Spatially Targeted LTV Policies and Collateral Values *

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Abstract

Governments regulate household leverage at a national level, even when credit and housing market conditions vary across locations. We document that loan-to-value limits targeting specific neighborhoods can curb local house price growth. We combine administrative data from Taiwan covering the universe of mortgages, personal income tax returns, geocoded housing transactions, and bank balance sheets. Applying matched difference-in-differences and border difference-in-discontinuity designs, we find leverage limits are effective at persistently reducing local house prices in expensive, high-income neighborhoods, without reducing delinquency or inducing mortgage credit rationing. Consumers avoid place-based mortgage restrictions by obtaining inflated appraisals and moving to less regulated areas.

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 5% in catchment areas relative to nearby neighborhoods not subject to LTV restrictions. This price drop is concentrated in higher-income 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. We uncover quantitatively similar results when we instead estimate standard difference-in-differences specifications using never-treated districts faraway from the policy border to define the control group of properties, indicating that our baseline strategy of zooming in on properties around the border is not simply picking up secular pricing trends particular to the commuting zone containing regulated areas. Moreover, these effects are due to a decline in credit demand rather than credit rationing or lenders steering consumers towards property purchases in unregulated areas carrying higher internal rates of return. We compute banks' branch-level exposure to LTV tightenings based on their pre-existing propensities to originate mortgages in newly regulated areas to document null effects of lenders' direct or indirect branch network exposure on mortgage origination.

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.

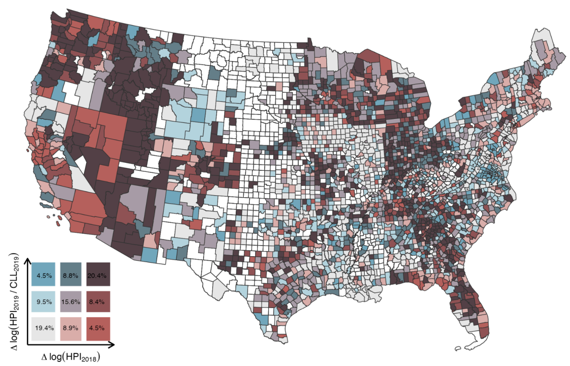
Matched difference-in-differences estimates at the loan-level reinforce the findings in our more macro analysis in which we look at spillovers to untreated loans attached to properties located in regulated areas. We match pairs of loans within treated areas on the parent bank and borrower characteristics 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 delinquency or charge-off rates. 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 (2023) 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 access on local house price trends.

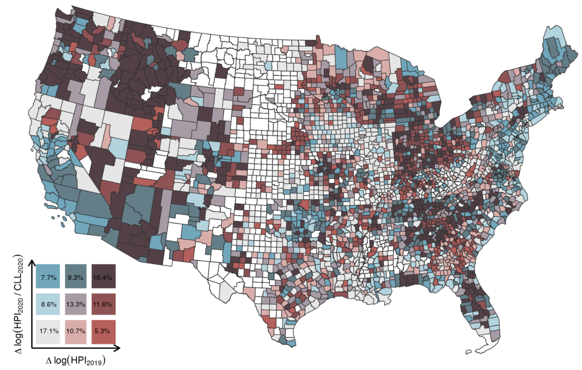
To illustrate the distinction between place-based and spatially targeted mortgage policies like the ones we analyze, Figure 1 plots the bivariate distribution across U.S. counties of YOY log changes in the ratio of house prices to the CLL (y-axis) against one-year lagged log house price growth (x-axis). We perform this exercise for the years 2019 to 2022. 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; large swathes of the country swing between over-regulated and under-regulated over the course of the post-COVID boom. Policies like the FHA and CLL thresholds are akin to the "soft" LTV limits enacted outside the U.S., which can take the form of

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

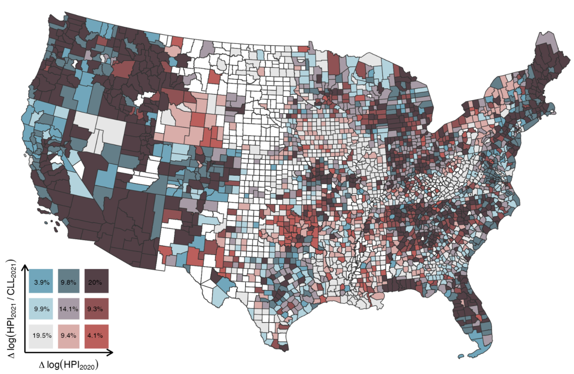
A. 2019 CLL Stringency



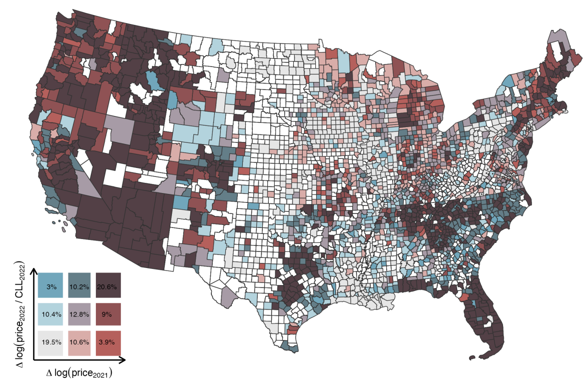
B. 2020 CLL Stringency



C. 2021 CLL Stringency



D. 2022 CLL Stringency



Notes: Each panel in the figure shows a two-way map comparing U.S. county-level log house price growth between two years $t-1$ and t (x-axis) to the log change in the ratio of $t+1$ prices relative to the conforming loan limit (CLL) for that county-year observation (y-axis). We do this for years $t+1 \in \{2019, 2020, 2021, 2022\}$ 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.

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 access in each U.S. 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" area located on the policy border.

We find 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 tend towards 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 5% drop in housing prices is due to spatial contamination of the difference-in-differences. Once we subtract out this spillover, we obtain a conservatively estimated price-leverage ratio elasticity of 0.75. 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 provision 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 the government's valuation 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 instead focus 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.

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

[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 conforming loan limit 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.

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 home LTV ratios remain underexplored.[2]

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). 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 the financial sector. Transfer taxes on housing sales are, in theory, isomorphic to downpayment constraints to the extent that sellers can pass through the tax to the buyer by charging higher prices, thus necessitating more cash for the downpayment (Koetter, Marek, & Mavropoulos 2021). Chi, LaPoint, & Lin (2023) 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 use our reduced form estimates to conclude spatially targeted LTV limits offer an 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

[2]Other papers focus on heterogeneous effects of leverage limits on household decisions. Higgins (2024) 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, Eeerola 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.

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 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

[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.

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, home buyers 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 threshold, 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 at least 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

[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.

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.

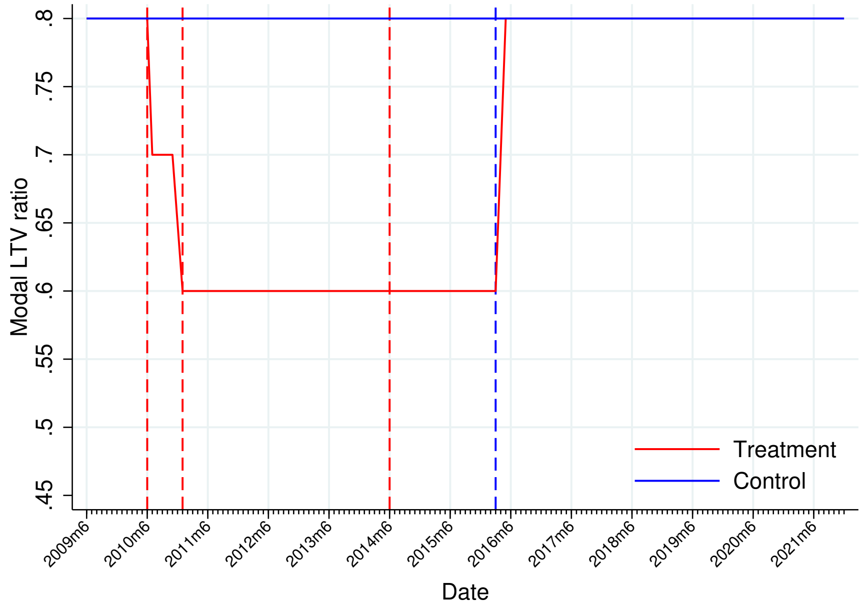
size, after which the maximum LTV was set to 140% of the official (i.e. for tax purposes) appraisal value. Despite there being such a high leverage limit, the lack of a centralized public mortgage insurer like the FHA in the U.S. led lenders to require a 20% downpayment to originate most home mortgages. 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

[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 (≈ 2.3 million USD) within the treated districts and 40 million NTD (≈ 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.

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

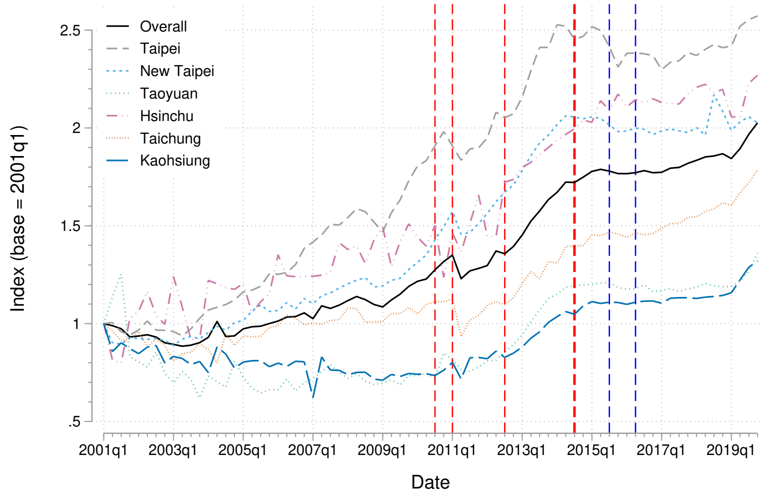


Notes: The figure plots the statistical mode of the LTV ratio over time for loans in the treatment group (red) and control group (blue). Treatment and control groups are defined according to the targeted segments of the mortgage market listed in Table 1. Vertical red dashed lines delineate reform events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

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 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

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 (2023), 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.

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 10% and 15% 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.

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

[7] We describe the hybrid repeat sales approach used to estimate these price indices in our companion paper Chi, LaPoint, & Lin (2023). The approach is similar to the one we adopt below for estimating district-level price growth via equation (2.1).

with the greatest *ex ante* price growth? To answer this question, we further decompose house price 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. To maintain 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 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.[8] 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.

[8] The difference-in-differences (DiD) split in Table 2 between treated vs. not-yet-treated districts is analogous to the estimator proposed by de Chaisemartin & D'Haultfœuille (2020), just as the split comparing treated to never-treated districts corresponds to the Sun & Abraham (2021). However, we do not adopt a staggered DiD approach which stacks up the multiple policies as an identification strategy. The reason for this is that the 2010 and 2014 reforms described in Table 1 were fundamentally distinct policies in how they defined LTV ratios, as illustrated by our results on the appraisal gap in Section 5.3 and results on the 2010 loophole in Appendix C.3.

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.

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:[9]

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

[9]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.

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.[10]

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 by concatenating 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 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 the 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

[10] We exclude refinancing events from our transaction sample. While the LTV limits we study apply to both for-purchase and refinance loans, refinancing is a relatively rare event during our time sample due to the fact that all mortgages are floating rate contracts and the Central Bank policy rate was flat over the time period.

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 (2023) 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 impute what 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 track home buyers across multiple mortgages 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:

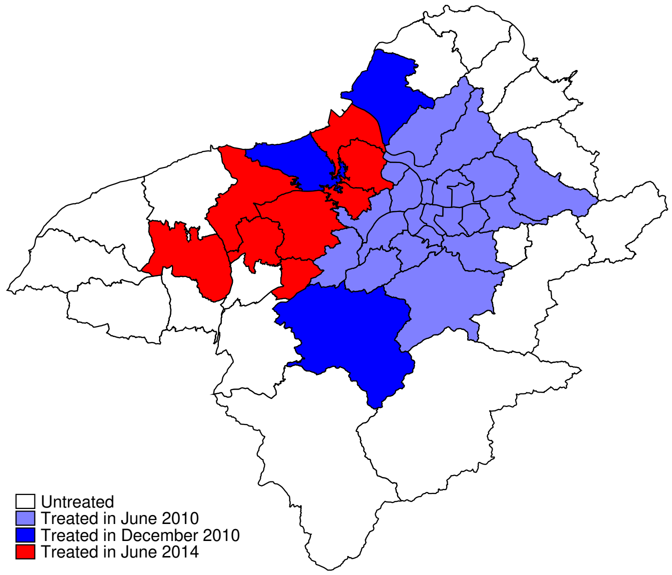
1. Exclude individuals who take out a mortgage before the policy that is far away from the 60% LTV ratio cap.

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 $\mathbf{X}_{i,t-1}$, including salary income, birth year cohort, educational attainment, and the parent bank with which they take out the loan.[11] 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, and for the post-reform period we use 55-59% LTV mortgages. This bandwidth defines our sample restriction in step 1.

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

[11] We use birth year cohort rather than age because the goal of this exercise is to match otherwise identical borrowers except one borrower faces a more stringent mortgage regulatory environment than the other. Prior studies in the LTV literature (e.g. Kabaş & Rozbach 2021; Tzur-Ilan 2023) match on age at origination rather than birth year since birth dates are typically redacted by the data provider.

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.

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.[12]

[12] 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).

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:

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

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 districts 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 properties.[13] $Post_{d,t}$ 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.[14]

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

[13] 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.

[14] To our knowledge, this is the first implementation of a border diff-in-disc design in the financial intermediation literature. Huang (2008) adopts a similar, but more discretized version of our equation (4.2) in which border county-pairs are further subdivided into segments to study the real effects of bank branching deregulation.

[15] 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).

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.[16]

The set of indicators ϕ_i^s splits the policy boundaries pictured in Figure 4 into 2 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.[17]

Still, there are several potential problems associated with interpreting the diff-in-disc 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 is 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 in on neighborhoods on either side of the policy border

[16]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 collinear with the border segment dummies ϕ^s or even with the district fixed effects ξ_d for small enough bandwidths.

[17]Depending on the specification, 10-20% of border segment fixed effects are also multicollinear with the time and district fixed effects, so our preferred specification does not include them.

which are more likely to be similar on unobservable and observable characteristics, including the recent house price path. This is the main strength of the border diff-in-disc approach, as opposed to the more aggregated border county-pair difference-in-differences design commonly used in the labor economics literature to examine the effects of minimum wage (Dube, Lester, & Reich 2010) or unemployment insurance policies (Hagedorn et al. 2016). We estimate larger point estimates $\widehat{\gamma}$ for prices when we use district-border pairs rather than our diff-in-disc specification (4.2). 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.[18] Selecting relatively small bandwidths reduces the diff-in-disc model (4.2) to a bias-corrected difference-in-differences model applied to border-district pairs (Dieterle, Bartalotti, & Brummet 2020).[19]

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:

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

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 changes 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, 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 (Dieterle, Bartalotti, & Brummet 2020; 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.

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[18] 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.

[19] The correction for aggregation measurement error generally entails controlling for distance between sub-district units (or, sub-county units like a Census block group in the U.S.) and the policy border. We do not need to apply such an approximation here because our data are already fully disaggregated to the housing transaction level.

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.

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 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' non-primary residences. 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 spatial spillovers from treated to 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.[20]

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%.[21] For both reforms, the loan amount and price per square meter decline after the tightening;

[20] 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 for the 2014 reform. Using a stricter LTV bandwidth choice (e.g. $\pm 2\%$) modulates these differences along the income dimension in the matched loan sample.

[21] 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	14.94	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	538.51	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 ±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. For each reform, we only include in the sample loans in counties which were newly regulated. We deflate annual income from nominal to real terms using the CPI. See Section 4.1 for details on the matching procedure.

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 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 plot the ATTs with respect to different LTV bandwidth choices in Appendix C.1. The coefficients are stable with respect to bandwidths between 2% and 6%, but with a bias-variance tradeoff. Bias increases with increasing bandwidth because the treatment and control groups become more dissimilar, but at the same time sample size increases, leading to tighter confidence intervals.

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 (%)		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 (%)		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 initial interest rate (% of loan amount), 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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

We also consider whether the pricing of the loan changes.[22] 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 lower costs of private mortgage insurance premia to the consumers.[23] 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. In Appendix D.3, we compute realized and expected internal rates of return (IRRs) for the matched sample of loans and find that IRRs fall by 150-300 basis points around the 2014 reform but remain unchanged around the 2010 reform, consistent with the intuition that mortgage insurance premia decrease with the actual LTV.

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. 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

[22] 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.

[23] 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.

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 ($\approx$ 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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

matched loans for each reform:

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

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 a regression, rather than estimating separate ATTs by income quantile, to preserve statistical power.[24]

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 reforms if they have higher income, but the point estimates for β_5 are far from statistically significant for each reform.[25] A negative sign on β_5 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 generated 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.[26] 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 C.1.

[24] Our finding of a null result on delinquency could be due to the relatively low mortgage delinquency rates for the targeted population of second home investors. Average ever-delinquency rates for the matched sample of loans are 0.7% for the 2014 reform (0.6% for the untreated, 0.8% for the treated), and 0.3% for the matched sample around the 2010 reform. For U.S. residential mortgages originated by commercial banks the delinquency rate has historically fluctuated between 2% and 3%. See the FRED series: https://fred.stlouisfed.org/series/DRSFRMACBS.

[25] 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.

[26] 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.

5.1.2 Diff-in-Disc 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.[27] 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 5% relative decline in house prices within the policy region. This result is robust to accounting for 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.[28] 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 restrictions is not simply driven by individuals downsizing by substituting towards smaller homes. We show in Appendix C.4 that price declines due to the LTV tightening are concentrated in neighborhoods with the highest income residents, meaning that targeted leverage limits improve the affordability of areas previously inaccessible to lower-income prospective homebuyers.

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).[29] 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

[27] 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.

[28] 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%.

