

The Anatomy of the Transmission of Macroprudential Policies

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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. 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 between credit and house prices and slows house price growth in “hot” housing markets. Banks whose lending to households is more affected by the regulatory constraint drive this reallocation, but also substitute their risk-taking into holdings of securities and corporate credit.

SINCE 1990, POLICYMAKERS IN MORE than 60 countries have adopted macroprudential policies that aim to limit household leverage and thereby slow

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down the feedback loop between credit and house prices. By showing that a buildup of household leverage eventually leads to busts, lower output growth, and higher unemployment (Jorda, Schularick, and Taylor (2016), Mian, Sufi, and Verner (2017)), the academic literature has highlighted the importance of these policies.

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, and bank security-level holdings, we study the 2015 introduction of LTV and LTI 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 to 2008, and 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 effective immediately after the announcement. The goal, according to 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—somewhat counterintuitively—that while the lending limits affect a large share of the residential mortgage market, mortgage issuance has continued to grow since the policy introduction. We show that this result is due to the market moving to different borrowers and geographies to “conform” with the new limits. Our analysis of this reallocation leads to three main findings. First, mortgage credit is reallocated from low- to high-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, which slows 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 affect savings rates, and in turn aggregate demand and real economic activity.

¹ Alam et al. (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-to-GDP ratio increased from 55% to 101% between 2002 and 2007, followed by a GDP growth rate of −10% and an unemployment rate change of +8% over the next three years.

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. The increase in “conforming” issuance offsets the collapse in the issuance of those mortgages that exceed the newly imposed limits, leaving aggregate issuance little affected. However, in the cross section of counties, “hot” housing markets (typically urban counties) are closer to the limits than “cool” housing markets (typically rural counties), and in the cross section of borrowers, high-income borrowers are more distant from the limits than low-income borrowers. In particular, 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 affect house prices through the mortgage credit channel via both partial equilibrium and general equilibrium effects. For example, lower mortgage credit to hot housing markets might reduce house prices, reducing households’ borrowing capacity. The 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% postregulation. This evolution is driven by hot housing markets—these markets’ house price growth was well above 20% and rapidly increasing at the time of announcement, but collapsed to around 4% postregulation. However, house price growth remained stable at the prepolicy level in cool housing markets. Thus, 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 across cool and hot housing markets is more pronounced for larger properties, which are 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 these findings via a bank portfolio choice channel of macroprudential policies. Consider an environment in which 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

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

choice. Specifically, banks reallocate their portfolio to fund some positive net present value (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 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 prepolicy 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. Prepolicy, bank equity returns are positively correlated with real estate returns but become insensitive to real estate and positively correlated with nonfinancial 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 policies in this literature is based on the observation that agents overborrow in good times, not internalizing all of 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 contributed to the rapid appreciation of house prices (Mian and Sufi (2009), Favara and Imbs (2015), Di Maggio and Kermani (2017), Adelino, Schoar, and Severino (2020), Mian and Sufi (2022)). The housing price collapse that followed worked its way through the

nonperforming loans and are adjusting their balance sheets to comply with new capital and liquidity regulations.

⁴ See Aikman et al. (2019), Freixas, Laeven, and Peydro (2015), Claessens, Ghosh, and Mihet (2013), Claessens (2015), and Gambacorta and Murcia (2020) for excellent overviews of macroprudential policies.

balance sheets of households (Hall (2011), Eggertsson and Krugman (2012), Mian, Rao, and Sufi (2013), Mian and Sufi (2014), Jones, Midrigan, and Philippon (2020)) and intermediaries (Gertler and Kiyotaki (2010), 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 showing how macroprudential policies that aim to limit household leverage operate in practice. We do so by jointly analyzing mortgage credit, house prices, and their feedback loop. We document 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 toward nonmortgage risky assets. Our paper is (less directly) related to the literature on macroprudential policies that aim to limit risk-taking, both empirical and theoretical.^{5,6}

A few other papers analyzing LTV/LTI limits imposed in countries other than Ireland find results consistent with ours. By analyzing the bunching behavior around regulatory thresholds, DeFusco, Johnson, and Mondragon (2020) show that the Dodd-Frank “Ability-to-Repay” rule (similar to an LTI limit) successfully reduced borrower leverage, and Van Bakkum et al. (2019) show that LTV limits caused Dutch borrowers to increase their down payments. Although they do not analyze the role of banks, Tzur-Ilan (2020) and Igan and Kang (2011) show that borrowers move away from hot housing markets, slowing house price growth in Israel and Korea, respectively.⁷

Finally, analyzing the same Irish macroprudential policy we study, Kinghan, McCarthy, and O’Toole (2019) show that LTV fell for first-time and 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, to the best of our knowledge, our analysis represents the most comprehensive analysis of how macroprudential policies in mortgage credit have operated in practice in a country, and highlights the importance of how banks reallocate credit both across and away from mortgage markets.

The rest of the paper is structured as follows. Section I describes the data and the setting. Section II documents the mortgage credit reallocation.

⁵ Our paper is also related to the empirical literature on macroprudential policies (Aiyar, Calomiris, and Wieladek (2014), Jimenez et al. (2017), Ayyagari, Beck, and Martinez Peria (2019), Gropp et al. (2019), Benetton (2021), Benetton et al. (2021), De Marco, Kneer, and Wieladek (2021)).

⁶ The theoretical literature on macroprudential policies (Landvoigt and Begenau (2022), Eleney, 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, Matyunina, and Ongena (2022) 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, Badarinar, 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.

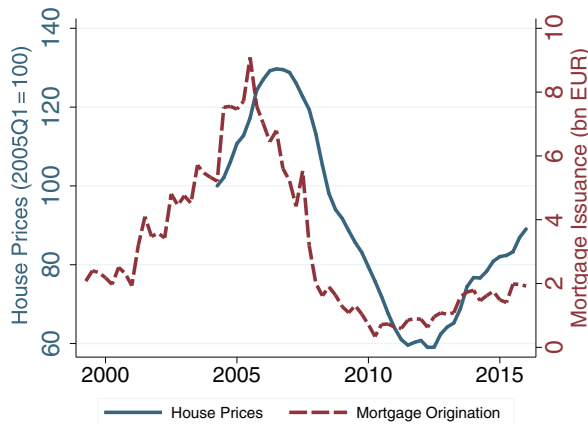


Figure 1. Ireland real estate boom-bust. This figure shows real estate prices from 2005Q1 to 2016Q4 (left axis) and residential mortgage issuance from 2000Q1 to 2016Q4 (right axis). Sources: Department of Housing, Planning and Local Government and Central Statistics Office. (Color figure can be viewed at wileyonlinelibrary.com)

Section III analyzes house prices. Section IV presents the bank portfolio choice channel. Section V discusses financial stability, and Section VI discusses the macroeconomic effects. Section VII concludes.

I. Setting and Data

Section I.A provides background on the Irish residential mortgage market. Sections I.B and I.C then describe the macroprudential policy and our data, respectively.

