Spatially Targeted LTV Policies and Collateral Values [*]

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Abstract

Many governments regulate household leverage at a national level, even when credit and housing market conditions vary substantially across locations. We explore the efficacy of loan-to-value (LTV) limits targeted towards specific neighborhoods as a macroprudential policy designed to curb local house price growth. We combine administrative data from Taiwan covering the universe of mortgage loans, personal income tax returns, a public database of geocoded housing transactions, and bank branch balance sheets. Applying a series of matched difference-in-differences and border difference-in-discontinuity designs, we find that leverage limits are effective at reducing local house prices compared to alternative policy instruments such as transfer taxes, with no effects on delinquency rates. In response to a statutory tightening of the maximum LTV ratio to 60% from the standard 80% for mortgages on second homes, house prices decline by 6% in policy catchment areas relative to nearby neighborhoods not subject to LTV restrictions. However, we uncover two kinds of efficiency costs associated with place-based mortgage restrictions: (i) real commuting costs driven by homeowners sorting into neighborhoods where credit is easier to obtain, and (ii) mispricing, or "noise" costs, as banks and prospective homebuyers face incentives to obtain inflated appraisals to avoid the limits.

Keywords: loan-to-value ratio, place-based mortgage restrictions, macroprudential policy, intermediation, collateral misreporting, house prices, border discontinuity, location sorting

JEL classifications: E61, G21, G28, R21, R31, R38

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1 Introduction

With property values in many major real estate markets skyrocketing since the Global Financial Crisis, policymakers have been experimenting with combinations of credit constraints and sales or capital gains taxes to moderate housing price growth and prevent systemic risks to the banking sector. Among the most common macroprudential policy instruments are risk weights, reserve requirements, and strict loan-to-value (LTV) caps imposed on banks. Cerutti et al. (2018) count 97 distinct episodes of tightening and loosening caps on LTV ratios in mortgage markets in their database spanning 64 countries over 2000-2014. However, despite the fact that credit and housing market conditions vary substantially across locations, all of these policy interventions entailed rules that applied nationwide rather than locally.

We provide new evidence of how conditioning mortgage credit provision on *ex ante* local house price growth can cool housing markets, subject to both real and financial tradeoffs. We study a set of top-down mortgage market interventions through which the Central Bank of Taiwan imposed maximum allowed LTV ratios for particular neighborhoods which were experiencing rapid house price growth. Using a series of border difference-in-discontinuity designs, in response to a drop in the maximum LTV ratio to 60% from the standard 80%, we find that house prices decline by 6% in catchment areas relative to nearby neighborhoods not subject to LTV restrictions. The price drop is concentrated in neighborhoods which on the eve of the reforms experienced the largest house price gains, and the effect is quantitatively unchanged after conditioning on address-level differences in transit access, property characteristics, as well as topographic heterogeneity around different policy border segments. The idea underlying our border discontinuity strategy for uncovering effects of the reforms is that the government's selection of neighborhoods within cities created multi-dimensional discontinuities in latitude-longitude such that we can compare otherwise similar groups of properties, with common taxing jurisdictions, except that a subset are eligible for mortgages with higher leverage ratios.

Interestingly, house price levels in leverage regulated areas do not recover following the removal of all LTV restrictions and remain flat for several years thereafter. We argue that the succession of increasingly stringent reforms negatively altered investors' expectations about the path of future house prices, leading to persistently depressed demand for homes in treated areas. This is consistent with the survey experiments conducted in Fuster & Zafar (2016, 2021) who show the negative effects of downpayment constraints on home purchase decisions continue even after such hypothetical constraints are relaxed. The macro time series patterns echo our conclusions using finer definitions of treatment. Price levels in previously hot markets remained stable to such an extent that the Central Bank enacted no further mortgage market reforms until 2021, when work-from-home behavior during the COVID-19 crisis quickly pushed up demand for residential space.

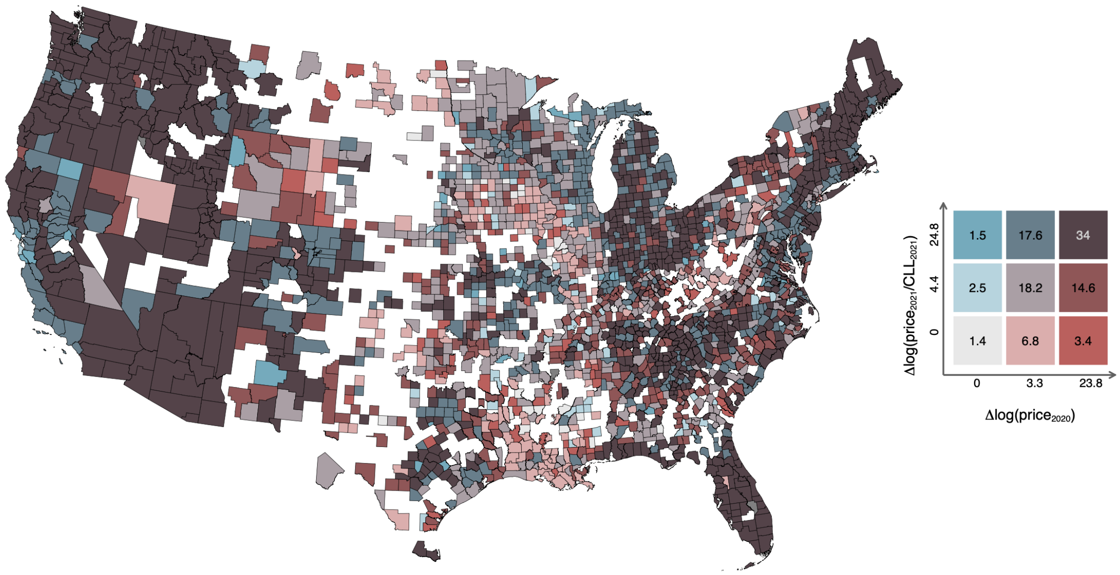
Matched difference-in-differences estimates at the loan-level reinforce the findings in the more macro analysis in which we look at spillovers to untreated loans attached to properties located in regulated areas. We match pairs of loans within treated areas on the parent bank and borrower characteristics which appear on standard mortgage loan applications (e.g. income, age, education), but each loan is on either side of the statutory LTV cutoff. Banks pass through the reduced costs from paying mortgage insurance premia to borrowers in the form of lower interest rates. For both reforms we consider, the loan amount and price per square meter decline after the tightening, while there is no change in other contract terms (e.g. loan maturity). The results hold even after controlling for observable differences in property characteristics or varying the bandwidth for the matched sample of loans with LTVs just above or just below the statutory threshold. Hence, even within the group of treated neighborhoods, localized strict LTV limits reduce housing demand.

Mortgage policy can be place-based even if not directly targeting particular parts of the country. For instance, Gupta, Hansman, & Mabille (2022) show that geographic variation in the eligibility for FHA loans, which are subsidized loans allowing U.S. borrowers to make downpayments as low as 3.5%, effectively closes off access to high cost of living neighborhoods to Black borrowers who are more likely to struggle to make higher downpayments. Another example is the conforming loan limit (CLL) – a home value cutoff above which banks cannot sell a loan to Fannie Mae and Freddie Mac. Jumbo loans above the CLL are subject to more stringent applicant screening and carry higher interest rates. While there is some cross-county variation in the CLL and FHA limits, both limits are set according to national formulas which track national rather than local house price growth. This is in contrast to the policies we examine in this study in which the government conditioned neighborhood-level credit rationing on local house price trends.

To illustrate the distinction between place-based and spatially targeted mortgage policies like the ones we analyze here, Figure 1 plots the bivariate distribution across U.S. counties of YOY log changes in the ratio of 2021 house prices to the 2021 CLL (y-axis) against log house price growth during the early COVID-19 outbreak period, measured from 2019Q3 to 2020Q3 (x-axis). The ratio of prices to the CLL captures the tightness of credit conditions due to regulation of the secondary mortgage market. Counties on the top right-hand side of the 2×2 grid where price growth is pronounced and the CLL is becoming more binding are macroprudentially regulated, whereas blue areas are over-regulated relative to the recent path of local house prices. Policies like the FHA and CLL thresholds are akin to the "soft" LTV limits enacted elsewhere, which can take the form of risk weights or capital requirements for banks approving higher leverage mortgages. Our findings speak to both the question of the effectiveness of soft vs. strict LTV limits and point to what might happen if the formula determining the CLL in Figure 1 were altered so that the credit rationing in each county were indexed to recent house price growth in a macroprudential fashion.

A key complication in answering whether spatially targeted vs. nationwide leverage restrictions are preferable is that by exacerbating variation in credit access regulators risk exporting property booms to less regulated areas. This is because borrowers may substitute away from purchasing homes in neighborhoods where more upfront cash is required, thereby sorting into places where homes were previously in less demand. We use our border discontinuity research design to isolate and quantify this spillover by comparing treatment effects with and without excising a "donut hole"

FIGURE 1. House Price Growth vs. Conforming Loan Limits during the COVID-19 Boom



Notes: The figure shows a two-way map comparing U.S. county-level log house price growth between 2019Q3 and 2020Q3 (x-axis) to the log change in the ratio of 2021 prices relative to the conforming loan limit (CLL) for that county-year observation (y-axis). The ratio of prices relative to the CLL is one measure of the extent to which leverage limits bind in a given part of the country. Counties which are shaded more towards the blue part of the scale are those for which *ex ante* price growth was low relative to the change in single-family home affordability with standard mortgages. On the other end, counties shaded towards the red end of the scale are those for which *ex ante* price growth was high, and yet affordability did not change much. Counties in white represent counties for which the housing market is too thin to estimate a price index based on transaction volume. Numbers within the grid boxes represent the percentage of counties in that grid cell, whereas the axis ticks are terciles. In Appendix A, we describe the data sources and computations underlying this figure, and present similar figures showing the geographic dispersion in how much leverage restrictions bind for other time periods and specific types of mortgage loans.

area located on the policy border.

Borrowers do migrate across the policy border to avoid the more restrictive credit regimes, leading to an uptick in mortgage originations and prices in nearby untreated border neighborhoods. However, this behavior is limited to unregulated areas very close the border, such that positive cross-border demand spillovers quickly decay to zero at distances beyond 4 km, as commuting costs increase exponentially in utility terms further from the center of the commuting zone (Monte, Redding, & Rossi-Hansberg 2018). At most, 2 p.p. of our estimated 6% drop in housing prices is due to spatial contamination of the difference-in-differences. Once we subtract out this spillover, we obtain a price-leverage ratio elasticity of roughly 1. Overall, we conclude spatially targeted LTV policies can be effective at curtailing price growth in hot housing markets without exporting local housing booms to other, nearby neighborhoods. Such policies effectively smooth out house price growth over slightly larger areas.

On top of the real cost of distorting household location choices by inducing borrowers to buy homes, another issue is that the concept of value in assessing compliance with LTV limits is open to interpretation by borrowers and lenders. Both counterparties might seek artificially inflated appraisal values to report lower LTVs, leaving the loan amount unchanged. Avoidance through misreporting of collateral values was rampant during the U.S. 2000s subprime mortgage era, when buyers and sellers colluded to inflate sale values (Ben-David 2011), and banks encouraged real estate appraisers on their payroll to inflate home values (Agarwal, Ben-David, & Yao 2015; Griffin 2021). During that period, due to the "originate-to-distribute model," banks were also complicit in exaggerating the quality of mortgages sold to RMBS pools by engaging in lax screening (Keys et al. 2010; Purnanandam 2011), inserting false information about borrower home equity values into contractual disclosures (Piskorski, Seru, & Witkin 2015), or failing to report second liens and owner-occupancy status (Griffin & Maturana 2016a,b).

Given that appraisals are sticky and thus influence future sale prices (McMillen & Singh 2022), a cost to LTV caps is that they can contribute to local mispricing ("noise") in the housing market, undoing some of the stated objectives of these policies to achieve affordability. This drawback applies to any credit rationing rule anchored to non-market values. Consequently, we find prices continued to rise, and sales volume increased by 16% in treated relative to non-border untreated neighborhoods following an earlier series of Taiwan's LTV reforms which defined leverage caps in terms of a bank-provided appraisal value. In contrast, once the rule was changed so that the cap was defined as a fraction of the transaction price or appraisal value, whichever was lower, prices declined in treated areas, and sales volume cratered by 23% in treated relative to untreated neighborhoods away from the border.

We formally test for collateral misreporting by introducing the notion of an "appraisal gap," or the difference between the lender's appraised value for the house and what the government thinks it is worth towards calculating the property tax base. In treated areas, the gap between bank and government appraisals increases by 13% after the lowering of the LTV cap to 60%. The appraisal gap remains even if we compare capped and uncapped loans issued by the same parent bank, or run specifications allowing for benign time drift in between tax appraisal and loan origination dates. While previous studies have instead focused on the gap between loan-to-value and loan-to-price ratios (Montalvo & Raya 2018; Galán & Lamas 2023), using measures which are a function of transaction prices to quantify the degree of collateral misreporting is unsatisfactory because, as we show, tightening leads to local price declines, which would lead to a mechanical increase in the gap between appraised values and transaction prices even if no misreporting occurred. An advantage to our setting is we know the precise formula determining for-tax purpose appraisals in each location and tax year, and therefore can benchmark lenders' appraisals against an official non-price valuation.

Academic interest in the potential power of LTV limits as a macroprudential tool has also exploded since the 2000s global boom-bust cycle. Yet, evidence on the effectiveness of these policies at lowering prices is mixed. de Araujo, Blanco Barroso, & Gonzalez (2020) study a lowering of the LTV limit from 96.5% to 90% in Brazil and conclude treated borrowers finance homes that are 4-6% more affordable after conditioning on zip code fixed effects. Han et al. (2021), in contrast, document bidding wars for properties priced just below the \$1 million threshold for a lower LTV cap in Toronto.[1] A more general source of tension in this literature is that whether LTV limits bind is a statement about individuals, not the overall market. Hence, a finding that prospective home buyers treated by the policy substitute towards cheaper housing, as in the case of Israel's 2010 LTV tightening (Laufer & Tzur-Ilan 2021), could also be consistent with an overall muted effect on prices if lower price growth in one neighborhood is offset by increased price growth in another.[2]

We contribute to the large literature on regulating household leverage by accounting for spatial tradeoffs inherent in LTV policies. Uniquely, Taiwan's LTV experiments singled out specific neighborhoods in three large cities by imposing more stringent 60% LTV caps for second home purchases. In 2009, Korea introduced "speculative zones" in Seoul which were subject to lower LTV limits (Igan & Kang 2011), and in 2016 Norway imposed additional credit-tightening measures on second home buyers in Oslo relative to buyers in the rest of the country (Aastveit, Juelsrud, & Wold 2020). With the exception of Acharya et al. (2022) who show loan originations shift away from urban towards rural areas in Ireland, and Tzur-Ilan (2023) who provides evidence that households purchase homes in lower-SES areas in response to Israel's soft and strict LTV limits, the spatial dimensions of borrower and lenders' responses to capping leverage ratios remain underexplored.

While many studies have emphasized the successes of LTV ratio caps in curbing credit growth in emerging markets (e.g. Akinci & Olmstead-Rumsey 2018), others have documented substitution towards unregulated sources of credit to finance home purchases (DeFusco & Paciorek 2017). A running theme in the work on mortgage credit limits is that LTV policies may have muted effects on the overall amount of household debt originated. Given the rise of shadow banking and fintech in the U.S., targeting of intermediaries is another issue at stake for policymakers (Buchak et al. 2018). Consequently, environments such as the Netherlands where borrower alternatives to bank-based mortgage credit are limited have had more success at reducing leverage via LTV constraints (Van Bekkum et al. 2022). Buyers in Taiwan have limited financing options outside traditional bank-issued mortgages, and thus the effects on quantities in our setting are dramatic.