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

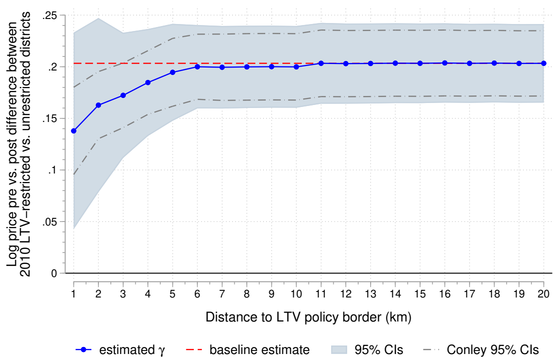
TABLE 6. Pooled Border Difference-in-Discontinuity Estimates of Pricing Effects of 2014 Reform

	(1)	(2)	(3)	(4)	(5)	(6)
$LTVCap \times Post$	-0.078^{***}	-0.058^{***}	-0.054^{***}	-0.050^{***}	-0.051^{***}	-0.066^{***}
	(0.021)	(0.009)	(0.010)	(0.010)	(0.010)	(0.014)
	[0.013]	[0.008]	[0.007]	[0.007]	[0.006]	[0.009]
Sample	Buildings	Buildings	Buildings	Buildings	Buildings	All
Bandwidth (km)	20	20	20	20	20	20
$f(lat, lon)$	Linear	Linear	Linear	Linear	Quadratic	Linear
District & Time FEs	✓	✓	✓	✓	✓	✓
$g(DTrain)$	✓	✓	✓	✓	✓	✓
Property controls		✓	✓	✓	✓	✓
Census controls			✓	✓	✓	✓
Border segment FEs				✓	✓	✓
N	107,405	107,405	107,405	107,405	107,405	136,274
# districts	74	74	74	74	74	74
Adj. R^2	0.376	0.823	0.823	0.835	0.836	0.635

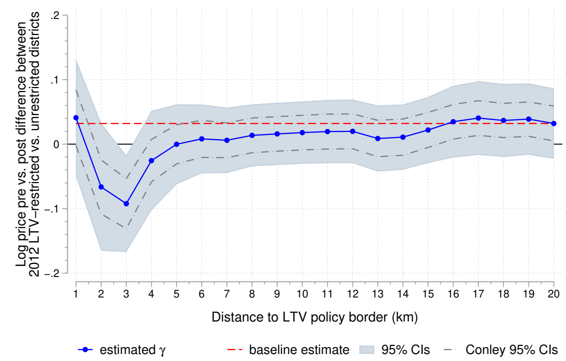
Notes: The table shows results from estimating versions of the pooled border difference-in-discontinuity model in equation (4.2) for the main reform of interest which tightened LTV limits in specific districts in June 2014. The outcome in each regression is the log house price. All specifications include day-of-week, week-year, and holiday fixed effects as well as $g(DTrain)$, which 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. The last three columns include 2 km border segment fixed effects to account for topographical differences along the policy border which might impact changes in housing demand. 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

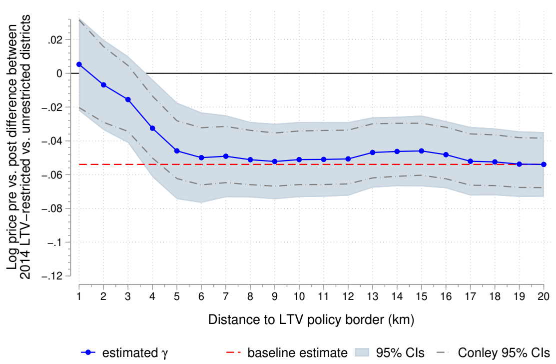
A. December 2010 Reform: LTV Limits as a Fraction of Collateral Value



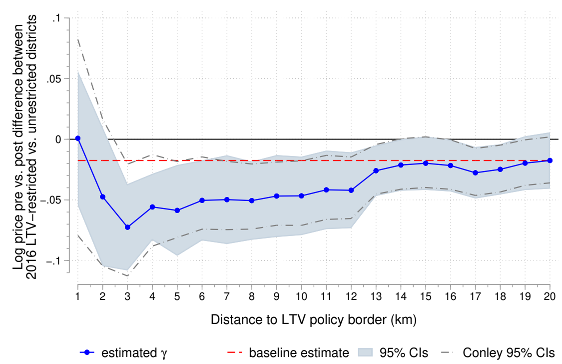
B. June 2012 Reform: High-End Properties but no Border Change (Placebo)



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 20 km for the 2010 reform, 13 km for the 2012 reform, 1 km for the 2014 reform, and 1 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.

6.[30] 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), except for distances very close to the policy border, 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.[31] In Appendix C.3 we compare sales volume around the 2010 reform for buildings, which could use the loophole, to land-only transactions (which could not) and find transaction volume precipitously drops for the latter (see Table C.3). This provides further evidence that anchoring the LTV limit to non-market valuations led to misreporting and thus no negative impact on prices or quantities for most transactions which pertained to mortgaged buildings.

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 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 complete loosening of restrictions. 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"

[30] 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.

[31] 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.

markets relative to other localities while limiting distortions induced by mortgage regulation.[32]

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.[33] 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.

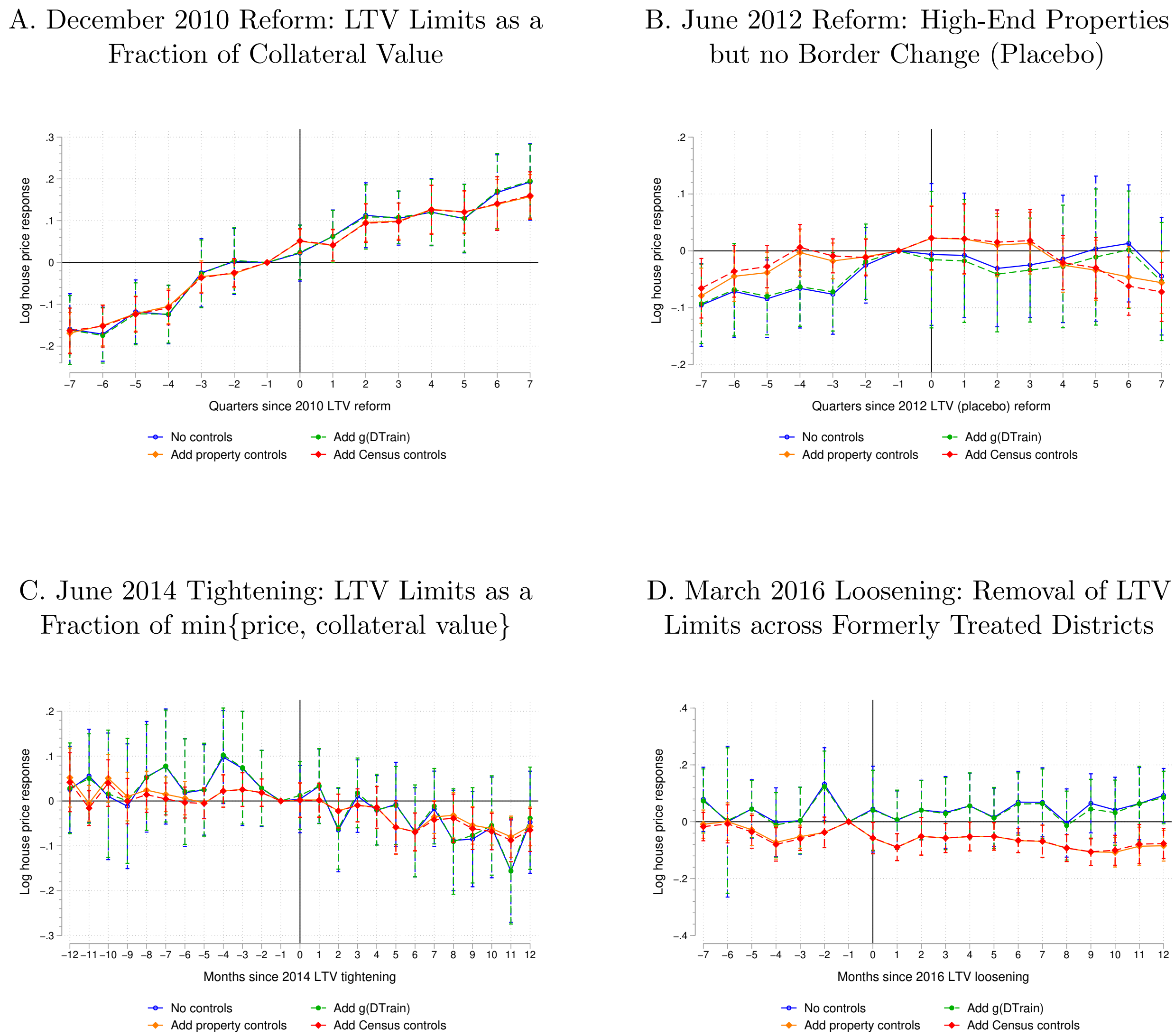
The patterns in Figure 6 and the district-level quality-adjusted price growth estimates in Table 2 point to strong pre-trends in the Central Bank's selection of treated and untreated districts prior to the 2010 reform, but not for the 2014 reform. Figure 6 traces out the dynamic border discontinuity 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.

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 motivations of limiting leverage for investors in areas with strong price growth. We present additional tests of our interpretation in Appendix C.4, where we permute the set of properties in the control group either by using not-yet-treated neighborhoods for the 2010 reform, or using never-treated neighborhoods far away from the policy border as the control group for each reform. This has little quantitative impact on our results, because varying the border distance bandwidth is equivalent to taking a convex combination of these alternative control group sets, and our point estimates in Figure 5 are largely invariant to this bandwidth choice. We show in Appendix C.5 that our border diff-in-disc estimates are similar but more pronounced at close distances to the border when we instead use

[32] 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.

[33] 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



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 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 3 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.

city-level rather than district-level borders to define $LTVCap_{i,c}$ and the bandwidth parameter, which suggests that spatial targeting of leverage limits might be improved by taking into account how commercial banks define the geographic scope of their mortgage markets.

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:

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

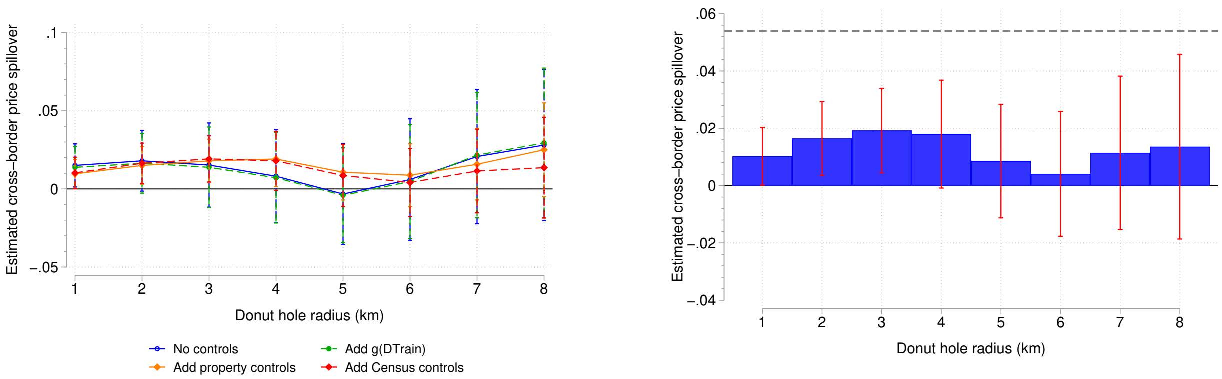
where we formally define the hemi-circle as $\mathcal{H}(r) := \{i | 0 \leq x(i) \leq r\}$ for property i's kilometer distance $x(i)$ from the border. For example, let q denote our distance bandwidth over which we run the border discontinuity regression. If we set $r = q$ to be our baseline distance 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) and de Chaisemartin & D'Haultfœuille (2023).

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 $\widehat{\gamma} - \widetilde{\gamma}$ then identifies the cross-border demand effect of the reform if sales within the donut hole comprise 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

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 a full set of time 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.

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 $\widehat{\gamma} - \widetilde{\gamma}$ 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 utility as an exponential function of distance (Monte, Redding, & Rossi-Hansberg 2018), the estimated spillover declines in magnitude and statistical significance as we move further from the border, from roughly 2 p.p. of the 5% baseline effect at close distances to zero after 4 km. We consider 1 km $\leq r \leq$ 8 km.[34] 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.[35]

Subtracting our upper bound 2 p.p. spillover effect from our baseline estimate of a 5% 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 3%: 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 $\epsilon = \widetilde{\gamma}/\%\Delta LTV \approx 3\%/4\% = 0.75$, or a semi-elasticity of $\widetilde{\gamma}/\Delta LTV = 3\%/2$ p.p. $= 1.5$. 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, 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

[34] There are very few treated observations in the regulated areas but beyond 8 km from the policy border. Hence, at donut hole radii greater than 8 km the estimation sample is mostly properties in the control group.

[35] 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.

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. A simple difference-in-differences in means 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 (see Appendix C.2).

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.[36] 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 show in Appendix E.1 that this drop in sales volume around June 2014 led to only minor short-run declines in revenue from the deed (i.e. stamp duty) tax. In Appendix D.4, we further decompose quantity responses to the LTV reforms and isolate credit supply responses using a shift-share design comparing branches which are more or less exposed to the regulation based on their *ex ante* mortgage lending patterns. Our evidence is inconsistent with lenders steering or rationing credit, pointing to household demand as the dominant force.

We formally test for 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 d where the property is located to the extent that appraisals

[36] 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. See Appendix Appendix C.2 for details.

are anchored to the official one conducted by the local government every three years.[37]

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 infrequently 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 B.[38]

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.[39] We use district-level quarterly index levels obtained via estimating equation (2.1) district by district to interpolate market appreciation in land between revaluation dates. Conditional on these adjustments, (5.3) is 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

---

[37] 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.

[38] 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.

[39] 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 a non-issue if we simply restrict our sample to apartment purchases.

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

$$
\begin{aligned}
Gap_{i,b,d,t} &= \alpha + \gamma_1 \cdot Post_t + \gamma_2 \cdot LTV\_District_{i,d} + \gamma_3 \cdot \Big(Post_t \times LTV\_District_{i,d}\Big) \\
&+ \gamma_4 \cdot 2nd\_Mrtg_i + \gamma_5 \cdot \Big(Post_t \times 2nd\_Mrtg_i\Big) + \gamma_6 \cdot \Big(LTV\_District_{i,d} \times 2nd\_Mrtg_i\Big) \qquad (5.4) \\
&+ \gamma_7 \cdot \Big(Post_t \times LTV\_District_{i,d} \times 2nd\_Mrtg_i\Big) + \mathcal{D}(t,t^*) + \theta' \cdot \mathbf{X}_{i,t} + \beta' \cdot \mathbf{X}_{b,t-1} + \eta_b + \xi_d + \delta_t + \varepsilon_{i,d,b,t}
\end{aligned}
$$

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 and property characteristics at origination, and $\mathbf{X}_{b,t-1}$ includes one-year lagged bank characteristics. 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 reforms.

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 $\mathcal{D}(\cdot)$. 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.[40]

[40]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*. When we include non-apartment transactions, our triple differences estimates of $\widehat{\gamma}_7$ in equation (5.4) 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.

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
District & Time FEs	✓	✓	✓	✓	✓	✓	✓	✓
Bank FEs	✓	✓	✓	✓	✓	✓	✓	✓
Lagged 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 B for full details on how we calculate 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 one-year lagged 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. Lagged bank controls include cash holdings, deposits, total assets, accounts receivable, total loans issued, total liabilities, 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. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

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.[41] For instance, column (6) indicates that the appraisal gap 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. Unit prices for 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 regions very close to the policy border, we uncover positive pricing spillovers equivalent to under half (2 p.p.) of the 5% 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 the incentives to misreport diminish once the limits are redefined as a function of both appraisal values and contract prices.

[41] 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.

Our findings shed light on the effectiveness of spatially targeted LTV policies relative to alternative instruments such as home buyer restrictions (Francke et al. 2023) or transfer taxes on investment properties, which are also frequently enacted to cool down housing markets and which have been shown to generate large welfare losses (Chi, LaPoint, & Lin 2023). Using our reduced form estimates, we calculate welfare losses in housing consumption value terms in Appendix E.2 and show that spatially targeted LTV limits result in losses at least half as severe as those induced by housing transfer taxes. Due to stickiness in the valuation of the real estate tax base, the drop in transaction volumes generated by these leverage restrictions has only minor short-run effects on revenues. Even net of borrowers' avoidance strategies, restricting leverage for relatively price-sensitive home buyers like investors can improve affordability for owner-occupiers in the *ex ante* least affordable neighborhoods without creating a large wedge between the demand and supply of housing units.

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Internet Appendix to

Spatially Targeted LTV Policies and Collateral Values

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A Spatial Variation in U.S. Mortgage Leverage Limits

In this appendix, we describe the data sources and methodology underlying the bivariate map shown in Figure 1 to illustrate imperfect targeting of the U.S. conforming loan limit (CLL). The purpose of this exercise is to demonstrate how the spatially targeted nature of the reforms we analyze in Taiwan might apply to counterfactual scenarios for countries like the U.S. in which household leverage is not currently regulated at the local level in a macroprudential fashion.

The CLL is set by a nationally applicable formula based on index values maintained by the Federal Housing Finance Agency (FHFA), and it is the main mechanism through which household mortgage credit is statutorily restricted in the U.S. Households receive implicit financing subsidies if their conventional loan origination amount falls below the CLL, as the Government Sponsored Enterprises (GSEs), Fannie Mae and Freddie Mac, can purchase their loan on the secondary market if the loan also satisfies certain underwriting creditworthiness criteria.[1] Any loans originated above the CLL are considered "jumbo" loans, and are either held on lenders' balance sheets as portfolio loans or private-label securitized. Loutskina & Strahan (2009) document that banks are more willing to originate jumbo loans if they have greater holdings of liquid assets. Because the CLL rations credit by imposing an illiquidity tax on banks, the policy is akin to risk weighting schemes (sometimes known as "soft" LTV limits) adopted in non-U.S. countries.

The formula determining the CLL for each county can be summarized via:

$$CLL_{i,t} = \alpha \cdot \mathbb{1}\{HighCoL\}_{i,t-1} + (1 + \%\Delta HPI_{t-1,t}) \times \overline{CLL}_{t-1} \tag{A.1}$$

where i indexes counties, and $\mathbb{1}\{HighCoL\}_{i,t-1}$ is a dummy indicating whether i is a high cost-of-living (CoL) county for which the intercept α shifts upwards according to the lagged median home price in the area.[2] The remaining non-high CoL counties (94% of all counties) receive the standard CLL allowance, which is the lagged normal limit ($\overline{CLL}_{i,t-1}$) inflated up by the percentage growth in the *national* house price index.[3] The initial CLL was set to be uniform across counties, and the FHFA only implemented the allowance for high CoL areas as part of the 2008 Housing and

[1] Because Ginnie Mae mainly purchases loans from government programs (e.g., FHA or VA loans), there are certain cases in which it is allowed to purchase loans above the CLL. See Ginnie Mae's program guidelines: https://www.ginniemae.gov/issuers/program_guidelines/MBSGuideLib/Chapter_24.pdf.

