# The Anatomy of the Transmission of Macroprudential Policies

Viral V. Acharya, Katharina Bergant, Matteo Crosignani, Tim Eisert, and Fergal McCann*

November 2021

*Viral V. Acharya is at NYU Stern, NBER and CEPR. Katharina Bergant is at the IMF. Matteo Crosignani is at the New York Fed. Tim Eisert is at Erasmus University Rotterdam and CEPR. Fergal McCann is at the Central Bank of Ireland. We thank Sergey Chernenko, Stijn Claessens, Anthony DeFusco, Adam Guren, Peter Koudijs, Tim Landvoigt, Philip Lane, David Moreno, Tomasz Piskorski, José-Luis Peydró, Andrea Polo, Andres Sarto, Roberto Savona, Tess Scharlemann, Sanjay Singh, Patrick Verwijmeren, James Vickery, and Wei Xiong for their comments. We also thank participants at the NBER Summer Institute (CF), WFA-CFAR Corporate Finance Conference (Olin), 11th Annual Paul Woolley Centre Conference, AFA, EFA, Labor and Finance Group Conference (Chicago Booth), CEPR-Bank of Finland Monetary Policy and Reality Conference, UCLA-FRBSF Conference on Housing, Financial Markets, and Monetary Policy, Workshop on Housing and Household Finance (Oslo), RFS Banking Conference (Rome), "Exploiting Microdata to Understand Real-Financial Linkages" Workshop (Central Bank of Chile), BIS "Impact of Banking Regulation on Financial Markets" Conference, CREDIT Conference, Bocconi Carefin Conference, MFA, CSEF Banking Conference, 14th Meeting of German Economist Abroad, Fed "Day Ahead" Conference, and seminar participants at Fed Board, NY Fed, Boston Fed, UNSW, University of Melbourne, Michigan Ross, Central Bank of Ireland Financial Stability Reading Group, ECB, FDIC, TU Munich, IWH Halle, Johns Hopkins Carey, and Frankfurt School of Finance & Management. We thank Ronan Lyons for helping us interpret the house price data. The views expressed in this paper are solely the responsibility of the authors and should not be interpreted as reflecting the views of the New York Fed, the Central Bank of Ireland, the International Monetary Fund, or anyone associated with these institutions. All results have been reviewed to ensure that no confidential information is disclosed. All authors declare that they have no relevant or material financial interest that relates to the research described in this paper. All errors are our own. This version: November 2021.

**ABSTRACT**

We analyze how regulatory constraints on household leverage—in the form of loan-to-income and loan-to-value limits—affect residential mortgage credit and house prices as well as other asset classes not directly targeted by the limits. Supervisory loan level data suggest that mortgage credit is reallocated from low- to high-income borrowers and from urban to rural counties. This reallocation weakens the feedback loop between credit and house prices and slows down house price growth in "hot" housing markets. Banks whose lending to households is more affected by the regulatory constraint drive this stabilizing reallocation; these same banks, however, substitute their risk-taking into holdings of securities and corporate credit.

# I. Introduction

Policymakers have recently proposed and implemented macroprudential policies aimed at limiting household leverage to slow down the feedback loop between credit and house prices. The academic literature—by showing that build-up of household leverage eventually leads to busts, lower output growth, and higher unemployment (Mian, Sufi, and Verner, 2017; Jorda, Schularick, and Taylor, 2016)—has highlighted the importance of these policies, adopted by more than 60 countries since 1990.

In this paper, we provide a comprehensive analysis of the most widely used type of macroprudential regulations, namely, policies that limit household leverage in the residential mortgage market, by imposing upper bounds on loan-to-value (LTV) and loan-to-income (LTI) ratios of household mortgages.[1]

Combining county-level house price data, loan-level data on residential mortgages and credit to firms, as well as bank security-level holdings, we study the 2015 introduction of loan-to-value and loan-to-income limits for residential mortgages issued by Irish banks. The policy was introduced in the aftermath of a dramatic boom-bust cycle that led to the financial crisis of 2007–08, which in turn forced the government to adopt a costly bailout (Acharya, Drechsler, and Schnabl, 2014).[2] The lending limits under the policy affect 43% of the typical mortgage origination and are immediately effective after the announcement. The goal, in the words of the then central bank governor Patrick Honahan, was to *"prevent another psychological loop between credit and prices and credit"* and *"keep banks and borrowers safe."*

We document—a somewhat counter-intuitive fact—that whereas the lending limits affect a large share of the residential mortgage market, mortgage issuance keeps growing after the policy introduction. We show that this is a result of the market moving to different borrowers and geographies to "conform" with the new limits. Our analysis of this reallocation provides three main findings. First, mortgage credit is reallocated from low- to high-

[1] Alam, Alter, Eiseman, Gelos, Kang, Narita, Nier, and Wang (2019) collected data from 1990 to 2016 on macroprudential policies in 134 countries. LTV and LTI limits have been adopted by 60 and 42 countries, respectively. LTV limits are the most widely used tool in advanced economies. See Cerutti, Claessens, and Laeven (2017) for another cross-country database.

[2] The household debt/GDP ratio increased from 55% to 101% from 2002 to 2007, followed by a GDP growth rate of −10% and an unemployment rate change of +8% over the next three years.

income borrowers and from counties where borrowers are close to the lending limits (typically urban areas) to counties where borrowers are more distant from the lending limits (typically rural areas). Second, in "hot" housing markets, this reallocation is effective in slowing down house price growth and short-circuiting the feedback loop between credit and house prices. This effect slows down the buildup of vulnerabilities caused by the rapid rise in mortgage credit—an often stated goal of lending limits. Third, this reallocation is consistent with a *bank portfolio choice* channel as banks more affected by the limits drive the mortgage credit reallocation that stabilizes house prices; however, these same banks also take more risk in their securities holdings and corporate credit, two asset classes not targeted by the policy. In addition to this important risk-shifting effect, survey evidence suggests that the policy might have affected savings rates, potentially affecting aggregate demand and real economic activity.

Specifically, we find that 43% of the mortgage issuance in the year before the policy would have been affected if the LTV and LTI limits had been in place during this period. Nevertheless, the increase in "conforming" issuance offsets the collapse in the issuance of those mortgages that exceed the newly imposed limits, leaving aggregate issuance barely affected. However, not every mortgage is affected in the same way. In the cross-section of counties, "hot" housing markets (typically urban counties) are closer to the limits than "cool" housing markets (typically rural counties). In the cross-section of borrowers, high-income borrowers are more distant from the limits than low-income borrowers. We show that, after the policy introduction, residential mortgage issuance moves from hot to cool housing markets and from low- to high-income borrowers.

The macroprudential policy might work through the mortgage credit channel in affecting house prices via both partial equilibrium and general equilibrium. For example, lower mortgage credit to hot housing markets might reduce house prices, reducing the households' borrowing capacity. The resulting lower local demand for mortgage credit might further reduce local house prices. In other words, the partial equilibrium effect of the policy might be amplified by this feedback loop. Following the methodology in Mian, Sarto, and Sufi (2019), we compare estimated coefficients with and without county-time fixed effects, which absorb the general equilibrium forces at the county level. This comparison suggests that the macroprudential policy weakened the sizable general equilibrium *"loop between credit and prices and credit"* at work before the policy introduction.

Next, we analyze the evolution of house prices and show that it is consistent with the observed geographic reallocation of mortgage credit. The average house price growth, around 14% year-on-year (YoY) and rapidly increasing at the time of the policy announcement, stabilized below 10% post regulation. This evolution is driven by hot housing markets. These markets had house price growth well above 20% and rapidly increasing at the time of announcement, but collapsing to around 4% post regulation. On the other hand, house price growth remained stable at the pre-policy level in cool housing markets. As a result, the lending limits substantially reduced the geographical heterogeneity in house price growth.

The evolution of house prices is also consistent with the reallocation of mortgage credit across the income distribution. We show that the differential evolution of house price growth in cool and hot housing markets is more pronounced for larger properties, more likely to be purchased by high-income borrowers. Consistent with these results, survey evidence suggests that households closer to the limits (young and low-income) increased their savings rates around the policy implementation.

We rationalize the collection of these findings via a *bank portfolio choice* channel of macroprudential policies. Consider an environment where banks solve their portfolio problem subject to a binding balance-sheet constraint (e.g., a regulatory capital constraint) and face an additional constraint, such as mortgage-lending limits.[3] As mortgage-lending limits are introduced, banks alter their portfolio choice. Specifically, banks reallocate their portfolio to fund some positive NPV projects that were not funded before the introduction of the mortgage-lending limits. The marginal profitability of projects determines which projects are funded after the introduction of the mortgage-lending limits.

Our sample banks likely rationed high-income and rural borrowers before the policy introduction and reallocated their mortgage credit from hot to cool housing markets and from low-income to high-income borrowers after the policy introduction. This evidence is consistent with a bank portfolio choice channel as, due to their past realized defaults, mortgages extended to high-income and rural borrowers have the highest predicted probability

[3]In our setting, residential mortgages are almost entirely issued by banks that hold them on balance sheet. Irish banks are also likely constrained at this time as they hold a large stock of non-performing loans and are adjusting their balance sheets to comply with new capital and liquidity regulations.

of default, making them more expensive from a regulatory and/or economic capital standpoint.

We further test the portfolio choice channel of macroprudential policy by exploiting banks' branch networks as a source of variation to capture banks' exposure to hot housing markets and, in turn, their exposure to the lending limits. In particular, for each bank $b$, we calculate the deviation of bank $b$'s average market share in hot housing markets from bank $b$'s average market share in the entire country—with market shares calculated using the number of branches. After showing that more-exposed banks drive the aggregate reallocation, we document that banks increased their risk taking in asset classes not targeted by the lending limits. In particular, we analyze banks' holdings of securities and credit to firms, which together with residential mortgages, capture 80% to 90% of banks' assets. More-exposed banks increase their holdings of high-yield securities more than less-exposed banks, relative to the pre-policy period, controlling for stringent security-time and bank-time fixed effects. Similarly, more-exposed banks increase their corporate lending (higher volumes and lower rates), targeting mostly risky borrowers.

Finally, Irish banks' equity returns are also affected by the introduction of the mortgage policy in a manner consistent with the portfolio choice channel. Pre-policy, bank equity returns are positively correlated with real estate returns but become insensitive to real estate and positively correlated with non-financial firms' equity returns after the policy introduction.

Our findings have important implications for the literature on macroprudential policies and their impact.[4] The rationale for macroprudential

[4]See Aikman, Bridges, Kashyap, and Siegert (2019), Freixas, Laeven, and Peydro

policies in this literature is based on the observation that agents over-borrow in good times, not internalizing all the costs of their financing choice (Lorenzoni, 2008; Bianchi, 2011; Bianchi and Mendoza, 2012, 2018). Empirically, the increase in mortgage credit in the United States (U.S.) contributed to the rapid appreciation of house prices (Favara and Imbs, 2015; Mian and Sufi, 2009, forthcoming; Adelino, Schoar, and Severino, 2020; Di Maggio and Kermani, 2017). The housing price collapse that followed worked its way through the balance sheet of households (Mian, Rao, and Sufi, 2013; Mian and Sufi, 2014; Hall, 2011; Eggertsson and Krugman, 2012; Jones, Midrigan, and Philippon, 2020) and intermediaries (Gertler and Kiyotaki, 2011; He and Krishnamurthy, 2013; Brunnermeier and Sannikov, 2014; Chodorow-Reich, 2014), contributing in turn to the Great Recession.

Our analysis contributes to this literature by explaining how macroprudential policies aimed at limited household leverage operate in practice. We do this by jointly analyzing mortgage credit, house prices, and their feedback loop, and showing that the portfolio reallocation of mortgage lenders plays an important role in the transmission of macroprudential policies. In particular, while house prices are overall stabilized by mortgage reallocation, banks shift their portfolios towards non-mortgage risky assets. Our paper is (less directly) related to the literature on macroprudential policies aimed at

(2015), Claessens, Ghosh, and Mihet (2013), Claessens (2015), and Gambacorta and Murcia (forthcoming) for excellent overviews of macroprudential policies.

limiting risk-taking, both empirical and theoretical.[5] [6]

A few other papers analyzing LTV/LTI limits imposed in countries other than Ireland also find results consistent with ours. By analyzing the bunching behavior around regulatory thresholds, DeFusco, Johnson, and Mondragon (2020) show how the Dodd-Frank "Ability-to-Repay" rule (similar to a LTI limit) successfully reduced borrower leverage, and Van Bekkum, Gabarro, Irani, and Peydro (2019) show LTV limits caused Dutch borrowers to increase their downpayments. Although they do not analyze the role of banks, Tzur-Ilan (2020) and Igan and Kang (2011) show borrowers move away from hot housing markets, slowing down house prices in Israel and Korea, respectively.[7]

Finally, analyzing the Irish macroprudential policy we study, Kinghan, McCarthy, and O'Toole (2019) show that LTV fell for first-time and

[5]Our paper is also related to the empirical literature on macroprudential policies (Aiyar, Calomiris, and Wieladek, 2014; Jimenez, Peydro, Ongena, and Saurina, 2017; Gropp, Mosk, Ongena, and Wix, 2019; Benetton, forthcoming; Benetton, Eckley, Garbarino, Kirwin, and Latsi, forthcoming; De Marco, Kneer, and Wieladek, forthcoming; Ayyagari, Beck, and Martinez Peria, 2019).

[6]The theoretical literature on macroprudential policies (Landvoigt and Begenau, forthcoming; Elenev, Landvoigt, and van Nieuwerburgh, 2021; Begenau, 2020; Kashyap, Tsomocos, and Vardoulakis, 2020; Malherbe and Bahaj, 2020) has mainly focused on policies (mostly capital requirements) aimed at limiting bank risk-taking.