Finally, we acknowledge that regulators have many policy instruments at their disposal to achieve the twin goals of improving housing affordability and preventing excessive risk-taking by

[1] There is a large related literature in macro-finance which finds a robust causal relation between credit provision and house prices at more aggregated levels. Related to the map in our Figure 1, Loutskina & Strahan (2015) use the fraction of CBSA-level lending below the CLL as a shock to house price growth; Greenwald & Guren (2021) use this instrument to calibrate a structural model in which credit conditions explain between one-third and one-half of the 2000s U.S. boom. Favara & Imbs (2015) use the deregulation of interstate banking as a shock to mortgage credit supply. Blickle (2022) adopts distance between competing banks as an instrument for deposit growth in Switzerland. Mian & Sufi (2022) document MSA-level variation in shadow bank-financed mortgages led to more speculative investment and a rise in house price growth expectations.

[2] Other papers focus on heterogeneous effects of leverage limits on household decisions. Higgins (2021) studies Ireland's 2015 LTV tightening and shows poorer borrowers respond by buying cheaper houses, while richer households take out smaller loans. In the Finnish context, Eerola et al. (2022) show that strict LTV limits disproportionately impact below-median income renters who are looking to buy homes for the first time. Kabaş & Rozbach (2021) study a Norwegian LTV reform and argue that policies which reduce household leverage can have positive effects on labor market outcomes by encouraging job search effort among displaced workers.

the financial sector. Transfer taxes on housing sales are, in theory, isomorphic to downpayment constraints to the extent that sellers pass through the tax to the buyer by charging higher prices, thus necessitating more cash for the downpayment (Koetter, Marek, & Mavropoulos 2021). In our companion paper (Chi, LaPoint, & Lin 2022), we use causal estimates from a reform to structurally estimate a heterogeneous investor model and find that transfer taxes are largely unsuccessful at achieving a moderation in housing price growth and generate large welfare losses equal to 56% of housing consumption. Soft LTV limits which impose a "tax" on banks who originate high leverage loans also appear ineffective at influencing prices and default rates (DeFusco, Johnson, & Mondragon 2020). Although we find no impact of changes in maximum leverage ratios on subsequent delinquency rates, we conclude spatially targeted LTV limits offer a relatively efficient way to dampen house price growth relative to these other policies.

The remainder of the paper proceeds as follows. Section 2 offers background details on the mortgage market in Taiwan and offers a timeline of recent household leverage restrictions. Section 3 describes the data sources we draw on for our analysis. Section 4 introduces the matched difference-in-differences and border difference-in-discontinuities methodologies we apply to LTV restrictions in this context. Section 5 examines how LTV limits influence home values, loan contract terms, bank behavior, and household location sorting for different segments of the housing market. Section 6 concludes with a discussion of implications for macroprudential policy.

2 Background: Mortgage Market in Taiwan

We describe mortgage loan contracts and the bank regulatory environment in Taiwan. We provide a timeline of policy changes to loan-to-value (LTV) limits we consider in our empirical analysis.

2.1 Key Mortgage Contract Features

Borrowers can obtain either fixed rate mortgages (FRMs) or adjustable rate mortgages (ARMs) in Taiwan. Fully amortizing ARMs with terms between 15 and 30 years are the dominant form of contract for non-government sponsored loans.[3] Banks do not offer FRMs except under special government sponsored programs.[4] ARMs are typically indexed to the one-year benchmark deposit interest rate, which is set by the Central Bank of the Republic of China (Taiwan). Banks set the interest rate on an ARM loan to be a fixed margin above the one-year (or two-year for ARMs with

[3]ARMs are the most prevalent type of contract in many countries outside the U.S., including Australia, Greece, Ireland, Italy, Sweden, Finland, Portugal, and Spain. All 13 OECD countries in the panel of aggregate loan volumes constructed by Badarinza, Campbell, & Ramadorai (2018) have higher ARM market share than the U.S. In most non-OECD countries, ARMs are either the dominant or only type of contract (Cerutti, Dagher, & Dell'Ariccia 2017).

[4]There are two dozen government sponsored programs offering FRMs which cater to younger, low-income households purchasing owner-occupied properties, civil servants, indigenous peoples, and households affected by disasters. These loans are exclusively issued by one development bank directly owned by the government, and constitute less than a 0.01% market share. We exclude such government-backed loans from our analysis.

a two-year reset period) certificate of deposit interest rate they offer to consumers. Hence, both the index and margin components of the interest rate have time-varying bank-specific components.

Mortgage lending is regulated by the Financial Supervisory Commission (FSC). Both the Central Bank of Taiwan and the FSC are under the executive branch of the government. The FSC sets standards for collateral valuation, requires lenders to conduct thorough credit checks, and regulates the fees and charges that can be applied to mortgages. While the Central Bank has the authority to mandate strict LTV limits or capital requirements, the FSC is tasked with ensuring lenders adhere to limits dictated by the Central Bank.

Lenders typically determine the appraisal value of a property by enlisting an independent appraiser to verify the LTV ratio, but can alternatively use proprietary automated valuation models (AVMs). The FSC may conduct regular inspections and audits to ensure that lenders are adhering to these standards and to identify potential risks in the mortgage market. When conducting an appraisal, lenders are required to consider factors such as the property's location, size, age, condition, zoning classification, and local market trends. Additionally, mortgage insurance is required for all mortgages in Taiwan, and insurance premia are usually rolled into the monthly mortgage payment. This is in contrast to places like the U.S., U.K., and Canada, where the borrower is only required to pay insurance premia as part of the monthly payment while the mortgage LTV exceeds some threshold ratio (80% in the U.S.). The fact that mortgage insurance is always required means interest rates are more tightly linked to the borrower's choice of LTV ratio.

There are two features about Taiwan's regulatory system which render strict LTV limits a potentially more effective tool for curbing housing demand. One is that the non-traditional, or shadow banking, sector accounts for a negligible dollar share of all mortgage loans originated, less than 2% over our sample period. 83% of loans are issued by banks, 7% by credit unions, 9% by insurance companies, and 1% by post office savings banks. Second, even if shadow banks can gain market share from traditional lenders in response to tightening LTV restrictions by offering more attractive contract terms, the LTV limits applied uniformly to all lenders. This means prospective borrowers in targeted regions cannot simply avoid leverage limits by obtaining loans from non-traditional banks. Still, even if shadow banks are subject to strict LTV limits, homebuyers might seek personal loans or credit lines to comply with an increase in downpayment requirements. We confirm that there is no run-up in unsecured consumer credit in targeted cities relative to non-targeted cities following declines of the maximum allowable LTV ratio.

Finally, banks approve mortgage loan applications subject to borrower characteristics and capital requirements, both of which are common features in well-developed mortgage markets such as the U.S. Banks use the borrower's credit score, age, occupation, income, education, and information about the household's balance sheet to screen applications. There is a 60% debt-to-income (DTI) ratio, whereby banks are unlikely to approve loans to borrowers if monthly existing plus new debt expenditure would exceed 60% of monthly income.[5] For borrowers, total (real + financial) marked-to-market assets must be 150% of total debt. Overall, the screening process and contract characteristics for mortgage loans in Taiwan mirror those of other mortgage markets.

2.2 Timeline of Leverage Restrictions

Like many governments and banking regulatory agencies worldwide, the Central Bank of Taiwan became concerned about the sharp rise in house prices in particular neighborhoods in the northern part of the island. After the Global Financial Crisis, it was alleged in the popular media that rapid house price appreciation was tied to owners taking out several mortgages to buy investment properties. This led the Central Bank to enact a series of restrictions on maximum loan-to-value (LTV) ratios for certain property segments and geographic submarkets. In Table 1, we list all recent reforms to allowable LTVs for mortgage loans. Prior to 1989, there was no regulatory limit on loan size, after which the maximum LTV was set to 140% of the official (i.e. for tax purposes) appraisal value. Despite there being such a high leverage limit, the lack of a centralized public mortgage insurer like the FHA in the U.S. led lenders to require a 20% downpayment to originate most home mortgages. On the eve of the recent reforms which began in June 2010, fewer than 5% of loans were issued at an LTV above 80%.

In the main empirical results of Section 5 we focus on the LTV regulatory regimes created by the borders and 60% maximum ratios set in June 2010, December 2010, and June 2014. These reforms are unique in a worldwide context in that the leverage ratio restrictions only apply to loans attached to properties in specific districts (roughly the equivalent in population to a typical U.S. town). For the remainder of the paper, we interchangeably refer to "neighborhoods" and "districts." We map how the policy borders shift over time in Figure 4, since we exploit the geographic boundaries dividing regulated vs. unregulated districts in a border difference-in-discontinuity design we introduce in Section 4.2.[6] All LTV limits were removed in two stages across the originally treated districts in August 2015 and March 2016.

Figure 2 shows that the LTV reforms had the intended effect on measured leverage ratios. We sort newly originated loans into treatment and control groups according to the criteria in Table 1 and compute the loan-to-value using the bank's appraisal value as the value in the denominator. We then take the mode of the ratio for the treatment and control groups in each month. For example, a loan issued for purchase of a second home in a targeted district in Taipei in 2011 could not have a

[5]The Banking Act, Article 72-2 prohibits banks from issuing residential loans which total more than 30% of bank capital (deposits + bond issues); this is slightly lower than the 35% minimum risk weight applied to U.S. mortgages under Basel III rules adopted in 2013. Campbell, Ramadorai, & Ranish (2015) study variation in mortgage risk weights across loans originated by a large Indian bank and find that there is a negative relation between risk weights and delinquency rates, even conditional on the LTV ratio and interest rate.

[6]We ignore aspects of each reform which only applied to very high-end properties, where the price cutoffs determining treatment are 70 million NTD ($\approx$ 2.3 million USD) within the treated districts and 40 million NTD ($\approx$ 1.3 million USD) in untreated districts; these properties are well above the top 1% of the price distribution within each district, and we winsorize prices in our main specifications at the 99th percentile.

TABLE 1. Summary of LTV Reforms: Targeted Segments and Restrictions

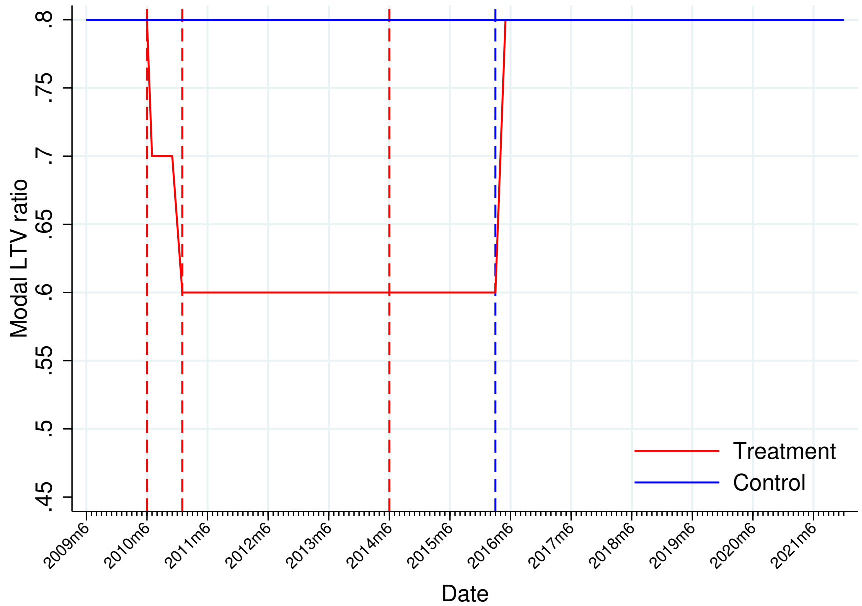
Effective date	Type	Property target	Region	Buyers	Maximum LTV
March 1, 1989	T	Land, residential and non-residential properties	All regions	Individuals and institutions	140% of the current appraisal value
June 25, 2010	T	Second (mortgaged) homes	Taipei and New Taipei (22 districts)	Individuals	70% of the collateral value
December 31, 2010	T	Second (mortgaged) homes	Taipei and New Taipei (+3 districts)	Individuals and institutions	60% of the collateral value
		Land	All regions	Individuals and institutions	65% of $min(price, collateral\ value)$
June 22, 2012	T	High-end properties	All regions	Individuals and institutions	60% of $min(price, collateral\ value)$
June 27, 2014	T	Second (mortgaged) homes	Taipei, New Taipei, Taoyuan (+ 8 districts)	Individuals	60% of $min(price, collateral\ value)$
		Third (mortgaged) homes	All regions	Individuals	50% of $min(price, collateral\ value)$
		High-end properties	All regions	Individuals	50% of $min(price, collateral\ value)$
		Residential properties	All regions	Institutions	50% of $min(price, collateral\ value)$
August 14, 2015	L	Third (mortgaged) homes	All regions	Individuals	60% of $min(price, collateral\ value)$
		Second (mortgaged) homes	New Taipei and Taoyuan (− 6 districts)	Individuals	No LTV limit
		High-end properties	All regions	Individuals and institutions	60% of $min(price, collateral\ value)$
		Residential properties	All regions	Institutions	60% of $min(price, collateral\ value)$
March 25, 2016	L	High-end properties	All regions	Individuals and institutions	60% of $min(price, collateral\ value)$
		All other mortgages	All regions	Individuals and institutions	No LTV limit

Notes: The table lists the history of recent laws in Taiwan which altered statutory maximum leverage limits for mortgage loans. Each policy applied to both bank and non-bank mortgage originators. The type column refers to whether the reform resulted in a tightening ("T") or loosening ("L") of limits. The property target column lists segments of properties and loans subject to the legal change, while the region column notes whether there were specific areas targeted. Some restrictions applied to either only institutional or individual property buyers. The final column describes the precise functional form determining the LTV limit.

LTV ratio at origination exceeding 60% of appraised value. The dashed lines indicate the tightening (red) and loosening (blue) of LTV limits resulting in changes to the maximum ratio percentage. The modal ratio remains at 80% for loans in the control group throughout the sample period, while ratios in the treatment group bunch around the limit during each regulatory regime. Measured LTV ratios also track the limits if we instead plot the median or average LTV ratio; the average drop in observed ratios around the June 2014 reform among mortgages in the treatment group relative to those in the control group was 2 p.p.

However, the definition of the maximum allowable LTV ratio changed over time because in some regimes the bank's appraisal value counted towards the denominator, but after June 2014, the relevant denominator was the minimum of the transaction price and the appraisal value. The Central Bank's goal in making this distinction between loan-to-value and loan-to-price (LTP) was to deter borrower-lender collusion leading to inflated appraised values to originate larger loans. The formula change meant that this avoidance strategy would only be feasible in cases where the transaction price exceeded the "true" appraisal value. Figure 2 smooths out any collateral misreporting because we use the same measure of the valuation in the denominator over time even though there are important distinctions between price and value across the reform thresholds. We quantify the gap in bank and official appraisals in Section 5.3 and show that this gap grows by between 4% and 6% for treated loans following LTV tightenings. Thus, a portion of the loans used to construct Figure 2 achieved permissible LTV ratios through collateral misreporting rather than by rationing credit relative to market value.