[2] As of 2024, high CoL counties consist of areas where 115% of local median home value exceeds the baseline limit value $\overline{CLL}_t$, with a cap such that $\alpha \leq 0.5 \cdot \overline{CLL}_t$. Such counties include those in Alaska and Hawaii and the D.C., Los Angeles, San Francisco, Denver, Seattle, New York City, and Boston metro areas. See the FHFA Conforming Loan Limit Addendum: https://www.fhfa.gov/DataTools/Downloads/Documents/Conforming-Loan-Limit/FHFA-CLL-Addendum-CY2024.pdf.

[3] The loan limits apply to single family residences and small multi-family properties, with higher limits for the latter depending on the number of units between 2 and 4. Hence, if s refers to the market segment, there is also a segment-specific limit $\overline{CLL}_{s,t-1}$.

Economic Recovery Act (HERA).[4]

Loutskina & Strahan (2015) document at the CBSA level that the share of borrowers around the CLL cutoff negatively predicts future house price growth, and expansions in the limit result in larger local house price gains in areas where more borrowers are constrained by the limit. By studying a context like Taiwan's policy experiments in which the leverage limit was directly anchored to the local recent path of house prices, we can begin to answer what would happen to prices and credit access in the U.S. if the formula in (A.1) were rewritten so that the $(1 + \%\Delta HPI_{t-1,t})$ were replaced with a county-specific term $(1 + \%\Delta HPI_{i,[t-1,t]})$.

We construct the maps in Figure 1 to show that the CLL in its current form results in imperfect targeting of credit subsidies if the goal is to limit house price growth or prevent systemic risk to the financial sector due to over-provision of mortgage credit. In particular, we download the list of single-family limits by county from the FHFA and Zillow's county-level price indices for single-family residences.[5] We use house prices from Zillow rather than the FHFA's own price indices, which are an input to the limit calculation in (A.1), because the latter are only available at annual frequency. The fact that Zillow provides its indices at monthly frequency allows us to better match the timing conventions inherent in how FHFA calculates its limits. In particular, the $\%\Delta HPI_{t-1,t}$ term in (A.1) is defined as the YOY national change in house prices between the third quarter of the current year and Q3 of the preceding year. However, using the FHFA's county-level all transaction (AT) index to construct Figure 1 results in minimal changes to the maps. We collapse the Zillow indices to the quarterly frequency by averaging across months in the quarter and to the annual frequency by averaging across months in the year.

The y-axis in the bivariate maps of Figure 1 is the YOY growth in the ratio of the annual HPI to the conforming loan limit set for that year. The x-axis is lagged YOY house price growth between Q3. We rank counties based on terciles of the y-axis and x-axis variables. For instance, in 2021 the cross-county correlation between these two objects is only 29%, indicating that the current calibration of the CLL imperfectly tailors leverage limits to local house prices for the majority of U.S. counties. There is generally strong autocorrelation between the extent to which the CLL binds across counties, but the COVID-19 crisis flipped many counties (e.g. in California and in the Northeast) from having a CLL anchored to local house price growth, to having an unanchored CLL during 2020-2021.

[4]HERA also resulted in the FHFA adopting a "hold harmless," approach to updating the CLL. This means that the county-level limit only adjusts upward and never downward, even if the house price index declines in a county such that the county is no longer considered to be in the high CoL category. Therefore, in the post-HERA world the $\%\Delta HPI_{[t-1,t]}$ term in equation (A.1) is replaced by $\%\Delta HPI_{[t-1,t]} \times \mathbb{1}\{\Delta HPI_{[t-1,t]} > 0\}$.

[5]Annual growth rates from Zillow's home value indices including vs. excluding condos have a 99.7% correlation. The data can be downloaded at: `https://www.zillow.com/research/data/` (accessed August 29, 2023).

B Details on Appraisal Gap Measurement

We introduce the concept of an appraisal gap in Section 5.3 to show that households partially avoid the 60% LTV limit by obtaining inflated appraisals. Our gap measure, defined by (5.3), compares the appraisal value, $A_{i,b,d,t}$, observed for property i on the loan application with bank b to an official appraised value, A^*_{i,d,t^*}, determined by the local district tax assessor for the same property. Given that A^* does not appear on loan application materials, this appendix provides further details on how we back out A^* from the data.

We reconstruct tax assessed housing values A^* from the following official formula which applies to all districts d:

$$A^*_{i,d,t^*} = standard\_value_{i,c,t^*} \times size_i \times (1 - \delta_{i,d,t^*} \times age_{i,t^*}) \times \zeta_{i,d,t^*} \tag{B.1}$$

where the standard value (price in NTD per m^2) varies by city c, revaluation year t^*, the construction and build completion dates for the house, as well as the floor space, building material, and usage of the property (e.g. apartment vs. a detached single-family residence).[6] The depreciation factor δ_{i,d,t^*} is defined in percentage terms and varies by district d, year, the build completion date, and building material. There are upper bounds on the depreciation by age interaction term in (B.1), above which an older property is assumed not to depreciate further.[7] Since the δ_{i,d,t^*} take into account the completion date of any renovations, the product $\delta_{i,d,t^*} \times age_{i,t^*}$ represents depreciation in years of "effective age." Equation (B.1) follows standard computer-assisted mass appraisal (CAMA) modeling conventions adopted by local tax assessors in the U.S. (IAAO 2017).

Importantly for our analysis, the model format in (B.1) does not vary over our sample period, and local governments rarely update individual factors. Even if a district does update their standard value or depreciation factor in a new revaluation year, this occurs with at least a one-year lag and therefore would not be reflected in our definition of the gap in (5.3). Further, our triple differences specification in equation (5.4) compares second to first properties located in the same district, and none of the terms in (B.1) varies by the ordinality in an investor's portfolio. In short, this means that changes to local property tax assessment methods are not driving our measurement of the widening gap between loan collateral and official appraised values around the 2014 LTV reform.[8]

We directly observe the determinants of the standard value, depreciation factor, and size and age

[6] Official documentation of this formula can be found at the eTax Portal of the Ministry of Finance here (Chinese language only): `https://www.etax.nat.gov.tw/etwmain/tax-info/understanding/tax-saving-manual/local/house-tax/5qYVKWW`.

[7] For some building material × effective build date cells, there is no district-specific depreciation factor. The typical cap on cumulative depreciation is 60% for buildings which describe the majority of home transactions in our sample.

[8] 82% of transacted housing units in our sample are in non-steel reinforced concrete buildings. Within this structure type, average parameters in LTV-regulated areas include a standard value of 5,528 NTD per m^2 ($\approx$ 173 USD), an annual depreciation rate of 1.12%, and a depreciation cap of 60%.

of the property. We hand-collect the standard value for each city and the depreciation factors for each district, while we obtain the age and size of the property from the mortgage application form.[9] However, we do not observe the unpublished road adjustment factor ζ_{i,d,t^*}, which accounts for the hedonic value of both the width of the front-facing road adjoining the property and the property's position on that road. We therefore impute the road adjustment rate using three-step procedure:

1. For each property in the confidential tax returns available through the Financial Information Agency of the Ministry of Finance, we take logs on both sides and rearrange (B.1) to obtain $\log(\zeta_{i,d,t^*})$ as the difference between the log appraisal value and portion of the log appraisal value due to the standard value, size, and effective age of the property.

2. We cannot directly merge the tax data from the Ministry of Finance to the loan-level data provided by the Central Bank to guarantee individuals' privacy. We therefore regress $\log(\zeta_{i,d,t^*})$ on a full set of district by revaluation year fixed effects and exponentiate to obtain the rescaled point estimates on each dummy $\widehat{\zeta}_{d,t^*}$ which can then be merged back to the loan-level data by the property's district and the last t^* before the origination date.[10]

3. Using the loan-level data, we obtain an estimated government appraisal value $\widetilde{A}^*_{i,c,t^*}$ by combining via (B.1) the $size_i$ and age_{i,t^*} from the collateral reporting form, the known $standard\_value_{i,c,t^*}$ and depreciation factor series we hand-collected for each area, and the merged in $\widehat{\zeta}_{d,t^*}$.

Implicitly, our procedure assumes that within the same district-revaluation year, the contribution of the road position to local government valuations is the same for each property. This assumption will generally not hold, and so the estimate $\widetilde{A}^*_{i,d,t^*}$ we obtain is an approximation of the true official appraisal value. However, this approximation is not an issue for our research design as long as the true property-level road adjustment factors ζ_{i,d,t^*} do not vary, on average, across the treatment and control groups before vs. after the LTV limit. Hence, in our triple differences specification (5.4), for this assumption to be problematic it would need to be the case that the road adjustment factor varies within district across first and second mortgaged properties, even after controlling for other observable property characteristics which may be collinear with road positioning.

One plausible reason why investment properties might systematically differ from non-investment properties in terms of the building's position on a street or street width is that the former are more likely to be high-rise apartment units. Apartments in such buildings are more likely to be in zoning areas with higher minimum road widths, which will carry greater road adjustment factors. However, we show in our main set of results for the appraisal gap analysis in Table 7 that the appraisal gap is of a similar magnitude to the full sample estimates when we restrict the sample

[9] The standard value and depreciation schedules are available in PDF form from each local government's website. We provide a spreadsheet with links to each of these PDFs in our replication files.

[10] In practice, most of the variation in ζ_{i,d,t^*} is across districts, as many districts do not update their road adjustment factor vector during our sample period.

to apartment units. This test suggests that differences in the appraisal value A^* due to unobserved differences in the road width of the property are not driving our results on avoidance of LTV limits through inflated appraisals.

C Additional Results for LTV Limit Policies

We present several sets of additional results in this appendix related to tracing out effects of the spatially targeted LTV policies on housing market outcomes:

- The matched DiD estimated effects we present for loan contract features in Table 4 are relatively invariant to our choice of LTV percentage bandwidth. Moreover, we find null effects for the placebo group of loans within regulated counties which are tied to mortgages on primary residences and therefore are not subject to LTV limits (Appendix C.1).
- Our finding of an appraisal gap in Section 5.3 is robust to rescalings of the gap between loan appraisal values and official appraisals (Appendix C.2).
- Transaction volume drops off substantially in regulated counties relative to unregulated (non-border) counties after the 2014 reform. However, we do not see the same decline in home sales volumes after the 2010 reform due to the more pronounced misreporting loophole used to avoid the LTV limits during that policy regime (Appendix C.3). This result for the 2010 reform is driven by the set of mortgaged non-land transactions for which the LTV limit was defined as a function of only the appraisal value rather than a market price.
- Our baseline results from Section 5.1.2 – indicating that prices continued on trend in regulated areas under the 2010 LTV reform (with loophole) but that the 2014 LTV reform (closed loophole) led to lower house prices in the directly regulated areas – hold when we use alternative control groups of properties, including never-treated and not-yet-treated districts as control groups (Appendix C.4).
- The 2014 LTV policy generated heterogeneous effects on prices according to *ex ante* neighborhood average income. Price declines were more pronounced in higher-income areas which were subject to the binding LTV limits under that reform (Appendix C.4).
- Our main results showing a drop off in prices close to the regulated side of the border hold if we define treatment at the level of cities rather than districts and redraw the policy border (Appendix C.5). This finding suggests regulators might improve the spatial targeting of household leverage limits by redefining policy areas according to bank branch locations rather than administrative boundaries.

C.1 Additional Matched DiD Results

Figure C.1 plots point estimates from the matched DiD estimator defined in Section 4.1 by choice of LTV bandwidth of $\pm x\%$ around the 60% regulatory cutoff for six loan outcomes. The charge-off flag uses the full performance information for the matched sample of loans, tracking each loan up to five years after its origination.[11] Vertical lines in each graph indicate the point estimate obtained under our baseline bandwidth of $\pm 4\%$, as reported in Table 4 for each reform. The differences we uncover for matched samples of loans are similar across bandwidth choices, even though we face a bias-variance tradeoff in that smaller bandwidths restrict the sample sample size but potentially reduce bias by comparing more similar loans. For mortgage loans tied to second properties, which are directly regulated, we find robustly negative effects on loan origination amounts, prices per square meter, interest rates, and closing costs, with null effects on loan charge-off rates.

C.2 Additional Appraisal Gap Results

Table C.2 re-estimates the triple differences specification given by equation (5.4), but with the appraisal gap defined using the alternative scaling proposed by Kruger & Maturana (2021). This alternative scaling measure is the gap between the bank and official appraisal in levels, divided by the average appraisal value, or $(A - A^*)/0.5(A + A^*)$. We find a robustly positive effect on the triple interaction term $Post_t \times LTV\_District_{i,d} \times 2nd\_Mrtg_i$ from equation (5.4), indicating that the appraisal gap grew for the regulated segment of the mortgage market after the 2014 LTV tightening. The interpretation of the estimates under this scaling is that the appraisal gap grew by between 2% to 3% relative to the midpoint between the loan appraisal and the last available official appraisal value.

C.3 Effects of LTV Reforms on Transaction Volume

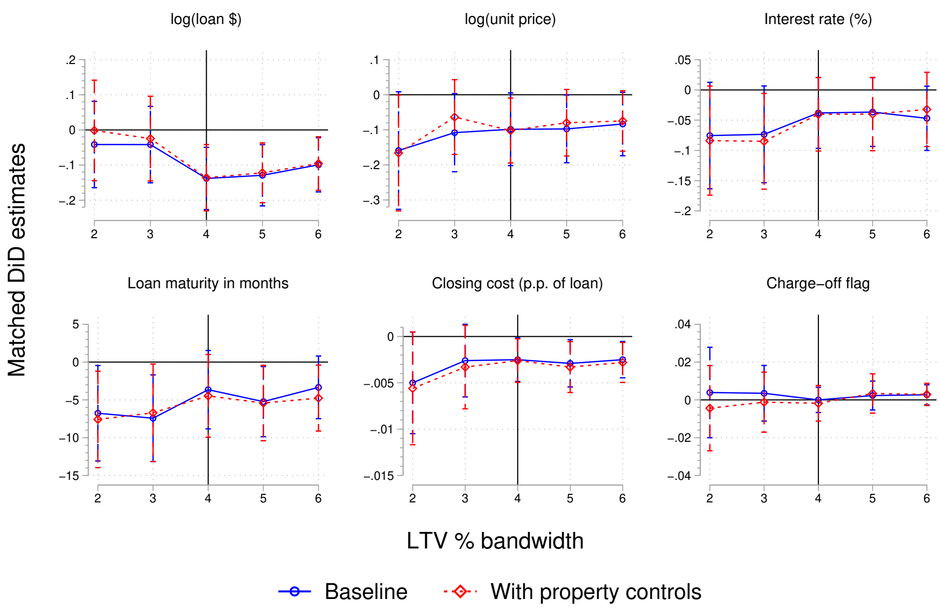
Although most of our analysis focuses on pricing, here we show that home sales volume moves in a similar fashion to prices around the 2010 and 2014 LTV reforms. We start by performing simple difference-in-differences (DiD) in means tests in Figure C.2, where we compare under each reform home sales volume by pricing bin in treated districts to untreated districts which are not adjacent to the policy border. Home sales volume actually continues to increase in regulated districts relative to the unregulated ones following the December 2010 series of LTV limits, while it declines after the June 2014 LTV limit expansion.

As documented in Section 5.3, the discrepancy is due to a loophole which made it easier to circumvent the earlier LTV limit via collateral inflation. During the first LTV tightening in December 2010, the LTV limit was set at 60% of the loan appraisal value. A simple

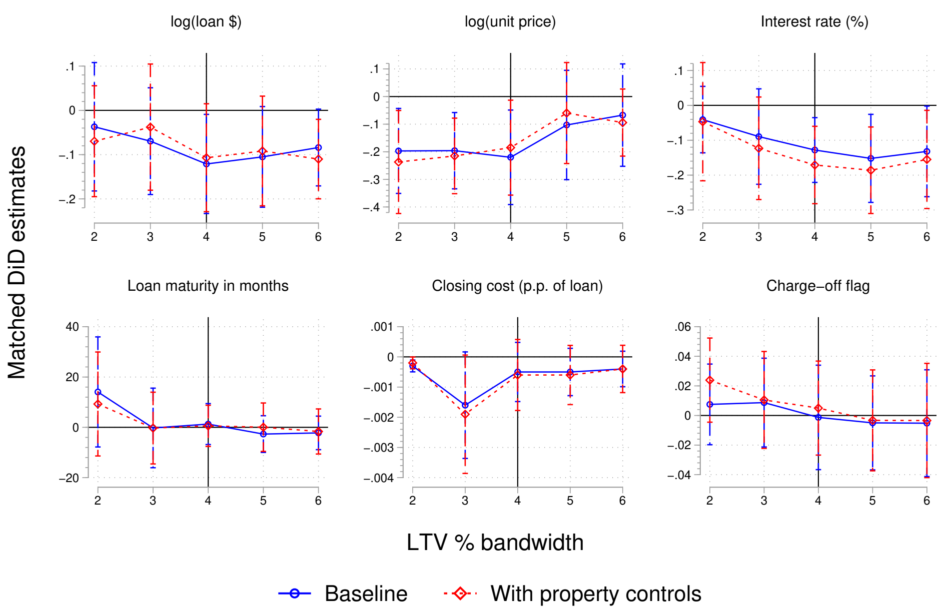
[11] This tracking horizon is truncated by the length of our sample rather than constituting an econometric parameter choice.

FIGURE C.1. Matched DiD Estimates of LTV Limits on Loan Outcomes by Bandwidth

A. 2010 LTV Tightening



B. 2014 LTV Tightening



Notes: Each panel in the figure shows the results from estimating average treatment effects on the treated using the matched difference-in-differences estimator in (4.1), according to our choice of LTV bandwidth $\pm x\%$ along the x-axis. We show results for six loan contract outcomes: log loan amount originated, log price per square meter (unit price), the initial interest rate (% of loan amount), the maturity of the loan (in months), closing costs (in additional p.p. of the loan), and the charge-off flag. The charge-off flag is a flag equal to one if the loan is subsequently written off by the bank over its entire performance history. We perform this exercise separately for the 2010 reform (Panel A) and 2014 reform (Panel B), both of which featured tightenings of the maximum LTV on second mortgages in regulated areas. See Section 4.1 for details on implementation of the matched DiD method. Point estimates in red residualize on a vector of property characteristics. Bars indicate 95% confidence intervals obtained from clustering standard errors at the bank and distrct level. We follow the same estimation procedures as in Section 5.1.