[7]Auer and Ongena (2019) and Basten (2020) show that capital buffers on Swiss residential lending led to higher growth in commercial lending and shifted mortgages from less to more resilient banks, respectively. Using Singaporean data, Agarwal, Badarinza, and Qian (2019) show that policies that impose limits on LTV cause an increase in high-LTI issuance. These papers do not analyze house prices or banks' risk exposure.

subsequent-time buyers. Compared with their paper, we focus on house prices and document a reallocation of mortgage credit across the income and geographical distributions.

In summary, our analysis—to the best of our knowledge—represents the most comprehensive analysis of how macroprudential policies in mortgage credit have operated in practice in a country, highlighting the importance of how banks reallocate credit both across and away from mortgage markets.

The rest of the paper is structured as follows. Section II describes the data and the setting. Section III documents the mortgage credit reallocation. Section IV analyzes house prices. Section V presents the bank portfolio choice channel. Section VI discusses financial stability. Section VII discusses the macroeconomic effects. Section VIII concludes.

# II. Setting and Data

Section A provides some background on the Irish residential mortgage market. Section B and Section C describe the macroprudential policy and our data.

## *A. Residential Mortgage Credit in Ireland*

In the years leading up to 2000, Ireland experienced a period of steady economic growth often interpreted as a healthy convergence of the "Celtic Tiger" with the rest of the European Union. However, the surge in output from 2003 to 2007 was of a different type, fueled by a construction boom financed through bank credit (Honohan, 2010). In Figure 1, we show the

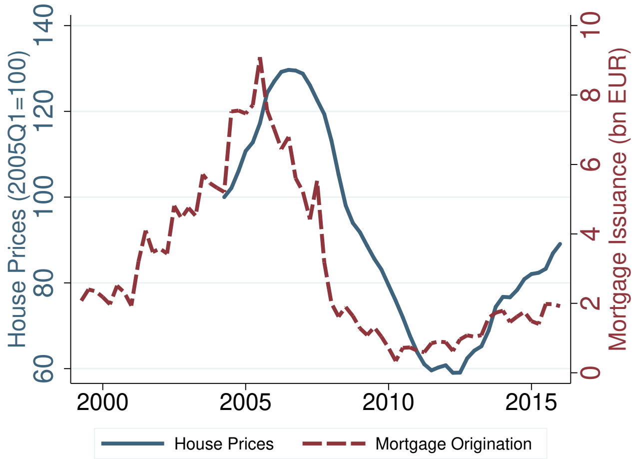


**Figure 1. Ireland Real Estate Boom-Bust.** This figure shows real estate prices from 2005Q1 to 2016Q4 (left axis) and residential mortgage issuance from 2000Q1 to 2016Q4 (right axis). Sources: Department of Housing, Planning and Local Government and Central Statistics Office.

issuance of residential mortgages (dashed line) from 2000 to 2016 and observe a stark increase from 2002 to 2007. Issuance then collapsed and started increasing again in 2013. House prices (solid line) followed a remarkably similar pattern.[8]

During the bust of 2007–10, prices declined sharply and construction activities collapsed. The fall in quarterly Gross National Product (GNP) is estimated to be about 17%.[9] In addition to the sharp decrease in real

[8]In the internet appendix, we show that the boom-bust cycle in the housing market has been more pronounced in Ireland compared with the U.K., the euro area, and the U.S. around the same period. After the bust, mortgage originations and house prices rebounded, mostly driven by "pent up" demand of households whose incomes were not affected by the crisis but avoided buying during the bust (the share of renters that became first-time buyers had fallen to 2% in 2012 and then rose steadily up towards 7–8%). This increased demand interacted with a weakened supply elasticity.

[9]The Irish economic performance is better measured with GNP because GDP is in-

estate prices, the increase in unemployment from 4.6% in 2007 to 13.3% in 2010 left many households unable to service their debt. This increase in non-performing mortgage credit led to losses for banks that consequently experienced funding dry-ups.[10] In September 2008, public funds had to be used to recapitalize almost all large domestic credit-taking institutions, which needed further government assistance in March 2011 (Lane, 2011; Acharya, Drechsler, and Schnabl, 2014).

## B. *The February 2015 Mortgage Lending Limits*

To avoid a recurrence of this boom-bust cycle, the central bank introduced new macroprudential rules. In the words of Patrick Honahan, at that time governor of the Central Bank of Ireland, *"What we are trying to prevent is another psychological loop between credit and prices and credit. If we avoid that, we can keep banks safe, we can keep borrowers safe."*

The idea of introducing lending limits was first discussed in October 2014 and announced and immediately implemented on February 9, 2015 (implementation date).[11] In Table I, we provide an overview of the limits on LTV

flated by profits of international companies transferred to Ireland because of low corporate tax.

[10]Almost all mortgages in Ireland are held on banks' balance sheets. No active securitization market exists. Securitization is solely used to create collateral eligible to be pledged at the ECB and in certain recent cases to transfer nonperforming exposures off banks' balance sheets. Risk transfer off banks' balance sheets is not common. Refinancing mortgages are not part of our sample. They account for less than 15% of total mortgage issuance during our sample period.

[11]The lending limits were first mentioned in a paper (Consultation Paper 87) published

| Regulation | Target Group | Limits | Allowances for each bank |
|---|---|---|---|
| **LTV limits** | For primary dwelling homes: | **First-Time Buyers**: Sliding LTV limits from 90%* **Subsequent Buyers**: 80% | 15% of new lending can be above limits |
| | For buy-to-let: | 70% LTV limit | 10% of new lending can be above limits the buy-to-let limit is allowed |
| **LTI limits** | For primary dwelling homes: | 3.5 times income | 20% of new lending above the limit is allowed |
| **Exemptions** | **From LTV limit** Borrowers in negative equity | **From LTI limit** Borrowers for investment properties | **From both limits** * Switcher mortgages * Restructuring of mortgages in arrears |

*A limit of 90% LTV applies to the first €220,000 of the value of a residential property and a limit of 80% LTV applies to any value of the property thereafter.

**Table I. Lending Limits.** This table shows a summary of the limits. Source: Central Bank of Ireland.

and LTI ratios on new originations of residential mortgages. The LTI limit is 3.5. The LTV limit depends on the type of borrowers.[12] Lending for primary-dwelling housing (PDH) is limited to 80% LTV. For first-time buyers (FTBs), a more generous LTV limit of 90% is imposed for houses up to €220,000. For any amount exceeding €220,000, the excess amount over €220,000 faces a 80% LTV limit. The measures impose a stricter threshold of 70% for buy-to-let (BTL) properties.[13]

on October 7, 2014 to stimulate discussion and available on the Central Bank of Ireland website (link). The limits were announced and implemented on February 9, 2015, with no limits being in place prior to this date. Mortgages issued after February 9, 2015 could exceed the lending limits if approved before February 9, 2015.

[12]Given that banks have to use an independent valuation, house prices are unlikely to be manipulated. Incomes are more prone to manipulation by loan officers and borrowers but this manipulation would work against us, making it more difficult to find a reallocation across counties and across the income distribution.

[13]First-time buyers are four percentage points or 30% less likely to default than subsequent-time buyers in Ireland (Kelly, O'Malley, and O'Toole, 2014). In addition to

## C. Data

The core of our final data set is the result of combining loan level information on residential mortgages and credit to firms, bank security level holdings, and county level house prices. The loan level data and security register are proprietary data sets obtained from the Central Bank of Ireland.

First, we observe loan level data on the issuance of residential mortgages at a daily frequency from January 2013 to June 2016. We observe all outstanding residential mortgages by the most significant institutions that have to submit loan level data to the Central Bank of Ireland.[14] This sample covers more than 90% of the domestic market and consists of the five largest banks. The data set also contains information on borrower income and demographics (e.g., age, marital status) and mortgage type (e.g., first-time buyer, buy-to-let).

Second, we observe loan level data on bank credit to firms at a semi-annual frequency from June 2013 to June 2016. At the bank-firm-period level, we observe credit granted and drawn and the rate charged by banks.

loans that are exempted from the rule, banks can issue loans exceeding the limits to a small share of borrowers (see last column of the table). In November 2016, the rules were relaxed, extending the LTV limit for FTBs to 90%. The analysis of this subsequent period goes beyond the scope of this paper.

[14] Irish banks that received a public bailout are required to report loan level data. The rest of the significant mortgage issuers in Ireland submit loan level data following the encouragement from regulators and in accordance with data submissions required by the ECB-SSM Comprehensive Assessment in 2013. More information is available in the internet appendix.

We match this information with firm characteristics such as the county of incorporation, industry, and size (very small/SME/large). We observe the borrower rating assigned to each loan from internal rating models of each lender. The Central Bank of Ireland internal mapping scales are used to classify each internal rating into a consistent categorization between 1 and 6. It ranges from 1 (highest-quality borrower) to 5 (very risky borrower) for non-defaulted loans and equals 6 for defaulted loans. The data have one main limitation. In contrast to most credit registries, our borrower identifier is consistent within a bank over time but not across banks.

Third, we observe bank security level holdings at a quarterly frequency from January 2011 to June 2016. At the security-bank-quarter level, we observe each security $s$ identified by an International Securities Identification Number (ISIN) held by bank $b$ at time $t$. We match this information with security characteristics (e.g., yield) from Datastream.

Fourth, at the bank-month level, we observe monthly balance sheet items from the European Central Bank Individual Balance Sheet Statistics (IBSI).

Fifth, at the county-period level, we observe quarterly house prices from the Irish property website Daft.ie. This data set is publicly available and regularly updated with quarterly reports published on the website. The statistics are based on properties advertised on the website for a given period. The average monthly sample size for sale properties is 5,000. Indices are quality adjusted, holding the mix of characteristics constant.

# III. Mortgage Credit Reallocation

In this section, we document four facts. In Section A, we show that the lending limits affect more than one third of the market but the originations of residential mortgages seem almost unaffected by these limits. In Section B, we show that borrowers are differentially exposed to the limits, with low-income borrowers and borrowers located in "hot" (mostly urban) housing markets being more affected than high-income borrowers and borrowers located in "cool" (mostly rural) housing markets. In Section C, we show that after the policy mortgage credit is reallocated from low- to high-income borrowers and from hot to cool housing markets. In Section D, we show that the policy was effective in weakening the feedback loop between mortgage credit and house prices.

## *A. Evolution of Residential Mortgage Issuance*

The lending limits affected a large fraction of the mortgage market as 43% of the volume of residential mortgage issuance (35% of the number of mortgages issued) from October 2013 to September 2014 would have been affected if the policy had been in place during that period. Out of the total €1.6 billion in mortgages in our sample in that period, non-conforming (i.e., not complying with the new rules) mortgages accounted for €0.7 billion. The LTV limits affected the largest fraction of the market. LTV-non-conforming mortgages accounted for €0.5 billion and LTI-non-conforming mortgages accounted for €0.3 billion. Moreover, approximately half of the

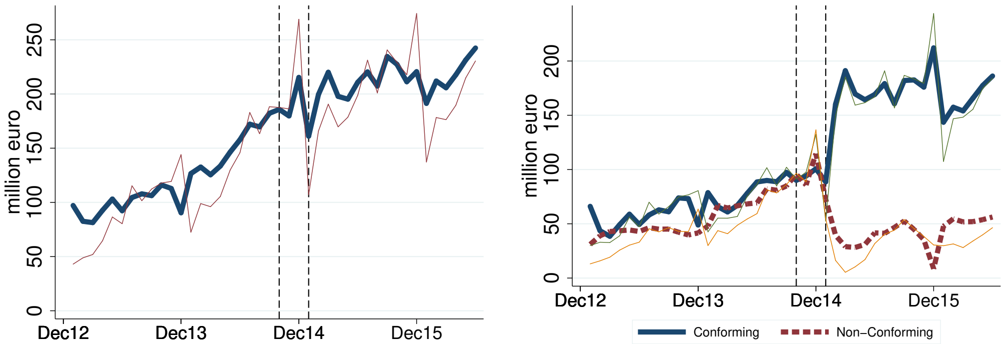


**Figure 2. Aggregate Residential Mortgage Issuance.** This figure shows the aggregate residential mortgage issuance of our sample banks from January 2013 to June 2016. The left panel shows total issuance. The right panel shows issuance of conforming (solid line) and non-conforming (dashed line) mortgages. Thick lines are seasonally adjusted and thin lines are not seasonally adjusted. The vertical dashed lines indicate the first rumors and the implementation of the lending limits. Source: Central Bank of Ireland.

LTI-non-conforming mortgages were also LTV-non-conforming.[15]

Whereas the lending limits affected more than one third of residential mortgage issuance, mortgage originations seem almost unaffected by the policy. In the left panel of Figure 2, we show the evolution of aggregate mortgage issuance from January 2013 to June 2016. We find that mortgage credit growth—high since the beginning of 2014—did not collapse after the implementation of the lending limits. This aggregate evidence suggests an increase in the issuance of *conforming* mortgages might have compensated the mechanical reduction of the issuance of *non-conforming* mortgages, as banks followed the new rules.[16] In the right panel, we show the evolution

[15] 2,797 LTV non-conforming mortgages and 1,467 LTI non-conforming mortgages were issued from October 2013 and September 2014. The limits are more binding for second and subsequent time buyers and buy-to-let buyers compared with first-time buyers.

[16] The non-conforming issuance is strictly positive after the policy implementation be-

of originations of conforming (solid line) and non-conforming (dashed line) mortgages and confirm the two time-series diverge starting in February 2015.

## B. *Exposure to the Lending Limits*

We now show that borrowers are differentially exposed to the lending limits: (i) low-income borrowers are more exposed than high-income borrowers and (ii) borrowers located in hot housing markets, mostly urban counties, are more exposed than borrowers located in cool housing markets, mostly rural counties.

In Table II, we show how borrowers across counties and across the income distribution differ along observable characteristics. We divide households who obtain a mortgage in the year prior to the policy in five quintiles based on their income (Q1 is the bottom quintile, Q5 is the top quintile) and in borrowers located in hot housing markets (counties that experienced a rapid house price appreciation before the policy, top panel) and cool housing markets (counties that experienced a more moderate house price appreciation before the policy, bottom panel). Across the income distribution, high-income borrowers tend to (i) have lower LTV and LTI, (ii) be older and more likely to be married, and (iii) less likely to be first-time buyers than lower-income borrowers. The table also shows that the income distribution is positively skewed as the average income of the top quintile is almost double the average income of the fourth income quintile. Somewhat mechanically, the distance

cause the new rules allow banks to exceed the limits for a limited fraction of their issuance. In the internet appendix, we show mortgage originations keep increasing even when weighted by LTV and LTI.