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 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

⁸ In the Internet Appendix, which may be found in the online version of this article, we show that the boom-bust cycle in the housing market has been more pronounced in Ireland compared with the United Kingdom, the euro area, and the United States 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 that avoided buying during the bust (the share of renters that became first-time buyers (FTBs) had fallen to 2% in 2012 and then rose steadily toward 7% to 8%). This increased demand interacted with a weakened supply elasticity.

During the bust of 2007 to 2010, 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 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 nonperforming 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, then 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 and LTI ratios on new originations of residential mortgages. The LTI limit is 3.5. The LTV limit depends on the borrower type.¹² Lending for primary-dwelling housing (PDH) is limited to 80% LTV. For 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 an 80% LTV limit. The measures impose a stricter threshold of 70% for buy-to-let (BTL) properties.¹³

⁹ Irish economic performance is better measured with GNP because GDP is inflated by profits of international companies transferred to Ireland because of low corporate taxes.

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 European Central Bank (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 on October 7, 2014 to stimulate discussion and available on the Central Bank of Ireland website (<https://www.centralbank.ie/docs/default-source/financial-system/loan-to-value-and-loan-to-income-restrictions/gns-2-1-1-macro-prudential-mortgage-lending.pdf?sfvrsn=6>). 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 FTBs are 4 percentage points (pp) or 30% less likely to default than subsequent-time buyers in Ireland (Kelly, O'Malley, and O'Toole (2014)). In addition to loans that are exempted from the

Table I
Lending Limits

This table shows a summary of the limits. Source: Central Bank of Ireland.

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

^aA 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.

C. Data

The core of our final data set is a 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., FTB, BTL).

Second, we observe loan-level data on bank credit to firms at a semiannual 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. We match this information with firm characteristics such as the county of incorporation, industry, and size (very small/small and medium-size/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 nondefaulted 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 ECB 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 properties listed for sale is 5,000 units. Indices are quality adjusted, holding the mix of characteristics constant.

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%. Analysis of this subsequent period is 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 encouragement from regulators and in accordance with data submissions required by the ECB-SSM (Single Supervisory Mechanism) Comprehensive Assessment in 2013. More information is available in the [Internet Appendix](#).

II. Mortgage Credit Reallocation

In this section, we document four facts. In Section II.A, we show that the lending limits affect more than one-third of the market but the originations of residential mortgages appear to be almost unaffected by these limits. In Section II.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 II.C, we show that after policy implementation, mortgage credit is reallocated from low- to high-income borrowers and from hot to cool housing markets. In Section II.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. Of the total €1.6 billion in mortgages in our sample in that period, nonconforming (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-nonconforming mortgages accounted for €0.5 billion and LTI-nonconforming mortgages accounted for €0.3 billion. Moreover, approximately half of the LTI-nonconforming mortgages were also LTV-nonconforming.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 *nonconforming* mortgages, as banks followed the new rules.¹⁶ In the right panel, we show the evolution of originations of conforming (solid line) and nonconforming (dashed line) mortgages and confirm that the two time series diverge starting in February 2015.

¹⁵ From October 2013 to September 2014, 2,797 LTV nonconforming mortgages and 1,467 LTI nonconforming mortgages were issued. The limits are more binding for second and subsequent time buyers and BTL buyers compared with FTBs.

¹⁶ The nonconforming issuance is strictly positive after the policy implementation because the new rules allow banks to exceed the limits for a limited fraction of their issuance. In the [Internet Appendix](#), we show that mortgage originations continue to increase even when weighted by LTV and LTI.

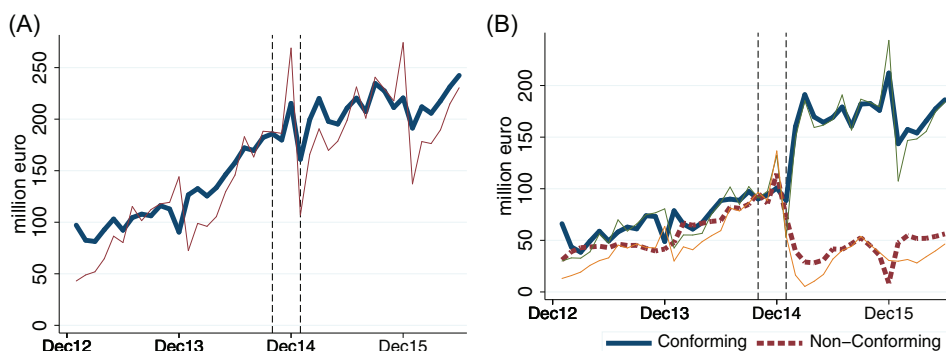


Figure 2. Aggregate residential mortgage issuance. This figure shows the aggregate residential mortgage issuance of our sample banks from January 2013 to June 2016. The left panel shows total issuance. The right panel shows issuance of conforming (solid line) and nonconforming (dashed line) mortgages. 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. (Color figure can be viewed at wileyonlinelibrary.com)

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 into five quintiles based on their income (Q1 is the bottom quintile, Q5 is the top quintile) and into borrowers located in hot versus cold housing markets (counties that experienced a rapid vs. more moderate house price appreciation before the policy, top and bottom panel, respectively). Across the income distribution, high-income borrowers (i) have lower LTV and LTI, (ii) are older and more likely to be married, and (iii) are less likely to be FTBs 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 that of the fourth income quintile. Somewhat mechanically, the distance 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 BTL 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

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 summary statistics for the subsample of counties with high (low) house price appreciation in the prepolicy period. Source: Central Bank of Ireland.

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

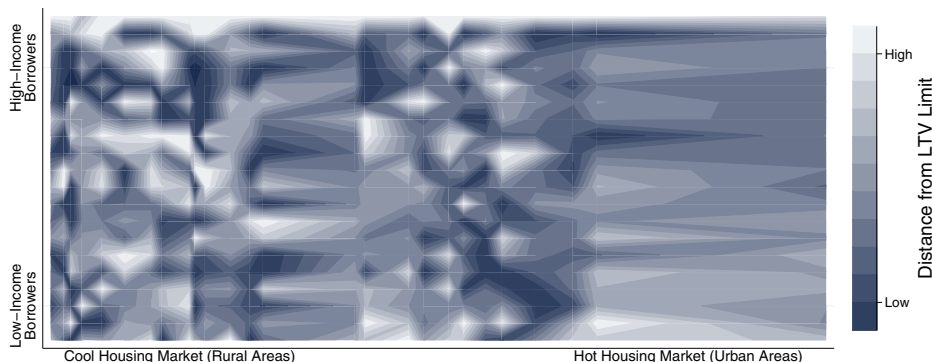


Figure 3. Exposure to LTV lending limits across counties and incomes. This figure shows the exposure to the LTV lending limit across counties and across the income distribution of borrowers. The x-axis shows counties ranked according to the house price appreciation prepolicy (from 2012Q4 to 2014Q3). 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. (Color figure can be viewed at wileyonlinelibrary.com)

period.¹⁷ We calculate the mean of this borrower-level distance at the income bucket-county level, where we group borrowers into 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 Figures 3 and 4, we show that the borrowers that are more exposed to the lending limits, in particular, the LTI limits, tend to have low income and tend to be located in hot housing markets—predominantly urban counties that experienced rapid house price appreciation before the policy. On the x-axes, counties are weighted by size (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 into 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 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

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 to zero. Second, given the different scale of LTI and LTV, we rescale the two distances to have a mean of zero and a standard deviation of one. Third, we calculate the minimum of these two limits.