FIGURE 2. First Stage Effects: Modal LTV Ratios for Treatment vs. Control Loan Contracts



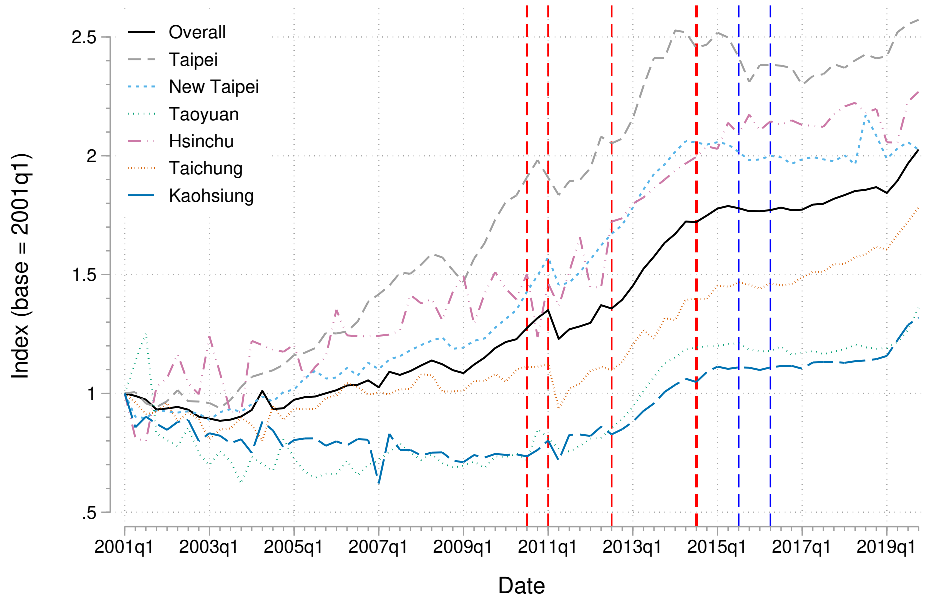
Notes: The figure plots the statistical mode of the LTV ratio over time for loans in the treatment group (red) and control group (blue). Treatment and control groups are defined according to the targeted segments of the mortgage market listed in Table 1. Vertical red dashed lines delineate reform events resulting in lower maximum LTV ratios for certain loans, while the blue dashed line in March 2016 indicates the relaxation of LTV limits.

Was the Central Bank's rationing of mortgage credit through LTV limits justified by trends in house price growth? Figure 3 plots quality-adjusted price levels for the top six cities in Taiwan by population and an overall index including property transactions across the entire island.[7] The sales data indicate substantial price appreciation over a short time period. House prices grew by 81% in Taipei but only by 22% in the overall market between 2001Q1 and 2010Q1, with 33 p.p. of the increase in Taipei due to appreciation within one year (2009). The dashed red lines in Figure 3 indicate policy events in which the Central Bank tightened the LTV limit, while the blue dashed lines indicate the staggered removal of LTV limits for non-high-end properties in August 2015 and March 2016. For the cities containing treated districts – Taipei and New Taipei in 2010, plus Taoyuan in 2014 – price growth turns negative and then moderates in the quarters immediately following the December 2010 and June 2014 reforms. Prices do not rebound in treated cities after the removal of all LTV limits in March 2016. Our loan-level and property-level specifications in Section 5 corroborate these aggregate time series patterns.

Did the Central Bank spatially target restrictions by lowering maximum LTVs in housing markets with the greatest *ex ante* price growth? To answer this question, we further decompose house price

[7]We describe the hybrid repeat sales approach used to estimate these price indices in our companion paper Chi, LaPoint, & Lin (2022). The approach is similar to the one we adopt below for estimating district-level price growth via equation (2.1).

FIGURE 3. Quarterly Housing Price Levels and LTV Reforms



Notes: Panel A of the figure plots nominal quality-adjusted housing price index levels computed in Chi, LaPoint, & Lin (2022), including properties for all of Taiwan ("Overall") and for the top six cities. Dashed vertical lines indicate the reforms to LTV ratios detailed in Table 1 for which we analyze the housing market impacts in Section 5.

changes by computing growth rates in quality-adjusted average prices for different definitions of treated and untreated districts over the different LTV regime periods listed in Table 1. We pool the June and December 2010 reforms into a single policy event given that only two quarters passed in between their implementation, and that the only change for property mortgages between June and December was an expansion of the border encompassing treated districts.

We obtain quality-adjusted price growth by separately estimating for treatment and control districts the following regression and then transforming the estimated quarter fixed effects:

$$\log p_{i\in g,q} = \delta_q^g + \gamma_b^g + \beta^{g\prime} \cdot \mathbf{X_{i\in g,t}} + \varepsilon_{i\in g,q} \tag{2.1}$$

$$\Delta \widetilde{P}_{q,q+1}^g = \exp(\widehat{\delta}_{q+1}^g)/\exp(\widehat{\delta}_q^g) - 1 \tag{2.2}$$

where i denotes a property sale, q refers to a quarter-year period, and g indexes the district type. The block fixed effects γ_b^g control for all time-invariant observed or unobserved characteristics within a neighborhood block. Since we quickly lose statistical power in estimating (2.1), we include a parsimonious set of property-level controls in the vector $\mathbf{X_{i\in g,t}}$: 5-year building age bin dummies, building material dummies, log floor space, a dummy for whether the unit is in a high-rise apartment building, and a linear spline in distance to the nearest train station.

Table 2 shows that the Central Bank's initial 2010 LTV tightening campaigns targeted districts with higher *ex ante* house price growth even after we residualize by city block to account for

TABLE 2. Spatial Targeting of LTV Reforms based on *ex ante* House Price Growth

	$\%\Delta\widetilde{P}_{08Q1-10Q1}$		$\%\Delta\widetilde{P}_{10Q2-12Q2}$		$\%\Delta\widetilde{P}_{12Q2-14Q2}$		$\%\Delta\widetilde{P}_{14Q3-16Q3}$		$\%\Delta\widetilde{P}_{16Q4-18Q4}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Dec. 2010 Treated Borders										
Treated districts	27.2%	16.4%	34.2%	12.6%	18.6%	28.9%	−5.8%	−3.0%	21.8%	−0.2%
Untreated border districts	3.7%	2.0%	37.5%	37.9%	35.2%	29.9%	0.7%	2.0%	3.4%	5.3%
Untreated non-border districts	1.5%	1.1%	12.9%	10.0%	29.1%	25.9%	6.2%	8.0%	7.0%	5.6%
B. June 2014 Treated Borders										
Treated districts	17.2%	14.7%	30.3%	12.2%	25.5%	33.5%	−4.5%	−3.8%	1.8%	1.5%
Untreated border districts	5.5%	3.1%	21.1%	35.2%	16.2%	19.9%	3.8%	4.6%	4.9%	2.5%
Untreated non-border districts	1.4%	0.7%	12.3%	9.6%	27.9%	22.9%	7.0%	8.1%	7.2%	6.0%
Property controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
City block FEs		✓		✓		✓		✓		✓

Notes: The table displays quality-adjusted price growth for subsets of treated and untreated districts and for two-year windows around each LTV reform according to the system of equations defined by (2.1) and (2.2). To account for the possibility of households' moving their home purchases across the policy border, we distinguish between untreated districts along the borders created by the December 2010 and June 2014 reforms and untreated districts away from the border. All regressions include vector of property characteristics, including 5-year building age bins, building material dummies, log floor space, a dummy indicating whether the unit is in a high-rise apartment building, and a linear spline in distance to the nearest train station with kink points estimated via 20 quantile bins. We include only non-land transactions in the estimation sample. See map provided in Figure 4 for a depiction of the policy borders.

neighborhood-specific valuation components (Panel A of columns 1 and 2). The same is true for the 2014 reforms (Panel B of columns 5 and 6). The untreated border districts in Panel A correspond to the not-yet-treated districts which were later regulated in June 2014. We distinguish between untreated districts adjacent to the policy border during each LTV regime and untreated non-border districts, because the spatially targeted nature of the leverage limits incentivized homeowners to substitute towards properties on the untreated side of the border, which would push prices up within the group of all untreated districts. We show in Section 5.2 that this sorting effect was quantitatively small and limited to very close distances of within 4 km to the 2014 policy border. Overall, for each reform, the point estimates we obtain from our border difference-in-discontinuity design in Section 5.1 are close to the covariate-adjusted differences in means implied by comparing the treated and untreated district groups in Table 2.

3 Data Sources

Our main dataset consists of loan-level data covering the period from June 2009 to December 2021 collected from credit reports filed by banks to the Joint Credit Information Center maintained by the Central Bank of Taiwan. The credit reports describe the origination and monthly performance of the universe of mortgage loans in Taiwan and consist of the following five forms:[8]

1. **Monthly credit balance report** that tracks contract-level characteristics, such as the outstanding loan value, interest rate (i.e. index + margin for adjustable rate loans), start and end dates of the mortgage, and delinquency flags. Banks are required to submit credit balance reports for all loans which have not yet fully amortized. For each loan originated, the credit balance report is filed starting in the first month when a payment is due. This report also includes the unique borrower id, contract id, advanced contract id, and collateral id. The difference between the contract id and the advanced contract id is that the former is plan-specific, as one mortgage between a borrower and a bank may involve different plans subject to different interest rates and payments. Different plans for purchasing the same collateral are then assigned the same advanced contract id.

2. **Borrower background report** is an unbalanced panel that includes the borrower id and collects borrower characteristics, such as their salary income, occupation, years at current job, age, education level (i.e. high school, associate's degree, college degree, master's degree, or doctorate), flag for owning owner-occupied properties, zip code-level contact address, age, and gender. These demographic characteristics are collected by the loan officer at the time the borrower applies for a new loan. Therefore, the borrower background report is only updated by the lending bank when the borrower initiates a new loan application or tries to refinance an existing mortgage.

3. **Collateral report** that contains the collateral id and collects characteristics for the property attached to the mortgage, including its zip code, the lender's appraisal value, the tax appraisal value for the land portion of the property, usage (e.g. residential vs. mixed-use), building material, number of floors (or floor of the unit if an apartment), floor space, transaction price, and transaction date. We merge the credit balance, borrower background, and collateral reports via the concatenation of the unique borrower id and collateral id.

4. **Cover page** indicating the branch and parent bank which originated the loan.

5. **ID matching form** that matches the contract id, an advanced contract id which links multiple mortgages to the same borrower, and collateral id.

To compute LTV ratios, we combine the mortgage's loan value at origination and the collateral's appraisal value. There are several general issues associated with calculating an LTV ratio in credit registry data that motivate our sample selection. First, an originated loan contract can sometimes lead to multiple report entries when the borrower applies for several mortgage plans simultaneously to execute a single transaction. For this reason, we restrict to loans in which the borrower applies to only one plan under a given mortgage. Second, it is possible that a borrower purchases multiple

[8] There is a separate form filed for originations of home equity lines of credit (HELOC). HELOCs are not subject to LTV limits, so we exclude them from our analysis.

properties under a single advanced contract id. Hence, we only keep mortgages that correspond to one property used as collateral. Third, a single property can be pledged as collateral for different mortgages of a given borrower if the borrower applies for a new mortgage after the existing mortgage was partially repaid (i.e. a second mortgage). To avoid these cases in which the precise LTV cannot be determined, we restrict to observations for which there is a one-to-one mapping between collateral id and mortgage contract id for a given borrower.

After applying these sample restrictions, we obtain our estimation sample by merging the monthly credit balance report with the borrower background information provided at the time of application by matching on the borrower id. Next, we merge this file to the ID matching form using the contract id and advanced contract id. Finally, we use the collateral id from the ID matching form to merge this file with the collateral report to pick up property characteristics, including the key zip code variable we use to assign location-based treatment status.

Finally, we merge in three sets of supplemental data sources to our credit registry extract:

Bank balance sheets and branch characteristics. To account for the fact that lender responses to mortgage regulation could depend on factors such as their profitability and size, we merge in bank balance sheet information from Taiwan Economic Journal (TEJ+). TEJ+ contains balance sheet line items, income statements, cash flows, capital expenditures, and deposit and loan information for all domestic banks and foreign banks with a presence in Taiwan through the Banking Bureau, a financial supervisory entity. We scraped branch addresses from each parent bank's website to compute branch distances to policy borders and to account for differences in borrowers' physical access to loan officers across regulated and unregulated districts. We do the same for credit union branches which are not included in TEJ+ and match the branches to the parent's balance sheet in TEJ+ using the name string. Our dataset contains 155 parent lenders with 5,242 branch offices between them.

Public housing transaction records. Due to the confidential nature of credit registry data, we cannot directly geocode the properties attached to mortgages while accessing the data within the secure research facility. To work around this issue, we obtain the precise latitude and longitude (out to six decimals) and distance measures to local amenities like schools and train stations for a database of public housing transaction records with complete addresses. Chi, LaPoint, & Lin (2022) describe the construction of this housing transaction database. We then merge in the public transaction records into the credit registry based on sale price, zip code, transaction date, and floor space (up to a margin of error). This step is only necessary for the border discontinuity designs described in Section 4.2.

Census economic indicators and topography. We compile from Taiwan's economic Census annual district-level variables such as unemployment rates, after-tax income, household expenditures, population, average household size, savings rates, and homeownership rates. We also download data on slope and elevation as a continuous function of latitude/longitude to account for granular differences in residential development due to topography.

4 Empirical Strategies

In this section we describe the matched difference-in-differences and border discontinuity designs we adopt to analyze the effects of changes to spatially targeted LTV policies on local housing markets and loan outcomes.

4.1 Matched Difference-in-Differences

The main identification challenge in the LTV or downpayment constraint literature is that macroeconomic conditions may influence real estate markets in such a way that obscures the true effect of the LTV cap change itself. Since many papers rely solely on bank-loan level data, the standard approach is to match loans on observables before and after the reform to create comparison groups to what impute mortgage LTV choices would have looked like in the absence of the new leverage limit (Abadie & Imbens 2011). This can be thought of as an implementation of "propensity score-weighted difference-in-differences (DiD)."

We adopt a modified version of this estimation procedure which takes advantage of (i) the panel dimension of the credit registry data, and (ii) information about the precise location and physical characteristics of collateralized properties. We can therefore take out individual fixed effects for frequent home buyers and condition on a richer set of demographics than the standard age and income variables observable on a typical credit application. Although the matched DiD estimator only yields treatment effects for a very restrictive definition of the treatment group, we view it as a necessary part of our analysis in that it allows us to benchmark our results to those already obtained in the literature on LTV limits and to gauge the extent to which failing to account for potential unobservables on borrower and collateral dimensions might bias the measured effects.

Consider the June 2014 LTV reform for prospective second home buyers who afterwards can take out a mortgage with a maximum 60% LTV ratio. Since all second home buyers will be below the limit after the intervention, we need to "fill in" missing data on which home buyers would have chosen an LTV ratio above 60% in the absence of the reform. We do so through the following steps:

1. Exclude individuals who take out a mortgage before the policy that is far away from the 60% LTV ratio cap.

2. Match borrowers who chose a loan slightly below the 60% cap after the policy to the "closest" borrower from the period before the policy but buying a property *within the same district*. Matching occurs along a vector of borrower characteristics $\mathbf{X}_{i,t}$, including salary income, age, educational attainment, and the parent bank with which they take out the loan. In our baseline analysis we use a symmetric bandwidth of $\pm 4\%$ on either side of the maximum LTV threshold for the 2014 reform. This means for the pre-reform period we focus on 61-65% LTV mortgages (step 1), and for the post-reform period (step 2) we use 55-59% LTV mortgages.

3. Sort the matched borrowers into treatment and control groups. The control group consists of borrowers who chose the same LTV ratio before and after the policy, but slightly below the cutoff, while the treatment group chose to be above the LTV cutoff before the policy.