**Hot Housing Markets (Urban Counties)**

| | Unit | *Income Quintiles* Bottom | Q2 | Q3 | Q4 | Top |
|---|---|---|---|---|---|---|
| *Borrower Characteristics* | | | | | | |
| Income | € | 33,353 | 47,715 | 64,875 | 92,168 | 172,009 |
| Married | % | 16.6 | 34.3 | 44.7 | 64.2 | 68.4 |
| Age | Years | 34.2 | 35.6 | 35.7 | 37.4 | 40.0 |
| First-Time Buyer | % | 83.3 | 75.9 | 68.6 | 44.7 | 24.7 |
| Buy-to-Let | % | 1.1 | 2.6 | 2.3 | 3.8 | 7.3 |
| *Loan Characteristics* | | | | | | |
| Size | € | 108,821 | 141,815 | 191,243 | 237,807 | 280,653 |
| LTV | % | 77.2 | 76.7 | 79.0 | 76.9 | 72.1 |
| LTI | Units | 3.5 | 3.3 | 3.2 | 2.9 | 2.4 |
| House Value | € | 154,230 | 202,438 | 260,519 | 331,049 | 453,416 |
| Term | Months | 336.0 | 331.0 | 336.4 | 322.1 | 292.2 |
| Fixed Rate | % | 44.2 | 43.1 | 43.2 | 35.9 | 26.6 |
| Rate | % | 4.1 | 4.2 | 4.2 | 4.2 | 4.2 |

**Cool Housing Markets (Rural Counties)**

| | Unit | *Income Quintiles* Bottom | Q2 | Q3 | Q4 | Top |
|---|---|---|---|---|---|---|
| *Borrower Characteristics* | | | | | | |
| Income | € | 32,021 | 47,599 | 64,931 | 90,994 | 156,749 |
| Married | % | 16.5 | 38.2 | 54.9 | 71.5 | 58.3 |
| Age | Years | 34.2 | 34.9 | 37.0 | 38.4 | 40.1 |
| First-Time Buyer | % | 82.2 | 80.4 | 60.3 | 36.0 | 31.7 |
| Buy-to-Let | % | 2.3 | 2.0 | 4.5 | 5.2 | 10.6 |
| *Loan Characteristics* | | | | | | |
| Size | € | 83,256 | 111,588 | 138,590 | 173,037 | 145,027 |
| LTV | % | 77.3 | 77.8 | 76.9 | 75.9 | 70.6 |
| LTI | Units | 2.9 | 2.7 | 2.5 | 2.3 | 1.8 |
| House Value | € | 115,835 | 150,799 | 189,671 | 238,668 | 249,379 |
| Term | Months | 317.1 | 324.4 | 312.5 | 297.9 | 274.5 |
| Fixed Rate | % | 45.6 | 40.5 | 36.6 | 33.1 | 20.4 |
| Rate | % | 4.2 | 4.3 | 4.3 | 4.3 | 4.3 |

**Table II. Summary Statistics.** This table shows household and loan characteristics by household income quintile during the 12-month period before the first rumors about the policy (October 2013 to September 2014). Income quintiles are adjusted monthly for wage inflation. The top (bottom) panel shows the summary statistics for the subsample of counties with high (low) house price appreciation in the pre-policy period. Source: Central Bank of Ireland.

from the LTI limit increases monotonically with income. This monotonicity does not apply to the distance from the LTV limit as high-income borrowers tend to face stricter limits because they are often buy-to-let or second- or subsequent-time buyers. Across the distribution of counties, borrowers located in hot housing markets borrow more, have higher LTI, and purchase more expensive properties compared with borrowers located in cool housing markets.

To measure the distance of borrowers from the lending limits, we calculate what would have been the distance from the limits for each borrower in the year before the policy, assuming that the limits were in place during that period.[17] We calculate the mean of this borrower level distance at the income bucket-county level, where we group borrowers in 20 buckets (ventiles) based on the national income distribution. The thresholds of the income buckets are adjusted monthly for wage inflation using OECD data. In sum, we obtain a *Distance* variable that captures the exposure to the lending limits across the 26 counties and across the income distribution.

In Figure 3 and Figure 4, we show that the borrowers that are more exposed to the lending limits, in particular LTI limits, tend to have a low income and tend to be located in hot housing markets, namely counties, predominantly urban, that experienced a rapid house price appreciation before

[17]We proceed in three steps. First, we calculate the distance of each mortgage from its LTV and LTI limit during the 12 months before the first rumors about the limits. If the mortgage exceeds one limit, we set its distance equal to zero. Second, given the different scale of LTI and LTV, we rescale these two distances to have a mean of zero and a standard deviation of one. Third, we calculate the minimum of these two limits.

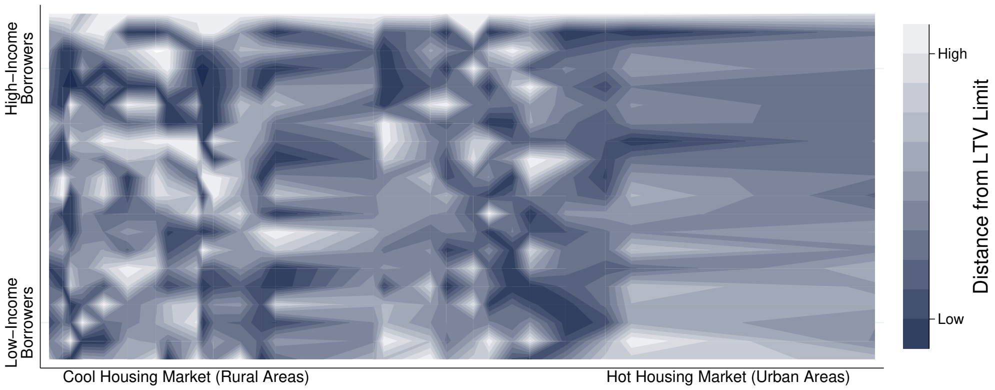


**Figure 3. Exposure to the LTV Lending Limits Across Counties and Incomes.** This figure shows the exposure to the LTV lending limit across counties and across the income distribution of borrowers. The x-axis shows counties ranked according to the house price appreciation pre-policy (from 2012Q4 to 2014Q3). The interval for each county on the x-axis is proportional to its population. The y-axis shows borrowers ranked according to their position in the income distribution (20 ventiles). Each point in the map indicates the distance from the LTV lending limits. Darker colors indicate a higher distance.

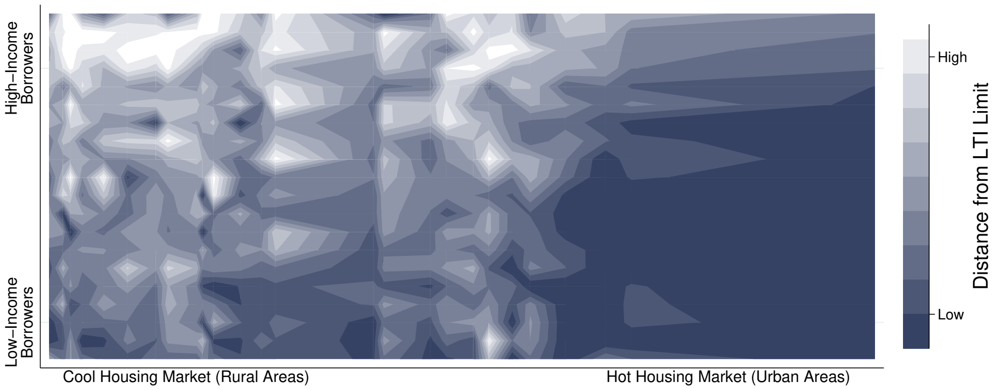


**Figure 4. Exposure to the LTI Lending Limits Across Counties and Incomes.** This figure shows the exposure to the LTI lending limit across counties and across the income distribution of borrowers. The x-axis shows counties ranked according to the house price appreciation pre-policy (from 2012Q4 to 2014Q3). The interval for each county on the x-axis is proportional to its population. The y-axis shows borrowers ranked according to their position in the income distribution (20 ventiles). Each point in the map indicates the distance from the LTI lending limits. Darker colors indicate a higher distance.

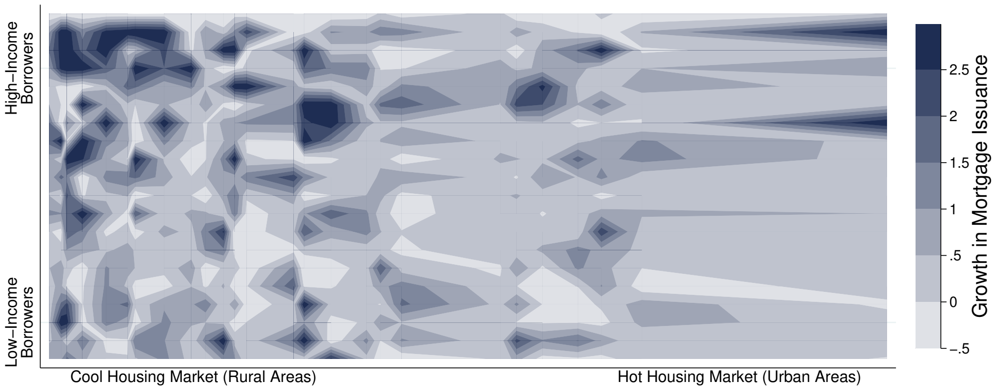


**Figure 5. Reallocation of Mortgage Credit.** This figure shows the reallocation of mortgage credit across counties and across the income distribution of borrowers. The x-axis shows counties ranked according to the house price appreciation pre-policy (from 2012Q4 to 2014Q3). The space taken by each county on the x-axis is proportional to its size (population). The y-axis shows borrowers ranked according to their position in the income distribution (20 ventiles). Each point in the map indicates the change of mortgage issuance in the post-period (February 2015 to January 2016) compared with the pre-period (February 2014 to January 2015). Darker colors indicate higher growth of issuance.

the policy. On the x-axes, counties are size weighted (larger counties take up a larger interval on the axis) and ordered based on their house price appreciation before the policy: cool housing markets on the left and hot housing markets on the right. On the y-axes, borrowers are grouped and ordered in 20 buckets based on their position in the income distribution: low-income borrowers on the bottom and high-income borrowers on the top. A point in these heatmaps is an income bucket-county pair and darker colors indicate a lower distance from the limits. Perhaps not surprisingly, we observe darker colors toward the bottom and the right of the LTI graph, suggesting that low-income borrowers and borrowers located in hot housing markets are closer (more exposed) to the LTI lending limit. This heterogeneity across counties is intuitive. Borrowers located in counties that experienced a rapid house price appreciation before the policy are more likely to borrow close to the to-be-imposed LTI limits. Interestingly, we do not find a similar pattern for the LTV limit.[18]

## *C. Reallocation of Residential Mortgage Credit*

We now document a mortgage credit reallocation from hot to cool housing markets and from low-income to high-income borrowers around the policy implementation.

Figure 5 is another heatmap where, for each income bucket-county pair, we show the change in mortgage origination from the pre-policy period (February 2014 to January 2015) to the post-policy period (February 2015 to January 2016). Darker colors indicate high credit growth. We observe darker colors on the left, toward the top, and especially in the top-left corner. In sum, this figure documents that the growth in issuance after the policy implementation has been driven by cool housing markets and high-income borrowers.[19]

In Figure 6, we confirm that this reallocation takes place around the implementation of the lending limits. The solid line corresponds to those

[18]In Figure 12 in the appendix, we show that the most exposed counties are located around the Dublin area. In the internet appendix, we show that more-exposed counties are more densely populated and have more residents compared with less-exposed counties.

[19]While there is anecdotal evidence of borrowers moving from Dublin to its commuting towns, the counties with the largest change in mortgage credit are too far from large cities for its residents to commute.

borrowers that are closer to the lending limits (lowest tercile of the distance distribution). The dashed red line indicates borrowers that are more distant from the lending limits (highest tercile of the distance distribution). As suggested by Table II, low-distance borrowers tend to be low-income borrowers and located in urban counties while high-distance borrowers tend to have a higher income and are predominantly located in rural counties. In the top panel, we observe that originations and loan size are very similar for the two groups of borrowers before the policy introduction. After the policy implementation, the evolution of originations and loan size flattens for low-distance borrowers while it keeps increasing for high-distance borrowers. In the bottom panel, we show that LTV and LTI, both higher for low-distance borrowers in the pre-policy period, tend to converge after the policy implementation.

