Spatially Targeted LTV Policies and Collateral Values

Chun-Che Chi [†] Cameron LaPoint [‡] Ming-Jen Lin [§]

[†]Academia Sinica [‡]Yale SOM [§]National Taiwan University

Bank of England

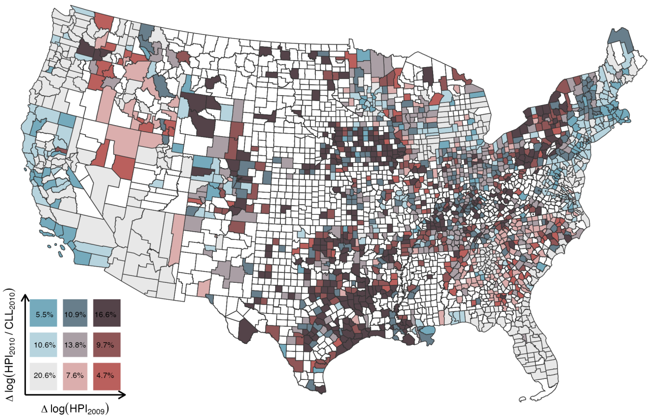
October 30th, 2024

Motivation: regulating leverage to cool housing demand

- Housing increasingly unaffordable in major cities around the world
  - Concentrated in supply-constrained superstar cities with high amenity value
- Many types of policy experiments conducted to bring prices down
  - Taxes: transaction, capital gains, vacancy, foreign homebuyer surcharges, etc.
  - Most recent studies show transfer taxes distortionary and $P \uparrow$ through lock-in effects
  - **Mortgage regulation:** downpayment requirements, insurance, bank quotas/risk weights
- U.S. has **conforming loan limits (CLL)** which positively co-move with house prices
  - Cutoffs for whether Fannie/Freddie can buy mortgages on secondary market
  - For jumbo loans above cutoff higher required income and credit score $\implies$ rationing
  - Exceptions for certain high cost areas set by 2008 law (e.g. D.C., NYC)

U.S. CLL formula imperfectly targets based on local ΔP

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



- Movements in leverage limits anchored to *national* housing cycle
- In practice, two ways to explicitly link to local prices:
  1. Bank origination costs tied to typical home value in an area $\longrightarrow$ "soft" limit, like CLL
  2. Or, set required downpayment percentage by locality $\longrightarrow$ **"strict" limit**
- What would happen if we replaced with $\%\Delta HPI_{i,[t-1,t]}$?

What we do

Research question

Are **spatially targeted** leverage limits preferred from an *efficiency* perspective as a way to "cool" housing markets? And how to quantify the **general equilibrium** costs?

- Study this question using a novel series of changes to strict LTV limits in Taiwan
  - Leverage policy part of Central Bank regulatory mandate (common outside U.S.)
  - **Select specific districts** to impose credit limits based on *ex ante* ΔP
- Loan-level data tracking origination and performance of **all** mortgages
  - Merge with info from administrative tax returns, public database of geocoded home sales, and bank balance sheets
  - Larger set of outcomes than other macropru studies $\rightarrow$ financial (mispricing) + real costs

Results: in what sense are these policies "successful"?

- Focus on 2014 reform: LTV drops from standard 80% to 60% for investment properties
  - Headline result: **house prices decline by 5-6%** in policy areas relative to nearby unregulated neighborhoods $\implies$ price-leverage ratio elasticity between 0.75 and 1
  - Affordability gains accrue to first-time homebuyers (FTHBs)
  - No average effect on loan delinquency outcomes or borrower creditworthiness
- **$\downarrow$ in origination amounts, sale prices, quoted rates for loans in treated areas**
  - Driven by demand, not supply: no evidence of banks rationing credit
- **But also $\downarrow$ in sales volume across price distribution relative to untreated areas**
  - Spatial pricing spillovers limited to 4 km distance to policy border $\longrightarrow$ real distortions
- **Evidence of avoidance through collateral misreporting**
  - Gap between bank and govt. appraisal widens by 15% $\implies$ observed LTV $<$ true LTV

Related work on housing market regulation

- **Macroprudential regulation of housing**
  - LTV limits: Igan & Kang (2011); Campbell, Ramadorai, Ranish (2015); Chen et al. (2016); Armstrong, Skilling, Yao (2019); Aastveit et al. (2020); de Araujo et al. (2020); Han et al. (2021); Acharya et al. (2022); Van Bekkum et al. (2022); Bolliger et al. (2022); Eerola et al. (2022); Tzur-Ilan (2023), Higgins (2024) and many more...
  - Other constraints [D(P)TI, quotas, risk weights, taxes, etc.]: Kuttner & Shim (2016); Cerutti, Claessens, Laeven (2017); DeFusco & Paciorek (2017); DeFusco, Johnson, Mondragon (2020); Benetton (2021); Deng et al. (2021); Hu (2022); Chi, LaPoint, Lin (2023)
- **Evidence of relationship between credit supply and house price growth**
  - Mian & Sufi (2011,22); Favara & Imbs (2015); Loutskina & Strahan (2015); Cerutti, Dagher, Dell'Ariccia (2017); Fuster & Zafar (2021); Greenwald & Guren (2021); Blickle (2022)
- **Adverse outcomes of lender-borrower collusion**
  - Collateral misreporting: Ben-David (2011); Agarwal, Ben-David, Yao (2015); Garmaise (2015); Piskorski, Seru, Witkin (2015); Griffin (2021); Kruger & Maturana (2021)
  - Credit screening standards: Keys et al. (2010); Purnanandam (2011); Ambrose, Conklin, Yoshida (2016); Griffin & Maturana (2016a,b); Mian & Sufi (2017)