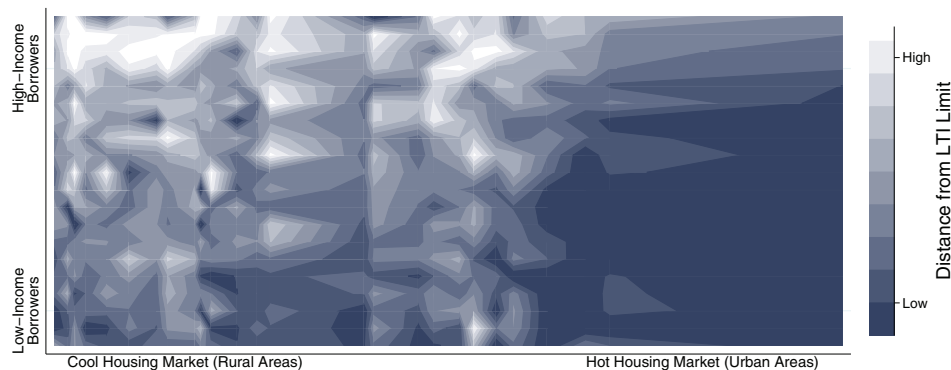


Figure 4. Exposure to LTI lending limits across counties and incomes. This figure shows the exposure to the LTI lending limit across counties and across the income distribution of borrowers. The *x*-axis shows counties ranked according to the house price appreciation prepolicy (from 2012Q4 to 2014Q3). 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. (Color figure can be viewed at wileyonlinelibrary.com)

experienced 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- to high-income borrowers around the policy implementation.

Figure 5 provides another heatmap where, for each income bucket-county pair, we show the change in mortgage origination from the prepolicy period (February 2014 to January 2015) to the postpolicy 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. 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 borrowers

¹⁸ In Figure B.1, 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.

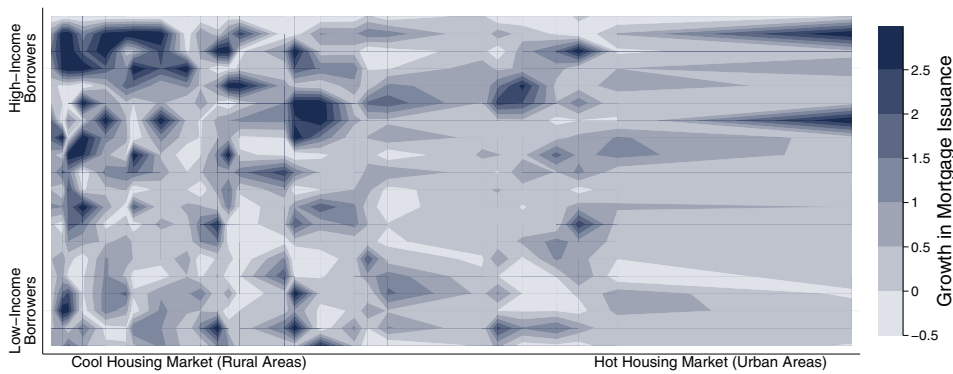


Figure 5. Reallocation of mortgage credit. This figure shows the reallocation of mortgage credit across counties and across the income distribution of borrowers. The x-axis shows counties ranked according to the house price appreciation prepolicy (from 2012Q4 to 2014Q3). The space taken by each county 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 in mortgage issuance in the postperiod (February 2015 to January 2016) compared with the preperiod (February 2014 to January 2015). Darker colors indicate higher issuance growth. (Color figure can be viewed at wileyonlinelibrary.com)

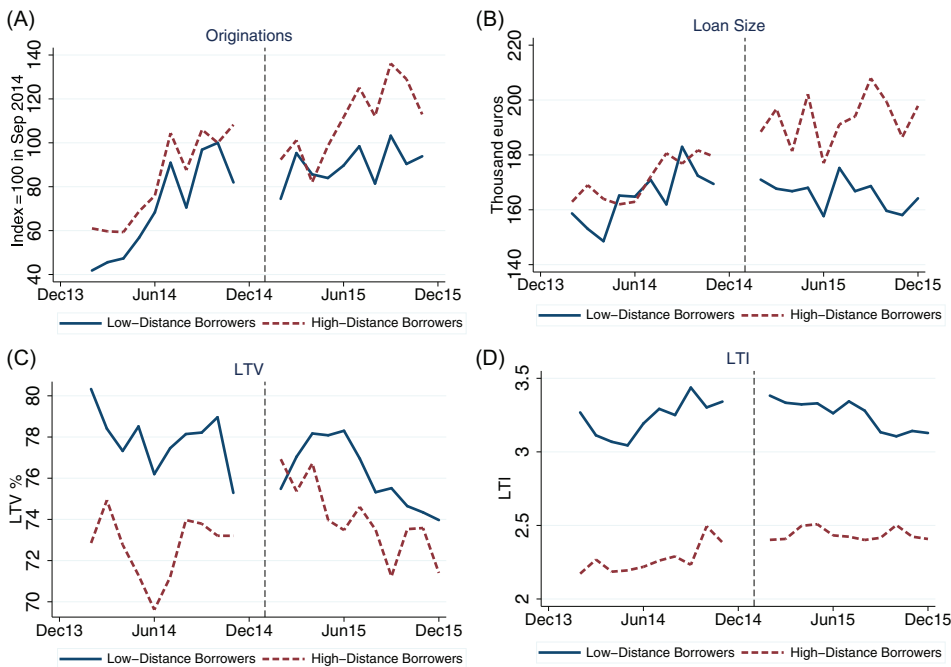


Figure 6. Mortgage credit reallocation, nonparametric evidence. These figures show mortgage origination volume, loan size, LTV, and LTI from February 2014 to January 2016. The dashed (solid) lines indicate high-distance (low-distance) borrowers, that is, the highest (lowest) tercile of the distance variable. Source: Central Bank of Ireland. (Color figure can be viewed at wileyonlinelibrary.com)

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 located in urban counties while high-distance borrowers tend to have higher income and are predominantly located in rural counties. In the top panel, we observe that originations and loan size are 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 prepolicy period, tend to converge after the policy implementation.