We confirm this reallocation estimating the following specification:

$$Y_{cht} = \alpha + \beta Post_t \times Distance_{ch} + X_{cht} + \gamma_{ct} + \eta_{ch} + \mu_{ht} + \epsilon_{cht} \tag{1}$$

where $c$ is a county, $t$ is a month, and $h$ is a borrower income bucket, where we divide borrowers into 20 income buckets. The sample includes 24 months and runs from February 2014 to January 2016. The key independent variable is the interaction term between a *Post* dummy equal to one from February 2015 to January 2016 (12-month period after the policy implementation) and the (pre-policy) distance from the lending limits for each income bucket-county pair, as defined in the previous subsection. We include as controls, in the vector $X$, the share of originations to first-time

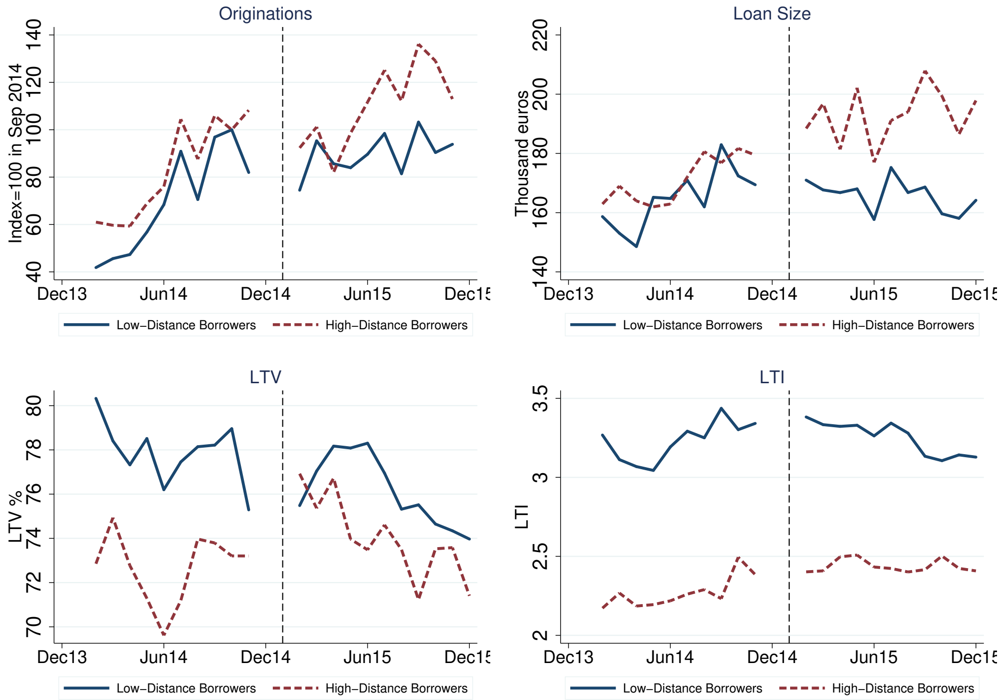


**Figure 6. Mortgage Credit Reallocation, Non-Parametric Evidence.** These figures show mortgage origination volume, loan size, loan to value, and loan to income from February 2014 to January 2016. The dashed (solid) lines indicate high-distance (low-distance) borrowers, namely the highest (lowest) tercile of the distance variable. Source: Central Bank of Ireland.

buyers and to buy-to-let investors. Finally, we saturate the specification with stringent fixed effects: county-time fixed effects capture county time-varying heterogeneity (e.g., county-specific demand for credit), income bucket-time fixed effects capture income bucket time-varying heterogeneity (e.g., income bucket-specific demand for credit), and county-income bucket fixed effects to capture time-invariant borrower and geographic characteristics. We double cluster standard errors at the county and income bucket level. This choice takes into account that the model errors are likely correlated within a county and within an income bucket as borrowers that are more exposed to the lending limits tend to have a low income and tend to be located in hot housing markets. We assume error independence across clusters. Formally, $E[u_i, u_j|\mathbf{x}_i, \mathbf{x}_j] = 0$, unless observations $i$ and $j$ share any cluster dimension (Petersen, 2009; Cameron, Gelbach, and Miller, 2011).

We show the estimation results in Table III. In the first three columns, the dependent variables are the logarithm of mortgage issuance, the average loan size, and the number of mortgages issued. We find that a one standard deviation increase in the distance from the LTI limits is associated with a 10.4% higher issuance and a 10.5% larger loan relative to the pre-period. We do not find a statistically significant effect on the number of mortgages. These estimation results confirm the reallocation of mortgage credit from low- to high-income borrowers and from low- to high-distance counties documented in the heatmap. In Table III, we also show that, while the LTV limits affect the largest fraction of the market, the reallocation is mostly driven by the LTI limits. This result is in line with our narrative of the reallocation as borrowers closer to the LTI limit tend to be disproportionately located in urban areas and tend to have a low-income, as shown in Figure 4.

### *D. General Equilibrium Multipliers*

In Table III, we show that a higher distance from the limits is associated with an increase in mortgage issuance after the policy introduction, capturing the partial equilibrium effect of the policy on mortgage issuance. We now estimate the general equilibrium effect and relate it to our partial equilibrium estimates.

We employ the methodology of Sarto (2018) and Mian, Sarto, and Sufi

| | Volume | Loan Size | No. Loans | LTV | LTI |
|---|---|---|---|---|---|
| LTI Distance × Post | 0.186** | 0.185*** | -0.571 | -0.790 | 0.408*** |
| | (0.071) | (0.054) | (0.521) | (1.545) | (0.068) |
| LTV Distance × Post | 0.094 | 0.080* | 0.490 | 11.285*** | 0.039 |
| | (0.066) | (0.046) | (0.446) | (1.661) | (0.052) |
| Fixed Effects | | | | | |
| County-Time | ✓ | ✓ | ✓ | ✓ | ✓ |
| Bucket-Time | ✓ | ✓ | ✓ | ✓ | ✓ |
| County-Bucket | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 7,188 | 7,188 | 7,191 | 7,100 | 7,045 |
| R-squared | 0.755 | 0.602 | 0.892 | 0.363 | 0.592 |

| | Volume | Loan Size | No. Loans | LTV | LTI |
|---|---|---|---|---|---|
| LTV Distance × Post | 0.186*** | 0.171*** | 0.240 | 11.021*** | 0.235*** |
| | (0.062) | (0.042) | (0.255) | (1.067) | (0.055) |
| Fixed Effects | | | | | |
| County-Time | ✓ | ✓ | ✓ | ✓ | ✓ |
| Bucket-Time | ✓ | ✓ | ✓ | ✓ | ✓ |
| County-Bucket | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 7,199 | 7,199 | 7,202 | 7,110 | 7,047 |
| R-squared | 0.754 | 0.602 | 0.892 | 0.369 | 0.588 |

| | Volume | Loan Size | No. Loans | LTV | LTI |
|---|---|---|---|---|---|
| LTI Distance × Post | 0.224*** | 0.218*** | -0.383 | 3.420** | 0.423*** |
| | (0.074) | (0.055) | (0.382) | (1.507) | (0.064) |
| Fixed Effects | | | | | |
| County-Time | ✓ | ✓ | ✓ | ✓ | ✓ |
| Bucket-Time | ✓ | ✓ | ✓ | ✓ | ✓ |
| County-Bucket | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 7,192 | 7,192 | 7,195 | 7,102 | 7,049 |
| R-squared | 0.754 | 0.602 | 0.892 | 0.356 | 0.593 |

**Table III. Reallocation of Mortgage Credit, Parametric Evidence, LTI Vs. LTV Limits.** This table shows estimation results from specification (1). The dependent variable is the logarithm of total mortgage volume, the logarithm of the average loan size, the number of loans issued, the value-weighted LTV, and the value-weighted LTI. LTI Distance and LTV Distance are the distances from the LTI lending limit and the LTV lending limit, at the county-income bucket level described in Section B, respectively. All regressions include the share of originations to first-time buyers and to buy-to-let investors at the county-time-income bucket level, county-time fixed effects, income bucket-time fixed effects, and income bucket-county fixed effects. Standard errors are double clustered at the county and income bucket level. Source: Central Bank of Ireland.

(2019). The intuition behind this approach is as follows. The estimated coefficient in (1) captures the partial equilibrium effect of the policy. The county-time fixed effects absorb the general equilibrium effect at the county level. For example, lower mortgage credit to hot housing markets might reduce house prices, reducing the households' borrowing capacity. The resulting lower local demand for mortgage credit might further reduce local house prices. The partial equilibrium effect of the policy might be amplified by this feedback loop. By collapsing our specification at the county-month level, the coefficients capture the general equilibrium effect of the policy. By comparing these estimates with the estimates in (1), we decompose the general equilibrium effect in its partial equilibrium component and a "general equilibrium multiplier."

More specifically, we estimate the following specifications:

$$\begin{aligned} Volume_{cht} &= \alpha + \beta_1^{PE} Distance_{ch} + \beta_2^{PE} Post_t \times Distance_{ch} + X_{cht} \\ &\quad + \gamma_{ct} + \eta_{ht} + \epsilon_{cht} \qquad (2) \\ Volume_{ct} &= \alpha + \beta_1^{GE} Distance_{c} + \beta_2^{GE} Post_t \times Distance_{c} + X_{ct} \\ &\quad + \gamma_{t} + \epsilon_{ct} \qquad (3) \end{aligned}$$

The unit of observation in the familiar first specification is county-month-income bucket. The unit of observation in the second specification is county-month. The superscripts $PE$ and $GE$ refer to partial and general equilibrium, respectively. The county-time fixed effects in the first specification capture the general equilibrium forces at work at the county level. Hence, by comparing the estimated coefficients in the two specifications, we can decompose

**Panel A**—Unit of obs: income bucket-county-time

| LHS: Credit | Full Sample | Hot Housing Markets | Cool Housing Markets |
|---|---|---|---|
| Distance × Post | 0.247*** | 0.206* | 0.301*** |
| | (0.060) | (0.105) | (0.074) |
| Distance | -0.230*** | -0.231** | -0.226*** |
| | (0.042) | (0.076) | (0.060) |
| Fixed Effects | | | |
| County-Time | ✓ | ✓ | ✓ |
| Bucket-Time | ✓ | ✓ | ✓ |
| Observations | 7,203 | 4,237 | 2,966 |
| R-squared | 0.701 | 0.755 | 0.433 |

**Panel B**—Unit of obs: county-time

| LHS: Credit | Full Sample | Hot Housing Markets | Cool Housing Markets |
|---|---|---|---|
| Distance × Post | 0.323 | 0.581* | 0.581* |
| | (0.193) | (0.295) | (0.301) |
| Distance | -4.182*** | -4.709*** | -1.368 |
| | (1.127) | (1.338) | (1.793) |
| Fixed Effects | | | |
| Time | ✓ | ✓ | ✓ |
| Observations | 622 | 311 | 311 |
| R-squared | 0.392 | 0.399 | 0.198 |

**Table IV. Partial and General Equilibrium Effects.** This table presents the estimation results from (2) in Panel A and (3) in Panel B. The dependent variable is the logarithm of total mortgage volume. *Distance* is the distance from the limits (mean at the relevant unit of observation). The share of originations to first-time buyers and to buy-to-let investors (mean at the relevant unit of observation) are included as controls. The standard errors are double clustered at the county and income bucket level in Panel A and clustered at the county level in Panel B. Source: Central Bank of Ireland.

the general equilibrium effect in (i) its partial equilibrium component and (ii) its general equilibrium multiplier. More specifically, by comparing $\widehat{\beta_1^{GE}}$ and $\widehat{\beta_1^{PE}}$, we obtain the general equilibrium multiplier before the policy was introduced. Similarly, by comparing $\widehat{\beta_2^{GE}}$ and $\widehat{\beta_2^{PE}}$, we obtain the general equilibrium multiplier after the policy introduction.

Table IV shows the estimated coefficients. In the pre-policy period, the general equilibrium effect is large ($-4.18$), driven by hot housing markets. This effect can be decomposed in its partial equilibrium component of 6% ($-0.23/-4.18$) and its general equilibrium multiplier of 94%. In the post-policy period, the multiplier drops to 23% and the general equilibrium effect is much smaller (0.32), and identical in hot and cool housing markets.

The large general equilibrium effect during the boom and the sudden reversal after the policy are consistent with the theory of feedback loops between credit and house prices. In the next section, we show that house price growth was rapidly increasing, especially in hot housing markets, before the policy, but then stabilized after the policy. As in Kiyotaki and Moore (1997), higher mortgage credit leads to higher housing demand and higher house prices. Higher house prices have a larger effect on borrowers' net worth than on down payments and, therefore, relax the borrowers' collateral constraint, allowing them to obtain a larger mortgage. In the context of the 2002–05 boom in the U.S., Mian, Sarto, and Sufi (2019) find a 80% general equilibrium multiplier. Our larger multiplier during the boom is consistent with the Irish setting, prone to extreme boom-bust episodes like the 2002–10 one.

Short-circuiting the feedback loop between credit and house prices was one of the goals of the policy. In the words of governor Honahan: "*What we're trying to do is prevent that psychological loop between credit and prices and credit. If we can avoid that we can keep banks safe, we can keep borrowers safe.*" While likely intended, the drop in the general equilibrium effect in the post-policy period cannot solely be attributed to the lending limits.

In the next section, we discuss how the announcement of the lending limits likely signaled that policymakers were concerned by the rapidly appreciating housing market, affecting, in turn, agents' expectations. Consistent with an active role of policymakers in the housing market in this period, the government adopted eight policies, mostly tackling housing affordability, between October 2013 and October 2016 (see internet appendix). These policies do not constitute an identification concern as most of them were effectively help-to-buy schemes and our effects (i) kick in exactly around the implementation of the lending limits (e.g., Figure 2 and Figure 9) and (ii) are theoretically motivated (reallocation across counties and across the income distribution) by the bank portfolio choice channel presented in Section A. Moreover, the survey data presented in the internet appendix suggests that agents anticipated, at the time of the first rumors, a decline in house prices mainly because of the soon-to-be announced limits.

# IV. House Prices

In this section, we show that the time-series evolution of house prices is consistent with the mortgage credit reallocation documented in the previous section.

First, we show non-parametric evidence. In the left panel of Figure 7, we show yearly growth in house prices from January 2011 to June 2017. House price growth stopped increasing at the time of the first rumors about the policy and then stabilized around 10% after the implementation. In the right panel, we plot house price growth for high-distance or cool counties (solid line) and low-distance or hot (dashed line) counties. Low-distance counties experienced a stark contraction of house price growth after the policy implementation, whereas house price growth remained stable at the pre-policy level in high-distance counties.[20] In the internet appendix, we show the slowdown in house price growth in low-distance counties is driven by small properties, and the relative stability of house price growth in high-distance counties is driven by large properties. This evidence is consistent with the documented credit reallocation across counties and, to the extent that property size is correlated with the income of the buyers, with the reallocation across the distribution of borrowers' income.[21]

The time series evidence above shows that the increase in house price appreciation stopped at the time of the rumors of the lending limits, before the actual policy implementation. In the internet appendix, we show that

[20]The housing supply is more elastic in high-distance than low-distance counties, potentially explaining why house price growth did not increase in high-distance counties after the policy. Granted planning permissions did not change in low-distance counties (66% in 2012Q4-2014Q4; 69% in 2014Q4-2016Q4) but substantially increased in high-distance counties (-2% in 2012Q4-2014Q4; 81% in 2014Q4-2016Q4).