Related work on housing market regulation

- **Macroprudential regulation of housing**
- **Evidence of relationship between credit supply and house price growth**
- **Adverse outcomes of lender-borrower collusion**

What's new here...

1. **Spatially targeted nature of a national leverage policy $\longrightarrow$ spillovers**
2. Measure collateral revaluation by comparing loans to *administrative* appraisals
3. Setting features tightening and loosening of LTV limits, multiple years without successive reforms $\longrightarrow$ remove seasonality + GE effects
4. Ability to identify banks $\longrightarrow$ trace out what happens to profits + portfolio shifts

Background & Data

Mortgage market in Taiwan: a quick primer

- **Private bank lending market is ≈ 100% floating rate mortgage contracts**
  - Standard contract type resets rate each year ("tracker mortgage")
  - Small # of adjustable rate mortgages (ARMs) w/initial period where rate fixed
  - Indexed to bank-specific 1 or 2-year CD rate = weighted avg. of Treasury rates
- Fixed rate mortgages (FRMs) only offered on special govt. loans issued by public banks
- **Like U.S., standard pre-reform LTV is 80%, for similar reasons**
  - Banks explicitly set a maximum LTV they are willing to originate (private insurance)
  - Prepayment penalties, and no points rolled into closing costs
- LTV policies we study apply uniformly to traditional banks (90%) and shadow banking sector (10%) $\implies$ no avoidance through shopping across lender types

History of targeted LTV restrictions

Recent

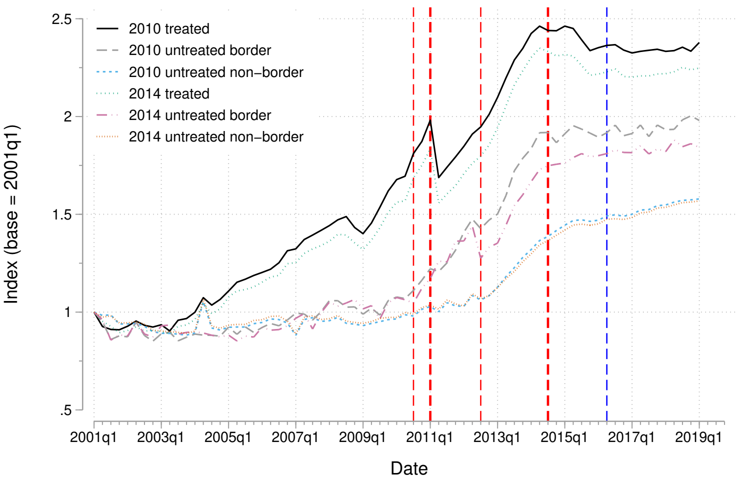
Effective date	Type	Property target	Region	Buyers	Maximum LTV
March 1, 1989	T	Land, residential and non-residential properties	All regions	Individuals and institutions	140% of the current appraisal value
June 25, 2010	T	Second (mortgaged) homes	Taipei and New Taipei (22 districts)	Individuals	70% of 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

- We focus on the June 2014 reform which tied LTV limits to **market prices**
- Use 2012 reform as a placebo since it only applied to very expensive homes

Spatial targeting clearly based on *ex ante* ΔP

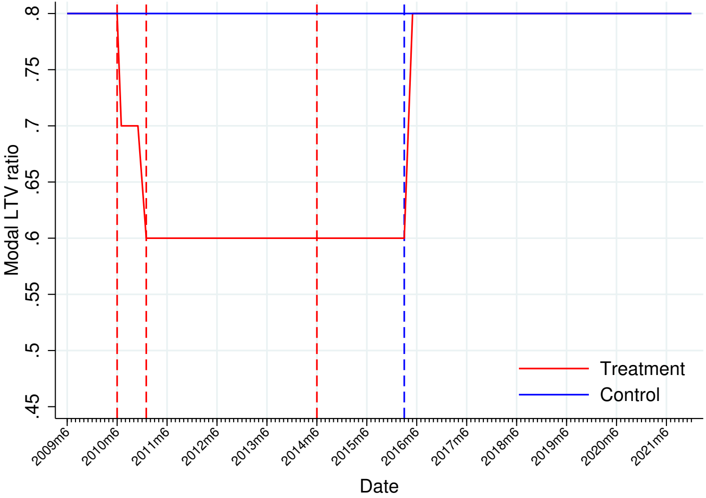
Full table Map

$$\log p_{i\in g,q} = \delta_q^g + \gamma_b^g + \beta^{g\prime} \cdot \mathbf{X_{i\in g,t}} + \varepsilon_{i\in g,q} \longrightarrow \widetilde{P}_q^g = \exp(\widehat{\delta}_q^g)$$