4. For each variable of interest, compute the average treatment effect on the treated (ATT) as:

$$ATT = \left(\overline{After} - \overline{Before}\right)_{treated} - \left(\overline{After} - \overline{Before}\right)_{control} \tag{4.1}$$

4.2 Border Difference-in-Discontinuity Designs

Besides the distinctions between various segments of the property market, 2010 and 2014 LTV reforms introduced physical boundaries determining which investment properties were subject to mortgage credit limits based on the address. We draw on the map in Figure 4 the policy boundaries for the mortgage tightening episodes accompanying the June 2010, December 2010, and June 2014 reforms. Properties located within certain districts of Taipei, New Taipei, and Taoyuan were subject to a 60% LTV cap on loans towards the purchase of second homes, while individuals investing in other areas were not. A district in Taiwan roughly corresponds to the population of a U.S. 5-digit zip code in dense metropolitan areas, with the average district containing 63,219 residents (2022 population) and spanning 98.35 square kilometers.[9]

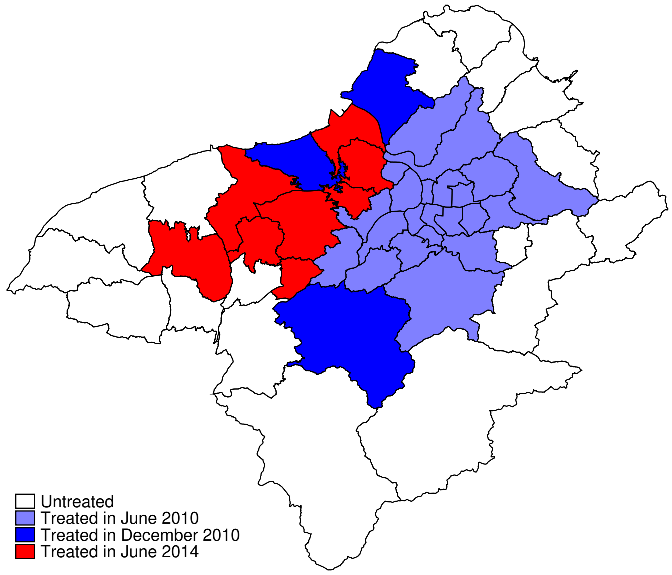
The shifting boundaries around these counties form multi-dimensional discontinuities in longitude-latitude, which we exploit by comparing outcomes for properties within each policy region vs. those properties located outside the policy region over time (Dell & Olken 2020; Méndez & Van Patten 2022). The following regression pooling effects over time implements this difference-in-discontinuity design:

$$Y_{i,d,t} = \gamma \cdot \left(LTVCap_{i,d} \times Post_{d,t}\right) + f(lat_i, lon_i) + g(DTrain_i) + \beta' \cdot \mathbf{X}_{i,d,t} + \xi_d + \delta_t + \sum_{s=1}^{N} \phi_i^s + \varepsilon_{i,d,t} \tag{4.2}$$

where $Y_{i,d,t}$ is an outcome attached to the sale of property i occurring at time t and located in district d. $LTVCap_{i,d}$ is a dummy equal to 1 if district d is located within a border set by a change to mortgage leverage policy. For instance, in the case of the 2014 reform, $LTVCap$ will be equal to one for outcomes attached to properties sold within areas subject to a 60% LTV limit. We write $LTVCap$ as indexed by i due to the restrictions – enumerated in Table 1 – applying only to mortgages originated for houses which are not the owners' first property or extremely high-end

[9]There are 50 districts included in the map in Figure 4. This means that, even at small distance bandwidths, we have enough clusters for standard errors to be consistently estimated in the face of spatial serial correlation when we cluster standard errors at the district level (Angrist & Pischke 2009, Ch. 8).

FIGURE 4. Geographical Depiction of Border Difference-in-Discontinuity Design



Notes: The map displays how the government rolled out LTV limits on second mortgages across districts in the Greater Taipei area between 2010 and 2014. Districts in white represent districts in which loans were never subject to LTV restrictions but shared a border segment with color-shaded districts which were subject to restrictions.

properties.[10] $Post_{d,t}$ is set equal to one when a transaction occurs after an LTV policy change impacting district d. The border discontinuity function $f(lat_i, lon_i)$ controls for smooth functions of the geographic location of property i. Based on Gelman & Imbens (2018), in our baseline analysis, we parameterize $f(\cdot)$ to be a linear function of latitude and longitude, although our results are invariant to specifying a quadratic function instead.

To separate the effects of mortgage restrictions on home sales due to the credit tightening itself rather than merely properties being close to a transit hub, we include on the right-hand side $g(DTrain_i)$, a linear spline in distance to the nearest train station, with kink points estimated via 20 quantile bins. To generate $g(DTrain_i)$, we calculate the distance of each property to the nearest city metro, commuter rail, and high speed rail station. We then take the minimum among these three distance measures as a proxy for $DTrain_i$. The median property sold in our dataset is located 1.8 km from the nearest train station, with an IQR of 3.1 km; for our baseline border discontinuity bandwidth of 20 km from the 2014 LTV policy border, the median sold property is 1.2 km from the nearest train station, with an IQR of 2.1 km. This indicates that the typical home in our setting is

[10]We can also estimate the triple-differences version of equation (4.2) where we decompose $LTVCap_{i,d} = LTV\_District_{i,d} \times 2nd\_Mrtg_i$, with $LTV\_District_{i,d}$ indicating treatment based on location, and $2nd\_Mrtg_i$ indicating that the mortgage is on a second or higher order property in the borrower's housing portfolio. Our baseline model instead tells us the effect on house prices in the broader market, which is important to the extent that transactions involving additional but not initial mortgages might be deterred altogether by tighter leverage limits.

located within a 15-minute walk to a train stop.[11]

We include day-of-week, week-year, and holiday fixed effects in δ_t to soak up housing market trends occurring at low and high frequency. We also include a vector of controls $\mathbf{X}_{i,d,t}$, consisting of property-level characteristics such as 5-year age bin dummies, building material dummies, log floor space, and a high-rise unit dummy. In some specifications we add the two-year lagged versions of the city-level unemployment rate, log disposable income, and average household size to $\mathbf{X}_{i,d,t}$ to account for time-varying demographic trends which could influence housing inventory.[12]

The set of indicators ϕ_i^s splits the policy boundaries pictured in Figure 4 into 10 km segments, equalling one if the property involved in transaction i is closest to segment s. We check the robustness of our results to removing from the estimation sample properties along boundary segments which overlap with physical borders which make new housing development difficult. These include mountainous areas around the border between the administrative cities of New Taipei and Yilan (the eastern border with the June 2010 set of treated districts). We apply the criterion introduced by Saiz (2010) that terrain in 90 square-meter grids with a slope of $> 15\%$ is deemed unsuitable for building new housing. We check for heterogeneous treatment effects by interacting the boundary segment fixed effects ϕ_i^s with the distance control function $f(\cdot)$, but find that this has little bearing on our estimates of the diff-in-disc parameter of interest γ.

There are several potential problems associated with interpreting the difference-in-discontinuity coefficient γ in equation (4.2) as the causal effect of imposing spatially targeted leverage limits on local house prices or other outcomes. One is that, besides the mortgage policy, other factors influencing housing market conditions could be discontinuously changing at the border. A standard way to test for this "no discontinuities" assumption inherent in regression discontinuity designs would be to re-estimate equation (4.2) using *ex ante* district-level characteristics as the outcome variable. We put on the left-hand side Census variables measured as of the first year before each LTV policy regime which does not overlap with a preceding LTV reform. Reassuringly, when we do so, we find that subsequent mortgage leverage regulation is uncorrelated with past economic performance as evidenced by disposable income and employment.

However, the house price growth tabulations in Table 1 clearly indicate that treated districts were not selected at random by the government; on average, they experienced larger price gains on the eve of each tightening reform. This means the parallel trends assumption would likely be violated for prices if we were to naively split the sample of transactions into groups either strictly within or strictly outside the policy border. For this reason, we narrow the estimation sample to properties within a distance bandwidth on either side of the reform. Selecting a bandwidth allows us to zoom

[11] The results are materially unchanged if we instead parameterize $g(DTrain_i)$ using a notion of walking time to the nearest station computed using the `georoute` package of Weber, Péclat, & Warren (2022).

[12] We include lagged versions of city-level economic indicators to account for potential reverse causality (i.e. leverage limits influence local population growth). It is also customary to include a vector of controls like slope, elevation, and temperature to account for differences in physical terrain across borders (e.g. Dell 2010), but these variables are colinear with the border segment dummies ϕ^s or even with the district fixed effects ξ_d for small enough bandwidths.

in on neighborhoods on either side of the policy border which are more likely to be similar on unobservable and observable characteristics, including the recent house price path. In what follows, we select 20 km as our baseline bandwidth and then demonstrate how the magnitudes of our point estimates $\widehat{\gamma}$ are minimally impacted by choosing smaller or larger thresholds.[13] This bandwidth selection effectively reduces the research design characterized by (4.2) to a difference-in-differences model applied to border-district pairs.

After selecting a distance bandwidth, we test the parallel trends assumption by extending equation (4.2) to allow the policy effects to vary over time:

$$\begin{aligned} Y_{i,d,t} = \sum_{\tau=-k}^{+k} \Big\{ \gamma_{t+\tau} \cdot LTVCap_{i,d} + \alpha_{t+\tau} \cdot f(lat_i, lon_i) \Big\} \\ + g(DTrain_i) + \beta' \cdot \mathbf{X}_{i,d,t} + \xi_d + \delta_t + \sum_{s=1}^{N} \phi_i^s + \varepsilon_{i,d,t} \end{aligned} \tag{4.3}$$

where τ indexes the number of quarters since the policy event at $\tau = 0$. For each reform, we set k to incorporate the largest possible number of quarters on each side of the policy change that results in a symmetric time window while simultaneously avoiding overlap with preceding or subsequent change to LTV limits. The dynamic specification also allows the gradient in two-dimensional space to flexibly vary over time by interacting the control function $f(\cdot)$ with time dummies $\alpha_{t+\tau}$.

Finally, honing in on a smaller area around each policy border allows us to compare observably similar properties which differ primarily on legal leverage limits before vs. after a reform. But households can avoid stricter leverage limits by substituting towards purchases on the other side of the border, and this sorting channel is likely stronger the smaller the bandwidth we set since implied commuting costs incurred through avoidance will be lower at short distances. Hence, γ in (4.2) captures a combination of the direct effect of LTV policies on treated housing markets plus spatial contamination bias due to indirect treatment of the untreated districts (Butts 2021). In Section 5.2, we specify the additional assumptions required to separate average treatment on the treated (ATT) from local average treatment effects (LATE) and quantify changes to housing demand on the untreated side of the border.

5 Housing Market Impact of Spatially Targeted LTV Limits

We use this section to describe our empirical results for how spatially targeted LTV limits influence house prices, loan volume, loan contract features, loan delinquency, and collateral misreporting.

[13] We search over bandwidths from 2 km from the border up to 49 km from the border, where the latter number is the maximum distance between the 2014 policy border and any point along the outside border of neighboring districts shaded in white in Figure 4. For reference, 20 km is the maximum distance between any two points within a district. For each property, we compute its distance to the border as the distance between the parcel and the *closest* point along the policy border. This rule also determines the assignment of properties to border segments ϕ_i^s.

5.1 Relative Effects on Housing & Loan Contract Outcomes

In this subsection, we present two sets of results: (i) effects of newly imposed LTV limits on loan outcomes *within* treated policy districts, and (ii) overall housing market pricing effects for regulated districts *relative* to the unregulated districts.

5.1.1 Evidence from Matched Loan Contracts

We begin by applying the matched DiD approach described in Section 4.1 to identify average treatment effects on treated (ATT) *loans*, meaning loan outcomes attached to properties which are located in treated areas *and* are investors' second homes. The matched DiD specification summarized by equation (4.1) compares outcomes for two second home loans in the same treated district to borrowers with similar salaried income levels, age, and educational attainment, but one loan has an LTV just below the statutory limit and the other has an LTV just above the limit. The loan-level and residential neighborhood-level effects of targeted leverage limits may diverge owing to different definitions of the treatment and control groups, as well as spillover effects from treated to control units in the neighborhood-level analysis. We isolate the spatial spillovers from homes in treated to those in control neighborhoods in Section 5.2.

Table 3 illustrates how borrower composition differs for treated and control loans around each of the two main LTV tightening episodes we study: one enacted December 2010 and the other in June 2014. Prior to imposing our matching algorithm, in both episodes untreated borrowers who were able to purchase investment properties with a lower downpayment had lower income, were less educated, and were slightly older. Hence, raising the required downpayment led to positive selection of borrowers in LTV-regulated districts. Such differences between treated and control groups become statistically negligible after we apply our matching procedure.[14]

Table 4 shows the results from the matched DiD procedure applied to the December 2010 and June 2014 reforms which lowered the allowable LTV limit on second homes in certain districts to 60%.[15] For both reforms, the loan amount and price per square meter decline after the tightening; these results hold even after adjusting for observable differences in property characteristics. There is marginally statistically significant decline in the loan maturity of 4 to 5 months for the 2010 reform. The fact that prices fall on a per unit of space basis demonstrates that households who would have normally borrowed at higher LTVs do not simply downsize to comply with the leverage tightening and instead seek lower quality homes. Consistent with it being easier to avoid the LTV

[14]Due to the spatial dimension of treatment, we have a more limited set of observations from which to select a nearest neighbor for each treated loan, which means some economically meaningful differences in borrower income remain even after matching.

[15]To be more precise, for the 2014 reform the treatment cutoff is $\max\{LTV, LTP\} \leq 60\%$ (Table 1). This means that after June 2014, if a loan sits above 60% LTV it can still be considered "treated" if the price is under the bank's appraisal value. Therefore, we take out the loans which have prices below the bank appraisal value in the post period to interpret this experiment as identifying the effect of lowering the LTV limit.

TABLE 3. Unmatched vs. Matched Sample Statistics

A. December 2010 LTV Tightening

	Unmatched			Matched		
	Pre-reform	Post-reform	t-stat	Pre-reform	Post-reform	t-stat
Annual income	607.66	743.97	5.77	655.80	699.88	1.43
Years of education	15.00	15.11	2.05	14.74	14.98	0.87
Birth year	1966.92	1968.81	9.22	1969.92	1970.09	0.79

B. June 2014 LTV Tightening

	Unmatched			Matched		
	Pre-reform	Post-reform	t-stat	Pre-reform	Post-reform	t-stat
Annual income	504.99	650.43	4.31	588.51	625.44	1.82
Years of education	14.59	14.73	1.94	14.37	14.28	-0.73
Birth year	1970.30	1971.95	5.65	1973.27	1973.67	0.89

Notes: This table shows the means and t-statistics for matched and unmatched borrowers' characteristics before and after the reform for second mortgagors within the same district. Annual income in thousands of NTD. Around each reform, we use an LTV bandwidth of $\pm 4\%$, meaning we compare post-reform loans below the 60% cutoff with an LTV of 55-59% to pre-reform loans above the cutoff with an LTV of 61-65%. We define the sample via the longest symmetric time window around each reform to avoid seasonality and implementation of new LTV policies. See Section 4.1 for details on the matching procedure.

limit through collateral misreporting in the 2010 regulatory environment, the unit price decline of 9.9% (10.4 log points) within the treated region is only half as large as the 17.7% (19.5 log points) decline observed during the 2014 reform.