[21]See the internet appendix. Table II shows that borrower income is strongly correlated with the price of the property purchased. In the internet appendix, we attempt to map the number of bedrooms to the income of buyers by regressing the price of the residential property collateralizing the residential mortgage (credit registry data) on property size-county level house price data. We find that these loadings are consistent with high-income (low-income) borrowers predominantly buying large (small) properties. Of course, this mapping is not perfect, because, for example, high-income borrowers might buy a one-bedroom property to rent it out.

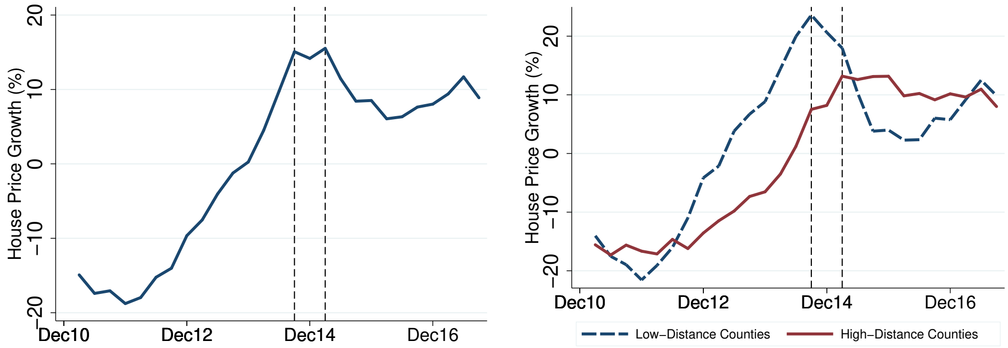


**Figure 7. House Price Changes.** The top panel of this figure shows the evolution of yearly house price growth. The bottom panel shows the evolution of yearly house price growth for high-distance and low-distance counties separately (groups split by median value). The vertical dashed lines indicate the first rumors about the limits and their implementation date. The sample period runs from January 2011 to June 2017. Source: Central Bank of Ireland, Daft.ie.

the first rumors about the lending limits caused households to revise their expectations about house prices downward, especially in hot housing markets. The survey data presented in the internet appendix show that this revision of expectations was mainly attributed to the introduction of the lending limits. That being said, the lending limits likely affected households' expectations also by signaling that policymakers were concerned about housing markets (especially in urban areas), consistent with the active role of the government in the housing market during our sample period (see internet appendix).

Second, we show parametric evidence consistent with the mortgage credit reallocation across counties and across the income distribution. In particular, we estimate the following specifications at the county ($c$) level and at the

| LHS: ΔHP | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| *Distance* × Size | | 0.006** | 0.006** | 0.006** | 0.006** |
| | | (0.002) | (0.002) | (0.002) | (0.002) |
| *Distance* | 0.272*** | 0.255*** | | 0.255** | |
| | (0.058) | (0.062) | | (0.062) | |
| *Size* | | 0.014*** | 0.014*** | | |
| | | (0.001) | (0.001) | | |
| Fixed Effects | | | | | |
| Country | | | ✓ | | ✓ |
| Size | | | | ✓ | ✓ |
| Observations | 54 | 270 | 270 | 270 | 270 |
| R-squared | 0.398 | 0.307 | 0.736 | 0.495 | 0.924 |

**Table V. House Prices and Lending Limits.** This table shows estimation results from specification (4) in column (1) and specification (5) in columns (2)-(5). The dependent variable is the change in house prices between 2014Q3 and 2016Q4. *Distance* is the county level distance from the lending limits. *Size* is the number of bedrooms (1 to 5). Standard errors clustered at the county level in parentheses. Source: Central Bank of Ireland, Daft.ie.

county-property type $(c, p)$ level:

$$\Delta HP_c = \alpha + \beta Distance_c + \epsilon_c \tag{4}$$

$$\Delta HP_{cp} = \alpha + \beta_1 Distance_c \times Size_p + \beta_2 Distance_c + \beta_3 Size_p + \epsilon_{cp} \tag{5}$$

where the dependent variable is the change in house prices from 2014Q3 to 2016Q4, *Distance* is the county level (pre-policy) distance from the lending limits, and *Size* is an integer equal to the number of bedrooms.[22] We interact

[22]The geographical breakdown of the house price data is more granular than the mortgage level data as we observe house price data for each of the 22 Dublin postal districts. Given that we cannot compute the distance from the lending limits at this more granular level, we assume the distance is constant within a county. We then cluster our standard errors at the county level to take into account that standard errors might be correlated

*Distance* with the measure of property size to check whether the effect of the lending limits changes depending on the type of property. We show the estimation results in Table V, where we cluster standard errors at the county level. This choice of clustering takes into account that model errors are likely correlated within a county while assuming error independence across counties.

The county level estimation in column (1) confirms the positive correlation between changes in house price growth after the policy and county level distance from the limits. In particular, a one standard deviation increase in the county-level distance is associated with a 4.2% change in house prices. In columns (2)-(5), we show the county-property size level estimation. We confirm that house price growth increased more in high-distance counties than in low-distance counties, and this different evolution is more pronounced for larger properties. These results are consistent with the documented reallocation of mortgage credit across counties and, to the extent that property size is correlated with the income of the buyers, across the income distribution.

Third, we show that, following the introduction of the lending limits, the geographical distribution of house price growth became less fat-tailed. The left and right panels of Figure 8 show the distribution of house price growth across counties before and after the policy, respectively. The distribution in the post-period is substantially less fat-tailed (standard deviation from 0.11 to 0.05), suggesting that the limits reduced the heterogeneity in house price growth. In the internet appendix, we show that the tails were not shrinking

within counties.

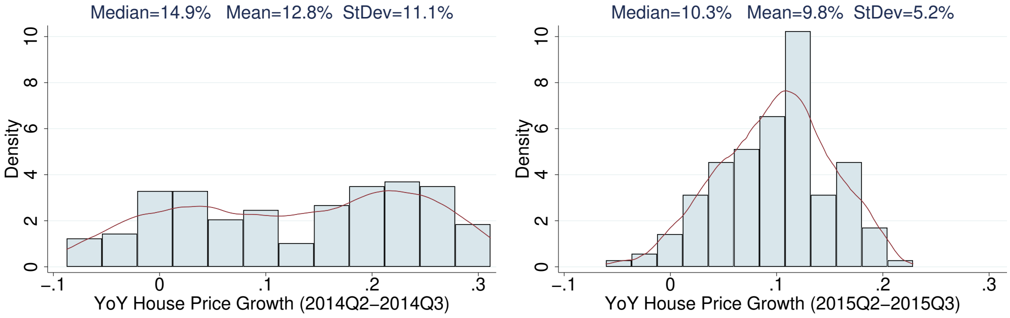


**Figure 8. Distribution of House Price Growth.** This figure shows the change in the distribution of house price growth. The left and right graphs show the density of YoY house price growth before (2014Q2-2014Q3) and after (2015Q2-2015Q3) the policy, respectively. Source: Daft.ie.

before the policy.

Our parametric and non-parametric results suggest that the lending limits contributed to reducing the yearly house price growth from around 15% to around 8%. In the context of Korea, Igan and Kang (2011) find that macroprudential regulation reduced house price growth by a monthly rate of 0.5%, a magnitude in line with the evidence in Figure 7. In the context of Israel, Laufer and Tzur-Ilan (2020) find that macroprudential regulation had a smaller effect, reducing prices for the most expensive properties by around 1% and for the market as a whole by 0.6%. Consistent with our findings, both these studies find that the effects are driven by metropolitan areas. Taken together with the general equilibrium estimates in Section D, our results also suggest that the housing market became more homogeneous across counties and less prone to feedback loops around the policy implementation. As discussed before, these changes are likely in part due to the lending limits and in part due to the changes in agents' expectations.

# V. Bank Credit Reallocation

In this section, we show that the reallocation of mortgage credit from hot to cool housing markets and from low-income to high-income borrowers is consistent with a "bank portfolio choice" channel. We present the bank portfolio choice channel in Section A. Consistent with this channel, in Section B and in Section C, we show that (i) the mortgage credit reallocation is driven by banks more exposed to the policy and (ii) banks, especially those more exposed to the policy, increase their risk exposure in asset classes not targeted by the limits, namely holdings of securities and credit to firms. In Section D, we show how the aggregate risk exposure of banks changed around the implementation of the policy.

## A. *Bank Portfolio Choice Channel*

According to the bank portfolio choice channel, the lending limits cause banks to reallocate their portfolio to fund projects that would have not otherwise been funded.[23] Consider an environment where banks solve their portfolio problem subject to a binding balance sheet constraint (e.g., regulatory capital constraint). Introduce in this setting, where banks cannot fund all positive NPV projects, a new constraint that prevents banks from investing in a specific asset (e.g., high-leverage mortgages). If this new constraint is also binding, banks reallocate their portfolio to fund some positive NPV

[23] In the internet appendix, we provide a formal characterization of banks' portfolio problem.

projects that were not funded before the introduction of the new constraint.[24]

This bank portfolio choice channel critically relies on the financial sector being constrained. In our empirical setting, Irish banks are adjusting to new capital and liquidity regulatory requirements and hold a large stock of non-performing loans. Moreover, there is no public support for the residential mortgage market and mortgages are almost entirely issued by banks that hold them on balance sheet.[25] In the context of the U.S., DeFusco, Johnson, and Mondragon (2020) find that debt-to-income limits have a sizable effect on quantities, a result that they attribute to a deterioration of the agency conflict between mortgage originators and mortgage investors. This friction is likely not at work in our context, where the mortgage market is structured in an originate-to-hold model. Note that, with the notable exception of the U.S., many countries around the world only have a small securitization market and do not provide a public support for the mortgage market.

The bank portfolio choice channel explains, in our context, why credit is reallocated from hot to cool housing markets and from low-income to high-income borrowers. The marginal profitability of mortgages is determined by mortgage rates and by the capital that banks need to set aside to fulfill their

[24]See Goel, Lewrick, and Tarashev (2020) for a model of how banks allocate capital across their business units when facing multiple constraints, including various regulatory ones. More generally, our results are consistent with a model where bank capital is "scarce" and the marginal equity issuance costs are very steep. See Harris, Opp, and Opp (2020) for a theoretical framework.

[25]Non-performing loans are about 25% of the value of outstanding loans at the time of the policy. Irish banks returned to profitability in 2014 for the first time since 2008. See Central Bank of Ireland (2014) for more details.

regulatory requirements. In our setting, the marginal profitability of mortgages is mostly determined by their capital requirements as banks are (i) likely capital constrained (see discussion above) and (ii) mortgage rates are relatively homogeneous (see Table II). The capital requirement on a mortgage loan is strongly influenced by the probability of default that banks predict using past data.[26] In Appendix A, we show that, due to their past realized defaults, mortgages extended to high-income and rural borrowers have the highest predicted probability of default, making them more expensive from a capital standpoint. Taken together, this evidence suggests that, consistent with the bank portfolio choice channel, our sample banks likely (i) rationed high-income and rural borrowers before the policy introduction and (ii) reallocated their mortgage credit from hot to cool housing markets and from low-income to high-income borrowers after the policy introduction.

## B. *Mortgage Credit Reallocation*

The bank portfolio choice channel has a clear cross-sectional prediction: Banks with a larger issuance of non-conforming mortgages in the pre-policy

[26] 90% of mortgages in Ireland fall under the Basel II IRB approach, which estimates capital requirements as follows: $CapitalRequirement = LGD \times N[(1-R)^{-0.5} \times G(PD) + (R/(1-R))^{0.5} \times G(0.999)] - PD \times LGD$, where $R$ is the correlation among mortgages (set at 0.15 for all banks), $EAD$ is the exposure at default, $LGD$ is the loss given default, $N$ is the standard normal distribution, and $G$ is the inverse of the standard normal distribution. While the $LGD$ and the $EAD$ largely depend on the size of the loan and the value of the collateral, with the $LGD$ additionally depending on country-specific factors (e.g., right of recourse, regulatory LGD floor), the $PDs$ are estimated using several borrower level characteristics obtained from historical data.

period drive the mortgage credit reallocation compared with banks with a larger issuance of conforming mortgages in the pre-policy period. We test this prediction using the geographical distribution of banks' branch network in 2014, before the policy implementation.[27] While all banks are nationally active, they differ in their geographical presence. The idea behind using banks' geographical network as a source of variation is to capture banks' exposure to hot housing markets and, in turn, their exposure to the lending limits. In particular, for each bank $b$, we define the variable $Exposure_b$ as the deviation of bank $b$'s average market share in hot housing markets from bank $b$'s average county-level market share in the entire country (with market shares calculated using the number of branches).[28]

We validate our measure in Figure 9, where we show the evolution of non-conforming mortgages (left panel) and conforming mortgages (right panel) issued by high-exposure banks (solid line) and low-exposure banks (dashed line). The figure shows that (i) non-conforming mortgage issuance, mechanically higher for high-exposure banks before the policy, collapses for both groups of banks after the policy implementation and (ii) conforming mortgage issuance increases sharply after the policy implementation, especially for high-exposure banks.

[27]The 2014 bank branch network is not available. Hence, we manually reconstruct it using the most recent branch network available at https://sortcode.bpfi.ie/Branches/DownloadAllCaptcha and news coverage of branch closures and openings since 2014. See the internet appendix for details.

[28]In the internet appendix, we show banks' summary statistics for high-exposure and low-exposure banks.