We confirm this reallocation by estimating:

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

where c is a county, t is a month, and h is a borrower income bucket, with borrowers divided 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 (prepolicy) 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 FTBs and to BTL 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 capture time-invariant borrower and geographic characteristics. We double-cluster standard errors at the county and income bucket levels. This choice accounts for the fact that 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 low income and tend to be located in hot housing markets. We assume error independence across clusters. Formally, $E[u_i, u_j | x_i, x_j] = 0$, 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 10.4% higher issuance and a 10.5% larger loan relative to the preperiod. 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 driven mostly by the LTI limits. This

Table III
Reallocation of Mortgage Credit, Parametric Evidence, LTI versus 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, value-weighted LTV, and 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 II.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 levels. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Central Bank of Ireland.

	Volume	Loan Size	No. Loans	LTV	LTI
LTI Distance \times Post	0.186** (0.071)	0.185*** (0.054)	-0.571 (0.521)	-0.790 (1.545)	0.408*** (0.068)
LTV Distance \times Post	0.094 (0.066)	0.080* (0.046)	0.490 (0.446)	11.285*** (1.661)	0.039 (0.052)
Fixed Effects					
County-Time	✓	✓	✓	✓	✓
Bucket-Time	✓	✓	✓	✓	✓
County-Bucket	✓	✓	✓	✓	✓
Observations	7,188	7,188	7,191	7,100	7,045
R^2	0.755	0.602	0.892	0.363	0.592

	Volume	Loan Size	No. Loans	LTV	LTI
LTV Distance \times Post	0.186*** (0.062)	0.171*** (0.042)	0.240 (0.255)	11.021*** (1.067)	0.235*** (0.055)
Fixed Effects					
County-Time	✓	✓	✓	✓	✓
Bucket-Time	✓	✓	✓	✓	✓
County-Bucket	✓	✓	✓	✓	✓
Observations	7,199	7,199	7,202	7,110	7,047
R^2	0.754	0.602	0.892	0.369	0.588

	Volume	Loan Size	No. Loans	LTV	LTI
LTI Distance \times Post	0.224*** (0.074)	0.218*** (0.055)	-0.383 (0.382)	3.420** (1.507)	0.423*** (0.064)
Fixed Effects					
County-Time	✓	✓	✓	✓	✓
Bucket-Time	✓	✓	✓	✓	✓
County-Bucket	✓	✓	✓	✓	✓
Observations	7,192	7,192	7,195	7,102	7,049
R^2	0.754	0.602	0.892	0.356	0.593

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 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 (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 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 into its partial equilibrium component and a "general equilibrium multiplier."

More specifically, we estimate the specifications

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

$$\begin{aligned} Volume_{ct} = & \alpha + \beta_1^{GE} Distance_c + \beta_2^{GE} Post_t \times Distance_c + X_{ct} \\ & + \gamma_t + \epsilon_{ct}. \end{aligned} \quad (3)$$

The unit of observation in the 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 the general equilibrium effect into (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 prepolicy period, the general equilibrium effect is large (−4.18), driven by hot housing markets. This effect can be decomposed into its partial equilibrium component of 6%

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 shares 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 levels in Panel A and clustered at the county level in Panel B. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Central Bank of Ireland.

Panel A. Unit of Obs: Income Bucket-County-Time			
LHS: Credit	Full Sample	Hot Housing Markets	Cool Housing Markets
Distance \times Post	0.247*** (0.060)	0.206* (0.105)	0.301*** (0.074)
Distance	-0.230*** (0.042)	-0.231** (0.076)	-0.226*** (0.060)
Fixed Effects			
County-Time	✓	✓	✓
Bucket-Time	✓	✓	✓
Observations	7,203	4,237	2,966
R^2	0.701	0.755	0.433
Panel B. Unit of Obs: County-Time			
LHS: Credit	Full Sample	Hot Housing Markets	Cool Housing Markets
Distance \times Post	0.323 (0.193)	0.581* (0.295)	0.581* (0.301)
Distance	-4.182*** (1.127)	-4.709*** (1.338)	-1.368 (1.793)
Fixed Effects			
Time	✓	✓	✓
Observations	622	311	311
R^2	0.392	0.399	0.198

(-0.23/ - 4.18) and its general equilibrium multiplier of 94%. In the postpolicy 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 borrowers' collateral constraint, allowing them to obtain a larger mortgage. In the context of the 2002 to 2005 boom in the

United States, 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 to 2010 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 postpolicy period cannot be attributed solely to the lending limits. In the next section, we discuss how the announcement of the lending limits likely signaled that policymakers were concerned about the rapidly appreciating housing market, which affected agents’ expectations, in turn. 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 the [Internet Appendix](#)). These policies do not constitute an identification concern as most were effectively help-to-buy schemes and our effects (i) kick in exactly around the implementation of the lending limits (e.g., Figures 2 and 9) and (ii) are theoretically motivated (reallocation across counties and across the income distribution) by the bank portfolio choice channel presented in Section IV.A. Moreover, the survey data presented in the [Internet Appendix](#) suggest that agents anticipated, at the time of the first rumors, a decline in house prices mainly because of the soon-to-be-announced limits.

III. 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.

We first report nonparametric 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 prepolicy level in high-distance counties.²⁰ In the [Internet Appendix](#), we show that 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

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 to 2014Q4; 69% in 2014Q4 to 2016Q4) but substantially increased in high-distance counties (−2% in 2012Q4 to 2014Q4; 81% in 2014Q4 to 2016Q4).

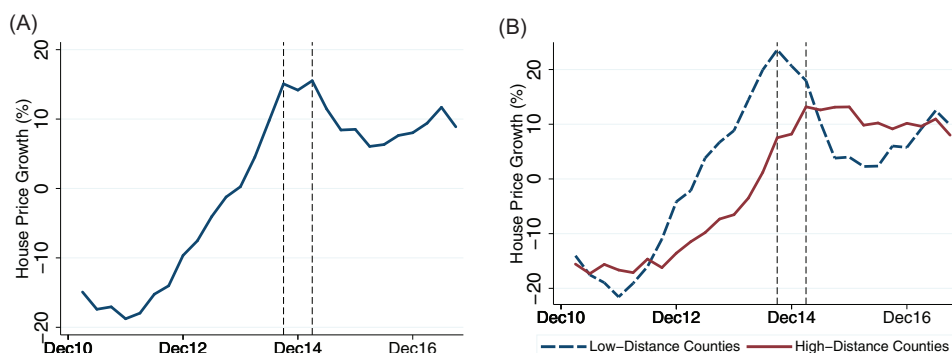


Figure 7. House price changes. The left panel of this figure shows the evolution of yearly house price growth. The right panel shows the evolution of yearly house price growth for high-distance and low-distance counties separately (groups split by median value). The vertical dashed lines indicate the 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. (Color figure can be viewed at wileyonlinelibrary.com)

with buyer income, 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 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 due mainly to the introduction of the lending limits. That being said, the lending limits likely affected households' expectations 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 the [Internet Appendix](#)).