We also consider whether the pricing of the loan itself changes.[16] For both reforms the sign on the loan interest rate is negative, although the estimated ATT is not statistically different from zero in the 2010 reform (Panel A of Table 4). When the LTV limit is more binding in 2014, we find interest rates decline by between 15 and 19 basis points (Panel B). This result is in contrast to the findings in several papers studying soft LTV limits – i.e. bank risk weights or capital requirements set in proportion to mortgage leverage. The reason is that banks in our setting charge lower interest rates on new loans after being forced to originate at a lower LTV because they pass through the

[16]Recall that all mortgages in our sample are floating rate loans for which the interest rate is equal to a certificate of deposit index rate and a spread, or margin, charged on top of this index. The index varies at the bank-time level, while the margin can vary by bank-time and contract type. To the extent both the index and the margin vary by bank-time, there is no meaningful distinction between the two interest rate components for loan pricing.

TABLE 4. Matched DiD Effects of LTV Limits on Second Mortgage Loans

A. ATT Estimates for December 2010 LTV Tightening

	log(loan amount)		log(unit price)		Interest rate (p.p.)		Maturity	
ATT	−0.130***	−0.128***	−0.092*	−0.104**	−0.029	−0.033	−4.329*	−5.111*
	(0.044)	(0.048)	(0.049)	(0.045)	(0.031)	(0.033)	(2.526)	(2.784)
Matched variables:								
District & bank	✓	✓	✓	✓	✓	✓	✓	✓
Salary income	✓	✓	✓	✓	✓	✓	✓	✓
Age	✓	✓	✓	✓	✓	✓	✓	✓
Education	✓	✓	✓	✓	✓	✓	✓	✓
LTV bandwidth	±4%	±4%	±4%	±4%	±4%	±4%	±4%	±4%
Property controls		✓		✓		✓		✓
N	4,052	3,742	3,962	3,656	4,052	3,742	4,052	3,742

B. ATT Estimates for June 2014 LTV Tightening

	log(loan amount)		log(unit price)		Interest rate (p.p.)		Maturity	
ATT	−0.110**	−0.096*	−0.230***	−0.195**	−0.148***	−0.190***	−1.474	−2.444
	(0.049)	(0.058)	(0.087)	(0.089)	(0.050)	(0.057)	(4.698)	(4.444)
Matched variables:								
District & bank	✓	✓	✓	✓	✓	✓	✓	✓
Salary income	✓	✓	✓	✓	✓	✓	✓	✓
Age	✓	✓	✓	✓	✓	✓	✓	✓
Education	✓	✓	✓	✓	✓	✓	✓	✓
LTV bandwidth	±4%	±4%	±4%	±4%	±4%	±4%	±4%	±4%
Property controls		✓		✓		✓		✓
N	966	920	952	906	966	920	966	920

Notes: The table displays results from estimating the average treatment effects on the treated (ATT) for key loan contract outcomes using the Abadie & Imbens (2011) estimator from equation (4.1) described in Section 4.1. We match observations between the treatment and control groups using sampling with replacement and minimize the Mahalanobis distance between the conditioning set of variables. We analyze the December 2010 reform in Panel A and the June 2014 reform in Panel B. We use an LTV bandwidth of ±4%, meaning we compare post-reform loans below the 60% cutoff with an LTV of 55-59% to those above the cutoff with an LTV of 61-65%. We consider the log of the principal at origination, the log of the property price per square meter (unit price), the interest rate (p.p.), and the maturity of the loan (in months). The interest rate is the rate initially applied at origination which includes teaser rates. In all columns, we match second mortgage loans on either side of the reform on the basis of the borrower's total salary income, birth year (age), bins for years of education, the parent bank originating the loan, and the district in which the property is located. Years of education computed from pre-college years of schooling and bins of associate's degree/certificate degree program, college degree, master's degree, or doctorate. In some columns we include a set of property characteristics, consisting of building age, dummies for structure material, dummies for number of floors in the house or floor within the building for apartment units. Standard errors two-way clustered at the bank and district level in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

lower costs of private mortgage insurance premia to the consumers.[17] A corollary of this finding is that the mortgage lending industry must be fairly competitive, otherwise banks with market power could simply continue to charge the same higher rates on the lower LTV loans to borrowers who have the same risk profile on *ex ante* characteristics, assuming that the risk profiles of the loan pools are otherwise unaffected.

Following the Global Financial Crisis, an often-stated goal of restrictions on mortgage lending is to prevent systemic risk to the banking sector due to originating mortgages to households at a high risk for delinquency, or to those who are likely to be underwater due to volatile property values.[18] We test in Table 5 whether the LTV limits were successful in reducing the incidence of overdue payments or loan charge-offs, both on average and for households at different points in the income distribution. We call a loan "ever-delinquent" if there is ever a missed payment recorded in the loan's performance history; therefore the ever-delinquent flag is the sum of 30-day, 60-day, and 90+ day delinquency flags. A loan gets charged-off if the lender writes off the loan as a loss and closes the account. While we follow the procedures in Table 4 of selecting a matched sample of loans originated within a symmetric time around each reform, to construct these two delinquency flags we track the loans over our full time sample, meaning we take the maximum of the sequence of delinquency dummies from origination up to December 2021.

To assess whether LTV limits result in differential effects on delinquency according to borrowers' *ex ante* creditworthiness, we estimate the following triple differences regression over our sample of matched loans for each reform:

$$\begin{aligned} Delinquent_{i,t} &= \alpha + \beta_1 \cdot Post_t + \beta_2 \cdot Post_t \times \mathbb{1}\{LTV > 60\%\}_j + \beta_3 \cdot Income_i \times Post_t \\ &+ \beta_4 \cdot Income_i \times \mathbb{1}\{LTV > 60\%\}_j + \beta_5 \cdot Income_i \times Post_t \times \mathbb{1}\{LTV > 60\%\}_j + \psi_{(i,j)} + \varepsilon_{(i,j),t} \end{aligned} \tag{5.1}$$

where $Delinquent_{i,t}$ is an indicator for whether loan i originated in period t is ever delinquent. $\mathbb{1}\{LTV > 60\%\}_j$ is a dummy for whether loan i is in the treatment group, defined by being matched to a loan j originated in the pre-reform period with an LTV over 60%. The standalone $\mathbb{1}\{LTV > 60\%\}_j$ dummy is absorbed by the matched loan-pair fixed effects $\psi_{(i,j)}$. We suppress bank and district subscripts since each pair is matched on those characteristics and consists of loans attached to properties in treated areas. We implement this version of our matched DiD design via regression, rather than estimating separate ATTs by income quantile, to preserve statistical power. We also estimate results using borrower debt-to-income (DTI) in lieu of income and find similar

[17]In the U.S., lenders charge private mortgage insurance (PMI) on mortgages above an 80% LTV. PMI is automatically removed from the monthly payment once the borrower pays down enough of the principal such that the LTV falls below 78%. This convention exists in the U.S. due to standards guiding what the government sponsored enterprises (GSE), Fannie Mae and Freddie Mac, consider conforming loans which they will purchase from lenders on the secondary market. In most other countries, there is no equivalent to the GSEs, and thus mortgage insurance premia are generally a continuous function of leverage.

[18]We isolate housing market macroprudential policies in the Cerutti et al. (2018) database and discuss their taxonomy in Appendix D.

TABLE 5. Matched DiD Effects of LTV Limits on Loan Delinquency

A. Estimates for December 2010 LTV Tightening

	Ever-delinquent flag			Charge-off flag		
$Post_t$	0.0007	0.0008	0.0011	0.0037	0.0056	0.0014
	(0.0004)	(0.0005)	(0.0007)	(0.0041)	(0.0042)	(0.0053)
$Post_t \times \mathbb{1}\{LTV > 60\%\}_j$	−0.0007	−0.0007	−0.0010	−0.0003	−0.0021	0.0039
	(0.0004)	(0.0005)	(0.0006)	(0.0048)	(0.0052)	(0.0072)
$Income_i \times Post_t$			−0.0004			0.0064
			(0.0003)			(0.0066)
$Income_i \times \mathbb{1}\{LTV > 60\%\}_j$			−0.0001			0.0001
			(0.0001)			(0.0012)
$Income_i \times Post_t \times \mathbb{1}\{LTV > 60\%\}_j$			0.0004			−0.0090
			(0.0003)			(0.0082)
LTV bandwidth	±4%	±4%	±4%	±4%	±4%	±4%
Property controls		✓	✓		✓	✓
N	4,052	3,742	3,742	4,052	3,742	3,742

B. Estimates for June 2014 LTV Tightening

	Ever-delinquent flag			Charge-off flag		
$Post_t$	−0.0058	−0.0076	−0.0089	−0.0017	−0.0010	−0.0025
	(0.0059)	(0.0071)	(0.0082)	(0.0125)	(0.0108)	(0.0128)
$Post_t \times \mathbb{1}\{LTV > 60\%\}_j$	0.0031	0.0039	0.0045	−0.0011	0.0048	0.0102
	(0.0040)	(0.0046)	(0.0058)	(0.0167)	(0.0162)	(0.0193)
$Income_i \times Post_t$			0.0019			0.0030
			(0.0023)			(0.0130)
$Income_i \times \mathbb{1}\{LTV > 60\%\}_j$			0.0001			0.0136
			(0.0020)			(0.0178)
$Income_i \times Post_t \times \mathbb{1}\{LTV > 60\%\}_j$			−0.0010			−0.0087
			(0.0028)			(0.0166)
LTV bandwidth	±4%	±4%	±4%	±4%	±4%	±4%
Property controls		✓	✓		✓	✓
N	960	922	922	960	922	922

Notes: The table displays results from estimating regression (5.1) using the sample of matched loans using the matching procedure described in Section 4.1. We analyze the December 2010 reform in Panel A and the June 2014 reform in Panel B. The first three columns set the outcome to be a flag equal to one if the borrower ever misses a payment on the loan. The last three columns set the outcome to be a charge-off flag equal to one if the lender ever writes off the loan and closes the account. We use an LTV bandwidth of ±4%, meaning we compare post-reform loans below the 60% cutoff with an LTV of 55-59% to those above the cutoff with an LTV of 61-65%. In all columns, we match second mortgage loans on either side of the reform on the basis of the borrower's total salary income, birth year (age), bins for years of education, the parent bank originating the loan, and the district in which the property is located. Years of education computed from pre-college years of schooling and bins of associate's degree/certificate degree program, college degree, master's degree, or doctorate. $Income_i$ is salary income in units of 1 million NTD (≈ 33,000 USD). In some columns we residualize outcomes for the matched sample on a set of property characteristics, including building age, dummies for structure material, dummies for number of floors in the house or floor within the building for apartment units. Standard errors two-way clustered at the bank and district level in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

null results for the triple interaction term, with estimates provided in Appendix B.

The results in Table 5 point to no effects of the spatially targeted LTV policies on delinquency on average or by borrower income. Borrowers who would have demanded higher leverage loans are less likely to default after the LTV limits if they have higher income, but the point estimates for γ are far from statistically significant for each reform and delinquency outcome.[19] A negative sign on γ is consistent with the sample of borrowers becoming more positively selected after the reform, as shown in Table 3; borrowers need to earn more income to cover the higher downpayment. We speculate that the LTV limits made no discernible improvement on delinquency because the treated group consists of loans taken out to purchase second properties in relatively affluent areas, and our bandwidth restricts to LTV limits below the standard 80% within the universe of mortgages.

If borrowers find it more difficult to finance a downpayment after facing new LTV restrictions, then they might prepay less by putting less money down at closing. To test this hypothesis we estimate ATTs using closing costs as the outcome variable.[20] While the sign is always negative for both the December 2010 and June 2014 reforms, the effect is only statistically significant for the matched loan sample around the earlier reform. Residualizing on property controls, closing costs decline by 0.26 p.p. of the loan amount (p-value = 0.031) during the December 2010 LTV regime. We report the full analysis for closing costs in the additional results of Appendix B.

5.1.2 Pricing Effects within Regulated Areas

We now ask whether the loan-to-value (LTV) reforms targeting loans for second homes had any effect on overall house prices in the treated relative to the untreated districts. Answering this question is relevant to assessing whether leverage limits enacted as macroprudential policy can achieve their often-stated goal of cooling housing markets.

Table 6 presents our main point estimates from estimating pooled difference-in-discontinuity regressions of the form in equation (4.2) with log home sale prices as the outcome.[21] We do this for the 2014 reform which lowered the allowed LTV on second home loans from 80% to 60% by setting the LTV limit for properties to be a function of the transaction price for the first time. Across all specifications in the table, we find the 2014 tightening resulted in at least a 6% relative decline in house prices within the policy region. This result is robust to accounting for

[19] We also test whether there are non-monotonicities in the relation between borrower creditworthiness and subsequent delinquency by substituting the continuous income variable for dummies indicating income quintiles, but once again find uniformly null effects.

[20] Closing costs here are defined as the difference between the total payment in the month of origination and the initial monthly payment, divided by the initial principal amount. Therefore, closing costs may also capture lenders' incentives to increase origination fees to make up any losses associated with originating lower-LTV mortgages. Such a pricing strategy would attenuate our effects on closing costs.

[21] One might argue that in this context using transaction values and including floor space or land area on the RHS is subject to a bad control problem because households might downsize their homes to comply with increases in required downpayments. We obtain very similar point estimates if we instead use log prices per square meter as the outcome variable and omit size variables from the RHS.

differences in transit access, the inclusion of controls for property and neighborhood characteristics, the parameterization of the location control function (linear vs. quadratic), or the decision of whether to include residential land parcel sales in the estimation sample.[22] The fact that the pricing effect is hardly attenuated when we control for floor space or land parcel size (compare column 2 to column 3) suggests that the negative pricing effect of credit rationing is not simply driven by individuals downsizing by substituting towards smaller homes.

Figure 5 compares confidence intervals obtained by clustering standard errors at the district level to those computed when we allow for spatial correlation of an unknown form between property observations, following Conley (2008).[23] The Conley variance-covariance estimator depends on a cutoff distance below which observations can be spatially correlated. Within each LTV reform sample, we conservatively set this cutoff parameter equal to the distance (in km) which *maximizes* the Conley standard errors for the most stringent version of our baseline specification (4.2) with a border distance bandwidth of 20 km and the full set of controls as included in column 4 of Table 6.[24] Ultimately, the statistical significance of the pricing effects of the LTV reforms in Table 6 and Figure 5 stays intact. For the key 2014 reform which linked LTV limits to contract prices (Panel C), all results are statistically significant at the 1% level regardless of the standard error estimator.

The results in Figure 5 also report the pooled diff-in-disc estimates for the other LTV reforms listed in Table 1, including the December 2010 reform which only pegged mortgage limits to the appraised collateral value, not the price. We argue in Section 5.3 that the incentive of bank-borrower pairs to inflate appraised values to avoid the limits was, for this reason, stronger during the 2010 reform than during the later 2014 episode which (partially) closed this loophole by setting the LTV limit to be a function of the transaction price. For the former reform, we find that house prices rose by roughly 20% after the reform in treated relative to control districts.[25]

Panel B studies the non-spatially targeted "placebo" reform in 2012 which imposed additional leverage limits only on "high-end" properties without changing the policy border previously drawn by the 2010 reform. In our baseline sample we winsorize prices at the 99th percentile, whereas the Central Bank's definition of a high-end property consists of homes sold well above the 99th percentile of prices. As expected, we estimate, for most bandwidths, a slightly positive but statistically

[22]Transactions involving only land, as opposed to standard home sales in which either a single-family home is sold as a building and land bundle or an apartment unit is sold, were initially subject to a strict LTV limit of 65% in December 2010 (cf. Table 1). In that sense, for land sales, the main change in moving from the 2010 to the 2014 mortgage regulatory regime was a reduction in the limit from 65% to 60%.

[23]The standard errors are even less conservative if we instead two-way cluster by district and time.