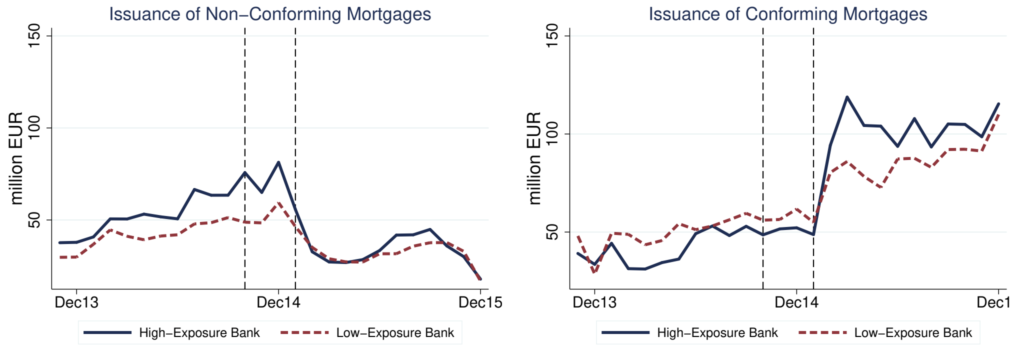


**Figure 9. Issuance and Bank Exposure.** The figure shows the issuance of non-conforming (left panel) and conforming (right panel) mortgages for high-exposure (solid line) and low-exposure (dashed line) banks. The vertical lines indicate the first rumors and the implementation date of the limits. All time series are seasonally adjusted. Source: Central Bank of Ireland.

Having shown non-parametric evidence of cross-sectional variation in bank mortgage credit reallocation, we estimate a triple difference-in-differences specification, obtained by adding the bank exposure variable to specification (1):

$$
\begin{aligned}
Y_{bcht} = \alpha &+ \beta_1 Post_t \times Distance_{ch} \times Exposure_b \\
&+ \beta_2 Distance_{ch} \times Exposure_b \\
&+ \beta_3 Post_t \times Distance_{ch} + \eta_{cht} + \mu_{bt} + X_{cht} + \epsilon_{bcht}
\end{aligned} \tag{6}
$$

where the unit of observation is bank $b$, county $c$, borrower income bucket $h$, and month $t$. Again, we divide borrowers into 20 income buckets and our sample period runs from February 2014 to January 2016. We saturate the specification with stringent fixed effects. We add bank-time fixed effects to ensure that our results are not driven by the non-random nature of bank exposure to the policy (e.g., larger banks being more exposed to the limits and changing their lending decision after the policy). We add county-time-income bucket fixed effects to ensure that our results are not driven by time-varying borrower heterogeneity (e.g., high-income borrowers in Dublin demanding less mortgage credit after the policy). We double cluster standard errors at the county and income bucket level to take into account the model errors are likely correlated within a county and within an income bucket. As in our analysis at the income bucket-county level, we assume $E[u_i, u_j|\mathbf{x}_i, \mathbf{x}_j] = 0$, unless observations $i$ and $j$ share any cluster dimension (Petersen, 2009; Cameron, Gelbach, and Miller, 2011)

We present the estimation results in Table VI. The independent variables are issuance volume, loan size, number of mortgage loans, LTV, LTI, and rate. Panel A includes county-time, county-bucket, and bank-time fixed effects. Panel B includes the more stringent county-bucket-time fixed effects. In the internet appendix, we show that the results are robust to the alternative two-way clustering at the county-bank and income bucket level. The positive coefficient $\beta_1$ shows the credit reallocation documented in Section III is primarily driven by banks more exposed to the limits. Given our definition of bank exposure, the opposite signs of $\beta_1$ and $\beta_2$ suggest that banks with a larger branch presence in hot housing markets were unable to keep the same type of origination in these markets, inducing them to move their origination in cool housing markets, such as more rural areas. Finally, although rates are falling for all borrowers during our sample period, the last column shows that more-exposed banks reduced mortgage rates more than less-exposed banks.[29]

[29]Irish banks do not offer mortgage rates based on the income of borrowers. Banks typically offer an interest rate-LTV schedule, allowing borrowers to self-select into products.

| Panel A | | Volume | Size | No. Loans | LTV | LTI | Rate |
|---|---|---|---|---|---|---|---|
| Distance × Exposure × Post | ($\beta_1$) | 9.055*** | 14.206** | -27.266 | 325.225** | 2.770 | -3.483* |
| | | (1.031) | (6.708) | (20.061) | (151.244) | (3.056) | (1.744) |
| Distance × Exposure | ($\beta_2$) | -13.071*** | -15.777* | 15.141 | -367.510** | -3.574 | 4.484*** |
| | | (3.252) | (8.228) | (20.849) | (143.516) | (2.747) | (1.567) |
| Distance × Post | ($\beta_3$) | 0.150*** | 0.286*** | -0.577 | 10.262*** | 0.190*** | 0.021 |
| | | (0.033) | (0.091) | (0.392) | (0.735) | (0.052) | (0.014) |
| Fixed Effects | | | | | | | |
| County-Time | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| County-Bucket | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Bank-Time | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | | 12,960 | 12,960 | 12,964 | 12,757 | 12,708 | 12,577 |
| R-squared | | 0.603 | 0.477 | 0.518 | 0.223 | 0.451 | 0.543 |

| Panel B | | Volume | Size | No. Loans | LTV | LTI | Rate |
|---|---|---|---|---|---|---|---|
| Distance × Exposure × Post | ($\beta_1$) | 11.596*** | 19.219** | -52.616 | 273.233** | 3.652 | -3.156 |
| | | (0.706) | (8.594) | (31.117) | (130.195) | (3.643) | (2.245) |
| Distance × Exposure | ($\beta_2$) | -15.861*** | -20.073* | 32.335 | -375.119** | -4.942* | 5.200* |
| | | (3.279) | (9.642) | (26.676) | (133.923) | (2.710) | (2.622) |
| Fixed Effects | | | | | | | |
| Bank-Time | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| County-Bucket-Time | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | | 12,960 | 12,960 | 12,964 | 12,757 | 12,708 | 12,577 |
| R-squared | | 0.796 | 0.768 | 0.647 | 0.656 | 0.740 | 0.764 |

**Table VI. Bank Mortgage Credit Reallocation.** This table presents the results from specification (6). Panel A includes county-time, county-income bucket, and bank-time fixed effects. Panel B includes bank-time and county-income bucket-time fixed effects. The sample period runs monthly from February 2014 to January 2016. The unit of observation is county-month-bank-income bucket. The dependent variables are the logarithm of mortgage volume, the logarithm of the median loan size, the number of mortgage loans issued, the value-weighted LTV, the value-weighted LTI, and the value-weighted rate. *Exposure* is defined in Section V, and *Post* is a dummy equal to one from February 2015 to January 2016. All regressions include the share of originations to first-time buyers and to buy-to-let investors at the county-time-income bucket level. Standard errors double clustered at the county and bucket level in parentheses. Source: Central Bank of Ireland.

## *C. Other Asset Classes*

In the previous sections, we have shown that, after the policy introduction, banks issued mortgages with similar characteristics to the mortgages

Banks have several ways to influence the rates charged to clients, including offering more fixed- or non-fixed-rate mortgages.

they previously issued, but partially reduced their total mortgage issuance. In this section, we show that banks, consistent with the bank portfolio choice channel, increased their risk-taking in their holdings of securities and credit to firms, two types of assets not targeted by the policy. These results are also consistent with the observation that banks were seeking hot housing markets in their pre-policy mortgage lending.

### *C.1. Security Holdings*

We use security level holdings data and examine whether banks changed their risk exposure in this asset class. Following Davis and Haltiwanger (1992), we define the "net buys" of security $s$ by bank $b$ from $t-1$ to $t$ as follows:

$$NetBuys_{s,b,t} = \frac{Holdings_{s,b,t} - Holdings_{s,b,t-1}}{0.5(Holdings_{s,b,t} + Holdings_{s,b,t-1})} \in [-2, 2] \qquad (7)$$

where *Holdings* is the euro value of holdings. Compared with percentage changes, this measure also captures final sales, corresponding to a value of -2, and initial purchases, corresponding to a value of 2.

We exploit again the cross-sectional heterogeneity in bank exposure to the lending limits. In particular, we estimate the following specification:

$$NetBuys_{sbt} = \alpha + \beta Exposure_b \times Post_t \times Yield_{st} + \gamma_{bt} + \eta_{st} + \epsilon_{sbt} \qquad (8)$$

where the unit of observation is security $s$, bank $b$, and quarter $t$ (we observe security level holdings at a quarterly frequency). The independent variable

| | Net Buys | | | | Buys | Sells |
|---|---|---|---|---|---|---|
| Exposure × Yield × Post | 0.097** | 0.084* | 0.289*** | 0.490*** | 1.786*** | -2.234*** |
| | (0.042) | (0.050) | (0.083) | (0.118) | (0.587) | (0.514) |
| Fixed Effects | | | | | | |
| Time | ✓ | | | | | |
| Security | ✓ | ✓ | | | | |
| Bank-Time | | ✓ | | ✓ | ✓ | ✓ |
| Security-Time | | | ✓ | ✓ | ✓ | ✓ |
| Observations | 7,707 | 7,707 | 7,707 | 7,707 | 7,707 | 7,707 |
| R-squared | 0.245 | 0.281 | 0.943 | 0.950 | 0.919 | 0.917 |

**Table VII. Bank Portfolio Reallocation, Holdings of Securities.** This table shows the estimation results from specification (8). The unit of observation is security-bank-quarter. The sample runs at a quarterly frequency from 2013Q1 to 2016Q2. The dependent variable is defined in (7). $Exposure$ is defined in Section V, $Post$ is a dummy equal to one from 2015Q2 onwards, and $Yield$ is the yield of the security. Double-interaction terms and uninteracted terms (when not absorbed by fixed effects) are not shown for brevity. Standard errors clustered at the security level in parentheses. Source: Central Bank of Ireland.

of interest is the triple interaction term between the bank exposure variable, a $Post$ dummy equal to one in the post period, and the yield of the security. In our most conservative estimation, we include bank-time and security-time fixed effects to capture time-varying bank heterogeneity and time-varying security heterogeneity, respectively.

We show estimation results in Table VII, where we progressively saturate the regression with more stringent fixed effects and cluster standard errors at the security level, assuming independence across clusters. Column (4) includes all the pairs of two-way fixed effects. The coefficient of interest, stable across specifications, indicates that more-exposed banks increase their holdings of risky securities compared with less-exposed banks after the policy implementation. In columns (5) and (6), we distinguish between the buying and selling behavior of banks. *Buys* is defined as the logarithm of the amount of security $s$ bought by bank $b$ at time $t$, and zero otherwise. Similarly, $Sells$ is defined as the logarithm of the amount of securities sold. We find that more-exposed banks buy more and sell less high-yield securities than less-exposed banks. Note that the magnitude of the estimated coefficient increases substantially once we include security-time fixed effects, suggesting that time-varying security characteristics likely affect the portfolio choice of high-exposure vs. low-exposure banks.

### *C.2. Credit to Firms*

We now use the corporate loan level data and ask whether banks changed their credit supply to firms. To this end, we estimate the following specification:

$$Y_{bclqt} = \alpha + \beta Post_t \times Exposure_b + \gamma X_{bt-1} + \delta_{bc} + \eta_{clqt} + \epsilon_{bclqt} \qquad (9)$$

We measure the credit provided by bank $b$ to firms in county $c$, industry $l$, of quality $q$ in semester $t$ (we observe credit to firms at a biannual frequency). We group firms into clusters based on their county, industry, and quality at time $t$ and investigate the lending behavior of banks to a cluster of firms (Acharya, Eisert, Eufinger, and Hirsch, 2018). We form clusters based on county and industry because firms in a particular industry in a particular county share many characteristics and are thus likely affected in a similar way by macroeconomic developments that might influence credit demand. Note that because we do not have a unique firm identifier across loans, we are unable to analyze credit extended to the *same* firm by different banks

(Khwaja and Mian, 2008). To determine the quality of a firm that receives a loan, we use the ratings obtained by the Central Bank of Ireland that employs a rating scale from 1 (best) to 5 (worst). These ratings come from the banks' internal models but are homogenized by the Central Bank of Ireland to ensure the rating classes correspond to similar probabilities of default. We divide firms into three quality buckets: high quality (rating 1-2), medium quality (rating 3-4), and low quality/high risk (rating 5).

The dependent variable is either the change in log (stock of) credit granted ($\Delta VOLUME$) or the change in the interest rate charged ($\Delta RATE$). Similarly to the previous section, we are interested in the coefficient of the interaction term between the *Post* dummy and the bank exposure to the policy. We include industry-county-quality-time fixed effects to control for credit demand of firms and other variables that are shared by firms of similar quality operating in the same county and industry. We also include bank-county fixed effects to capture time-invariant bank-county heterogeneity (e.g., time-constant heterogeneity in the geographical preference of banks). We cluster standard errors at the county level, assuming independence across clusters.