Table C.2. DDD Evidence of Collateral Misreporting: Alternative Appraisal Gap Measure

Transaction types:	All transactions		Apartment units	
	(1)	(2)	(3)	(4)
α	2.37***	2.18***	0.80	1.37**
	(3.19)	(2.84)	(1.36)	(2.20)
$Post_t$	−0.01	0.02**	−0.01	0.03**
	(0.49)	(2.29)	(0.36)	(2.92)
$LTV\_District_{i,d}$	0.24***	0.24***	0.27***	0.26***
	(9.11)	(6.44)	(8.95)	(7.13)
$Post_t \times LTV\_District_{i,d}$	−0.05**	−0.09***	−0.07*	−0.12***
	(1.96)	(5.83)	(1.81)	(6.19)
$2nd\_Mrtg_i$	0.03***	0.03***	0.02***	0.03**
	(4.22)	(5.56)	(3.18)	(3.14)
$Post_t \times 2nd\_Mrtg_i$	−0.02**	−0.02***	−0.01	−0.01
	(2.45)	(3.33)	(1.21)	(1.79)
$LTV\_District_{i,d} \times 2nd\_Mrtg_i$	−0.03***	−0.03***	−0.03**	−0.03***
	(3.39)	(5.91)	(3.02)	(3.63)
$Post_t \times LTV\_District_{i,d} \times 2nd\_Mrtg_i$	0.02***	0.03***	0.02*	0.03***
	(2.83)	(4.52)	(1.81)	(3.21)
$\mathcal{D}(t, t^*)$	−0.03*	0.00	−0.03**	0.00
	(1.93)	(1.32)	(2.28)	(1.49)
Drift function	dummy	linear	dummy	linear
District & Time FEs	✓	✓	✓	✓
Bank FEs	✓	✓	✓	✓
Lagged bank controls	✓	✓	✓	✓
Property controls	✓	✓	✓	✓
Borrower controls	✓	✓	✓	✓
N	41,178	40,112	29,797	29,268
Adj. R^2	0.66	0.73	0.70	0.77

Notes: The table presents coefficients obtained from estimating triple differences equation (5.4) with the appraisal gap defined à la Kruger & Maturana (2021) as the outcome: $Gap = (A - A^*)/0.5(A + A^*)$. 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 $(t - t^*)$. 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 B for full details on how we calculate 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 one-year lagged 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. Lagged bank controls include cash holdings, deposits, total assets, accounts receivable, total loans issued, total liabilities, 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. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

difference-in-differences shows that volume *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.[12] 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 conduct a covariate-adjusted version of the difference-in-differences tests in Table C.3. Collapse our housing transaction data to district $\times$ time cells and estimate regressions of the following form:

$$\log(Volume_{d,t}) = \gamma \cdot \Big(LTVCap_{d,t} \times Post_{d,t}\Big) + g(\overline{DTrain}_{d,t}) + \beta' \cdot \mathbf{X}_{c,t} + \xi_d + \delta_t + \varepsilon_{d,t} \qquad \text{(C.1)}$$

where the outcome is the (log) number of sales. $LTVCap_d$ is a dummy equal to one if the district is subject to LTV limits. The control group consists of districts which are never regulated and are non-adjacent to the regulated districts. Analogous to our border diff-in-disc design in equation (4.2), we include $g(\overline{DTrain}_{d,t})$, which refers to a linear spline in 10 decile bins of average distance to the nearest train station across all transactions in that district-month. The time-varying nature of $g(\overline{DTrain}_{d,t})$ controls for the possibility that limiting credit access might induce households to substitute towards cheaper properties within the same district which are further from transit. The vector of city-level Census demographic characteristics $\mathbf{X}_{c,t}$ includes the unemployment rate, log disposable income, and average number of persons in the household; our results are nearly unchanged if we instead lag these variables to account for the bad control problem.

Table C.3 shows on a covariate-adjusted basis that transaction volume continued to increase by 20% after the 2010 LTV reform. As described in Table 1, the LTV limit for mortgaged land was set to 65% $\times$ min{price, collateral value}, thus rendering it more difficult to avoid the limits through collateral inflation. Consequently, sales volume for land dramatically drops off, by over 50%, after the 2010 reform. This offers further evidence of the importance of anchoring valuations to market prices in designing LTV limit policies. Similarly, including transactions with residential land parcels attenuates the measured change in quantities for the 2010 reform by roughly one-half.

Since the formula was defined in proportion to min{price, collateral value} for both mortgaged buildings and land under the 2014 regime, altering the composition of the estimation sample along these dimensions barely influences the magnitude of the volume declines (Panel B).[13] We estimate

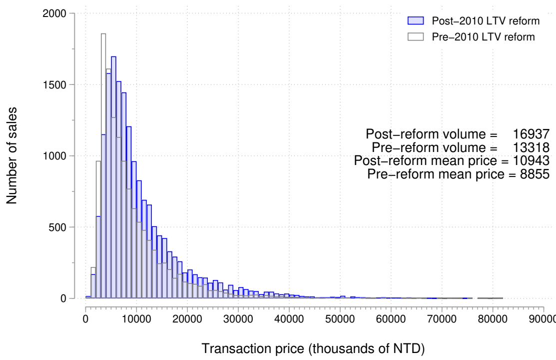
[12] The p-values on the Kolmogorov-Smirnov tests for each of the differences in Figure C.2 are all < 0.0001.

[13] We cannot fully separate land-only transactions around the 2014 reform due to how data are provided to the public. Therefore, we pool building transactions and sales in which land is also included.

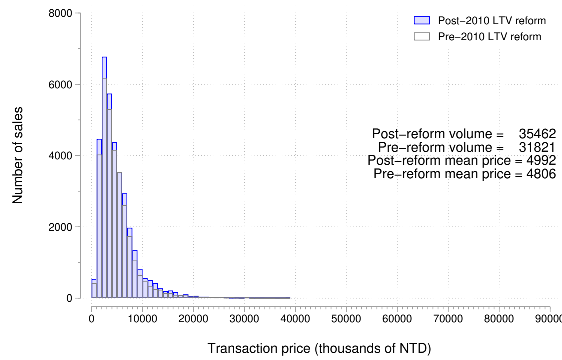
FIGURE C.2. Home Sales Volume Distributions in the 2010 vs. 2014 LTV Regimes

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

Treated Districts

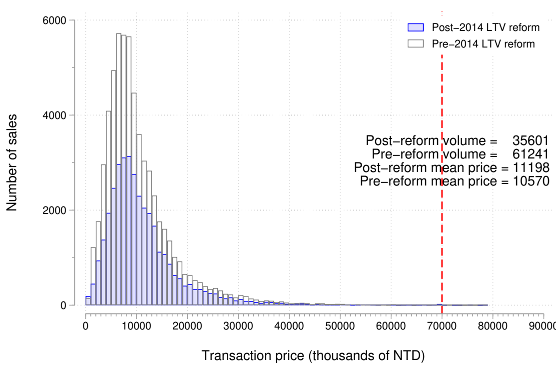


Untreated (Non-Border) Districts

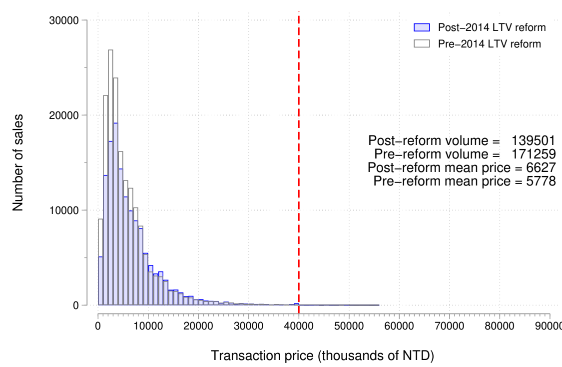


B. 2014 Reform: LTV Limits as a Fraction of min{price, collateral value}

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 only 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 $\pm 10 \times IQR$.

Table C.3. District-Level Volume Difference-in-Differences Estimates

A. 2010 LTV Tightening: Different Effects for Buildings vs. Land (Loophole)

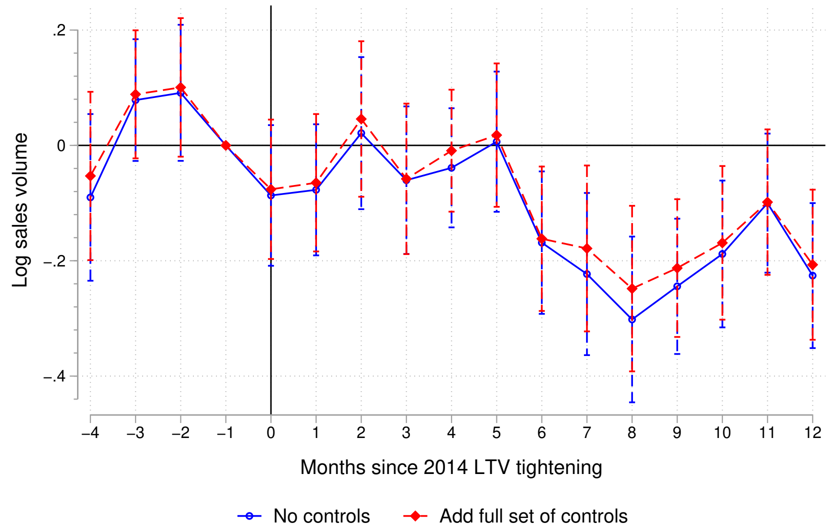
	(1)	(2)	(3)	(4)	(5)	(6)
$LTVCap \times Post$	0.238^{***}	0.236^{***}	0.182^{***}	-0.635^{***}	-0.636^{***}	-0.497^{***}
	(0.055)	(0.053)	(0.061)	(0.180)	(0.179)	(0.101)
Sample	Buildings	Buildings	Buildings	Land	Land	Land
District & Time FEs	✓	✓	✓	✓	✓	✓
Census controls		✓			✓	
Lagged Census controls			✓			✓
$g(\overline{DTrain})$			✓			✓
N	4,201	4,152	2,852	4,623	4,520	3,182
# districts	288	284	260	312	303	285
Adj. R^2	0.853	0.855	0.871	0.646	0.643	0.696

B. 2014 LTV Tightening: Similar Effects for Building vs. Land

	(1)	(2)	(3)	(4)	(5)	(6)
$LTVCap \times Post$	-0.326^{***}	-0.312^{***}	-0.258^{***}	-0.334^{***}	-0.317^{***}	-0.262^{**}
	(0.022)	(0.023)	(0.023)	(0.027)	(0.028)	(0.029)
Sample	Buildings	Buildings	Buildings	All	All	All
District & Time FEs	✓	✓	✓	✓	✓	✓
Census controls		✓			✓	
Lagged Census controls			✓			✓
$g(\overline{DTrain})$			✓			✓
N	6,462	6,382	5,276	6,467	6,387	5,282
# districts	297	291	272	297	291	272
Adj. R^2	0.913	0.917	0.909	0.921	0.924	0.917

Notes: The table displays results from estimating difference-in-differences equation (C.1) for the 2010 LTV reform (Panel A) and the 2014 reform (Panel B) with log district-level home sales volume as the outcome variable. The control group for which $LTVCap_d = 0$ consists of districts which are never regulated and are non-adjacent to the regulated districts. In each panel, the first three columns restrict to transactions involving only buildings or apartment units, while the latter three columns adds to the calculation of $Volume_{d,t}$ transactions of structures which may or may not include the sale of an attached land parcel. Columns 3 and 6 include one-year lagged Census controls for the 2010 reform, or one-quarter lags for the 2014 reform. We restrict to the longest possible time window of symmetric length around each reform to rule out the influence of other reforms. We exclude transactions involving institutions given that separate LTV limits applied to non-individual buyers regardless of property location (cf. Table 1). We identify institutional investors using standard text parsing methods (Lambie-Hanson, Li, & Slonkosky 2022). Robust standard errors clustered by district in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

FIGURE C.3. Dynamic DiD Estimates for Sales Volume around 2014 LTV Reform



Notes: We plot the estimated coefficients obtained from a dynamic version of equation (C.1) applied to the 2014 reform, with log sales volume at the district-month level as the outcome variable. The coefficients in blue correspond to the specification listed in column 1 of Panel B of Table C.3; those in red correspond to column 3 of the same table. Bars around each point estimate indicate 95% robust confidence intervals obtained by clustering standard errors at the district level.

the 2014 reform resulted in a 29% drop in home transaction volume in regulated districts (column 3). We plot the monthly dummies from the event study version of equation (C.1) in Figure C.3. Transaction volume sharply drops six months into the more stringent and geographically expansive LTV regime. This timing pattern mirrors the timing of the house price declines around the same reform in Panel C of Figure 5.

C.4 Additional Diff-in-Disc Pricing Results

Our baseline diff-in-disc pricing results in Section 5.1.2 zoom in on housing market activity around each policy border to isolate the causal effects of spatial LTV policies on overall house prices. Our regression discontinuity bandwidth parameter of distance to the policy border helps us compare otherwise similar properties in regulated vs. unregulated areas while flexibly controlling for the continuous evolution of across three-dimensional space. The bandwidth parameter can also be thought of as a particular convex combination of two different sets of control properties: those in never-treated districts and those in not-yet-treated or initially treated districts.

Our baseline diff-in-disc design raises several possible concerns which we address here by using alternative control group definitions. We show in Section 5.2 that spillover effects across the border appear to be sufficiently small such that there is still a sizeable direct negative effect on prices for the 2014 reform, but our donut hole approach relies on further parametric assumptions. For the 2010 reform which featured a loophole encouraging inflated appraisals to avoid the LTV limit, the presence of a strong pre-trend may simply reflect other changing housing market conditions unrelated to household leverage policies. After all, the initially treated set of districts were selected precisely because they had experienced the largest recent house price gains (see Table 2).

To address these concerns, we first consider never-treated districts faraway from the border (i.e. outside our bandwidth radius) as a control group to remove any influence of cross-border demand spillovers due to home investors sorting to the unregulated neighborhoods. Table C.4 documents that our point estimates of negative pricing effects in regulated areas are almost identical to those in Table 6 for the main 2014 reform.[14] This supports our donut hole design in that the exercise compares sales in regulated areas to those in unregulated areas which also lie in a different commuting zone. Similarly, in Table C.5 for the 2010 reform, prices continue on trend relative to the pre-reform period (as also shown in Panel A, Figure 5) even when we use the not-yet-treated districts (i.e. those regulated in 2014) or the faraway never-treated districts as control groups.

Given that the 2014 reform was successful at directly reducing prices in regulated districts – both those newly regulated and those previously regulated – we now turn to the question of heterogeneous effects across different neighborhoods. This is a natural question given that an objective of these targeted spatial LTV limits was to render housing more affordable for first-time homebuyers. We explore whether prices fell by more in *ex ante* high or low-income neighborhoods within regulated districts. We adopt two specifications. Our first one identifies income elasticities by augmenting our baseline diff-in-disc model in equation (4.2) and adding interactions with log *ex ante* average district-level income:

$$\begin{aligned}
\log(p_{i,d,t}) &= \gamma_1 \cdot \Big(LTVCap_{i,d} \times Post_{d,t}\Big) + \gamma_2 \cdot \Big(\log(Income_d) \times Post_{d,t}\Big) \\
&+ \gamma_3 \cdot \Big(\log(Income_d) \times LTVCap_{i,d} \times Post_{d,t}\Big) \\
&+ f(lat_i, lon_i) + g(DTrain_i) + \beta' \cdot \mathbf{X}_{i,d,t} + \xi_d + \delta_t + \sum_{s=1}^{N} \phi_i^s + \varepsilon_{i,d,t}
\end{aligned} \tag{C.2}$$

where γ_3 captures the percentage effect on house prices of a 1% increase in average resident income in a district which becomes subject to the strict 60% LTV limit in June 2014. Equation (C.2) might mask non-monotonicities in the relationship between neighborhood quality, as proxied by income, and changes in housing demand due to leverage restrictions. We therefore also run specifications

[14] Note that we do not consider the previously treated districts under the 2010 reform to be a valid control group for districts newly regulated under the 2014 reform. The reason is that, as discussed at length in Section 2 and Section 5.3, the 2010 and 2014 regimes defined the LTV using a different denominator, so one could argue that all of the regulated districts in 2010 were newly regulated in 2014.