[24]We search for the cutoff parameter which maximizes the Conley standard errors over the range of 2 km to 49 km, where the endpoint is set to be the longest distance between the 2014 policy border and any point along the outside border of the neighboring untreated districts. The resulting spatial correlation cutoff parameters are 49 km for the 2010 reform, 13 km for the 2012 reform, 2 km for the 2014 reform, and 2 km for the 2016 loosening.

[25]Putting aside the collateral misreporting loophole, the muted effect we uncover for the 2010 reform is consistent with the conclusions in Armstrong, Skilling, & Yao (2019), who study a series of LTV limit tightenings in New Zealand. Those authors found that these limits only started to have persistent negative effects on house price growth when *ex ante* price growth is not too extreme.

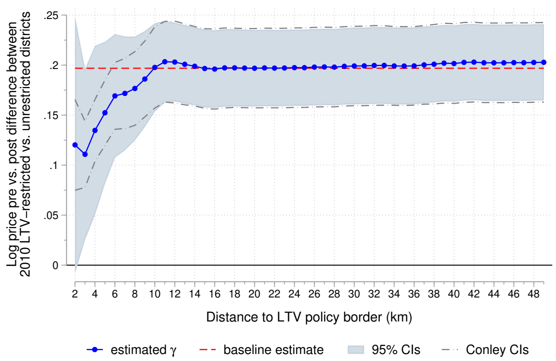
TABLE 6. Pooled Border Difference-in-Discontinuity Estimates of Pricing Effects of 2014 Reform

	(1)	(2)	(3)	(4)	(5)	(6)
$LTVCap \times Post$	-0.078^{***}	-0.078^{***}	-0.065^{***}	-0.059^{***}	-0.058^{***}	-0.077^{***}
	(0.019)	(0.018)	(0.010)	(0.009)	(0.010)	(0.013)
	[0.012]	[0.012]	[0.007]	[0.006]	[0.006]	[0.010]
Sample	Buildings	Buildings	Buildings	Buildings	Buildings	Buildings + land
Bandwidth (km)	20	20	20	20	20	20
$f(lat, lon)$	Linear	Linear	Linear	Linear	Quadratic	Linear
Time FEs	✓	✓	✓	✓	✓	✓
Border segment FEs	✓	✓	✓	✓	✓	✓
$g(DTrain)$		✓	✓	✓	✓	✓
Property controls			✓	✓	✓	✓
Census controls				✓	✓	✓
N	131,169	131,169	131,169	131,169	131,169	166,137
# districts	79	79	79	79	79	79
Adj. R^2	0.354	0.359	0.821	0.822	0.825	0.603

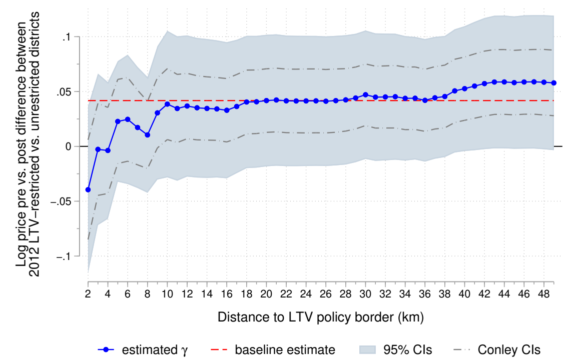
Notes: The table shows results from estimating versions of the pooled border difference-in-discontinuity model in equation (4.2) for the main reform of interest which tightened LTV limits in specific districts in June 2014. The outcome in each regression is the log house price. All specifications include 10 km border segment, day-of-week, week-year, and holiday fixed effects. $g(DTrain)$ refers to a linear spline in 20 quantile bins of distance to the train station closest to each property. The border discontinuity function $f(\cdot)$ is linear in latitude and longitude, or $f(x, y) = b_1x + b_2y$, except for column 5 in which we specify a quadratic function $f(x, y) = b_1x + b_2y + b_3(x \cdot y) + b_4x^2 + b_5y^2$. The set of property controls includes five-year bins of building age, building material dummies, log floor space, and a dummy for high-rise apartment units. For the final column, which includes transactions involving only land parcels, the set of property controls consists of log land area and a land only dummy. The set of Census controls includes two-year lags of the district-level unemployment rate, log disposable income, and average number of persons in the household. We set 20 km as our distance bandwidth by restricting to properties within 20 km on either side of the policy border pictured in Figure 4. We restrict to the longest possible time window of symmetric length around the 2014 reform to rule out the influence of other reforms, or April 2013 to July 2015. We winsorize prices at the 1st and 99th percentiles and restrict to arms-length transactions. Robust standard errors clustered by district in parentheses. Conley standard errors estimated with a maximal spatial correlation distance cutoff parameter of 2 km appear in brackets. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

FIGURE 5. Pooled Border Diff-in-Disc Estimates of Pricing Effects by Reform and Bandwidth

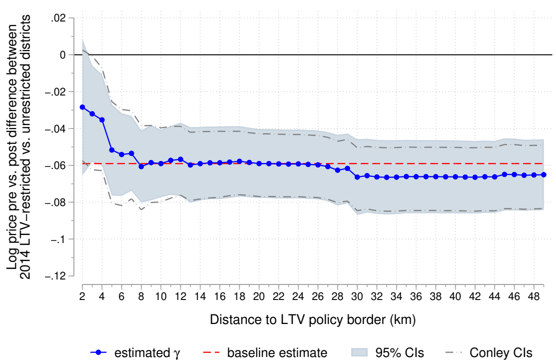
A. December 2010 Reform: LTV Limits as a Fraction of Collateral Value



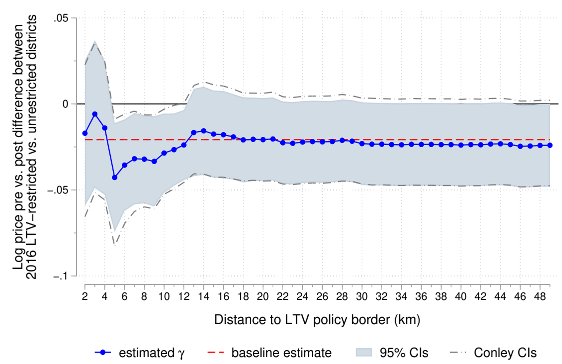
B. June 2012 Reform: High-End Properties but no Border Change (Placebo)



C. June 2014 Tightening: LTV Limits as a Fraction of min{price, collateral value}



D. March 2016 Loosening: Removal of LTV Limits across Formerly Treated Districts



Notes: Each panel shows for a given LTV regime how the pooled difference-in-discontinuity estimate obtained from equation (4.2) varies according to the assumed bandwidth restricting the sample to within a certain km to the policy border. Each regression incorporates the set of fixed effects and controls included in column 4 of Table 6, with a linear border discontinuity function $f(\cdot)$. The dashed red horizontal lines indicate the point estimate obtained by imposing our baseline bandwidth of 20 km. The dashed gray lines plot the 95% Conley standard error bands estimated with a maximal spatial correlation distance cutoff parameter of 49 km for the 2010 reform, 13 km for the 2012 reform, 2 km for the 2014 reform, and 2 km for the 2016 reform. The shaded area delineates the 95% robust confidence intervals obtained by clustering standard errors at the district level. In each panel we restrict to the longest possible time window of symmetric length around each reform to rule out the influence of preceding or subsequent reforms. We winsorize prices at the 1st and 99th percentiles and restrict to arms-length transactions in each regression. The 2012 reform is a placebo in that it only applied to incredibly high-end properties well above the 99th percentile of the price distribution in treated districts. The 2016 reform removed all lingering LTV limits except in cases of a high-end property sale. See Table 1 for details on the leverage restrictions accompanying each of the reforms.

insignificant price differential before the treated and untreated districts. Similarly, when we instead simply drop all properties which qualify as high-end sales, the locus of point estimates shifts further down onto the zero line and remains statistically insignificant.

Panel D studies the complete revocation of LTV limits within the formerly treated districts. If loosening restrictions generates symmetric effects on market prices compared to the tightening of restrictions, then we would expect prices to jump back up in formerly targeted areas. In contrast, we find a slightly negative and marginally statistically significant price differential of 2% remains over the two years after the loosening of restrictions.[26] The lack of a symmetric response is good news from the perspective of policy makers who are trying to lower price growth in formerly "hot" markets relative to other localities while limiting distortions induced by mortgage regulation. But why do prices not bounce back? One possibility is that the sequence of successively stricter leverage restrictions negatively altered investors' expectations about the path of future house prices, leading to persistently depressed demand. Fuster & Zafar (2016) document through survey responses that extensive margin homeownership decisions are sensitive to beliefs about future inflation even after hypothetical downpayment constraints are relaxed.[27] Another possibility is that a bump in demand in the previously credit-restricted areas was offset by stronger price growth due to households sorting into untreated districts across the border. We test the latter hypothesis in Section 5.3 and quantify for the 2014 and 2016 reforms the indirect effects on local house prices due to cross-border sorting.

The patterns in Figure 6 and the district-level quality-adjusted price growth estimates in Table 2 point to strong pre-trends in the Central Bank's selection of treated and untreated districts prior to the 2010 reform, but not for the 2014 reform. Figure 6 traces out the dynamic border discontinuity estimates from equation (4.3) for each reform using our baseline 20 km distance bandwidth and across different sets of control variables. Consistent with the tabulations in Table 2 showing that the initially targeted set of districts had 15.3 p.p. higher price growth than the never treated group (and 14.4 p.p. higher than the eventually treated group), there is a robust positive pre-trend in prices prior to the 2010 reform (Panel A), but not for the other reforms we study. That is, our evidence supports the parallel trends assumption for the 2014 LTV tightening and subsequent loosening, but not for the initial targeting.

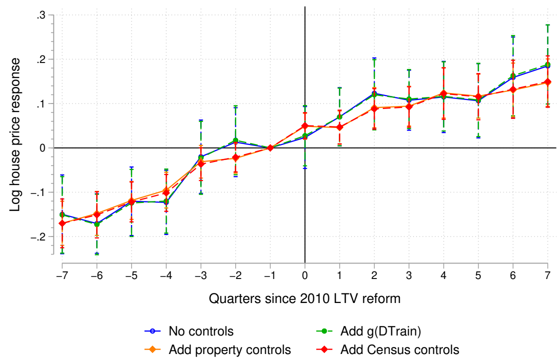
Therefore, we interpret our negative estimates for house prices after the 2014 LTV tightening to be causal in nature given the common price trend between newly formed border districts, but view the positive estimates for the 2010 reform as merely confirming the Central Bank's officially-stated

[26] One complication is that the loosening occurred in two stages. As noted in Table 1, the government lifted LTV restrictions for mortgages in a few border districts in August 2015 before lifting restrictions for the remaining treated districts in March 2016. We checked that the results for the 2016 loosening event hold even when we use a symmetric time window around March 2016 that cuts out the August 2015 reform and/or drop sales occurring in the areas where restrictions were lifted in August 2015 and focus only on segments of the border with districts which were treated continuously between June 2014 and March 2016.

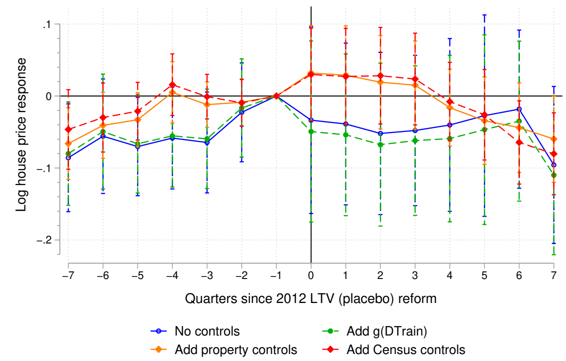
[27] Cusbert (2023) uses the experimental data from Fuster & Zafar (2021) to calibrate a heterogeneous user cost model, showing that relaxing downpayment constraints has large effects on willingness to pay but only half as large of an effect on housing prices. This is the case because households with high discount rates have WTPs which are both low and responsive.

FIGURE 6. Dynamic Border Diff-in-Disc Estimates of Pricing Effects by Reform

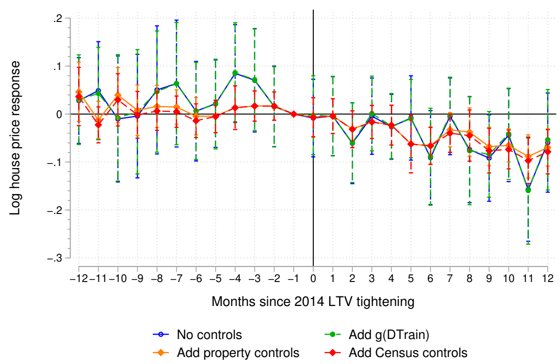
A. December 2010 Reform: LTV Limits as a Fraction of Collateral Value



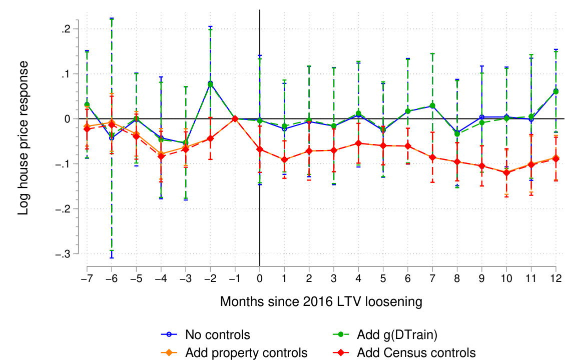
B. June 2012 Reform: High-End Properties but no Border Change (Placebo)



C. June 2014 Tightening: LTV Limits as a Fraction of min{price, collateral value}



D. March 2016 Loosening: Removal of LTV Limits across Formerly Treated Districts



Notes: Each panel shows for a given LTV regime how the dynamic difference-in-discontinuity estimates obtained from equation (4.3) evolve over time for different sets of controls. All regressions include time and border segment fixed effects with a linear border discontinuity function $f(\cdot)$ and a distance bandwidth of 20 km to the policy border. Coefficients plotted in green add the linear spline in distance to the nearest train station $g(DTrain)$, while those in orange add property-level characteristics as controls. The point estimates in red incorporate the full set of fixed effects and controls included in column 4 of Table 6. Transaction dates are only known up to the quarterly frequency during the time period covering the first two LTV reforms, while dates are known at the daily frequency in the bottom two panels. All point estimates are normalized to period right before the reform implementation date. The bars indicate the 95% robust confidence intervals obtained by clustering standard errors at the district level. In each panel we restrict to the longest possible time window of symmetric length around each reform to rule out the influence of preceding or subsequent reforms. We winsorize prices at the 1st and 99th percentiles and restrict to arms-length transactions in each regression. The 2012 reform is a placebo in that it only applied to incredibly high-end properties well above the 99th percentile of the price distribution in treated districts. The 2016 reform removed all lingering LTV limits except in cases of a high-end property sale. See Table 1 for details on the leverage restrictions accompanying each of the reforms.

motivations of limiting leverage for investors in areas with strong price growth. Because the 2010 and 2014 reforms differed in their definition of loan-to-value, identifying causal effects of the former policy is important for informing the design of macroprudential household leverage limits. To this end, in the next section we compare local treatment effects across the two rounds of restrictions using finer divisions of transactions into treated and control groups on the basis of location as well as loan and borrower characteristics around the LTV caps.

5.2 Cross-Border Housing Market Spillovers

Our baseline border difference-in-discontinuity estimates in Section 5.1.2 do not represent the direct effects of LTV policy limits on targeted neighborhoods in the sense that households may substitute toward home purchases in non-targeted areas to avoid mortgage restrictions and retain their desired downpayment percentage. If this is the case, then the negative relative effect of restricting the maximum LTV for second homes from 80% to 60% reflects the combination of two effects: (i) the direct effect on demand within the policy border region, and (ii) the indirect effect on housing demand within a bandwidth on the untreated side of the border.