We show estimation results in Table VIII. In Panels A and B, the dependent variable is the change in volume of credit and change in interest rate charged, respectively. Column (1) considers the full sample. The estimates document that more-exposed banks increase their lending volume to firms and decrease the price of corporate loans more than less-exposed banks. In a next step, we split our sample firms into risky (rating 5) and non-risky (rating 1-4) firms and re-run our specification (9) separately for these two groups of borrowers. The estimation results in columns (2) and (3) show that

Panel A

| LHS: $\Delta VOLUME$ | Sample: Total | Risky | Non-Risky | Total |
|---|---|---|---|---|
| Exposure × Post | 5.022*** | 8.137*** | 3.115* | |
| | (1.544) | (2.438) | (1.825) | |
| Exposure × Post × Risky | | | | 5.109** |
| | | | | (2.034) |
| Exposure × Risky | | | | 0.797 |
| | | | | (1.127) |
| Time-Varying Bank Controls | ✓ | ✓ | ✓ | ✓ |
| Fixed Effects | | | | |
| Industry-County-Quality-Time | ✓ | ✓ | ✓ | ✓ |
| Bank-County | ✓ | ✓ | ✓ | ✓ |
| Bank-Time | | | | ✓ |
| Observations | 10,092 | 3,227 | 6,865 | 10,092 |
| R-squared | 0.498 | 0.520 | 0.491 | 0.489 |

Panel B

| LHS: $\Delta RATE$ | Sample: Total | Risky | Non-Risky | Total |
|---|---|---|---|---|
| Exposure × Post | -3.117*** | -7.051** | -0.950 | |
| | (0.974) | (2.587) | (1.325) | |
| Exposure × Post × Risky | | | | -6.822** |
| | | | | (2.893) |
| Exposure × Risky | | | | 0.329 |
| | | | | (1.573) |
| Time-Varying Bank Controls | ✓ | ✓ | ✓ | ✓ |
| Fixed Effects | | | | |
| Industry-County-Quality-Time | ✓ | ✓ | ✓ | ✓ |
| Bank-County | ✓ | ✓ | ✓ | ✓ |
| Bank-Time | | | | ✓ |
| Observations | 10,007 | 3,183 | 6,823 | 10,007 |
| R-squared | 0.476 | 0.501 | 0.463 | 0.484 |

**Table VIII. Bank Portfolio Reallocation, Credit to Firms.** This table shows the estimation results of specification (9). The unit of observation is bank-industry-county-quality-time. The sample runs at a bi-annual frequency from 2013H1 to 2016H1. *Exposure* is defined in Section V and *Post* is a dummy equal to one from 2015H1 to 2016H1. A risky loan has a rating equal to 5. The dependent variables are the change in log (stock of) credit granted in Panel A and the (value weighted) change in the interest rate charged in Panel B. Standard errors clustered at the county level in parentheses. Source: Central Bank of Ireland.

although a credit expansion in the corporate sector occurs for both risky and non-risky firms, the effect is economically and statistically more pronounced for risky firms relative to the pre-period. A one standard deviation higher *Exposure* leads to a 6.3 percentage points higher credit supply to firms and a 10.3 percentage points higher credit supply to risky firms. These results are confirmed in the last column of Panel A, where we employ a triple interaction of our bank exposure variable with a *Post* dummy and a dummy for whether the borrowing firms are risky. The coefficient shows that the increase in loan volume is mostly driven by an increase toward risky firms. Similarly, in Panel B, we find that the decrease in the cost of bank loans is mostly benefiting risky firms.

## *D. Aggregate Magnitudes*

In this section, we analyze the overall changes in the balance sheet composition of Irish banks in response to the introduction of the lending limits.

Our results so far indicate that more affected banks increase their risk-taking in both corporate lending and security holdings. To provide more evidence on the economic magnitude of these shifts, we collect data on the total exposures to the mortgage market, corporate sector, and securities held by each of our sample banks. Note that we collect these data for the Irish businesses only and do not focus on the global portfolio of the internationally active banks. We scale the exposures such that the sum of corporate, mortgage, and security holdings adds to 100% to preserve the anonymity of banks in the affected and non-affected group. Importantly, in the raw data, these three categories make up 80% to 90% of the actual balance sheet size

| | Aggregate | | | Affected Banks | | | Non-Affected Banks | | |
|---|---|---|---|---|---|---|---|---|---|
| | 2014 | 2015 | $\Delta$ | 2014 | 2015 | $\Delta$ | 2014 | 2015 | $\Delta$ |
| Mortgage | 44.0% | 42.3% | −1.7pp | 49.5% | 45.6% | −3.9pp | 37.2% | 37.9% | +0.7pp |
| Corporate | 28.5% | 29.6% | +1.1pp | 19.7% | 21.7% | +2.0pp | 39.5% | 40.2% | +0.7pp |
| Security | 27.5% | 28.1% | +0.6pp | 30.8% | 32.7% | +1.9pp | 23.3% | 21.9% | −1.4pp |

**Table IX. Aggregate Banks' Balance Sheet.** This table show the share of each asset class on the banks' balance sheet in December 2014 and December 2015, respectively. We rescale the sum of these three categories to add up to 100%. Source: Banks' Annual Reports and Pillar III disclosures, EBA stress test 2016, EBA transparency exercise 2015.

for each of our sample banks.

Table IX presents an overview of how the balance sheets of Irish banks evolved around the policy introduction. The first two columns highlight the aggregate asset allocation of the Irish banking sector. Irish banks decreased their mortgage exposure by 1.7pp. This decrease is associated with a corresponding increase of 1.1pp in corporate lending and 0.6pp in security holdings. The rest of the table shows that this reallocation is driven by banks more exposed to the regulation. These banks, which have a higher share of their balance sheet allocated to mortgage lending than non-affected banks, reduced their mortgage credit by 3.9pp and increased their corporate lending by 2pp and security holdings by 1.9pp. Conversely, non-affected banks do not show such a reallocation pattern.

To assess whether the increase in corporate credit coincides with an increase in risk-taking, we also track changes in credit risk-weighted assets for corporate lending by our sample banks. Consistent with our evidence in the previous section, we find that the share of risk-weighted assets allocated to corporate lending increases by 7pp from 29% to 36% for affected banks.

The evidence presented in this section suggests that banks' credit reallocation is directly driven by the limits on loan leverage. In particular, the lack of reallocation by less-affected banks suggests that the general equilibrium effect operating through changes in expectations about future house prices is likely muted.

# VI. Financial Stability

In the previous section, we have shown that the credit reallocation caused by the lending limits is consistent with a bank portfolio choice channel. In this section, we analyze the effect of the limits on banks' overall risk exposure to real estate, non-financial firms, and government bonds.

To this end, we analyze the correlation of bank equity returns with the equity returns of portfolios that capture the performance of real estate firms, non-financial non-real estate firms, and government bonds around the implementation of the lending limits. We obtain equity returns of all stocks currently listed on Euronext Dublin from Reuters Datastream and manually classify stocks in three groups: bank stocks, real estate stocks, and non-financial non-real estate stocks. We also obtain 10-year Irish government bond yields from Reuters Datastream. We estimate the following specification:

$$R_t^{banks} = \alpha + Post_t + \sum_{i \in \mathcal{I}} \left( \beta^i R_t^i \times Post_t + R_t^i \right) + \epsilon_t \qquad (10)$$
$$\text{where} \quad \mathcal{I} = \{\text{Real Estate, Firms, Govt}\}$$

where the dependent variable is the mean return of bank stocks and the

| | $R^{Banks}$ | $R^{Banks}$ | $R^{Banks}$ | $R^{ExpBanks}$ | $R^{NoExpBanks}$ | *Placebo* $R^{Banks}$ | *Placebo* $R^{Banks}$ |
|---|---|---|---|---|---|---|---|
| Real Estate × Post | -1.125*** | -1.092** | -1.039*** | -1.716*** | 0.057 | 0.233 | -0.480 |
| | (0.403) | (0.416) | (0.352) | (0.553) | (0.385) | (0.421) | (0.624) |
| Real Estate | 1.298*** | 1.401*** | 1.265*** | 1.565*** | 0.764*** | 0.301 | 1.168** |
| | (0.266) | (0.264) | (0.236) | (0.365) | (0.254) | (0.391) | (0.567) |
| Firms × Post | 1.343*** | 1.400** | 0.852* | 1.689** | 0.651 | -0.049 | -0.525 |
| | (0.499) | (0.543) | (0.433) | (0.686) | (0.477) | (0.547) | (0.722) |
| Firms | -0.679** | -0.630* | -0.244 | -1.085** | 0.132 | 0.374 | 0.624 |
| | (0.317) | (0.339) | (0.270) | (0.435) | (0.302) | (0.521) | (0.651) |
| Govt × Post | -0.056 | 0.003 | -0.074 | -0.108 | 0.047 | -0.033 | -0.132 |
| | (0.117) | (0.159) | (0.077) | (0.161) | (0.112) | (0.053) | (0.123) |
| Govt | -0.013 | -0.055 | 0.027 | -0.002 | -0.035 | -0.008 | 0.047 |
| | (0.100) | (0.110) | (0.064) | (0.138) | (0.096) | (0.046) | (0.111) |
| Post | 0.005 | 0.006 | 0.003 | 999 | 999 | 0.033 | -0.000 |
| | (0.005) | (0.003) | (0.003) | 999 | 999 | (0.004) | (0.006) |
| Sample Period | ±3mo | ±2mo | ±4mo | ±3mo | ±3mo | ±3mo | ±3mo |
| Treatment Date | 9Feb15 | 9Feb15 | 9Feb15 | 9Feb15 | 9Feb15 | 9Jun15 | 9Jun14 |
| Observations | 130 | 87 | 173 | 130 | 130 | 131 | 131 |
| R-squared | 0.214 | 0.319 | 0.199 | 0.169 | 0.218 | 0.256 | 0.151 |

**Table X. Financial Stability.** This table presents the results from specification (10). In column (1), the sample period runs daily from November 9, 2014 to May 9, 2015 and the *Post* dummy is equal to one from February 9, 2015 onward. In columns (2) and (3), the sample period runs for 2 and 4 months around February 9, 2015, respectively. In columns (4) and (5), the dependent variable is the return of exposed (above median exposure) and non-exposed (below median exposure) banks, respectively. In columns (6) and (7), the sample period runs from 3 months around June 9, 2015 and June 9, 2014, respectively. Source: Reuters Datastream.

independent variables are (i) the mean return of real estate stocks (to capture mortgage credit), (ii) the mean return of non-financial non-real estate stocks (to capture the non-financial private sector), and (iii) the change in the 10-year Irish government bond yields (to capture holdings of securities). In our baseline estimation, the sample period runs daily from November 9, 2014 to May 9, 2015 (three months around the implementation date) and the *Post* is a dummy equal to one starting from February 9, 2015.

We show the estimation results in Table X. The interaction coefficients suggest that banks become less exposed to real estate and more exposed to the non-financial non-real estate private sector following the introduction of the limits, consistent with the reallocation documented in the previous section. In columns (2) and (3), we change the estimation period to two months and four months around the implementation date, respectively. Our results are robust. In columns (4) and (5), the dependent variable is the mean return of stocks of banks that are more exposed (above median exposure) and less exposed (below median exposure) to the policy, respectively. The estimation results confirm that the correlation in column (1) is driven by more-exposed banks that reduce their exposure to real estate and increase their risk exposure to the non-financial non-real estate private sector. To further confirm our findings, we run a placebo test in the last two columns, where the *Post* dummy is equal to one in the three months after June 9, 2015 and June 9, 2014, respectively. The coefficients of interest are not significant, suggesting that our effects are not present in times other than the treatment period.

# VII. Macroeconomic Effects

In this section, we present data on household savings and homeownership rate and discuss the potential equilibrium effects of the policy.

In Figure 10,we present data on savings behavior and homeownership rates from two household surveys run in 2013 and 2018 by the Central Statistics Office (CSO), the Irish national statistical office.[30] The top panels show

[30]We do not have reliable data on consumption and GDP closer to the policy implementation date or disaggregated at the county level. Data on GDP around the policy is

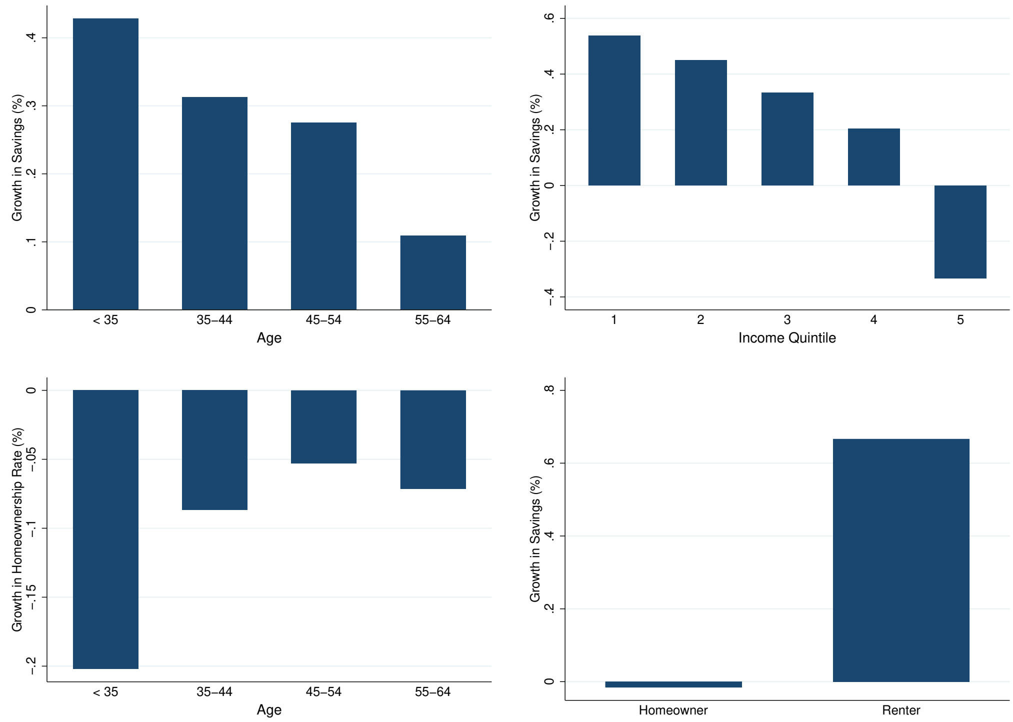


**Figure 10. Savings Behavior and Homeownership Rates.** This figure shows the change in the saving rate or the homeownership rate for different types of households. The change is calculated over two survey waves conducted by the Irish Central Statistics Office (CSO) in 2013 and 2018. The growth in savings depicts the %-change in the median savings reported by the households in each group. The growth in the homeownership rate represents the %-change in the fraction of households reporting to own a home. Source: Irish Central Statistics Office (CSO).

the growth in savings across the age distribution (top left) and across the income distribution (top right). The growth in savings rate between 2013 and 2018 declines monotonically with age and with income. The survey also asks households what the motive behind their savings behavior is and finds that the "purchase own home motive" almost doubles from 8.7% of

affected by a number of large multinational corporations that relocated their economic activities, and more specifically their underlying intellectual property, to Ireland, causing a 26% real GDP growth in 2015.

the cases in 2013 to 16.7% in 2018. The bottom panels show the growth in homeownership rate by age (bottom left) and the growth in savings by homeowners and renters (bottom right). Households under 35 years exhibit a 20% drop in homeownership rate, a significantly larger reduction than any other age group. Finally, the increase in savings rates is almost entirely driven by renters. The survey evidence shows that households closer to the lending limits (young and low-income) have driven the increase in savings rates around the implementation of the policy.