Table C.4. 2014 Border Diff-in-Disc Estimates: Faraway Never-Treated Districts as Control Group

	(1)	(2)	(3)	(4)	(5)	(6)
$LTVCap \times Post$	-0.076^{***}	-0.057^{***}	-0.058^{***}	-0.056^{***}	-0.056^{***}	-0.082^{***}
	(0.014)	(0.008)	(0.008)	(0.008)	(0.008)	(0.011)
	[0.009]	[0.005]	[0.005]	[0.005]	[0.005]	[0.008]
Sample	Buildings	Buildings	Buildings	Buildings	Buildings	All
$f(lat, lon)$	Linear	Linear	Linear	Linear	Quadratic	Linear
District & Time FEs	✓	✓	✓	✓	✓	✓
$g(DTrain)$	✓	✓	✓	✓	✓	✓
Property controls		✓	✓	✓	✓	✓
Census controls			✓	✓	✓	✓
Border segment FEs				✓	✓	✓
N	221,280	221,280	220,719	220,716	220,716	268,056
# districts	278	278	272	272	272	272
Adj. R^2	0.256	0.818	0.818	0.823	0.823	0.607

Notes: The table shows results from estimating versions of the pooled border difference-in-discontinuity model in equation (4.2) with log house prices as the outcome variable for the main reform of interest which tightened LTV limits in specific districts in June 2014. Unlike our baseline estimates in Table 6, here we use as a control group properties in districts never treated by any of the LTV reforms which are also located at least 20 km away from the border, while the treatment group includes properties in all districts located inside the 2014 policy border. The outcome in each regression is the log house price. All specifications include day-of-week, week-year, and holiday fixed effects as well as $g(DTrain)$, which refers to a linear spline in 20 quantile bins of distance to the train station closest to each property. The spatial control 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. The last three columns include 2 km border segment fixed effects to account for topographical differences along the policy border which might impact changes in housing demand. 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 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. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table C.5. 2010 Border Diff-in-Disc Pricing Estimates: Alternative Control Groups

A. Not-Yet-Treated Districts as Control Group

	(1)	(2)	(3)	(4)	(5)	(6)
$LTVCap \times Post$	0.219***	0.189***	0.127***	0.134***	0.134***	0.158***
	(0.037)	(0.025)	(0.029)	(0.029)	(0.029)	(0.033)
Sample	Buildings	Buildings	Buildings	Buildings	Buildings	All
$f(lat, lon)$	Linear	Linear	Linear	Linear	Quadratic	Linear
District & Time FEs	✓	✓	✓	✓	✓	✓
$g(DTrain)$	✓	✓	✓	✓	✓	✓
Property controls		✓	✓	✓	✓	✓
Census controls			✓	✓	✓	✓
Border segment FEs				✓	✓	✓
N	41,723	41,723	41,723	41,720	41,720	48,234
# districts	40	40	40	40	40	41
Adj. R^2	0.423	0.862	0.863	0.869	0.869	0.613

B. Faraway Never-Treated Districts as Control Group

	(1)	(2)	(3)	(4)	(5)	(6)
$LTVCap \times Post$	0.170	0.160***	0.228***	0.231***	0.226***	0.234**
	(0.100)	(0.034)	(0.065)	(0.059)	(0.060)	(0.091)
Sample	Buildings	Buildings	Buildings	Buildings	Buildings	All
$f(lat, lon)$	Linear	Linear	Linear	Linear	Quadratic	Linear
District & Time FEs	✓	✓	✓	✓	✓	✓
$g(DTrain)$	✓	✓	✓	✓	✓	✓
Property controls		✓	✓	✓	✓	✓
Census controls			✓	✓	✓	✓
Border segment FEs				✓	✓	✓
N	30,686	30,686	30,686	30,681	30,681	31,717
# districts	31	31	31	31	31	32
Adj. R^2	0.412	0.863	0.863	0.873	0.873	0.594

Notes: The table shows results from estimating versions of the pooled border difference-in-discontinuity model in equation (4.2) for the earlier reform which, due to a collateral valuation loophole, only partially tightened LTV limits in specific districts in June 2010 and December 2010. Unlike our baseline estimates in Table 6, here we use as a control group either: (i) in Panel A, properties in not-yet-treated districts which will later be regulated in 2014; or (ii) in Panel B, properties in districts never treated by any of the LTV reforms which are also located at least 20 km away from the border, while the treatment group includes properties in all districts located inside the 2014 policy border. The outcome in each regression is the log house price. All other details about the sets of fixed effects and controls are the same as in Table 6. We restrict to the longest possible time window of symmetric length around the 2010 reform to rule out the influence of other reforms, or 2008Q2 to 2012Q2. We winsorize prices at the 1st and 99th percentiles and restrict to arms-length transactions. Robust standard errors clustered by district in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

where we replace $\log(Income_d)$ with a set of dummies for the quintile of district income:

$$\log(p_{i,d,t}) = \gamma_1 \cdot \Big(LTVCap_{i,d} \times Post_{d,t}\Big) + \gamma_2 \cdot \Big(\sum_{q=2}^{5} \mathbb{1}_q\{Income_d\} \times Post_{d,t}\Big)$$
$$+ \gamma_3 \cdot \Big(\sum_{q=2}^{5} \mathbb{1}_q\{Income_d\} \times LTVCap_{i,d} \times Post_{d,t}\Big) \tag{C.3}$$
$$+ f(lat_i, lon_i) + g(DTrain_i) + \beta' \cdot \mathbf{X}_{i,d,t} + \xi_d + \delta_t + \sum_{s=1}^{N} \phi_i^s + \varepsilon_{i,d,t}$$

where the dummy for the bottom quintile, $\mathbb{1}_1(Income_d)$ is the reference category. We keep all other aspects of the specification and bandwidth selection criteria the same as in our previous models in Section 5.1.2.

Table C.6 presents results for both the continuous log-log specification (C.2) in the first four columns and the discretized one (C.3) in the last four columns. All columns indicate that the strict LTV limit imposed in 2014 had a disproportionate effect on higher-income neighborhoods. The implied elasticity is -0.1, meaning properties in a 10% more affluent district experiences a 1% greater decline in price after the policy. The effect is entirely concentrated in the top half of the income distribution. We note that this is not simply a mechanical result driven by our use of a distance bandwidth to zoom in around the policy border – there are both regulated and unregulated districts in each income quintile bin. Thus, spatially targeted LTV policies improve the affordability of homeownership in more exclusive neighborhoods. This could be a function of both the focus of these policies on restricting access to mortgage credit for investment properties, or the fact that house flippers tend to rely on higher-leverage loans anyways, as their investment strategies involve putting little money down to purchase and then betting on rapid price appreciation.

C.5 Diff-in-Disc Using Mortgage Market Boundaries

Despite the fact that the LTV limit policies targeted specific districts, one possibility is that discontinuities appear around borders corresponding to how mortgage lenders define geographic markets. If so, then regulators might improve the spatial targeting of household leverage limits, like the ones we study, by redefining policy areas according to bank branch locations rather than administrative boundaries. While the geographic scope of mortgage markets is an open research question, the commercial banks who comprise most of the loans originated in our setting operate many branches within each city. Multi-branch banks typically direct borrowers to the nearest branch office to file any necessary documents in person as part of the loan screening process. This means that the lender may act as if regulations in one district apply more broadly, such that there may be within-bank spillovers across branches in the same city.

To test for whether mortgage market boundaries play a role in the spatial targeting of leverage

Table C.6. Heterogeneous Pricing Effects of 2014 LTV Reform by Neighborhood Income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\log(Income) \times LTVCap \times Post$	-0.097^{***}	-0.103^{***}	-0.102^{***}	-0.104^{***}				
	(0.020)	(0.021)	(0.019)	(0.019)				
$2nd\_Quintile \times LTVCap \times Post$					-0.027	-0.027	-0.024	-0.025
					(0.021)	(0.020)	(0.018)	(0.018)
$3rd\_Quintile \times LTVCap \times Post$					-0.014	-0.021	-0.023	-0.024
					(0.023)	(0.022)	(0.020)	(0.020)
$4th\_Quintile \times LTVCap \times Post$					-0.045^{**}	-0.054^{***}	-0.045^{**}	-0.044^{**}
					(0.020)	(0.020)	(0.018)	(0.019)
$5th\_Quintile \times LTVCap \times Post$					-0.066^{***}	-0.074^{***}	-0.074^{***}	-0.072^{***}
					(0.021)	(0.021)	(0.019)	(0.019)
Sample	Buildings	Buildings	Buildings	Buildings	Buildings	Buildings	Buildings	Buildings
Bandwidth (km)	20	20	20	20	20	20	20	20
$f(lat, lon)$	Linear	Linear	Linear	Quadratic	Linear	Linear	Linear	Quadratic
District & Time FEs	✓	✓	✓	✓	✓	✓	✓	✓
$g(DTrain)$	✓	✓	✓	✓	✓	✓	✓	✓
Property controls	✓	✓	✓	✓	✓	✓	✓	✓
Census controls		✓	✓	✓		✓	✓	✓
Border segment FEs			✓	✓			✓	✓
N	105,569	105,569	105,569	105,569	105,569	105,569	105,569	105,569
# districts	73	73	73	73	73	73	73	73
Adj. R^2	0.823	0.824	0.835	0.836	0.823	0.824	0.835	0.836

Notes: The table shows results from estimating augmented 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. Columns 1 through 4 present results from the augmented model of equation (C.2), in which we interact regulatory treatment with the log of average district-level income in 2013. Columns 5 through 8 present results from equation (C.3) in which we instead interact regulatory treatment with dummies for the quintile of average district-level income in 2013, and the first quintile of district-level income serves as the reference category. The outcome in each regression is the log house price. All specifications include day-of-week, week-year, and holiday fixed effects, property controls, as well as $g(DTrain)$, which 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 columns 4 and 8 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. Some columns include 2 km border segment fixed effects to account for topographical differences along the policy border which might impact changes in housing demand. 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. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

limits, we rerun our baseline diff-in-disc equation (4.2) but redefine the border at the city level rather than the finer district level. Specifically, for each reform, we count a city as "treated" if any districts contained within its borders are subject to LTV limits and then draw the policy boundary including that city's boundary. Therefore for the 2010 LTV reform, we draw the policy boundary to include all of Taipei and New Taipei, and for the 2014 reform, we expand the boundary to include the city of Taoyuan. In equation (4.2), this corresponds to changing $LTVCap_{i,d}$ to $LTVCap_{i,c}$, where the latter is equal to one if property i is located in a city c containing a treated district d.

Figure C.4 demonstrates that the pricing discontinuities are sharper when we use the city-level boundaries instead of the district-level policy boundaries in our original Figure 5. For the main tightening reform in 2014 which applied to districts in three cities (Panel A), there is a clear discontinuity in prices of -3% within 2 km of the border. Whereas in our baseline analysis we only obtain statistically negative diff-in-disc pricing effects for the 2014 reform at distances of ≥ 4 km from the district-level border. This provides evidence that the geographical definition of a mortgage market, or the span of local banking networks, matters for the pass-through of leverage limits to house prices.

Importantly, the estimated pricing effects for the 2014 reform are stable at bandwidths beyond 8 km, which is roughly equal to the average distance between the city-level border segments and the "correct" district-level border segments. For instance, at our baseline bandwidth of 20 km that we impose throughout the paper, the pricing effect using city-level borders is -6%, compared to -5% with district-level borders. As before, in Panel B which uses the placebo 2012 reform which only applied to high-end properties while leaving policy borders unchanged, we find a null effect on prices around the city-level border. In Panel D, we still do not observe any rebound in prices around the city-level border after the loosening of LTV restrictions across all regions in 2016.

D Bank-Branch Profitability and Credit Supply Responses to LTV Limits

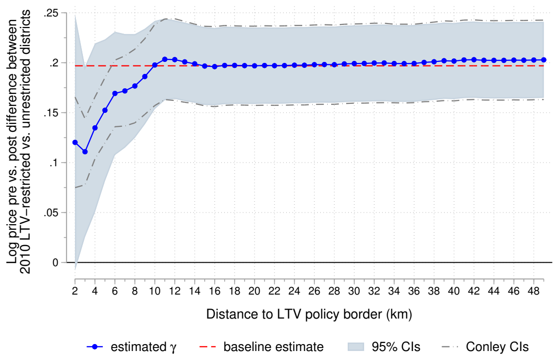
This appendix section covers how we measure profitability at the mortgage loan level using realized and excess internal rate of return (IRR) measures. We then use lenders' geographic exposure to regulated neighborhoods to tease out how the tightening of LTV limits impacts banks' profitability and how banks might alter their credit allocation decisions to mitigate losses.

D.1 Internal Rates of Return (IRR) on Floating Rate Loans

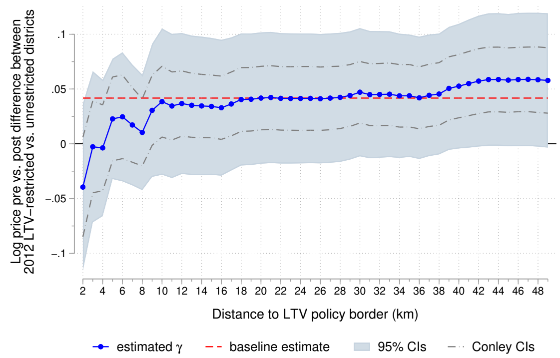
The internal rate of return is a constant discount rate which sets the present value sum of all cash flows (both inflows and outflows) associated with an investment to zero. It is widely used in finance because it not only measures how much cash relative to initial capital outlays an investment returns

FIGURE C.4. Pooled Border Diff-in-Disc Estimates Using City-Level Policy Borders

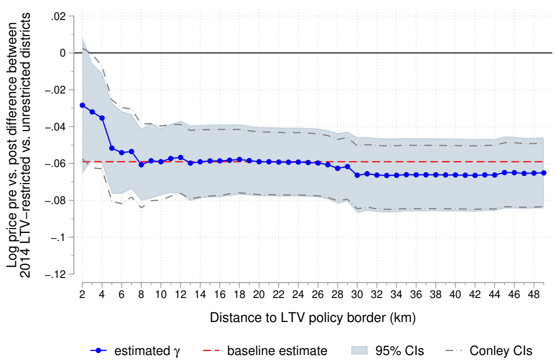
A. December 2010 Reform: LTV Limits as a Fraction of Collateral Value



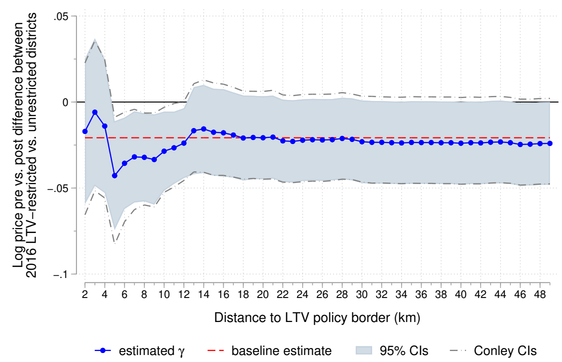
B. June 2012 Reform: High-End Properties but no Border Change (Placebo)



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. Relative to the analogous figure from our baseline analysis (Figure 5), we use city-level policy borders to account for the fact that commercial banks originating mortgages divide their operations into submarkets based on the larger city jurisdiction in which the property is located. 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 20 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.

– like an equity multiple or multiple on invested capital (MOIC) – but it also accounts for how quickly capital is returned to the investor over the life of the investment.

The IRR can be applied to the problem of a bank which originates a mortgage loan, retains that loan on their balance sheet, and then collects payments from the borrower over the term of the loan. Consider first the simple case where a bank originates a fixed rate mortgage (FRM) with amortization period of length N months and term T. For most residential mortgage loans, $N = T$, but there are some notable exceptions (e.g. Canada's mortgage market). For an FRM, we can solve for the constant monthly payment PMT – absent any refinancing, prepayment, delinquency, or default – using standard results about convergent geometric series:

$$L_0 = \sum_{s=1}^{N} \frac{PMT}{(1+i/12)^s} \implies PMT = L_0 \times \left(\frac{i/12}{1-(1/(1+i/12))^N} \right) \tag{D.1}$$

$$L_n = (1+i/12)^n \times L_0 - \sum_{s=0}^{n-1} (1+i/12)^s \times PMT$$

$$\implies L_n = (1+i/12)^n \times L_0 - \frac{PMT \cdot \Big((1+i/12)^n - 1 \Big)}{i/12} \tag{D.2}$$

where L_0 is the initial loan amount, and L_n is the account balance n months into the loan. We divide the nominal quoted rate i by 12 to obtain a monthly interest rate in contexts like the U.S. where loan contracts are not described in terms of annual interest rates.

To calculate the lender's IRR on investing in this loan over some general term $T \leq N$, we would solve for the monthly IRR following equation:[15]

$$L_0 = C_0 + \sum_{n=1}^{T} \frac{PMT}{(1+IRR)^n} + \frac{L_T}{(1+IRR)^T} \tag{D.3}$$

where PMT is given by (D.1) and C_0 are any closing costs paid by the borrower at the time of origination. Such closing costs might include any origination fees charged by the lender (e.g. appraisal fees) and points voluntarily paid by the borrower in exchange for a lower contract rate i.[16] The fees portion of C_0 generally scales with the size of the loan L_0. For a fully amortizing loan, $L_N = 0$, so if the bank holds the loan until maturity when $T = N$, the final term vanishes from the equation. The lender's IRR is increasing in the closing costs C_0 and decreasing over the term T, holding fixed all other parameters. Therefore, since $C_0 \propto L_0$, for a given house value V_0, an FRM loan with a higher loan-to-value (LTV) L_0/V_0 will yield a higher IRR for the lender.

In our setting in Taiwan and in the majority of OECD countries, adjustable-rate mortgages

[15] The annualized IRR is then $(1 + IRR)^{12}$.

[16] Although prevalent in the U.S., points are not a feature of the Taiwan mortgage market. Therefore, C_0 is only endogenous to the borrower's choice of lender, not to the borrower's choice of interest rate schedule.

(ARMs), for which the rate resets each year, are the norm. ARMs with either an initial fixed rate period or fully floating rate together account for over 99.9% of the mortgage market – both in terms of dollars or quantities of loans originated. Since the amortization and term are fixed at the time of origination, only i changes every m months after an initial period of length ℓ. i resets according to $i_{n,t} = r_{n,t} + \bar{\pi}_t$, where r is the index, generally a 1-year government bond or policy rate, and $\bar{\pi}_t$ is the margin, or constant spread over the index. Both the index and margin components of the mortgage rate have bank-specific components, but we suppress the bank subscript for ease of exposition. The margin $\bar{\pi}_t$ is fixed over the life of a loan, but can vary across loans depending on the date t when the lender originates. Margins are on average stable over time in the cross-section of bank loans.[17]

With these features in mind, the new set of equations governing the loan balance at each point in time is:

$$L_{n(\iota),t} = \underbrace{L_0 \times \prod_{s=1}^{n}(1 + i(s;t)/12)}_{\text{initial loan + interest accrued}} - \underbrace{\sum_{\iota=1}^{T/m} \sum_{s=1}^{m} (1 + i(\iota;t)/12)^s \times PMT(\iota;t)}_{\text{payments made}} \tag{D.4}$$

$$PMT(\iota;t) = L_{m\cdot\iota+1,t} \times \left(\frac{i(\iota;t)/12}{1 - (1/(1 + i(\iota;t)/12)^{N-n}} \right) \tag{D.5}$$

Equation (D.5) shows that the current monthly payment $PMT(\iota;t)$ is a function of the rate within the current reset period $\iota = \{1, 2, \ldots, T/m\}$ and the current loan balance $L_{n,t}$, which depends recursively on the history of past rates. $PMT(\iota, t)$ is constant within each reset period, and conditional on the new interest rate $i_{n,t}$, it amortizes the loan balance as of month $m \cdot \iota + 1$ over the remainder of the amortization period $N - n$. This is true regardless of the fact that $i_{n,t}$ can change at subsequent rate reset months. The realized IRR will be computed as in equation (D.3), but with the sequence of monthly payments determined by (D.4) and (D.5).