To the extent that the indirect effect due to cross-border location sorting exists, the stability of the pricing effects with respect to distance bandwidth displayed in Figure 5 indicates that such sorting must occur within a relatively short distance. Motivated by this observation, we consider an augmented version of equation (4.2) which adds a separate dummy indicating whether the house is located within a hemi-circle of radius r drawn around the border:

$$\begin{aligned} Y_{i,d,t} &= \gamma \cdot \Big(LTVCap_{i,d} \times Post_{d,t}\Big) + \eta \cdot \Big(\mathbb{1}\{i \in \mathcal{H}(r)\} \times Post_{d,t}\Big) \\ &+ f(lat_i, lon_i) + g(DTrain_i) + \beta' \cdot \mathbf{X}_{i,d,t} + \xi_d + \delta_t + \sum_{s=1}^{N} \phi_i^s + \varepsilon_{i,d,t} \end{aligned} \tag{5.2}$$

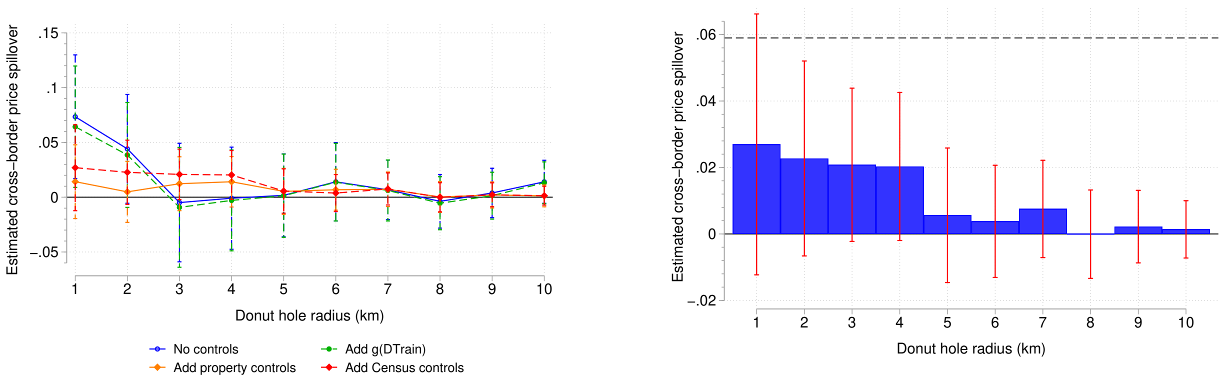
where we formally define the hemi-circle as $\mathcal{H}(r) := \{i | 0 \leq x(i) \leq r\}$ for property i's kilometer distance $x(i)$ from the border. For example, let q denote our distance bandwidth over which we run the border discontinuity regression. If we set $r = q$ to be our baseline distance bandwidth of 20 km from the border that we previously imposed, γ now captures the average price change on properties located close to the border and subject to leverage limits, but conditional on any observed change in prices for untreated properties within a symmetric 20 km on the other side of the border. While it may be tempting to interpret γ as the direct effect of the 2014 LTV policy and η as the cross-border demand spillover, this interpretation is confounded by the contamination effects of estimating regressions with multiple simultaneous treatments, as addressed in Goldsmith-Pinkham, Hull, & Kolesár (2022).

An alternative would be to estimate equation (4.2) over the sample excluding i within the "donut hole" $\mathcal{C}(r) := \{i | -r \leq x(i) \leq r\}$ and then compare the result, call it $\widetilde{\gamma}$, to the $\widehat{\gamma}$ obtained from the baseline estimates reported in Table 6 for the 2014 reform. The difference in coefficients $\widetilde{\gamma} - \widehat{\gamma}$ then

FIGURE 7. Estimated Cross-Border Spillover Effects of LTV Tightening

A. Spillover by Donut Hole Radius

B. Spillover Relative to Baseline Effect



Notes: The figure plots the estimated difference in coefficients $\widetilde{\gamma} - \widehat{\gamma}$ from implementing the donut hole procedure described in Section 5.2 for the 2014 LTV tightening. Panel A performs this procedure for different sets of controls, while Panel B uses the full set of controls as in column 4 of Table 6. $\widehat{\gamma}$ is the coefficient obtained from estimating (5.2) with log prices as the outcome variable, while $\widetilde{\gamma}$ is obtained from running the same regression but with sales within the donut hole of radius r excised. All regressions include time and border segment fixed effects with a linear border discontinuity function $f(\cdot)$ and a distance bandwidth of $q = 20$ km to the policy border. Coefficients plotted in green add the linear spline in distance to the nearest train station $g(DTrain)$, while those in orange add property-level characteristics as controls. The point estimates in red incorporate district-level Census controls. In Panel B, the gray dashed line indicates the baseline effect $\widehat{\gamma}$ obtained from estimating (4.2), so that the difference between the line and the blue coefficient bars provides an estimate of the direct (ATT) effect on the treated policy districts; the shaded blue bars correspond to points on the red line in Panel A. The bars indicate the 95% robust confidence intervals obtained by clustering standard errors at the district level. We winsorize prices at the 1st and 99th percentiles and restrict to arms-length transactions in each regression. We restrict to the longest possible time window of symmetric length around the reform which avoids other mortgage market reforms: June 2013 to June 2015.

identifies the cross-border demand effect of the reform if sales within the donut hole contains all sales to individuals who would have purchased in the treated policy districts in the absence of leverage restrictions but decided to buy across the border instead. However, we do not know the exact radius r that satisfies this condition. A candidate r is 4 km, based on the observation from Figure 5 that the $\widehat{\gamma}$ estimates are stable for bandwidths beyond that distance. This stability of the point estimates at longer bandwidths suggests that if the estimated effect is contaminated by cross-border sorting, then either much of this sorting occurs relatively close to the border within that 4 km radius, or shoppers who are induced by the LTV restrictions to shop across the border purchase properties in a uniformly distributed fashion across longer distances beyond the border. The possibility of uniformly distributed cross-border sorting is unlikely given that commuting costs to the central areas of Taipei and New Taipei – which are proportional to distances between properties and the nearest commuter train station – increase exponentially with distance to the border due to an increase in the sparsity of train stops.

In Figure 7, we report for the June 2014 LTV reform the estimated differences $\widetilde{\gamma} - \widehat{\gamma}$ with respect to different combinations of radii r to define the size of the donut hole and fixing a bandwidth of $q = 20$ km to define the size of the concentric outer ring, as in our baseline specifications in

Table 6. For donut hole radii ≤ 4 km, we find a positive cross-border demand spillover of the 2014 leverage tightening, suggesting that our initial estimates of the relative effect on the treated areas are an overestimate of the direct negative effect on prices (i.e. the ATT when treatment is based on location). In keeping with the intuition that commuting costs enter exponentially into the utility function with respect to distance (Monte, Redding, & Rossi-Hansberg 2018), we find the estimated spillover steeply declines in magnitude as we radiate out further from the border, from roughly 2 p.p. of the 6% baseline effect at close distances to zero after 4 km. We consider 1 km $\leq r \leq$ 10 km. Since $q > r$, there is a bias-variance tradeoff inherent in our choice of r. Choosing r to be closer to a given q results in a smaller sample to pin down $\widetilde{\gamma}$. At the same time, as $r \to q$, $\widetilde{\gamma} \to \widehat{\gamma}$ and the relative pricing effect of the reform approaches the average treatment effect on treated locations.

Overall, we uncover evidence of investors purchasing homes across the border to avoid leverage restrictions in directly treated districts, yet this indirect treatment effect accounts for, at most, one-third of the border difference-in-discontinuity effect in the baseline specification of equation (4.2). We conclude spatially targeted LTV policies can be effective at curtailing price growth in hot housing markets without exporting local housing booms to other, nearby neighborhoods. Such policies effectively smooth out house price growth over larger areas.[28]

Subtracting out the 2 p.p. spillover effect from our baseline estimate of a 6% drop in housing prices, we can compute a mortgage credit elasticity of local house prices. The average drop in observed LTVs around the June 2014 reform among loans in the treatment group relative to those in the control group was 4%: a drop from 60% to 55% in the treatment group, compared to a drop from 70% to 67% LTV in the control group. This yields a price-leverage ratio elasticity of $\epsilon = \widetilde{\gamma}/\%\Delta LTV \approx 4\%/4\% = 1$.[29] In the next subsection, we show that this calculation underestimates ϵ, because the $\%\Delta LTV$ we observe overstates the true decline in mortgage credit due to lenders and borrowers inflating collateral appraisal values to avoid leverage restrictions.

5.3 Avoidance through Misreporting of Collateral Values

One interesting feature of the LTV policies in Taiwan (cf. Table 1) is that leverage limits are defined as an explicit function of appraised collateral values for the property, and the functional form varies across policy regimes. Since collateral values are not directly tied to a market price, banks may have an incentive to artificially inflate appraised home values to continue originating higher LTV loans,

[28]Our results contrast with Deng et al. (2021) who conduct a city-level analysis of a bundle of home purchase restrictions targeted towards particular Chinese cities. Those authors show that there are significant pricing and durable goods spending spillovers to unregulated cities within a 2 hour (250 km) commuting distance to a regulated city. Regulated Chinese cities were subject to higher downpayment constraints, in addition to higher mortgage rates and in some cases outright purchase bans on investment properties. The policy regime we study in Taiwan instead targeted neighborhoods within cities by only imposing tighter leverage limits without instituting other restrictions.

[29]We obtain similar price elasticities out of mortgage leverage when we estimate DiD-IV specifications, where in the first stage we run equation (5.2) with the loan LTV as the outcome variable, and then use the resulting pass-through estimate to scale up our estimates of the reduced form effect of the regulation on prices. In Appendix D, we report elasticities estimated via the DiD-IV strategy and estimates from the donut hole regressions used to recover $\widetilde{\gamma}$.

which yield higher average internal rates of return conditional on bororwers' risk profiles. Similar collateral misreporting behavior has been documented during the 2000s U.S. boom; during that episode buyers and sellers colluded to inflate sale values (Ben-David 2011), and banks encouraged real estate appraisal firms on their payroll to inflate home values (Agarwal, Ben-David, & Yao 2015; Griffin 2021). Galán & Lamas (2023) provide evidence from Spain that the gap between loan-to-value and loan-to-price (LTP) widens around an 80% LTV threshold for banks' covered bond issues, but those authors do not examine loan outcome responses to leverage limit changes.

Such incentives are strongest during the first LTV tightening in December 2010, wherein the LTV limit was set at 60% of the loan appraisal value. Consequently, Panel A of Figure 8 shows the 2010 reform had limited real effects on home purchase volume. A simple difference-in-differences shows that volume actually *increases* by $(16,937/13,318)-(35,462/31,821) = 15.73\%$ in treated relative to (non-border) untreated districts after the 2010 reform; in treated districts, the price distribution continued to shift to the right despite (nominal) leverage restrictions. In contrast, sales volume declines dramatically, by $(35,601/61,241) - (139,501/171,259) = -23.32\%$, in treated relative to untreated (non-border) districts following the June 2014 LTV tightening, especially for properties at the bottom half of the price distribution (Panel B).[30] The 2014 revision of the LTV law partially corrected for this potential loophole by redefining LTV limits as the 60% of the minimum of *either* market prices or appraised values. Hence, under the 2014 regime, borrowers and lenders would only have incentive to inflate appraised values to skirt the LTV limits in cases where the contracted price was higher than the prevailing appraised value.

We formally test for and quantify the existence of collateral misreporting around each LTV reform by collecting publicly available, official appraisal information used by local governments to determine individuals' land value tax and building property tax liability. Doing so allows us to construct a notion of an "appraisal gap," which we define as the log of the difference between the bank's appraised collateral value A and the most recently available local property tax appraisal value A^*:

$$Gap_{i,b,d,t} = \log(A_{i,b,d,t} - A^*_{i,d,t^*}) \tag{5.3}$$

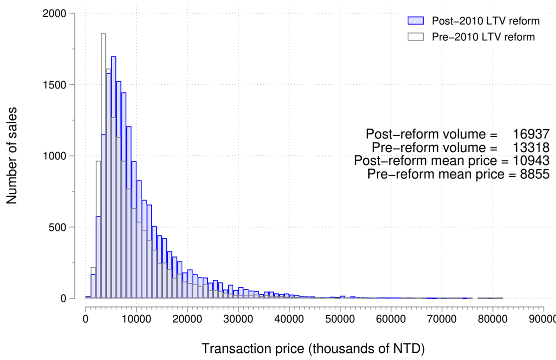
The appraisal value $A_{i,b,d,t}$ used to originate a loan attached to property i varies at the level of bank b and depends on the district jurisdiction d where the property is located to the extent that

[30] The difference-in-differences in means for prices are $(10,943/8,855) - (4,992/4,806) = 19.71\%$ for the 2010 reform and $(11,198/10,570)-(6,627/5,778) = -8.75\%$ for the 2014 reform. These differences in means nearly match the covariate-unadjusted local treatment effects from the border discontinuity design applied in Section 5.1.2. This complements the evidence in Figure 5 showing that our estimated treatment effects are relatively constant with respect to the choice of border distance bandwidth.