Second, we show parametric evidence consistent with 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 county-property type (c, p) level:

$$\Delta HP_c = \alpha + \beta \text{Distance}_c + \epsilon_c, \quad (4)$$

$$\Delta HP_{cp} = \alpha + \beta_1 \text{Distance}_c \times \text{Size}_p + \beta_2 \text{Distance}_c + \beta_3 \text{Size}_p + \epsilon_{cp}, \quad (5)$$

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.

Table V
House Prices and Lending Limits

This table shows estimation results from specification (4) in column (1) and specification (5) in columns (2) to (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 are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Central Bank of Ireland, Daft.ie.

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

where the dependent variable is the change in house prices from 2014Q3 to 2016Q4, *Distance* is the county-level (prepolicy) distance from the lending limits, and *Size* is an integer equal to the number of bedrooms.²² We interact *Distance* with the measure of property size to check whether the effect of the lending limits changes with 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 accounts for the fact 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) to (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),

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 that

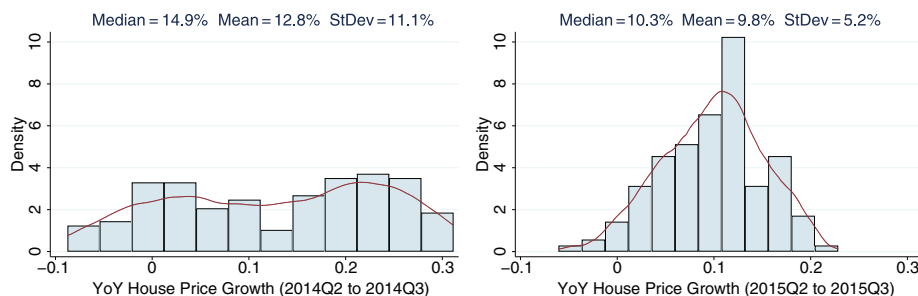


Figure 8. Distribution of house price growth. This figure shows the change in the distribution of house price growth. The left and right graphs show the density of year-over-year house price growth before (2014Q2 to 2014Q3) and after (2015Q2 to 2015Q3) the policy, respectively. Source: Daft.ie. (Color figure can be viewed at [wileyonlinelibrary.com](https://onlinelibrary.wiley.com/doi/10.1111/jofi.13170))

suggesting that the limits reduced the heterogeneity in house price growth. In the [Internet Appendix](#), we show that the tails were not shrinking before the policy.

Our parametric and nonparametric 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 (2021) 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 of these studies find that the effects are driven by metropolitan areas. Taken together with the general equilibrium estimates in Section II.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 due in part to the lending limits and in part to the changes in agents' expectations.

IV. 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 IV.A. Consistent with this channel, in Sections IV.B and IV.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 IV.D, we show

the distance is constant within a county. We then cluster our standard errors at the county level to account for the fact that standard errors might be correlated within counties.

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 otherwise not been funded.²³ Consider an environment in which banks solve their portfolio problem subject to a binding balance sheet constraint (e.g., regulatory capital constraint). In this setting, where banks cannot fund all positive NPV projects, introduce 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 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 nonperforming loans. Moreover, there is no public support for the residential mortgage market, and mortgages are issued almost entirely by banks that hold them on balance sheet.²⁵ In the context of the United States, 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 not likely to hold in our context, where the mortgage market is structured in an originate-to-hold model. Note that with the notable exception of the United States, many countries around the world have only a small securitization market and do not provide 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 regulatory requirements. In our setting, the marginal profitability of mortgages is determined mostly by their capital requirements as banks are likely capital constrained (see discussion above) and, 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

²³ In the [Internet Appendix](#), we provide a formal characterization of banks' portfolio problem.

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 constraints. More generally, our results are consistent with a model in which bank capital is "scarce" and the marginal equity issuance costs are very steep. See Harris, Opp, and Opp (2020) for a theoretical framework.

²⁵ Nonperforming loans comprise 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.

²⁶ In Ireland, 90% of mortgages fall under the Basel II IRB approach, which estimates capital requirements as follows: $CapitalRequirement = LGD \times N[(1 - R)^{-0.5} \times G(PD) + (R/(1 - R))^{0.5} \times$

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 nonconforming mortgages in the prepolicy period drive the mortgage credit reallocation compared with banks with a larger issuance of conforming mortgages in the prepolicy 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 between bank b 's average market share in hot housing markets and 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 nonconforming 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) nonconforming 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.

Having shown nonparametric 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

$G(0.999)] - PD \times LGD$, where R is the correlation among mortgages (set to 0.15 for all banks), EAD is exposure at default, LGD is 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 depend largely 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 PD s are estimated using several borrower-level characteristics obtained from historical data.

²⁷ The 2014 bank branch network is not available, and thus 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.

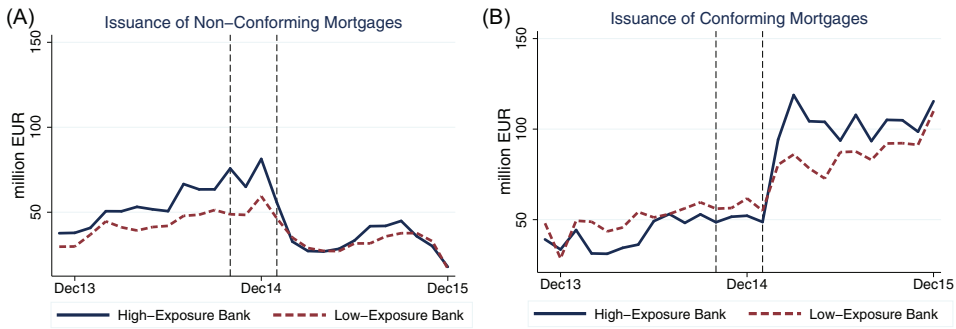


Figure 9. Issuance and bank exposure. The figure shows the issuance of nonconforming (left panel) and conforming (right panel) mortgages for high-exposure (solid line) and low-exposure (dashed line) banks. The vertical lines indicate the first rumors and the implementation date of the limits. All time series are seasonally adjusted. Source: Central Bank of Ireland. (Color figure can be viewed at wileyonlinelibrary.com)

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 nonrandom 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 levels to account for the fact that 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 $E[u_i, u_j | x_i, x_j] = 0$, 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

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 a county-month-bank-income bucket. The dependent variables are the logarithm of mortgage volume, the logarithm of the median loan size, number of mortgage loans issued, value-weighted LTV, value-weighted LTI, and the value-weighted rate. *Exposure* is defined in Section IV, 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Central Bank of Ireland.