D.2 Forming Expected IRRs

Lenders offer consumers loan terms on the basis of the borrower's individual risk profile and their expectations of future market conditions. A borrower might be temporarily delinquent on payments or default on the loan. This would result in scenarios where the actual PMT collection is temporarily less than the PMT due, the borrower stops paying at some time $T < N$ and there is a charge-off rate $\xi < 1$ on the remaining balance, or some combination of the two. Lenders also form expectations

[17] In Taiwan, ARMs are typically indexed to the one-year benchmark deposit interest rate, which is set by the Central Bank. Taiwanese banks set the interest rate on an ARM loan to be a fixed margin above the one-year (or two-year for ARMs with a two-year reset period) certificate of deposit interest rate they offer to consumers.

over the path of future market index rates: $\mathbb{E}_0[r_{n,t}] = \mathbb{E}_0[i_{n,t}] - \bar{\pi}_0$.[18]

The gap between realized IRRs and risk-neutral expected IRRs gives us a sense of whether banks under-performed as a result of the policy. Focusing on this measure of underperformance, rather than simply comparing realized IRRs before vs. after the reform for regulated and unregulated loans, helps separate the mechanical negative effects of the reform on bank profitability due to the fact that IRRs are generally lower for lower-LTV loans. This is because, in practice, origination fees in C_0 are set to be proportional to the origination amount L_0.

We assume lenders form expectations of future market index rates via a 12-month lagged moving average of past rates. We then calculate the expected IRR at each point in time $t \leq T$, which we denote $IRR^{e,t}$. Analogously to (D.3), $IRR^{e,t}$ solves the equation:

$$L_0 = C_0 + \mathbb{E}_t\left[\sum_{n=1}^{t+1} \frac{PMT_n}{(1+IRR^{e,t})^n} + \frac{L_{t+1}}{(1+IRR^{e,t})^{t+1}}\right] \tag{D.6}$$

Since PMT_{t+1} is known as of time t due to the amortization schedule, the only forecasted term in (D.6) is $\mathbb{E}_t[L_{t+1}]$. A lender's expectation of the account balance on the loan in time $t+1$ is any interest that is expected to accrue on the preceding balance, which incorporates the forecasted path of future interest rates, less the expectation of total payments made by the borrower:

$$\mathbb{E}_t[L_{t+1}] = \mathbb{E}_t L_0 \prod_{s=1}^{t+1}\Big(1 + i_s/12\Big) - \mathbb{E}_t[TotalPaymentMade_{t+1}] \tag{D.7}$$

We calculate $TotalPaymentMade$ from the data via:

$$\mathbb{E}_t[TotalPaymentMade_{t+1}] = \Big(TotalPaymentMade_t + AccountsReceivable_t\Big) \cdot \mathbb{E}_t\Big(1 + i_{t+1}/12\Big) \tag{D.8}$$

where we use the observed payment made on the loan $TotalPaymentMade_t$ and any accounts receivable reported for the loan. We use the 12-month lagged moving average of average mortgage rates reported by the government to compute the expectation term in (D.8).[19]

D.3 Matched DiD Results for Return Outcomes

We use the return identities and empirical implementation of those identities described in the preceding subsection to examine the effect of the LTV reforms on realized and excess internal rates

[18] These expectations would also be a potentially important dimension of lenders' reaction to leverage regulation in mortgage markets where FRMs predominate. For FRMs, the lender bears the interest rate risk, since there is a risk that at some point in the amortization period the fixed interest rate on the loan will be lower than the rate which could be charged on a new loan contract. This pricing problem is analogous to how profit-maximizing firms set forward-looking prices when they face state or time-dependent nominal rigidities.

[19] We do not directly observe each lender's index rate. In practice, there is little deviation if we feed in a 12-month moving average which is specific to each lender's history of floating rate loans that recently experienced a rate reset.

of return on loans subject to the regulation. To briefly summarize our findings using the matched difference-in-differences strategy outlined in Section 4.1:

1. Once we residualize on property characteristics, we uncover no statistically significant effects on realized IRRs for the 2010 reform, regardless of our choice of LTV bandwidth or horizon, as shown in Panel A of Figure D.1. However, the sign of the point estimates is typically negative, consistent with the standard logic that lower LTV loans carry lower rates of return due to origination fees which are proportional to the size of the loan.

2. In Panel B of Figure D.1, we find a robust 150 to 300 basis point decline in realized IRRs for loans originated after the 2014 reform. This result is generally robust to the choice of LTV bandwidth around the 60% cutoff, although the effects are generally stronger at longer horizons. Note that due to the lower volume of loans originated around the 2014 regime and the fact that we only include newly regulated counties (given that some restrictions were already in place), for stringent bandwidths like $\pm 2\%$, we have less than 100 loans in the matched sample, which leads to large standard errors.

   In Taiwan, borrowers are not required to purchase mortgage insurance, irrespective of the initial down payment. Lenders are prohibited by law from mandating borrowers to buy mortgage insurance. There are no specific rules governing how banks insure themselves against the mortgages they own and how insurance premia are set. This means lenders fully bear the cost of the mortgage insurance and pass insurance costs through to the borrowers in the form of higher loan interest rates. This regulatory environment rationalizes the lower interest rates charged by lenders when they are forced to originate lower LTV loans during the 2014 regime.

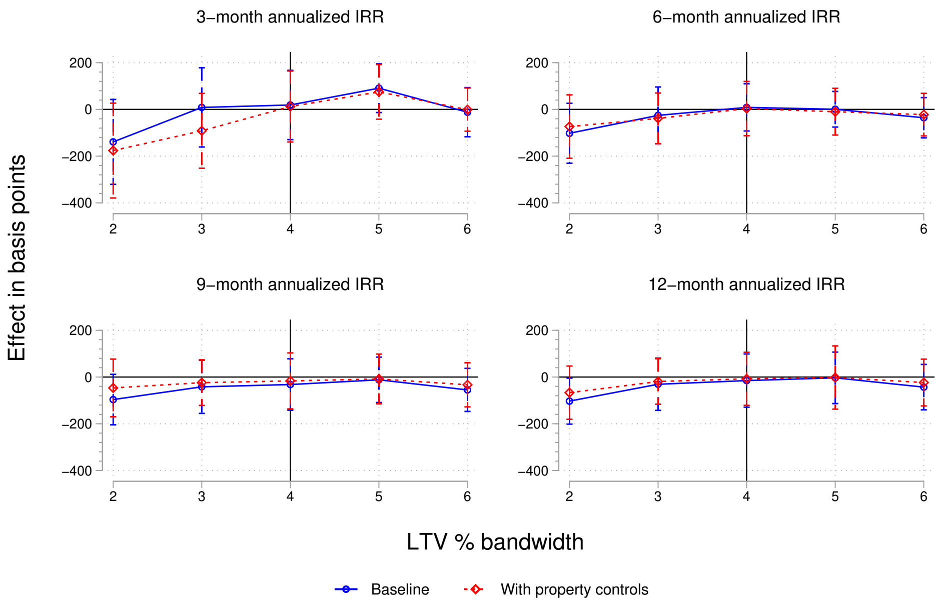
3. For the 2010 reform, we find no statistically significant effects on excess IRRs within one year since origination (Panel A, columns 5 through 8 of Table D.1). For the 2014 reform, we find large negative effects on excess IRRs within a year since origination, but these effects are more sensitive to the choice of bandwidth given the smaller sample of loans disbursed (Panel B, columns 5 through 8 of Table D.1). We define the excess IRR at a given horizon t months into the loan, $(IRR^{e,t} - IRR^{t})$, with expected $IRR^{e,t}$ defined by (D.6), and realized IRR^{t} defined by equation (D.3).

   The fact that excess IRRs fell for the 2014 reform indicates that lenders became less over-optimistic conditional on the recent history of interest rates and delinquencies. Given that delinquency rates remained stable across both regimes (see Table 5), this can be explained by the fact that under the appraisal loophole of the 2010 regime, lenders' expected returns became less anchored to realized returns, and the (partial) closing of this loophole re-anchored those expectations about the costs of insuring or securitizing the loan portfolio.

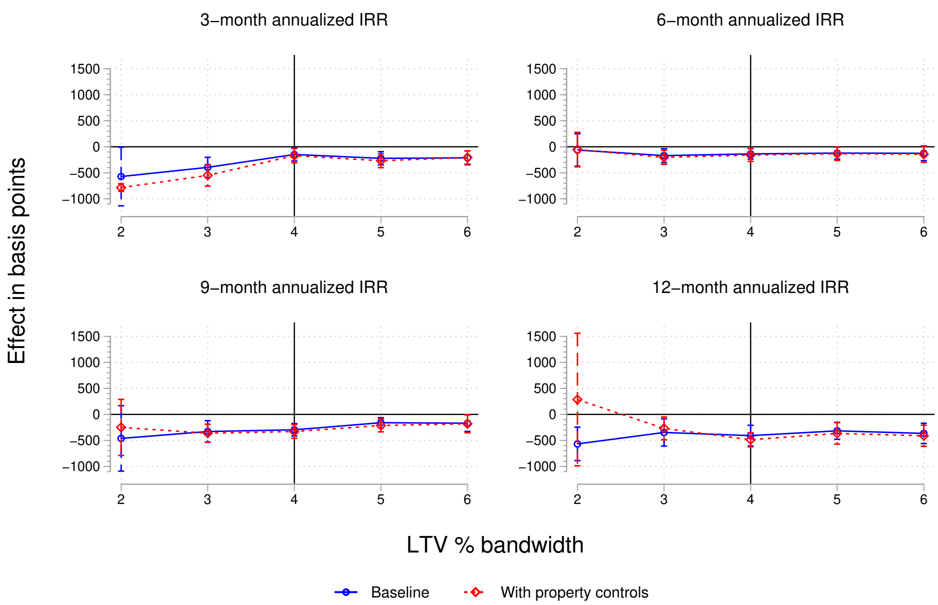
Note that the sizes for the samples used to obtain these results are smaller than for the other loan outcomes measured at origination in Table 4. This is due to the continuous cash flow data

FIGURE D.1. Matched DiD Estimates of LTV Limits on Realized IRRs by Bandwidth

A. 2010 LTV Tightening



B. 2014 LTV Tightening



Notes: Each panel in the figure shows the results from estimating average treatment effects on the treated using the matched difference-in-differences estimator in (4.1), according to our choice of LTV bandwidth $\pm x\%$ along the x-axis. The outcome in each panel is the annualized realized IRR at horizons of 3 months, 6 months, 9 months, or 12 months since origination, computed for each loan in the matched sample according to (D.3). The y-axis is scaled in terms of basis points. We perform this exercise separately for the 2010 reform (Panel A) and 2014 reform (Panel B), both of which featured tightenings of the maximum LTV on second mortgages in regulated areas. See Section 4.1 for details on implementation of the matched DiD method. Point estimates in red residualize on a vector of property characteristics. Bars indicate 95% confidence intervals obtained from clustering standard errors at the bank and distrct level. We follow the same estimation procedures as in Section 5.1.

Table D.1. Matched DiD Effects of LTV Limits on IRRs for Second Mortgage Loans

A. ATT Estimates for December 2010 LTV Tightening

	Realized IRR (IRR^{12})				Excess IRR ($IRR^{e,12} - IRR^{12}$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	−15.1	−7.5	−3.4	−2.1	54.4	48.8	51.9	60.0
	(58.1)	(57.9)	(56.2)	(69.0)	(50.8)	(53.0)	(61.8)	(65.9)
Matched variables:								
District & bank	✓	✓	✓	✓	✓	✓	✓	✓
Salary income	✓	✓	✓	✓	✓	✓	✓	✓
Age	✓	✓	✓	✓	✓	✓	✓	✓
Education	✓	✓	✓	✓	✓	✓	✓	✓
LTV bandwidth	±4%	±4%	±5%	±5%	±4%	±4%	±5%	±5%
Property controls		✓		✓		✓		✓
N	1,706	1,550	2,132	1,944	1,556	1,422	1,948	1,770

B. ATT Estimates for June 2014 LTV Tightening

	Realized IRR (IRR^{12})				Excess IRR ($IRR^{e,12} - IRR^{12}$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-408^{***}	-487^{***}	-316^{***}	-365^{***}	-262^{***}	-318^{***}	-210^{***}	-295^{***}
	(101)	(69.3)	(83.2)	(105)	(85.6)	(69.7)	(82.7)	(78.2)
Matched variables:								
District & bank	✓	✓	✓	✓	✓	✓	✓	✓
Salary income	✓	✓	✓	✓	✓	✓	✓	✓
Age	✓	✓	✓	✓	✓	✓	✓	✓
Education	✓	✓	✓	✓	✓	✓	✓	✓
LTV bandwidth	±4%	±4%	±5%	±5%	±4%	±4%	±5%	±5%
Property controls		✓		✓		✓		✓
N	172	164	180	176	162	152	172	172

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. The outcome in columns 1 through 4 of each table is the annualized IRR including cash flows 6 months after origination, calculated according to (D.3), while the outcome is the 6-month annualized excess IRR calculated by taking the difference between the lender's expected IRR in (D.6) and the realized IRR. All IRRs scaled in basis points. 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 even 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. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

required to measure the IRRs.[20] We use the 12-month IRRs as our preferred horizon in Table D.1, as monthly loan performance is most frequently updated in the credit registry on a quarterly basis and the 12-month horizon matches our moving average order used to compute the expectation term for expected IRRs in (D.8).[21]

D.4 How Do Banks Respond to the LTV Limits?

Given the preceding results in this appendix that realized and expected internal rates of return (IRR) earned by lenders fall after the imposition of LTV limits, a natural question is whether banks mitigate the drop in profitability by rationing mortgage credit to regulated areas, or steering borrowers towards loan contracts which are unregulated or carry higher IRRs. Answering this question addresses the larger question of how do spatially targeted LTV limits work to lower house prices – is it purely through demand, or is there a supply effect?

In the credit rationing scenario, banks reject more loans from borrowers buying a second home where the collateral is located in one of the treated districts. Such a reduction in credit supply across the distribution of home values would result in a shift inward in the credit supply curve, putting upward pressure on interest rates. Lenders can steer on the extensive margin by increasing rejection rates in regulated districts while lowering their screening standards in unregulated districts. This would give rise to spillovers in credit allocation across the LTV policy border and possibly fuel the demand spillovers we observe close to the border (at distances ≤ 4 km) in Section 5.2. However, borrowers for investment properties are relatively creditworthy and are thus unlikely to be turned away by this behavior.

Lenders can also steer borrowers within regulated areas through loan pricing, offering low mortgage rates on loans above the 60% LTV cap relative to what they would have offered in the absence of the policy. However, we find negative pass-through of LTV limits to interest rates on loans below the 60% threshold from our matched difference-in-differences analysis in Section 5.1.1. Within-district steering on the loan pricing margin, if it exists, attenuates the observed negative interest rate pass-through. Hence, if rationing occurs, then it must be that the demand effect of tightening LTV limits dominates for us to observe interest rate declines in directly regulated districts. We ultimately uncover little evidence of credit rationing, a fact we incorporate into our place-based welfare decomposition in Appendix E.2 by modeling the credit supply curve as static in response to the LTV cap.

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[20] We lack data to implement the matched DiD design for IRRs at longer horizons since loans drop out of the sample as $t \to \infty$. For some bandwidths we can follow loans originated around the 2010 LTV regime up to 48 months out and find that returns after 4 years fall by 150-200 basis points on an annualized basis.

[21] As shown for realized IRRs in Figure D.1, the estimated effects are almost identical for the 6-month vs. 12-month horizon, but the sample size is halved for the latter. For excess IRRs there sample size weakly increases with horizon, but is similar to the sample size for realized IRRs at 12 months. This is partly because some loans get refinanced or closed at the 12-month mark.

A common problem in the banking literature is that lending is an equilibrium object, and we do not observe the universe of loan applications and for that reason cannot examine rejection probabilities to directly measure lenders' credit rationing responses. Amiti & Weinstein (2018) propose a decomposition of the relative contributions of supply and demand to loan originations in the corporate lending context. This decomposition relies on identification of bank and borrower fixed effects. In mortgage markets, and especially in our setting in which we are focused on relatively short time windows around a particular reform, there are very few repeat borrowers with mortgages spanning multiple banks or branches, and such borrowers form a very selected sample.

Therefore, to tease out credit rationing responses we use measures of how *ex ante* geographically exposed lenders' loan portfolios are to the LTV regulations imposed on certain neighborhoods. The idea behind our strategy is that banks are differentially exposed to each LTV tightening based on the extent to which they specialize in mortgage lending to not-yet regulated segments of the market. The identifying assumption is that lending outcomes for branches and banks with a recently stronger history of mortgage lending to investors in regulated areas would have evolved similarly if there were no change to maximum LTV rules. Our research design is conceptually similar to the one adopted by Gilje, Loutskina, & Strahan (2016), who show that banks expand their mortgage lending at unexposed branches when they experience exogenous liquidity inflows (e.g. through shale discoveries) into areas where they have branches.

We define LTV regulation "exposure" as the dollar share of loans each branch j originates in treated areas within a year prior to each reform:

$$Exposure_j = \frac{\sum_{i=1}^{N_j} \left(Loan\_amt_{i,j} \times Treated_{i \in d} \right)}{\sum_{i=1}^{N_j} Loan\_amt_{i,j}} \tag{D.9}$$

where $Treated_{i \in d}$ indicates that loan i originated by branch j is located in a district d which will eventually be subject to the LTV cap according to the policy border drawn in 2010 or in 2014. $Loan\_amt_{i,j}$ is the dollar value of the mortgage amount at origination. We sum over all loans N_j originated by branch j in the year prior to the reform.

We can further decompose *Exposure* into separate measures using only unregulated mortgages on first properties or only the regulated mortgages on non-primary properties to compute the numerator. Similarly, we can compute $Exposure_b$ at the parent bank b level to show banks' top-down compliance with the policy. This measure of exposure is potentially non-binary even for mono-branch lenders, since branches can originate mortgages on properties outside the administrative area in which they reside; there are no restrictions on inter-jurisdictional banking.

Throughout this appendix, we restrict to a balanced panel of branches to isolate lenders' strategic decisions in responding to the LTV caps imposed on their neighborhood from any churn in the loan portfolio due to branch entry and exit. Our results are qualitatively similar if we compute *Exposure* over alternative time horizons, such as 6 months prior to each reform, or using loans originated in a

12-month window lagged relative to the reform date to allow for anticipation effects.[22] We prefer to use a full 12-month window prior to the reform to account for seasonality in mortgage demand. In our main set of results, we find no evidence of anticipatory effects of each LTV limit announcement on house prices.