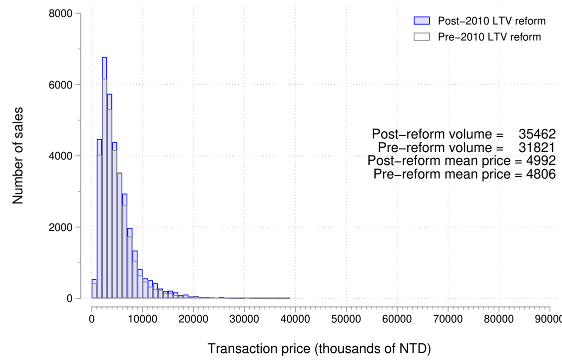
FIGURE 8. Home Sales Volume Distributions in the 2010 vs. 2014 LTV Regimes

A. 2010 Reform: LTV Limits as a Fraction of Collateral Value

Treated Districts

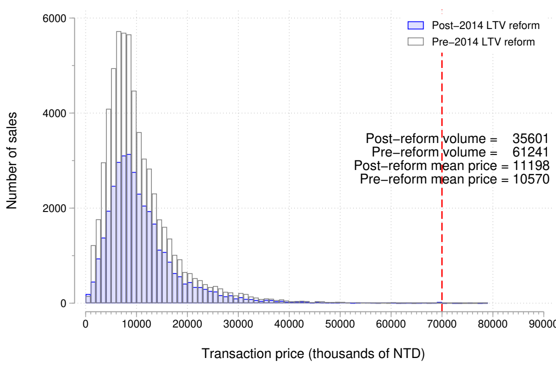


Untreated (Non-Border) Districts

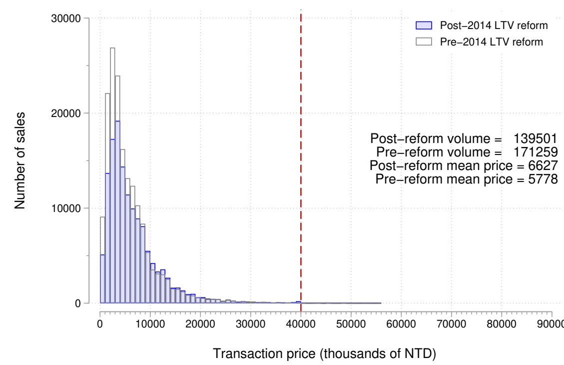


B. 2014 Reform: LTV Limits as a Fraction of min{price, collateral value}

Treated Districts



Untreated (Non-Border) Districts



Notes: Each panel plots the pre-reform to post-reform distribution of home purchase volume by transaction price, split into bins of 1 million NTD ($\approx$ 33,000 USD). Left-hand side panels do this for districts subject to LTV limits under the 2010 regime (Panel A) or 2014 regime (Panel B), while right-hand side panels include sales in untreated districts. For each reform, we exclude from the untreated group transactions in districts bordering the treated districts (see the map in Figure 4) given the evidence in Section 5.2 of spatial contamination due to shifting of purchases across the policy border. In each panel, we restrict to the longest possible symmetric time window around each reform that allows us to avoid overlap with the implementation date of any previous or subsequent reform. We exclude transactions involving institutions given that separate LTV limits applied to non-individual buyers regardless of property location (cf. Table 1). For similar reasons, we take out sales of land parcels. We identify institutional investors using standard text parsing methods (Lambie-Hanson, Li, & Slonkosky 2022). Vertical dashed lines in Panel B indicate the cutoff for "high-end properties" which are subject to LTV restrictions regardless of the first vs. second mortgage status of the borrower; the limits are 70 million NTD within the treated districts and 40 million NTD in untreated districts ($>$ 99th price percentile within each group). We separately censor the treated and untreated group distributions at the median price $\pm 10 \times IQR$.

appraisals are anchored to the official one conducted by the local government every three years.[31]

The official appraisal $A^*_{i,c,t}$ is observed for year t^*, which is the most recent appraisal year occurring before an origination in year $t \geq t^*$. The longest possible gap between the loan origination date and the official appraisal is therefore three years. We account for the portion of the gap between A and A^* due to inflation in market conditions rather than collateral misreporting via a two-step procedure. First, for the building component of A^*, we apply the exact hedonic formula underlying each district's appraisal method, which consists of loading factors that vary by neighborhood. These loading factors are updated each year and applied to characteristics which are immutable in the short-run: namely, building age and floor space. Thus, even though a revaluation is performed every three years, we can calculate what the valuation would have been if a loan issued in $t > t^*$ had instead been originated during a revaluation year ($t = t^*$). We offer a more detailed discussion of the property tax system and valuation model used by local governments in Appendix C.[32]

Second, in our regression specifications we include either a dummy $\mathbb{1}\{t = t^*\}$ or a linear trend for the difference $t - t^*$ to control for situations where the bank may simply move their collateral appraisal in lockstep with the tax authority if the origination year is the same as the most recent tax appraisal year. Including a drift function term $\mathcal{D}(t, t^*)$ in the regression helps mitigate mismeasurement in A^* arising from the fact that there is no official hedonic model determining the for-tax-purpose valuation of the land portion of the property.[33] We use district-level quarterly index levels obtained via estimating equation (2.1) district by district to interpolate market appreciation in land in between revaluation dates.[34] Conditional on these adjustments, we interpret (5.3) as the deviation of the bank's appraisal from the tax appraised value due to banks obtaining inflated appraisals through adjustment of a proprietary AVM, borrowers shopping around for higher valuations from professional real estate appraisers, or a combination of the two (i.e. collusion).

We estimate the following triple differences model to study how the appraisal gap in (5.3) moves

[31] An alternative measure of collateral misreporting would be the gap between the bank's appraisal and the transaction price, as examined in Galán & Lamas (2023). There is a clear problem with using market prices to infer the degree of collateral misreporting in response to leverage limit changes. We show using a battery of methods in Section 5.1.2 that tightening leads to local price declines, which would lead to a mechanical increase in the gap between appraised values and transaction prices even if no misreporting occurred.

[32] After applying these procedures, 99.2% of the property transactions in our sample have $A > A^*$, or a positive gap, indicating that the log scaling we apply to our definition of *Gap* imposes very little censoring on our sample. For comparison, Kruger & Maturana (2021) show 60% of a sample of U.S. securitized mortgages during the 2000s boom have bank appraisals that are strictly greater than an independent AVM valuation.

[33] For apartment units, which comprise 99.4% of transactions in the districts targeted by the LTV restrictions (74.5% in the untreated group), the building tax appraisal is the entire official appraised value. Thus, the salutary drift in appraisal values for land is no non-issue if we simply restrict our sample to apartment purchases.

[34] We compare official land appraisals during revaluation years to our appraisals imputed between t^* and t, and find the latter to be greater, on average. Inflating valuations using a quasi-repeat sales index level thus over-estimates A^* and produces conservative estimates of *Gap*. Our difference-in-differences estimates of $\widehat{\gamma}_3$ in equation (5.3) will be conservative for the main 2014 LTV reform given the statistics in Table 2 showing that index price growth was more muted in treated districts relative to control districts in the post-reform period (2014–2016). Moreover, a "highest and best use" principle applies to land value assessments, meaning that the land appraisal is a function of the building's characteristics even though the components are assessed separately for different tax bases.

around each LTV reform for mortgages on second vs. first homes:

$$\begin{aligned}
Gap_{i,b,d,t} &= \alpha + \gamma_1 \cdot Post_t + \gamma_2 \cdot LTV_District_{i,d} + \gamma_3 \cdot \Big(Post_t \times LTV_District_{i,d}\Big) \\
&+ \gamma_4 \cdot 2nd_Mrtg_i + \gamma_5 \cdot \Big(Post_t \times 2nd_Mrtg_i\Big) + \gamma_6 \cdot \Big(LTV_District_{i,d} \times 2nd_Mrtg_i\Big) \qquad (5.4) \\
&+ \gamma_7 \cdot \Big(Post_t \times LTV_District_{i,d} \times 2nd_Mrtg_i\Big) + \mathcal{D}(t, t^*) + \beta' \cdot \mathbf{X}_{i,t} + \xi_d + \delta_t + \eta_b + \varepsilon_{i,d,b,t}
\end{aligned}$$

where $LTV\_District_{i,d}$ is a dummy equal to unity if loan i is attached to a property located in a district d where an LTV limit applies, and $Post_t$ indicates the origination took place after the enactment of a new LTV law. $2nd\_Mrtg_i$ is a dummy equal to one if loan i is attached to a borrower who is using the funds towards purchase of a second property.

Our coefficient of interest is γ_7 which captures the average change in the appraisal gap for mortgage loans attached to second properties in treated vs. control districts after the reform. A finding of $\gamma_7 > 0$ is evidence in favor of collateral misreporting. The inclusion of bank fixed effects η_b allows us to compare appraisal gaps for two properties with loans from the same parent lender, where one property is located in an area targeted by the LTV policy and the other is not. We include a full set of time fixed effects δ_t to strip out seasonality in housing market conditions and within-month variation in the intensity of mortgage loan processing due to volume quotas faced by loan officers (Giacoletti, Heimer, & Yu 2022). The vector $\mathbf{X}_{i,t}$ consists of borrower, property, and bank characteristics at origination. The intercept term α captures the average (log) appraisal gap in the pre-reform period. In estimating equation (5.4), we restrict to the longest possible symmetric time window around the reform that allows us to avoid overlap with the implementation date of any previous or subsequent reform.

We present results from estimating equation (5.4) in Table 7, which shows evidence of a positive change in housing collateral misreporting due to the 2014 LTV tightening. For second mortgage transactions in treated neighborhoods, the gap between bank and government appraisals increases by 9% to 13% depending on how we parameterize the drift function $f(\cdot)$. In columns (3), (4), (7), and (8), we estimate collapsed difference-in-differences versions of (5.4) in which we compare second mortgages in treated districts to a control group consisting of all mortgage loans towards purchases of primary residences and/or any loans to properties outside the treated districts. The results are similar, but attenuated, relative to the full triple differences specifications; for second apartment units in treated areas, the appraisal gap widens by 6% relative to untreated property appraisals.

The large estimated intercept term α points to endemic collateral misreporting at the start of the 2014 tightening due to the previous LTV regime imposing strong incentives for borrower-lender pairs to inflate loan appraisals.[35] For instance, column (6) indicates that the appraisal gap

[35] Unfortunately, our sample size drops by 75% if we estimate (5.4) for a symmetric window around the earlier December 2010 reform. The reason is we lack all the inputs needed to construct $Gap_{i,b,d,t}$ in equation (5.3) for the earlier time period. Suggestively, and consistent with our story, the increase in the appraisal gap is larger than it is after the 2014 tightening, with $\widehat{\gamma}_7 = 0.262$ (t-stat = 1.58) for the all transaction sample and a linear drift function.

TABLE 7. DDD Evidence of Collateral Misreporting: Increase in Appraisal Gap

Transaction types	All transactions				Apartment units			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
α	14.19***	14.23***	15.37***	15.05***	13.43***	13.11***	14.08***	13.33***
	(5.62)	(5.56)	(5.52)	(5.40)	(6.93)	(6.74)	(7.24)	(7.18)
$Post_t$	0.08***	0.08***	0.01	0.00	0.08	0.06	−0.01	−0.02
	(3.62)	(3.19)	(0.50)	(0.10)	(1.70)	(1.52)	(0.53)	(1.04)
$LTV\_District_{i,d}$	0.82***	0.79***			0.90***	0.83***		
	(4.86)	(4.68)			(4.55)	(4.44)		
$Post_t \times LTV\_District_{i,d}$	−0.10***	−0.11***			−0.12**	−0.11**		
	(3.83)	(3.61)			(2.58)	(2.37)		
$2nd\_Mrtg_i$	0.09**	0.13***			0.07	0.12**		
	(2.53)	(5.82)			(1.32)	(3.01)		
$Post_t \times 2nd\_Mrtg_i$	−0.07*	−0.10***			−0.05	−0.10**		
	(1.91)	(4.77)			(0.99)	(2.46)		
$LTV\_District_{i,d} \times 2nd\_Mrtg_i$	−0.15***	−0.19***	−0.05	−0.06*	−0.13**	−0.18***	−0.06**	−0.07***
	(3.06)	(4.94)	(1.31)	(1.85)	(2.18)	(3.42)	(1.96)	(3.12)
$Post_t \times LTV\_District_{i,d} \times 2nd\_Mrtg_i$	0.09**	0.13***	0.03	0.04**	0.09*	0.14***	0.05**	0.06***
	(2.46)	(5.75)	(1.42)	(2.03)	(1.81)	(3.46)	(2.05)	(3.10)
$\mathcal{D}(t, t^*)$	−0.05**	−0.00	−0.06***	−0.00***	−0.06**	−0.00***	−0.08***	−0.00***
	(2.45)	(1.38)	(2.69)	(4.65)	(2.85)	(3.14)	(3.40)	(4.45)
Drift function	dummy	linear	dummy	linear	dummy	linear	dummy	linear
Time FEs	✓	✓	✓	✓	✓	✓	✓	✓
District & bank FEs	✓	✓	✓	✓	✓	✓	✓	✓
Bank controls	✓	✓	✓	✓	✓	✓	✓	✓
Property controls	✓	✓	✓	✓	✓	✓	✓	✓
Borrower controls	✓	✓	✓	✓	✓	✓	✓	✓
N	41,015	40,123	41,015	40,123	29,648	29,283	29,648	29,283
Adj. R^2	0.56	0.55	0.54	0.54	0.62	0.61	0.60	0.60

Notes: The table presents coefficients obtained from estimating triple differences equation (5.4) with the appraisal gap defined in (5.3) as the outcome. To account for discrete jumps in banks' collateral appraisals due to timing around tax revaluation years, we include a dummy function $\mathbb{1}\{t = t^*\}$ or a linear function $(t - t^*)$. Columns (3), (4), (7), and (8) represent collapsed difference-in-differences versions of (5.4) in which the control group includes both first mortgages in treated districts and all loans in untreated districts. The estimation sample includes properties for which we can observe an official appraisal value and a second mortgage loan was originated within a two-year symmetric window around the enactment of the June 2014 LTV limit tightening. See Appendix C for full details on how we calculated appraisal gaps. All regressions include district and parent bank fixed effects, as well as month-year, week-of-month, and day-of-week dummies. All regressions include a vector of borrower, property, contract, and bank controls. Borrower controls include education, work experience, age, flag for owning self-occupied properties, and income bins. Property and contract controls include building age, dummies for structure material, dummies for number of floors in the house or floor within the building for apartment units, floor space, usage, and fees other than mortgage payment paid in the first month. Bank controls include cash holdings, deposits, total assets, accounts receivable, total loans issued, total liability, and profit. The "all transactions" columns include loans for purchases of detached single-family homes, residential land, and apartment units. t-statistics in parentheses obtained from standard errors two-way clustered by bank and district. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

for apartment units increased by 15%, or 74,078 NTD ($\approx$ 2,321 USD) on top of an average pre-reform gap of 493,856 NTD ($\approx$ 15,476 USD). The appraisal drift term is always negative and is statistically significant in most columns, suggesting that our approach for updating official appraisals between revaluation years adjusts for mechanical changes in the appraisal gap due to cross-sectional differences in local housing market inflation.

6 Conclusion

We study the implications of macroprudential mortgage market restrictions which tie the stringency of leverage limits to *ex ante* house price growth within a well-defined geographical area. We analyze several spatially targeted loan-to-value (LTV) tightening and loosening episodes in Taiwan using administrative credit registry data tracking the performance of all mortgage loans. We apply a series of difference-in-differences designs matching treated borrowers who would have chosen mortgages above the permissible LTV cutoff to control borrowers who choose an allowed LTV ratio before and after the policy. Unit prices for matched properties with newly originated loans dramatically fall, and banks partially pass through declines in the cost of insuring what would have been riskier mortgages by offering borrowers lower interest rates without altering other contract terms.

Yet, we document two sources of efficiency costs associated with implementing such place-based mortgage restrictions. First, using several border difference-in-discontinuity designs which omit donut hole or hemi-circle regions very close to the policy border, we uncover positive pricing spillovers equivalent to about half (2 p.p.) of the 4% price decline in directly treated neighborhoods around the 2014 reform for which LTV limits were well-defined. This implies spatially targeted LTV policies can be effective at curtailing price growth in hot housing markets by smoothing out local housing booms over larger geographic areas. Further, increased demand for cross-border properties is limited to within a 4 km distance to the policy border, suggesting real costs in the form of selection into longer commute times to the CBD are minimal.

Second, collateral misreporting – as measured by the gap between property appraisals used by banks to originate loans and appraised values computed from official formulas for determining tax liability – becomes more commonplace as LTV limits tighten. We find borrowers and banks are able to almost completely avoid the real consequences of leverage restrictions by obtaining inflated home appraisals when the maximum LTV is determined solely by appraised collateral values; the result is no statistically distinguishable change in loan volume or amounts originated after an initial tightening. The appraisal gap persists but diminishes once the limits are redefined as a function of both appraisal values and contract prices.

These findings shed light on the effectiveness of spatially targeted LTV policies relative to alternative instruments such as homebuyer restrictions or transfer taxes which are also frequently enacted to cool down housing markets. At the same time, place-based mortgage restrictions introduce "whack-a-mole" problems similar to those which have been documented in other types of macroprudential regimes. The fact that borrowers can shop across branches within their preferred parent bank means that some of the risks inherent in higher household leverage may be partially exported from *ex ante* high price growth to *ex ante* low price growth areas. Moreover, banks face strong incentives to collude with borrowers to originate more profitable loans, as witnessed in the years preceding the Global Financial Crisis in the U.S. This fact, combined with persistent feedback of inflated appraisal values into market prices, implies tightening leverage limits widens wedges between fundamental and perceived housing values.

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