Panel A						
	Volume	Size	No. Loans	LTV	LTI	Rate
Distance \times Exposure \times Post (β_1)	9.055*** (1.031)	14.206** (6.708)	-27.266 (20.061)	325.225** (151.244)	2.770 (3.056)	-3.483* (1.744)
Distance \times Exposure (β_2)	-13.071*** (3.252)	-15.777* (8.228)	15.141 (20.849)	-367.510** (143.516)	-3.574 (2.747)	4.484*** (1.567)
Distance \times Post (β_3)	0.150*** (0.033)	0.286*** (0.091)	-0.577 (0.392)	10.262*** (0.735)	0.190*** (0.052)	0.021 (0.014)
Fixed Effects						
County-Time	✓	✓	✓	✓	✓	✓
County-Bucket	✓	✓	✓	✓	✓	✓
Bank-Time	✓	✓	✓	✓	✓	✓
Observations	12,960	12,960	12,964	12,757	12,708	12,577
R ²	0.603	0.477	0.518	0.223	0.451	0.543
Panel B						
	Volume	Size	No. Loans	LTV	LTI	Rate
Distance \times Exposure \times Post (β_1)	11.596*** (0.706)	19.219** (8.594)	-52.616 (31.117)	273.233** (130.195)	3.652 (3.643)	-3.156 (2.245)
Distance \times Exposure (β_2)	-15.861*** (3.279)	-20.073* (9.642)	32.335 (26.676)	-375.119** (133.923)	-4.942* (2.710)	5.200* (2.622)
Fixed Effects						
Bank-Time	✓	✓	✓	✓	✓	✓
County-Bucket-Time	✓	✓	✓	✓	✓	✓
Observations	12,960	12,960	12,964	12,757	12,708	12,577
R ²	0.796	0.768	0.647	0.656	0.740	0.764

two-way clustering at the county-bank and income bucket levels. The positive coefficient β_1 shows that the credit reallocation documented in Section II is driven primarily by banks more exposed to the limits. Given our definition of bank exposure, the opposite signs of β_1 and β_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

C. Other Asset Classes

In the previous sections, we show that after the policy introduction, banks issued mortgages with similar characteristics as the 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 prepolicy 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:

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

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

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

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 of interest is the triple interaction term between the bank exposure variable, a *Post* dummy equal to one in the postperiod, 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

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

Table VII
Bank Portfolio Reallocation, Holdings of Securities

This table shows the estimation results from specification (8). The unit of observation is a 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 IV, *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 are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Central Bank of Ireland.

	Net Buys				Buys	Sells
Exposure \times Yield \times Post	0.097** (0.042)	0.084* (0.050)	0.289*** (0.083)	0.490*** (0.118)	1.786*** (0.587)	-2.234*** (0.514)
Fixed Effects						
Time	✓					
Security	✓	✓				
Bank-Time		✓		✓	✓	✓
Security-Time			✓	✓	✓	✓
Observations	7,707	7,707	7,707	7,707	7,707	7,707
R^2	0.245	0.281	0.943	0.950	0.919	0.917

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. The variable *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 versus 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

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

We measure the credit provided by bank b to firms in county c and 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 et al. (2018)). We form clusters based on county and industry because firms in a particular industry in a particular county share many characteristics and thus are 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 employ 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 the log (stock of) credit granted ($\Delta VOLUME$) or the change in the interest rate charged ($\Delta RATE$). Similar to the previous section, we are interested in the coefficient on 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 report the estimation results in Table VIII. In Panels A and B, the dependent variable is the change in the volume of credit and the change in the interest rate charged, respectively. Column (1) considers the full sample. The estimates show 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 nonrisky (rating 1–4) firms and rerun our specification (9) separately for these two groups of borrowers. The estimation results in columns (2) and (3) show that although a credit expansion in the corporate sector occurs for both risky and nonrisky firms, the effect is economically and statistically more pronounced for risky firms relative to the preperiod. A one-standard-deviation higher *Exposure* leads to a 6.3 pp higher credit supply to firms and a 10.3 pp higher credit supply to risky firms. These results are confirmed in the last column of Panel A, where we employ the triple interaction between our bank exposure variable, a *Post* dummy, and a dummy for whether the borrowing firms are risky. The coefficient shows that the increase in loan volume is driven mostly by an increase toward risky firms. Similarly, in Panel B we find that the decrease in the cost of bank loans mostly benefits 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 Irish businesses only and do not focus on the global portfolio of the internationally active banks.

Table VIII
Bank Portfolio Reallocation, Credit to Firms

This table shows the estimation results of specification (9). The unit of observation is at the bank-industry-county-quality-time level. The sample runs at a biannual frequency from 2013H1 to 2016H1. *Exposure* is defined in Section IV 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 the 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 are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Central Bank of Ireland.

Panel A				
LHS: $\Delta VOLUME$	Sample			
	Total	Risky	Nonrisky	Total
Exposure \times Post	5.022*** (1.544)	8.137*** (2.438)	3.115* (1.825)	
Exposure \times Post \times Risky				5.109** (2.034)
Exposure \times 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^2	0.498	0.520	0.491	0.489
Panel B				
LHS: $\Delta RATE$	Sample			
	Total	Risky	Nonrisky	Total
Exposure \times Post	-3.117*** (0.974)	-7.051** (2.587)	-0.950 (1.325)	
Exposure \times Post \times Risky				-6.822** (2.893)
Exposure \times 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^2	0.476	0.501	0.463	0.484

Table IX
Aggregate Banks' Balance Sheet

This table shows 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.

	Aggregate			Affected Banks			Nonaffected Banks		
	2014	2015	Δ	2014	2015	Δ	2014	2015	Δ
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

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 nonaffected groups. Importantly, in the raw data these three categories make up 80% to 90% of the actual balance sheet size 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 1.7 pp. This decrease is associated with a corresponding increase of 1.1 pp in corporate lending and 0.6 pp 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 nonaffected banks, reduced their mortgage credit by 3.9 pp and increased their corporate lending by 2 pp and security holdings by 1.9 pp. Conversely, nonaffected 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 7 pp from 29% to 36% for affected banks.

The evidence presented in this section suggests that banks' credit reallocation is driven directly 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.

V. Financial Stability

In the previous section, we show 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, nonfinancial 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, nonfinancial nonreal 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 into three groups: bank stocks, real estate stocks, and nonfinancial nonreal estate stocks. We also obtain 10-year Irish government bond yields from Reuters Datastream. We estimate the following specification:

$$R_t^{banks} = \alpha + Post_t + \sum_{i \in \mathcal{I}} (\beta^i R_t^i \times Post_t + R_t^i) + \epsilon_t, \quad (10)$$

$$\mathcal{I} = \{\text{Real Estate, Firms, Govt}\},$$

where the dependent variable is the mean return of bank stocks and the independent variables are (i) the mean return of real estate stocks (to capture mortgage credit), (ii) the mean return of nonfinancial nonreal estate stocks (to capture the nonfinancial private sector), and (iii) the change in the 10-year Irish government bond yield (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 *Post* is a dummy equal to one starting on February 9, 2015.

We report the estimation results in Table X. The interaction coefficients suggest that banks become less exposed to real estate and more exposed to the nonfinancial nonreal 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 and four months around the implementation date, respectively. Our results continue to hold. 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 nonfinancial nonreal 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.

VI. Macroeconomic Effects

In this section, we present data on household savings and the 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

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 two and four months around February 9, 2015, respectively. In columns (4) and (5), the dependent variable is the return of exposed (above-median exposure) and nonexposed (below-median exposure) banks, respectively. In columns (6) and (7), the sample period runs three months around June 9, 2015 and June 9, 2014, respectively. ****p* < 0.01, ***p* < 0.05, **p* < 0.1. Source: Reuters Datastream.