Our $Exposure$ measure also picks up the fact that collateral values for loans originated before the LTV tightening may fall due to changes in broader housing market demand, as documented in our main set of results in Section 5.1.2. Favara & Giannetti (2017) show that mortgage lenders internalize the negative local price externalities of calling in bad loans and foreclosing.[23] Hence, lenders more exposed to LTV limits on specific neighborhoods may have additional incentives to steer, since their profitability falls not only due to the fall in credit demand (i.e. lower IRRs on new loans) but also from declines in the recovery value of existing collateral assets.

Figure D.2 separately plots the distribution of branch-level $Exposure_j$ prior to the 2010 and 2014 reforms for regulated investor mortgages vs. unregulated mortgages on primary residences. For the treated investor mortgages, the share of unexposed branches (i.e. those with $Exposure_j = 0$) rises by 6 p.p. before the 2010 regime vs. on the eve of the 2014 regime. Meanwhile, there is a small 1 p.p. decline in the fraction of unexposed branches in terms of origination of the primary mortgages. There is otherwise little change in the distribution of exposure outside of the endpoints of $Exposure_j = 0$ or $Exposure_j = 1$. Hence, some branches which previously originated all their investor mortgage loans in treated districts stop originating them altogether after the initial wave of LTV caps imposed in 2010.

We examine shifts between the regulated and unregulated segments of the mortgage market at the branch level using the following triple differences specification:

$$\begin{aligned} L_{j,b,d,t} = \gamma_1 \cdot Post_t \times Treated_{j \in d} + \gamma_2 \cdot Post_t \times Exposure_j \\ + \gamma_3 \cdot Post_t \times Treated_{j \in d} \times Exposure_j + \eta_j + \theta_{d,t} + \varepsilon_{j,b,d,t} \end{aligned} \tag{D.10}$$

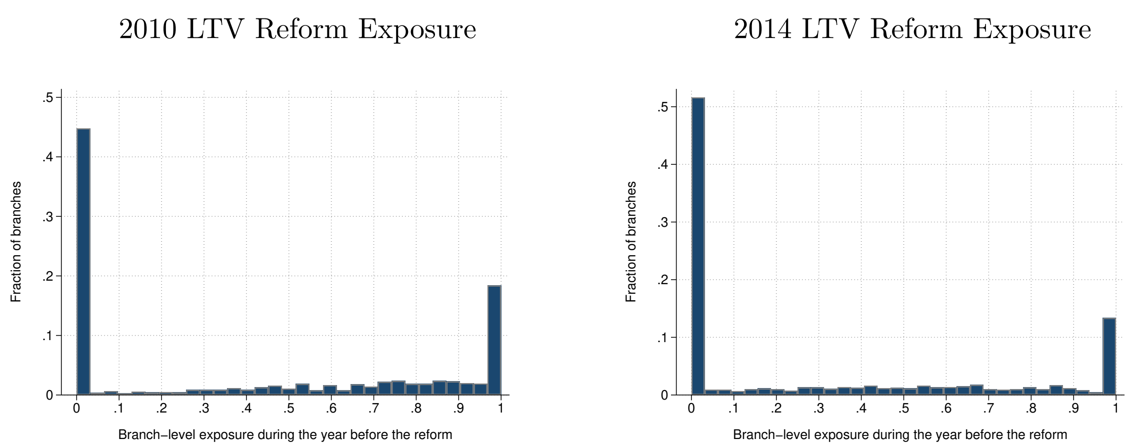
where the outcome L refers to log loan dollars originated or the log number of loans, either for mortgages on investment properties or for primary residences. The log specification removes the extensive margin response noted in Figure D.2. The dummy $Post_t$ refers to the post-LTV reform period, and the $Treated_{j \in d}$ dummy indicates whether branch j is located in a regulated district d. We include district $\times$ month-year fixed effects $\theta_{d,t}$ to soak up any district-specific time trends in lending. Within the set of branches in treated districts, the triple differences coefficient γ_3 captures the additional effect on lending outcomes of having a branch originate 100% of mortgages to investors vs. one which issues no loans subject to the LTV cap. Estimates of (D.10) in Table

[22] We cannot tabulate the total dollar value of all loans outstanding broken down by bank branch and district, since our loan-level data cover originations, and our sample only includes loans originated after June 2009.

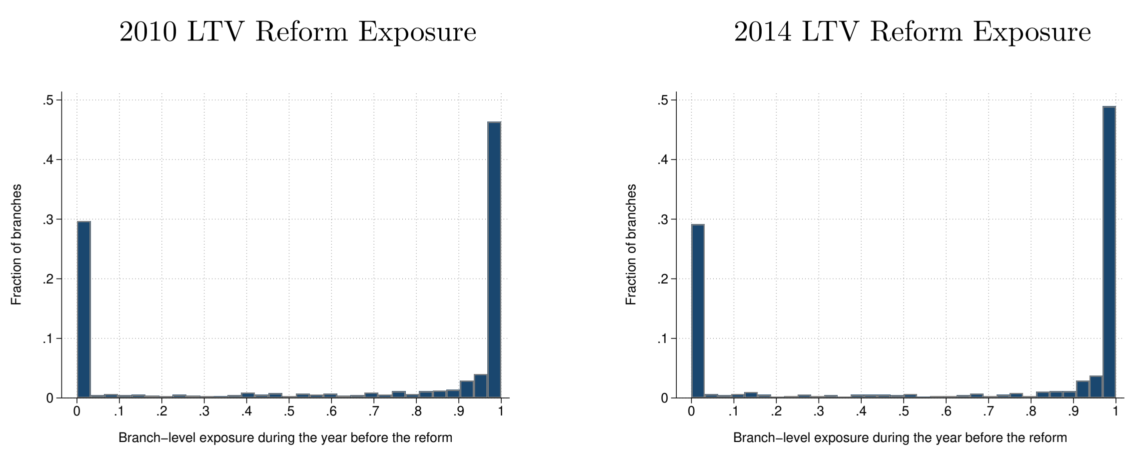
[23] A measure of LTV regulation exposure based directly on collateral values would require us to do book-to-market conversions for each asset. The loan-level data are not well-suited to applying a book-to-market conversion via hedonics given the limited amount of information about the home's physical characteristics reported on the loan origination forms (see Section 3 for details on the data).

FIGURE D.2. Distribution of Branch-Level Loan Exposure to LTV Regulation

A. Exposure Calculated using Mortgages for Investors Only



B. Exposure Calculated using Mortgages for Primary Residences Only



Notes: The plots display the distribution of branch-level exposure, $Exposure_j$, computed according to formula (D.9). In computing this measure, we sum over loans originated in treated areas within a year prior to each reform in 2010 (left panels) and 2014 (right panels). The top two plots (Panel A) show the distribution of $Exposure_j$ based on the soon-to-be treated investor mortgage loans, whereas the bottom two panels (Panel B) calculate exposure using mortgages used to purchase primary residences only. For each reform, we define exposure based on the set of districts treated under that reform's policy boundary, as pictured in Figure 4.

Table D.3. Branch-Level Credit Supply Responses to 2014 LTV Tightening

	1st mortgages		2nd+ mortgages	
Outcome:	log(loan \$)	log(# of loans)	log(loan \$)	log(# of loans)
$Post_t \times Treated_{j \in d}$	-0.02	-0.02	0.28^{**}	0.05
	(0.22)	(0.34)	(2.08)	(0.61)
	[0.16]	[0.24]	[1.80]	[0.42]
$Post_t \times Exposure_j$	-0.20^{***}	-0.13^{**}	-0.28^{***}	-0.06
	(2.79)	(2.54)	(2.31)	(1.01)
	[2.23]	[1.86]	[1.99]	[0.85]
$Post_t \times Treated_{j \in d} \times Exposure_j$	0.22^{**}	0.13^{**}	-0.38^{**}	-0.17^{**}
	(2.37)	(2.12)	(2.35)	(1.98)
	[2.01]	[1.53]	[1.90]	[1.39]
Branch FEs	✓	✓	✓	✓
District × month-year FEs	✓	✓	✓	✓
N	28,280	28,280	10,013	10,013
Adj. R^2	0.52	0.61	0.41	0.49

Notes: The table reports the estimated $\widehat{\gamma}_1$, $\widehat{\gamma}_2$, and $\widehat{\gamma}_3$ from the triple differences equation (D.10) using the directly regulated subset of loans (i.e. mortgages to investors) to construct the branch-level exposure measure in (D.9). We measure $Exposure_j$ using mortgages to investors originated on properties located in soon-to-be regulated areas but in the year prior to the 2014 reform. The first two columns use lending for purchases of primary residences ("1st mortgages") as the outcome, while the last two columns use lending for purchases of non-primary residences ("2nd+ mortgages") as the outcome. Each regression includes branch and district × month-year fixed effects. t-stats from standard errors clustered by bank-time in parentheses. t-stats from standard errors clustered by branch in brackets. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

D.3 show that banks clearly comply with the 2014 LTV cap. More exposed branches within the same district are 13.9% (13 log points) more likely to originate mortgages for primary residences and 15.6% (17 log points) less likely to originate mortgages for investment properties (or, 24.6% and 31.6% in dollar terms, respectively).

These results from examining individual branches still conflate supply and demand. It is not clear whether some branches strategically exit regulated segments of the mortgage market or if demand dries up at branches which previously catered to investors due to the increased downpayment requirement for purchases of non-primary residences. We examine whether banks strategically originate fewer or more loans after the introduction of LTV limits in neighborhoods where they operate by aggregating equation (D.9) across branches to define a network exposure measure:

$$\sum_{k \neq j}^{N_b} Exposure_{k,t-1} = \sum_{k \neq j}^{N_b} \left(\frac{\sum_{i=1}^{N_k} \left(Loan\_amt_{i,k} \times Treated_{i \in d} \right)}{\sum_{i=1}^{N(b)} Loan\_amt_{i,b}} \right) \tag{D.11}$$

Equation (D.11) quantifies how each parent bank b faces exposure through its constituent branches k. For each branch j within the parent bank, we add up loan exposure across all other branches $k \neq j$ for the N_k loans in each branch k, relative to all mortgage loans $N(b)$ originated across all branches. As in (D.9), we define branch network exposure in (D.11) using loans originated in the year preceding the 2014 reform and denote this via the $t-1$ subscript.

We use our network exposure to decompose how much of branch-level mortgage lending growth in response to the 2014 LTV limit cap is due to a branch's direct exposure, as defined by (D.9), versus the exposure of its peer branches. This leads us to the following specification:

$$\begin{aligned}\Delta L_{j,b,d,t,t+1} &= \alpha + \gamma_1 \cdot Exposure_{j,t-1} + \underbrace{\gamma_2 \cdot Exposure_{j,t-1} \times Treated_{j \in d}}_{\text{direct exposure}} \\ &+ \gamma_3 \cdot \textstyle\sum_{k \neq j}^{N_b} Exposure_{k,t-1} + \underbrace{\gamma_4 \cdot \sum_{k \neq j}^{N_b} Exposure_{k,t-1} \times Treated_{j \in d}}_{\text{indirect exposure through network}} + \xi_d + \varepsilon_{j,b,d,t,t+1}\end{aligned} \tag{D.12}$$

where the outcome is YOY growth in lending (in dollar or number of loan terms), computed by tabulating loans originated in the 12 months following the reform relative to those originated in the 12 months prior to the reform. All other variables in the regression are defined as before. We include district fixed effects to compare two branches in the same district but with different degrees of regulatory exposure through its parent bank's network.[24]

One hypothesis is that banks more exposed to regulatory risk through LTV limits might smooth out the shock across branches in their network. This is true when $\gamma_4 > 0$, since banks increase lending in directly exposed branches when their network of other branches is also more exposed given *ex ante* mortgage lending patterns to investors. In this case, banks effectively export reductions in credit mandated by spatially targeted LTV limits to unregulated areas. Conversely, if $\gamma_4 < 0$, then more indirect exposure through the branch network leads to lower lending growth, on average, at a directly exposed branch, suggesting there are strategic complementarities in lending decisions across branches.

Presenting the results from estimating equation (D.12) in Table D.4, we find that the coefficient on γ_4 is not statistically significant when we use growth in non-primary mortgage lending (2nd+ mortgages) as the outcome variable. This is true regardless of whether we measure lending growth in terms of dollars originated or the number of loans. The sign on the coefficient is positive ($\widehat{\gamma}_4 > 0$), pointing to the smoothing hypothesis, but we cannot reject the null of no branch network effects. In sum, we find no evidence of banks responding to the LTV limits by credit rationing in the regulated segments of the mortgage market.

[24]The parent bank fixed effects are collinear with the district fixed effects for most branches in (D.12), so we exclude them in Table D.4. Even if we exclude district fixed effects, we still fail to reject the null that the network interaction effect γ_4 is equal to zero.

Table D.4. Branch-Level Credit Supply Response through Parent Bank Network Exposure

	1st mortgages		2nd+ mortgages	
Outcome:	Δ log(loan \$)	Δ log(# of loans)	Δ log(loan \$)	Δ log(# of loans)
$Exposure_{j,t-1}$	0.043	0.017	−0.052	0.050
	(1.04)	(0.70)	(0.47)	(0.10)
$Exposure_{j,t-1} \times Treated_{j\in d}$	−0.041	−0.017	−0.018	−0.006
	(0.93)	(0.65)	(0.15)	(0.12)
$\sum_{k\neq j}^{N_b} Exposure_{k,t-1}$	0.062	0.074	−0.252	−0.068
	(0.22)	(0.37)	(0.35)	(0.20)
$\sum_{k\neq j}^{N_b} Exposure_{k,t-1} \times Treated_{j\in d}$	−0.015	−0.019	1.000	0.269
	(0.05)	(0.09)	(1.12)	(0.61)
District FEs	✓	✓	✓	✓
N	20,815	20.815	4,272	4,272
Adj. R^2	0.003	0.002	0.015	0.014

Notes: The table reports coefficients on own-branch and network exposure to the 2014 LTV tightening according to specification (D.12), with ΔL computed comparing loans in a one-year symmetric window on either side of the reform. The first two columns use lending for purchases of primary residences ("1st mortgages") as the outcome, while the last two columns use lending for purchases of non-primary residences ("2nd+ mortgages") as the outcome. We measure both own-branch $Exposure_{j,t-1}$ and network exposure $Exposure_{k,t-1}$ using 2nd+ mortgages originated on properties located in regulated areas but in the year prior to the 2014 reform. Own-branch exposure is defined by (D.9), and network exposure by (D.11). We rescale each term in the regression such that the coefficients in the table represent the effect of a 10 p.p. increase in $Exposure$. Each regression includes district fixed effects. t-stats from standard errors clustered at the branch level in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

E Welfare Decomposition of Spatial LTV Limits

There is an equity-efficiency tradeoff inherent in imposing LTV limits, as doing so crowds out profitable housing transactions that otherwise would occur with more lax downpayment requirements. But at the same time, by restricting mortgage access to housing investors, renters on the margin of homeownership and incumbent homeowners looking to move to previously expensive neighborhoods gain from reductions in local house prices. In this appendix, we show that losses in terms of forgone deed tax revenues and reductions in the value of housing trading are small relative to other nationwide policy alternatives such as housing transfer taxes.

E.1 Impacts of LTV Limits on Tax Revenues

So far we have not discussed the revenue implications of restricting access to household leverage. There are several real estate tax bases in Taiwan, including taxes paid by current homeowners, buyers, and sellers.[25] We focus on the largest of these tax bases, the deed tax, which is a 6% levy on the property's appraised value A^*_{i,d,t^*}, and is paid by buyers at the time of sale. We describe how the government computes A^*_{i,d,t^*} for each property in Appendix B.[26] Because A^* is only re-evaluated once every three years, changes in deed tax proceeds therefore reflect changes in quantities rather than changes in market prices, allowing us to isolate how rationing household credit reduces tax revenues independent of spillover effects to the valuation of surrounding properties.

We use confidential tax returns accessed through the Financial Information Agency of the Ministry of Finance. Our data consist of the universe of deed tax and personal income tax return filings spanning 2006 to 2015, which covers the main LTV reforms we analyze in the main text. We propose the following difference-in-differences specifications, the first of which we estimate at the individual property transaction level; we estimate the second equation by collapsing revenues to the district level.

$$DeedTax_{i\in d,my} = \alpha + \beta \cdot LTVCap_{i\in d} \times Post_{my} + \eta_i + \gamma_{my} + \epsilon_{i,my} \tag{E.1}$$

$$\sum_{i\in(d,my)} DeedTax_{i\in(d,my)} = \alpha + \beta \cdot LTVCap_d \times Post_{my} + \xi_d + \gamma_{my} + \epsilon_{d,my} \tag{E.2}$$

where in the property-level regression (E.1), $DeedTax_{i\in d,my}$ is deed tax revenues (thousands of NTD), $LTVCap_{i\in d}$ is a dummy equal to unity if the property is located in a district subject to LTV restrictions, and $Post_{my}$ is a dummy indicating the transaction occurs after the policy implementation. We define the variables analogously in the district-level equation (E.2). We estimate (E.1) as a repeat sales regression by including the property fixed effect η_i to capture all time-invariant quality dimensions of the house which might be capitalized into the tax bill. We include only non-border, unregulated districts in the control group to mitigate spatial contamination effects in our estimates of $\widehat{\beta}$.

Because we cannot attach a second mortgage flag to each transaction i, the estimates of the direct property-level effect on tax revenues in (E.1) are intent-to-treat. We cannot accurately identify a second mortgage flag in the tax return data for two reasons. The main reason is that our ability to identify leverage transactions relies on taxpayers claiming a mortgage interest payment deduction on their annual personal income tax return. A taxpayer would only elect to claim this deduction when tax savings under itemized deductions exceed those under the standard deduction, just like in the U.S. Second, taxpayers can choose between the two deduction methods every year, and so we

[25] See Appendix B of Chi, LaPoint, & Lin (2023) for a more in-depth description of the property tax and real estate transfer tax bases in Taiwan. Our confidential tax return data contain individual records for the deed tax (paid by buyers), building tax (paid by owners), and land value increment tax (paid by sellers). The deed tax is the equivalent of the Stamp Duty Tax that exists in the U.K. and in many British Commonwealth countries.

[26] Note that while we imputed one of the hedonic factors in equation (B.1) underlying A^* to estimate appraisal gaps, in the confidential tax return data we observe the actual A^* for each transaction. To preserve the privacy of the individuals involved in these transactions, we cannot merge the A^* in the tax return data to the loan-level data.