The rationale behind macroprudential policies is that lending limits during a credit boom alleviate the severity of future crises, by correcting aggregate demand and pecuniary externalities (Farhi and Werning, 2016) or by preventing the economy from falling in a debt trap (Mian, Straub, and Sufi, forthcoming). With the important caveat that the two survey snapshots are taken two years before and three years after the policy introduction, the responses suggest that the policy might have increased households' savings rate, in turn, lowering the aggregate demand in the economy, at least in the short-term. To the extent that household credit is needed to generate sufficient demand in the economy, the survey results above suggest that there might be a trade-off between alleviating the severity of crises and temporarily slowing down the economy.

Finally, the results presented in Section C and Section VI suggest that the lending limits are successful in reducing banks' exposure to the housing market but induce banks to increase their risk-taking in other asset classes not targeted by the policy, such as holdings of securities and lending to firms. By working against the goal of making the system safer, these spillover effects highlight the importance of other prudential policies and their coordination.

# VIII. Conclusion

We provide a comprehensive micro-level analysis of the transmission of macroprudential policies aimed at limiting household leverage in the residential mortgage market and, in turn, reducing the feedback loop between credit and house prices. Combining loan level data on residential mortgages, county level house prices, and detailed data on banks' other assets, we examine the February 2015 introduction of LTV and LTI limits in Ireland.

The policy caused a substantial reallocation of mortgage credit. We document a reallocation of mortgage credit from low- to high-income households and from hot, mostly urban, housing markets to cool, mostly rural, housing markets. This reallocation is effective in slowing down house price growth, and in turn, the feedback loop between mortgage credit and house prices, in hot housing markets. Consistent with constrained lenders adjusting their portfolio choice, more-affected banks drive this reallocation and also increase their risk exposure in credit to firms and holdings of securities, two assets not targeted by the limits.

Our analysis of macroprudential regulation opens up a promising area for future research. In particular, our results on bank asset allocation naturally call for the development of equilibrium models to measure how macroprudential regulation affects welfare and the likelihood of busts. Having documented how limits to household leverage affect bank portfolio choice and house prices in a partial equilibrium framework, we provide a set of correlations and suggested transmission mechanisms that these equilibrium models should take into account.

# Appendix A. Bank Mortgage Reallocation

In this section, we analyze banks' first order conditions and the marginal profitability of mortgages. The profitability of a mortgage can be decomposed in the interest earned and the capital cost. The interest earned by Irish banks on mortgages (decreasing during our sample period) has relatively little variation across mortgages, as shown in Table II. Hence, the profitability of mortgages is mostly determined by the capital cost, namely the regulatory capital that a bank needs to set aside for each mortgage loan. This cost is regulated in 31.14 of the Basel Regulation for calculating risk weighted assets for credit (BIS, 2019). The required capital depends on the predicted probability of default, according to the formulas (i) $K = \left(LGD * N\left(\frac{G(PD)}{\sqrt{1-R}} + \sqrt{\frac{R}{1-R}} * G * (0.999)\right) - PD * LGD\right)$ and (ii) $RWA = K * 12.5 * EAD$, where $R$ is the correlation among mortgages (set at 0.15 for all banks), $EAD$ is the exposure at default, $LGD$ is the loss given default, $N$ is the standard normal distribution, and $G$ is the inverse of the standard normal distribution. While the LGD and the EAD largely depend on the size of the loan and the value of the collateral, with the LGD additionally depending on country-specific factors (e.g., right of recourse, regulatory LGD floor), the PDs are estimated using several borrower level characteristics obtained from historical data.

Banks obtain the probability of default of a newly issued loan with an internal model estimated and back-tested with historical data. To mimic banks' (unobservable) models in a transparent way, we regress a default dummy variable on mortgage characteristics. In Table XI, we replicate banks' estimations using historical loan level data on loans issued before 2013. Independent variables include the most important loan characteristics (interest rate and interest rate type, LTI, LTV, loan size, geography) and borrower characteristics (borrower age, first-time buyers (FTB) status, buy-to-let (BTL) status, relationship status). The estimation results are intuitive. Riskier loans tend to be larger, have a higher interest rate, a higher LTI and LTV, and are more likely to have a standard variable rate (SVR) or a tracker than a fixed rate. Older borrowers and BTL investors are more likely to default. Borrowers in urban areas, FTBs, and couples are less likely to default.

In the left column of Figure 11, we show realized defaults of loans issued before 2013. In the right column, we show the probability of default predicted by the probit model in the first column of Table XI. Consistent with the estimation results, borrowers in Dublin (and affected counties more in general), FTBs, lower income borrowers, and younger borrowers are less likely to default (holding constant all other independent variables). The discussion of the effect of income in the 2007–10 bust is outside the scope of this paper. However, this table is consistent with the observation that higher income borrowers increased markedly their leverage during the boom in Ireland (Lydon and McCann, 2017).

This exercise shows that some loans are more costly, from a capital standpoint, than others. In particular, before the policy implementation, mortgages extended to urban areas, FTBs, and younger and lower income borrowers were cheap from a capital standpoint, in relative terms, given their lower estimated probability of default. As discussed in the main body, this evidence supports our interpretation that banks likely (i) rationed high-income and rural borrowers before the policy and (ii) reallocated their mortgage credit from hot to cool housing markets and from low- to high-income borrowers after the policy.

| | Default | Default | Default | Default | Default | Default |
|---|---|---|---|---|---|---|
| Interest Rate | 0.243*** | 0.244*** | 0.231*** | 0.242*** | 0.243*** | 0.230*** |
| | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) |
| LTI | 0.034*** | 0.038*** | 0.057*** | 0.034*** | 0.038*** | 0.057*** |
| | (0.003) | (0.003) | (0.003) | (0.002) | (0.003) | (0.003) |
| LTV | 0.001*** | 0.001*** | 0.002*** | 0.001*** | 0.001*** | 0.002*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Borrower Age | 0.003*** | 0.002*** | 0.003*** | 0.002*** | 0.002*** | 0.003*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Income | | 0.016*** | 0.033*** | | 0.015*** | 0.032*** |
| | | (0.003) | (0.004) | | (0.003) | (0.004) |
| House Price | | -0.019** | -0.003 | | -0.020** | -0.003 |
| | | (0.008) | (0.002) | | (0.008) | (0.002) |
| Loan Size | 0.081*** | 0.076*** | | 0.079*** | 0.075*** | |
| | (0.012) | (0.012) | | (0.013) | (0.013) | |
| Dublin | -0.143*** | -0.144*** | -0.121*** | | | |
| | (0.028) | (0.028) | (0.026) | | | |
| Urban | | | | -0.155*** | -0.155*** | -0.141*** |
| | | | | (0.028) | (0.028) | (0.027) |
| FTB | -0.185*** | -0.186*** | -0.199*** | -0.182*** | -0.183*** | -0.197*** |
| | (0.017) | (0.017) | (0.017) | (0.017) | (0.017) | (0.018) |
| BTL | 0.106*** | 0.103*** | 0.114*** | 0.112*** | 0.109*** | 0.120*** |
| | (0.037) | (0.037) | (0.036) | (0.036) | (0.036) | (0.034) |
| Couple | -0.035*** | -0.035*** | -0.023** | -0.034*** | -0.035*** | -0.023** |
| | (0.010) | (0.010) | (0.010) | (0.010) | (0.010) | (0.010) |
| SVR | 0.603*** | 0.604*** | 0.596*** | 0.601*** | 0.601*** | 0.594*** |
| | (0.017) | (0.017) | (0.017) | (0.016) | (0.016) | (0.017) |
| Tracker | 0.886*** | 0.887*** | 0.861*** | 0.884*** | 0.886*** | 0.859*** |
| | (0.027) | (0.027) | (0.026) | (0.026) | (0.026) | (0.026) |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 336,906 | 336,906 | 336,906 | 336,906 | 336,906 | 336,906 |
| Pseudo R-squared | 0.067 | 0.067 | | | | |

**Table XI. Probit Model, Historical Determinants of Default.** This table shows the results from a probit model estimated on historical loan level data. The dependent variable is a dummy equal to one for loans defaulted before 2013. The continuous independent variables are the interest rate, LTI ratio, LTV ratios, borrower age, household income, house price, and loan size. The last three are standardized. SVR (Tracker) is a dummy equal to one if the loan has a standard variable rate (has a tracker rate). Following Gaffney, Kelly, and McCann (2014), Dublin is a dummy equal to one if the loan was issued in Dublin. Urban is a dummy equal to one for the top five hot housing markets. The regression also controls for dummies whether the buyer was a first-time buyer (FTB), a couple, or a buy-to-let (BTL) investors. Standard errors clustered at the county level in parentheses. Source: Central Bank of Ireland.

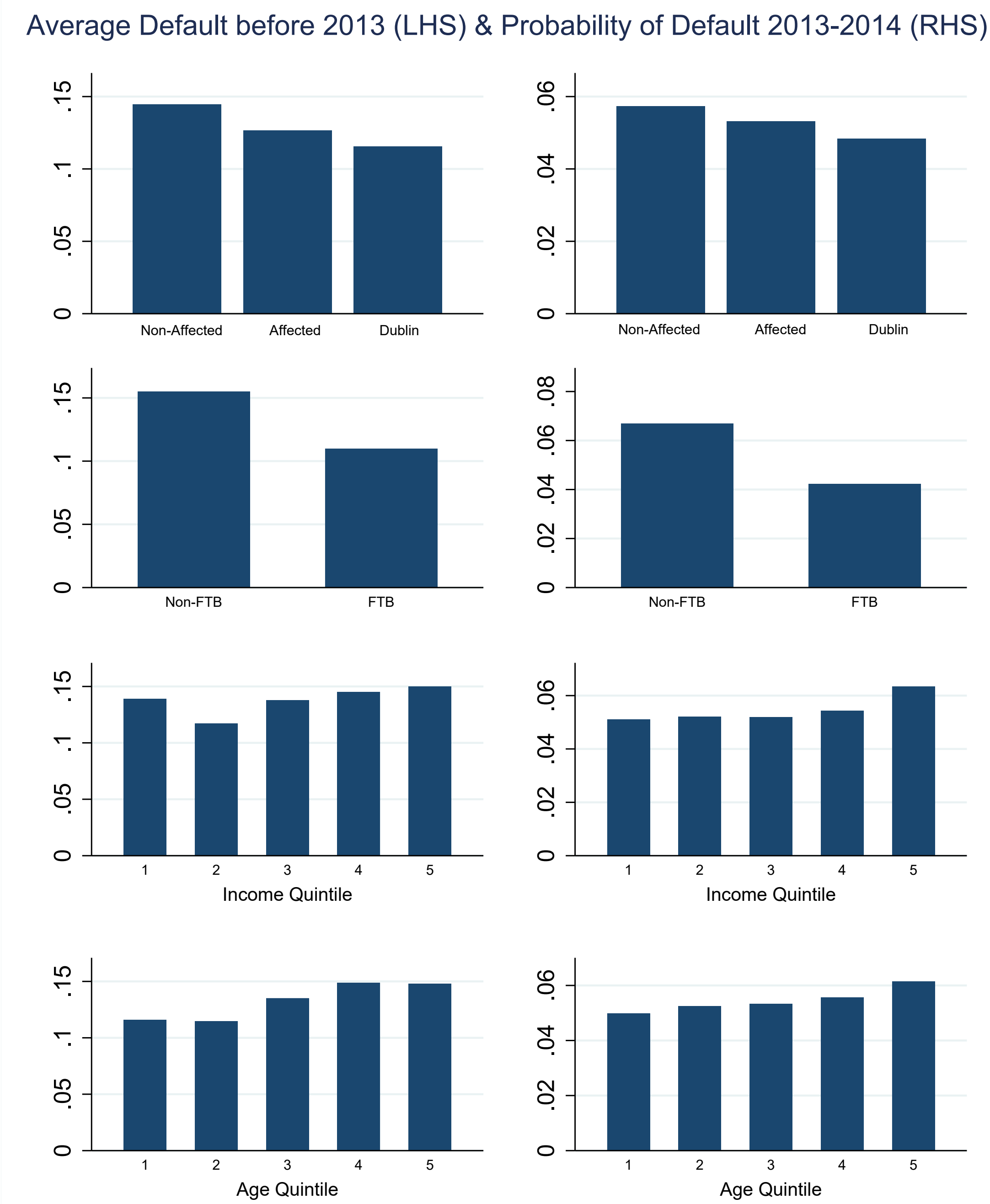


**Figure 11. Default Rates.** This figure shows average defaults (left column) and the estimated probability of default (right column). The estimated probability of defaults are obtained using the probit model in the first column of Table XI. Source: Central Bank of Ireland.

# Appendix B. Additional Figures

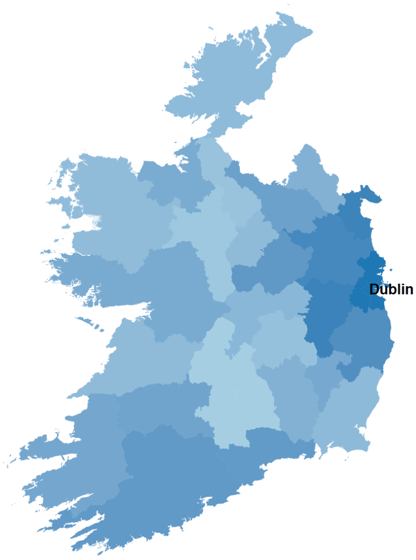


**Figure 12. Counties and Lending Limits.** This figure shows the county level distance from the limits. Darker colors indicate less distant counties. Source: Central Bank of Ireland.

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