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

Office (CSO), the Irish national statistical office.³⁰ The top panels show 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 both age and income. The survey also asks households for the motive behind their savings behavior and finds that the “purchase own home” motive almost doubles from 8.7% of 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 of age exhibit a 20% drop in homeownership rate, a significantly larger reduction than any other age group. Finally, the increase in savings rates is driven almost entirely by renters. The survey evidence shows that households closer to the lending limits (young and low

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

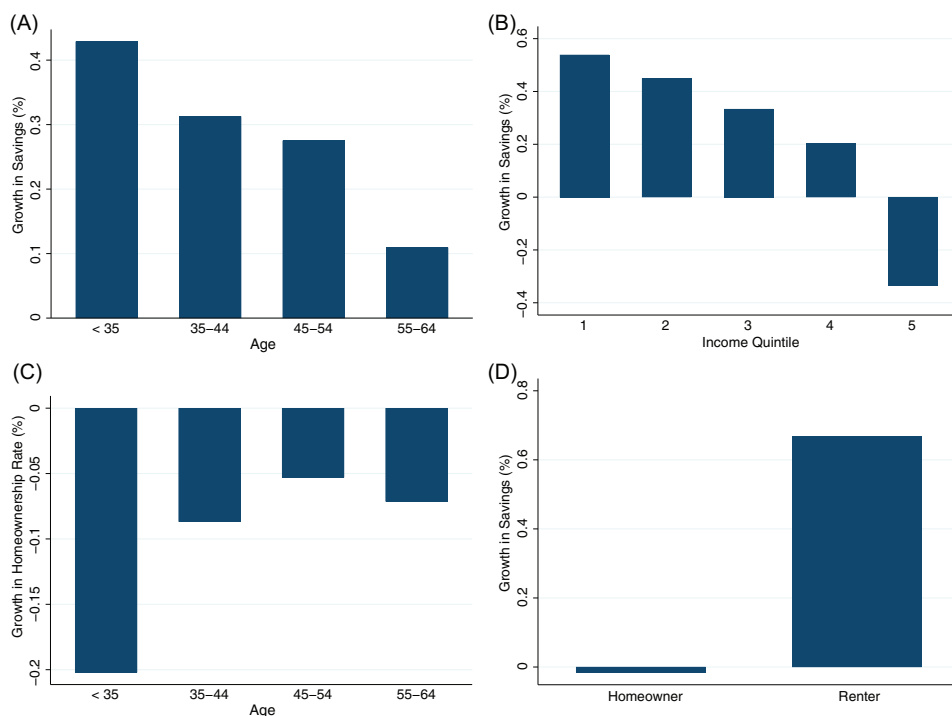


Figure 10. Savings behavior and homeownership rates. This figure shows the change in the savings rate and in the homeownership rate for different types of households. The changes are calculated over two survey waves conducted by the Irish Central Statistics Office (CSO) in 2013 and 2018. The growth in savings depicts the percent change in the median savings reported by the households in each group. The growth in the homeownership rate represents the percent change in the fraction of households that report owning a home. Source: Irish CSO. (Color figure can be viewed at [wileyonlinelibrary.com](https://onlinelibrary.wiley.com))

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 into a debt trap (Mian, Straub, and Sufi (2021)). 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, which reduces 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 the economy.

Finally, the results presented in Section IV.C and Section V 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.

VII. Conclusion

In this paper, we provide a comprehensive microlevel 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 led to 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.

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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 into 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 determined mostly 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 (Bank of International Settlements (2019)). The required capital depends on the predicted probability of default, according to the

formulas (i) $K = (LGD * N(\frac{G(PD)}{\sqrt{1-R}} + \sqrt{\frac{R}{1-R}} * G * (0.999)) - PD * LGD)$ and (ii) $RWA = K * 12.5 * EAD$, where R is the correlation among mortgages (set to 0.15 for all banks), EAD is exposure at default, LGD is loss given default, N is the standard normal distribution, and G is the inverse of the standard normal distribution. While LGD and EAD depend largely on the size of the loan and the value of the collateral, with LGD additionally depending on country-specific factors (e.g., right of recourse, regulatory LGD floor), the probabilities of default 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 using historical data. To mimic banks' (unobservable) models in a transparent way, we regress a default dummy variable on mortgage characteristics. In Table A.I, 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, loan to income [LTI], loan to value [LTV], loan size, and geography) and borrower characteristics (borrower age, first-time buyer [FTB] status, buy-to-let [BTL] status, and relationship status). The estimation results are intuitive. Riskier loans tend to be larger, have a higher interest rate, and 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 A.1, 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 A.I. Consistent with the estimation results, borrowers in Dublin (and affected counties in general), FTBs, lower-income borrowers, and younger borrowers are less likely to default (holding constant all other independent variables). Discussion of the effect of income in the 2007 to 2010 bust is outside the scope of this paper. However, this table is consistent with the observation that higher-income borrowers increased their leverage markedly 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 of the paper, 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.

Table A.I
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 ratio, 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 indicating whether the buyer was a first-time buyer (*FTB*), a couple, or a buy-to-let (*BTL*) investor. Standard errors clustered at the county level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: Central Bank of Ireland.

	Default	Default	Default	Default	Default	Default
Interest Rate	0.243*** (0.008)	0.244*** (0.008)	0.231*** (0.008)	0.242*** (0.008)	0.243*** (0.008)	0.230*** (0.008)
LTI	0.034*** (0.003)	0.038*** (0.003)	0.057*** (0.003)	0.034*** (0.002)	0.038*** (0.003)	0.057*** (0.003)
LTV	0.001*** (0.000)	0.001*** (0.000)	0.002*** (0.000)	0.001*** (0.000)	0.001*** (0.000)	0.002*** (0.000)
Borrower Age	0.003*** (0.001)	0.002*** (0.001)	0.003*** (0.001)	0.002*** (0.001)	0.002*** (0.001)	0.003*** (0.001)
Income		0.016*** (0.003)	0.033*** (0.004)		0.015*** (0.003)	0.032*** (0.004)
House Price		-0.019** (0.008)	-0.003 (0.002)		-0.020** (0.008)	-0.003 (0.002)
Loan Size	0.081*** (0.012)	0.076*** (0.012)		0.079*** (0.013)	0.075*** (0.013)	
Dublin	-0.143*** (0.028)	-0.144*** (0.028)	-0.121*** (0.026)			
Urban				-0.155*** (0.028)	-0.155*** (0.028)	-0.141*** (0.027)
FTB	-0.185*** (0.017)	-0.186*** (0.017)	-0.199*** (0.017)	-0.182*** (0.017)	-0.183*** (0.017)	-0.197*** (0.018)
BTL	0.106*** (0.037)	0.103*** (0.037)	0.114*** (0.036)	0.112*** (0.036)	0.109*** (0.036)	0.120*** (0.034)
Couple	-0.035*** (0.010)	-0.035*** (0.010)	-0.023** (0.010)	-0.034*** (0.010)	-0.035*** (0.010)	-0.023** (0.010)
SVR	0.603*** (0.017)	0.604*** (0.017)	0.596*** (0.017)	0.601*** (0.016)	0.601*** (0.016)	0.594*** (0.017)
Tracker	0.886*** (0.027)	0.887*** (0.027)	0.861*** (0.026)	0.884*** (0.026)	0.886*** (0.026)	0.859*** (0.026)
Year FE	✓	✓	✓	✓	✓	✓
Observations	336,906	336,906	336,906	336,906	336,906	336,906
Pseudo R^2	0.067	0.067				