Table E.1. Difference-in-Differences Estimates of LTV Limit Effects on Deed Tax Revenues

Policy date:	December 2010		June 2014	
Obs. unit:	Property	District	Property	District
α	27.90***	3291.10***	32.01***	3013.90***
	(54.57)	(46.58)	(106.81)	(79.74)
$LTVCap_{i\in d} \times Post_t$	1.32	$-3,597.70$***	-3.18	-2524.50***
	(0.94)	(-3.64)	(-1.60)	(-2.73)
Unit FEs	✓	✓	✓	✓
Month-year FEs	✓	✓	✓	✓
N	455,968	4,058	1,519,885	16,241
Adj. R^2	0.008	0.749	0.007	0.586

Notes: The table presents coefficients obtained from estimating difference-in-differences equation (E.1) at the property transaction level and (E.2) at the district-year level. In each regression, the outcome variable is deed tax revenues in thousands of NTD. We include a full set of time and unit fixed effects in each regression, such that for the property-level regressions we include month-year dummies and property fixed effects (repeat sales). We include only transactions with a mortgage in the property-level regressions to capture the direct impact on revenues raised from each transaction where the property buyer claims a mortgage interest deduction, whereas the district-level regressions include both leveraged and non-leveraged home purchases. As in the rest of the paper, we select the longest symmetric time window around each reform that avoids any other LTV interventions. t-statistics in parentheses obtained by clustering standard errors by district-date. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

cannot match mortgages to the associated housing collateral using the timing of the home purchase and the occurrence of mortgage interest payment deduction.

The estimation results in Table E.1 indicate that each LTV policy is revenue-neutral at the individual transaction level, but reduces deed tax revenues at the district level. The decrease in district-level revenues is due to a combination of two effects: (i) a direct negative effect on the value of the tax base due to the decline in transaction prices, as evidenced from our border diff-in-disc results in Section 5.1.2; and (ii) the fact that there are fewer transactions per treated district which are subject to the tax, as some people are discouraged from purchasing homes altogether, or those who would have purchased a second home using higher-LTV loans in regulated areas migrate to the unregulated side of the border, as we show in Section 5.2. The latter effect on revenues due to a decline in transaction volumes is more prominent given that the deed tax is based on a lagged appraised value A^*_{i,d,t^*}.

After the initial set of targeted LTV tightenings in December 2010, monthly district-level deed tax returns fell by 3.6 million NTD, or by 43.2 million NTD ($\approx$ 1.3 million USD) annually. Annual deed tax revenues declined by 30 million NTD ($\approx$ 0.9 million USD) for the second reform enacted in June 2014.[27] When summing up the districts' annual losses across the three cities regulated by the LTV limits (Taipei, New Taipei, and Taoyuan), losses are equivalent to 1% of total deed tax revenues collected in the tax year prior to the 2014 LTV reform. We use this statistic to decompose overall welfare losses associated with the LTV limits in the next subsection.

E.2 Reduced Form Welfare Analysis

We combine all our reduced form results on the effects of the 2014 LTV limit to estimate the percent welfare loss in housing consumption terms and compare this to similar estimates for housing transfer taxes, which are the most prominent alternative instrument used by policymakers to curb house price growth. Our approach to computing the welfare loss is similar in spirit to methods adopted in the place-based policy literature, including Busso, Gregory, & Kline (2013), Lu, Wang, & Zhu (2019), and LaPoint & Sakabe (2024). In those studies, the authors apply reduced form estimates from cross-sectional difference-in-differences (DiD) designs to form counterfactuals of market size in the absence of a reform. We take a hybrid approach which combines such place-based decompositions with standard public finance-style computations of Harberger triangles (e.g. Hines 1999). We use our average treatment on the treated (ATT) estimates to isolate welfare costs on the LTV-capped neighborhoods in our setting.

To guide our analysis, we start with a graphical representation in Figure E.1 of how the spatially targeted LTV limits shifted equilibria in the mortgage credit market (Panel A) and the broader housing market (Panel B), in light of our findings. We depict the 60% LTV limit as a quota in the mortgage credit market for total loan dollars originated towards purchases of investment properties. This imposes a cap (red vertical line) on the total amount of possible lending L^{**}, which is less than the previous equilibrium level of lending L^{*}. Credit demand L^D shifts inward due to the increase in the required downpayment (dp) to $dp' = 40\%$ from $dp = 20\%$. The credit supply curve L^S does not shift, given our lack of evidence in Appendix D.4 of banks rationing credit. The interest rate on regulated loans falls, as we find in our matched DiD results from Table 4 and Appendix D.3.

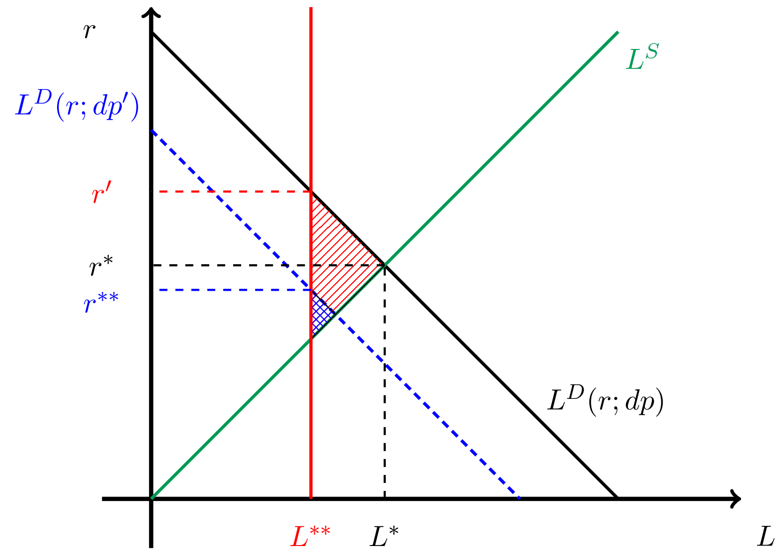
In the housing market (Panel B), the LTV limit results in a shift inward in the housing demand curve Q^D. Home buyers pay the *ad valorem* deed tax τ (described in Appendix E.1) which results in a deadweight loss at the new equilibrium quantity of transactions Q^{**}. Deed tax revenues collected from the new equilibrium are given by the gray shaded rectangle. Deed tax revenues, the buyer's surplus, and seller's surplus all shrink, resulting in a welfare loss in housing consumption terms given by the difference in the areas of the two trapezoids demarcated by the bases at Q^{**} and Q^{*}, less any change in the size of the deadweight loss from the deed tax.

In drawing the graph this way, we assume the housing inventory curve Q^S does not shift inward,

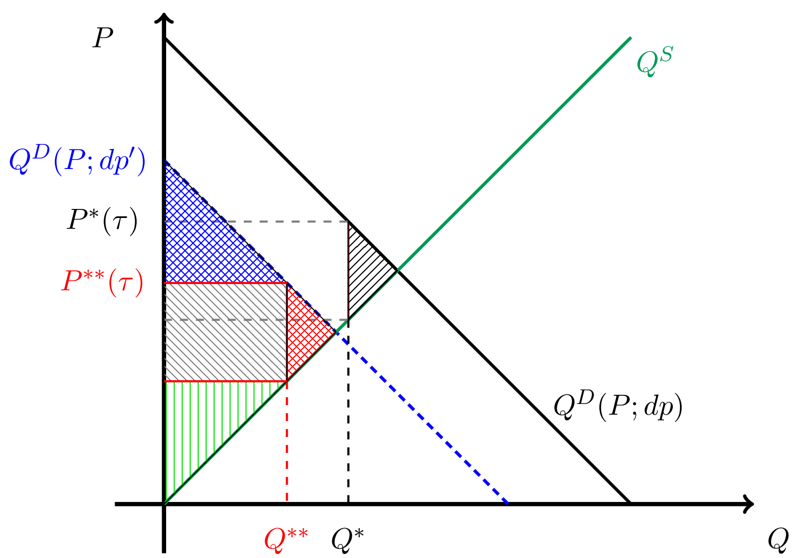
[27] If we instead use log revenues as the outcome variable, we find that district-level revenues fall by 1-2%, on average, after each LTV tightening relative to revenues collected during the preceding LTV regime. We do not use log revenues as our baseline specification because some districts report deed tax revenues in a lumpy fashion, meaning that there can be zero revenues reported in a district-month even if there are taxable transactions in that month.

FIGURE E.1. Graphical Depiction of Short-Run Welfare Losses from LTV Limit

A. Mortgage Credit Market (For Investment Purchases)



B. Home Purchase Market



Notes: The figure illustrates the impact on the local mortgage credit market (Panel A) and the local housing market (Panel B) of a spatially targeted LTV limit like the one enacted in Taiwan in June 2014. We depict the 60% LTV limit as a quota in the mortgage credit market for loans towards purchases of investment properties. This imposes a cap (red vertical line) on the total amount of possible lending L^{**}, which is less than the previous equilibrium level of lending L^*. Credit demand L^D shifts inward due to the increase in the required downpayment (dp) to $dp' > dp$. The credit supply curve L^S does not shift, given our lack of evidence in Appendix D.4 in favor of rationing. The LTV limit results in a shift inward in the housing demand curve Q^D. Home buyers pay the *ad valorem* deed tax τ (described in Appendix E.1) which results in a deadweight loss at the new equilibrium quantity of transactions Q^{**}. Deed tax revenues collected from the new equilibrium are given by the gray shaded rectangle. Deed tax revenues, buyer's surplus, and seller's surplus all shrink, resulting in a welfare loss in housing consumption terms given by the difference in the areas of the two trapezoids demarcated by the bases at Q^{**} and Q^*, less any change in the size of the deadweight loss (DWL). The sign on changes in prices and quantities in each market is consistent with our main empirical results in Section 5.

meaning we assume there is no lock-in effect resulting from the higher downpayment requirement. Unless investors believe that the LTV limits are temporary and there is strong market segmentation between transactions for investor buyer-seller pairs and other transactions, there is no clear reason why incumbent homeowners would be less likely to list their home for sale after the LTV reform. Incumbent investors in second homes still have a primary residence, and so unlike traditional mortgage lock-in effects resulting from a lack of mortgage portability (Fonseca & Liu 2023), if they decide to sell their second home they do not need to re-enter the mortgage market.

Based on Figure E.1, we can decompose the change in total surplus in the housing market into the change in the home buyer's surplus (ΔHB), the home seller's surplus (ΔHS), and the decline in deed tax revenues given the drop in transaction volume equal to $Q^* - Q^{**}$.

$$\Delta S = \Delta HB + \Delta HS + \tau \cdot \Delta(P \times Q) \tag{E.3}$$

where the deed tax rate $\tau = 6\%$. In Appendix E.1, we estimate that the 2014 LTV limit resulted in losses of 1% of total deed tax revenues in the regulated neighborhoods relative to revenues collected in the tax year prior to the reform. To compute losses due to the crowdout of housing transactions we need to make further assumptions. The textbook way of computing the buyer and seller surpluses would be to write down the formulas in terms of housing demand and supply elasticities and compute the areas of the triangles in Panel B of Figure E.1. To pin down the elasticities we would need to have access to differential shocks to demand and housing inventory. Otherwise we would need to examine proceeds from sale or capital gains income from housing transactions to separately estimate the effects on buyers vs. sellers.

We can instead compute the overall loss in terms of housing consumption by reformulating the decomposition in the following way:

$$\Delta C = \Delta(P \times Q) - \Delta DWL \tag{E.4}$$

In words, to compute the total housing consumption loss ΔC in regulated areas, we compute changes in the total amount of trade moving from equilibrium (Q^*, P^*) to (Q^{**}, P^{**}), but net out gains from the reduction in the deadweight loss (DWL) arising from the decline in housing transaction volume. Implicit in (E.4) is the idea that all deed tax revenues are rebated back to local homeowners, which is a reasonable assumption given that the deed tax is collected by local governments. A well-known result is that, up to a first-order approximation, the marginal DWL with respect to a sales tax is elasticity of equilibrium trade volume times the new equilibrium quantity, or:

$$\Delta DWL = \eta_Q \times Q^{**} = (dQ/d\tau) \cdot (P/Q) \times Q^{**} \tag{E.5}$$

Equation (E.5) tells us that the marginal DWL from a tax is the difference between the "mechanical" revenue loss if there were no change in prices used to compute the tax base, and the actual revenue

Table E.2. Welfare Losses Imposed by 2014 Spatial LTV Limit

	$\Delta \log P$	$\Delta \log Q$	$\%\Delta C$	$\%\Delta C \cdot \omega_{2009}$	$\%\Delta C \cdot \omega_{2013}$
Estimate Set #1:	-0.050^{***}	-0.258^{***}	26.5%	11.0%	11.4%
(Table 6-4; Table C.3-3)	(0.010)	(0.023)			
Estimate Set #2:	-0.056^{***}	-0.258^{***}	26.9%	11.1%	11.8%
(Table C.4-4; Table C.3-3)	(0.008)	(0.023)			
Estimate Set #3:	-0.066^{***}	-0.262^{***}	28.0%	11.6%	12.3%
(Table 6-6; Table C.3-6)	(0.014)	(0.029)			

Notes: The first two columns summarizes our average treatment effect on the treated (ATT) estimates presented elsewhere in the paper for the effects of the 2014 LTV limit on individual house prices and district-level transaction volume. Robust standard errors clustered by district in parentheses, with $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. The first set of estimates uses our preferred specifications, with references to the column of each table in each row. For instance, (Table 6-4; Table C.3-3) refers to column 4 of Table 6 and column 3 of Table C.3, respectively. The second row of estimates instead uses a difference-in-differences specification where we only include properties in never-treated districts faraway from the policy border in the control group to estimate $\Delta \log P$. The third row uses estimates including transactions involving only land parcels. In the fourth column, we combine our average treatment effect on the treated estimates in the first two columns via $\exp(\Delta \log P + \Delta \log Q) - 1$. The final two columns of the table scale down the measured decline in housing consumption $\%\Delta C$ by the fraction ω of total transaction value attributed to the 2014 set of regulated districts as of either 2009 or 2013.

loss. For most taxes this would be a difficult counterfactual to compute, but for the deed tax it is straightforward. As discussed in Appendix E.1, the fact that the deed tax is based on a lagged appraised value A^*_{i,d,t^*} means that the tax DWL is constant after a lowering of the LTV cap, so $\Delta DWL = 0$. Panel B of Figure E.1 is drawn to show this case. Therefore, all we need to compute the loss in housing consumption is to find $\Delta(P \times Q)$, for which we have detailed causal evidence.

Table E.2 offers a breakdown of our estimates of ΔC using estimates of the effects of the 2014 LTV reform on house prices and transaction volume for different definitions of treatment and control groups. In particular, we compute $\Delta \log C = \Delta \log P + \Delta \log Q$, and feed in our border diff-in-disc or DiD estimates using prices or quantities as the outcome variable. For our most conservative set of specifications (Estimate Set #1 in the table) the loss in housing consumption value is 26.5%. Using never-treated units as the control group (Estimate Set #2) or including land parcel sales (Estimate Set #3) leads to only slight increases in $\%\Delta C$.[28]

How do our welfare loss estimates compare to those imposed by other housing market restrictions? Koetter, Marek, & Mavropoulos (2021) illustrate that transfer taxes on housing sales are theoretically isomorphic to downpayment constraints, since sellers can pass through the tax burden to buyers by charging higher prices, which requires buyers to front more cash for the downpayment. Governments in 21 of the top 25 metros by investable residential real estate stock impose housing

[28] If we net out our upper bound estimate from Section 5.3 of a 2 p.p. pricing spillover effect at close distances ($\leq$ 4 km from the border), we obtain $\%\Delta C = -25.0\%$.

transfer taxes (Chi, LaPoint, & Lin 2023). Many governments in East Asia impose high transfer tax rates and two tax schedules whereby tax surcharges are greater for investment properties. Chi, LaPoint, & Lin (2023) structurally estimate a heterogeneous housing investor model using a nationwide transfer tax hike in the same context as ours and find the average welfare loss across the entire housing market is equivalent to 56% of housing consumption.

Since our estimates yield the loss in housing consumption within regulated districts, we scale down our estimates in proportion to the *ex ante* fraction of transaction volume attributed to regulated districts to render the welfare losses comparable to housing policies which apply nationwide. Computing total transaction values in 2009 (one year before the 2010 LTV policy), the fraction of trade across all eventually regulated districts is $\omega_{2009} = 41.4\%$ of the total; this same fraction computed using transaction values in 2013 (one year before the 2014 LTV policy) is $\omega_{2013} = 43.9\%$. This implies that the welfare loss in housing consumption terms when measured in proportion to transaction volume across all of Taiwan ranges from 11% to 12%. Hence, in the absence of any negative welfare consequences for unregulated neighborhoods, spatially targeted LTV limits induce welfare losses one-fifth as large as a comparable transfer tax targeting all districts.

Our calculations ignore any welfare losses incurred in the mortgage market, but we argue such losses are likely to be small. Panel A of Figure E.1 depicts a small DWL from the loan cap (the shaded blue triangle), but this represents one case that rationalizes our estimated causal effects of the LTV limit presented in Section 5. It could be that the resulting reduction in demand due to the higher downpayment requirement is enough to completely offset the distortionary effects of the quota on the mortgage market, eliminating the area given by the blue-shaded triangle. Another reason why there is a reduction in demand in the credit market is that the LTV policy regime generated negative beliefs about the future desirability of housing as an investment good in regulated areas. In Section 5.1.2 we find asymmetric effects of the complete relaxation of LTV limits across regulated districts after 2016. This is consistent with successively stricter leverage restrictions negatively altering investors' expectations about the path of future house prices, as has been documented in field experimental settings (Fuster & Zafar 2016; 2021). Ultimately, to answer this question and estimate the DWL in the credit market, we would need to estimate mortgage credit demand and supply elasticities, which is beyond the scope of this paper.

Finally, our estimates are partial equilibrium in the sense that we do not capture possible spillover effects to the rental market. Because lock-in effects from the LTV limits are likely to be limited, first-time home buyers, or current renters considering buying a house gain from lower house prices. Our analysis in Appendix C.4 shows that such gains in affordability in the ownership market are concentrated in neighborhoods with relatively high-income residents. If this substitution effect is strong enough, then we would expect rents in regulated districts to decline as residents substitute away from tenancy towards ownership. Unfortunately, we lack unit-level data on rents and can only observe total portfolio-level rental income reported on landlords' personal income tax returns, so we cannot directly test this hypothesis. We thus leave quantifying the strength of the redistributive effects of LTV policies on renters to future work.

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