Average Default before 2013 (LHS) & Probability of Default 2013-2014 (RHS)

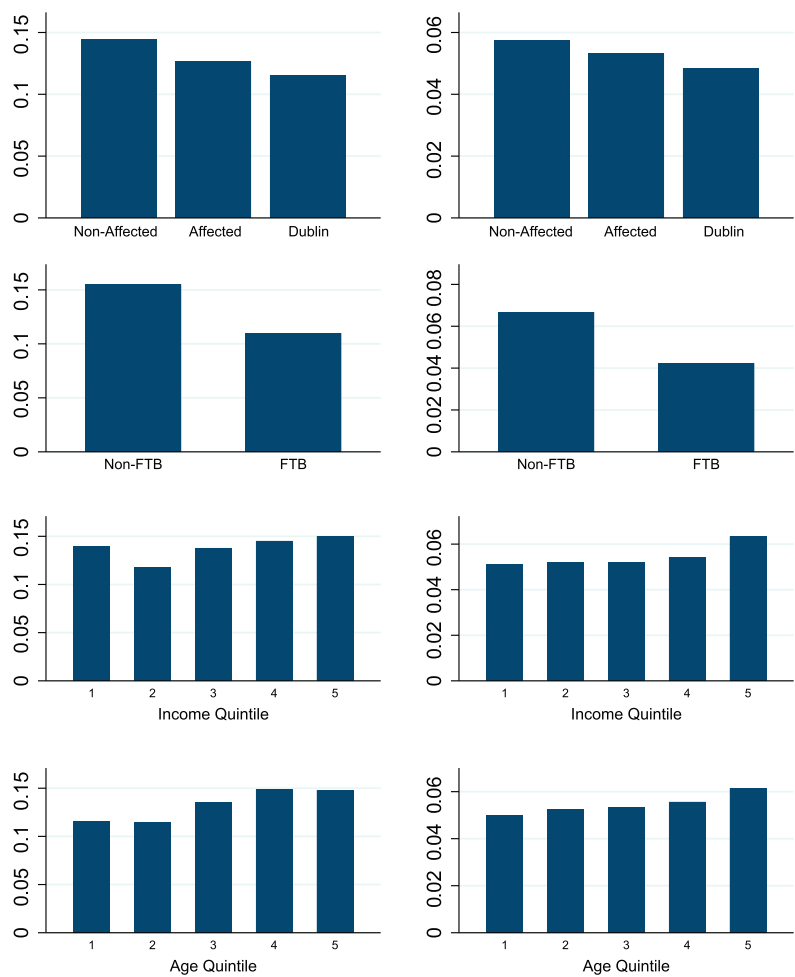


Figure A.1. Default rates. This figure shows average defaults (left column) and the estimated probability of default (right column). The estimated probability of defaults is obtained using the probit model in the first column of Table A.I. Source: Central Bank of Ireland. (Color figure can be viewed at wileyonlinelibrary.com)

Appendix B: Additional Figure

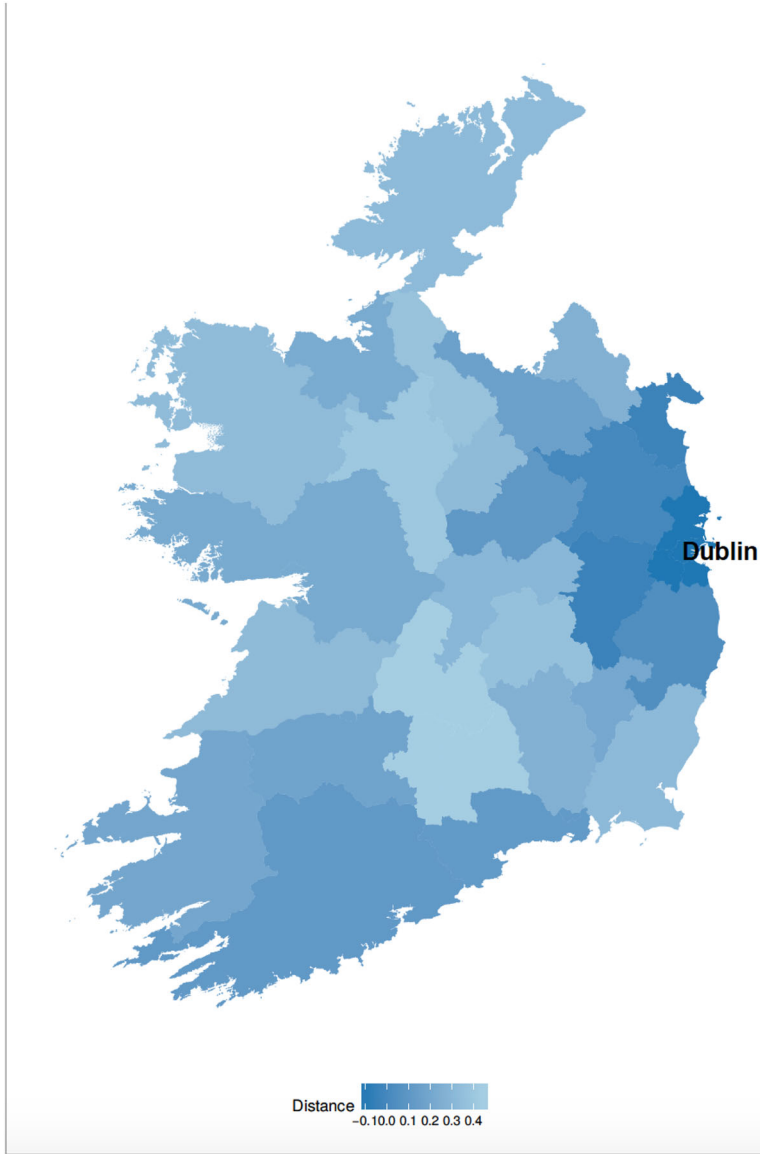


Figure B.1. Counties and lending limits. This figure shows the county-level distance from the limits. Darker colors indicate less distant counties. Source: Central Bank of Ireland. (Color figure can be viewed at [wileyonlinelibrary.com](https://onlinelibrary.wiley.com/doi/10.1111/jofi.13170))

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.
Replication Code.