- Govt. concerned about $\Delta P >> 0$ in Taipei area
- Setting features regimes lasting multiple years
  - Uncommon in literature
  - Govts. frequently tweak leverage limits post-GFC
  - Symmetric windows around reform remove seasonality in RE sales
- Test for asymmetry: several tightening events followed by loosening in 2016

Clear first stage: LTV ratios bunch around limits



- Treatment group varies over time according to:
  - First vs. **second** mortgages
  - **Neighborhoods** selected by Central Bank
  - Very high-end properties (> 1.3 mil. USD)
- All restrictions lifted in March 2016 except for high-end homes ⟶ test for symmetry
- We will show that due to changes in LTV limit formula, 2014 had biggest effects

2010 Q | 2014 Q | Treated LTV | Untreated LTV

Data infrastructure

- **Complete set of mortgage originations from ROC Central Bank (2009-21)**
  - Contract characteristics at origination + track loan performance over time
  - Info on borrowers: occupation, income, age, permanent address
- **Universe of personal tax records from Ministry of Finance (2006-16)**
  - Already linked with wealth estimates, property and deed tax assessments
  - Cannot merge directly with loan registry, but can merge with public property records
- **Merge with database of market prices and rents from public records**
  - Compiled + geocoded in our other paper on transfer taxes (Chi, LaPoint, Lin 2023)
- **Bank balance sheet data from TEJ+**
  - Link branch information to scraped addresses $\longrightarrow$ track (re)allocation of loans within bank

Empirical research designs to identify policy effects

1. **Matched DiD** (à la Abadie & Imbens 2011): standard approach in the literature
   - Match on observables to identify second mortgagors which would have asked for higher LTV but could not due to policy limits $\longrightarrow$ localized treatment on treated effect
2. **Border discontinuity designs** (Dell & Olken 2020; Méndez & Van Patten 2022)
   - Examine how outcomes vary around borders formed by spatial LTV policy
   - One of few applications of border diff-in-disc design in financial intermediation literature
3. **Border pair DiD** (Dube, Lester, Reich 2010; Hagedorn et al. 2016)
   - Tease out differences in confidential data between spatial and non-spatial LTV policies
4. **Branch-level DiD using exposure based on loan portfolio, bank exposure** Jump
   - Shift-share: parent banks and their branches face differential exposure to reform depending on where their loan originations were historically concentrated

Matched Diff-in-Diff Approach

Matched diff-in-diff implementation

- Fill in "missing" homebuyers who would have taken out (second) mortgage w/LTV above the limit according to following steps...
  1. Exclude individuals with an LTV ratio before policy that is far from cap
  2. Match borrower who chose loan slightly below the cap post-reform to nearest pre-reform borrower in same district according to $\mathbf{X_{i,t}}$ (age, income, educ.)
  3. **Control group** chooses same LTV ratio before and after policy, but slightly below cap
  4. **Treatment group** chooses to be above the LTV cutoff before the policy

$$ATT = \left(\overline{After} - \overline{Before}\right)_{treated} - \left(\overline{After} - \overline{Before}\right)_{control} \quad (1)$$

  5. Run regression on matched sample to account for other sources of observed heterogeneity

- Caveat: we have to match on location, which limits # of potential borrower matches, so some differences in income remain between treatment/control group Balance
  - Complement with border pair DiD designs which move away from statutory LTV cutoff

2014 MATCHED DIFF-IN-DIFF $\Longrightarrow$ SMALLER LOANS, $P\downarrow$, $r\downarrow$

	log(loan \$)		log(psm)		interest rate (%)	
ATT	-0.110^{**}	-0.096^{*}	-0.230^{***}	-0.195^{**}	-0.148^{***}	-0.190^{***}
	(0.049)	(0.058)	(0.087)	(0.089)	(0.050)	(0.057)
Matched variables:						
District & bank	✓	✓	✓	✓	✓	✓
Salary income	✓	✓	✓	✓	✓	✓
Age	✓	✓	✓	✓	✓	✓
Education	✓	✓	✓	✓	✓	✓
LTV bandwidth	$\pm4\%$	$\pm4\%$	$\pm4\%$	$\pm4\%$	$\pm4\%$	$\pm4\%$
Property controls		✓		✓		✓
N	966	920	952	906	966	920

- Compare 61-65% LTV to 55-59% LTV in symmetric window around each reform
- Lenders pass through decreased cost of insuring the loan to consumers IRRs 2010 reform
- *Ex ante* riskiness of loans changed due to **closing of misreporting loophole**

No effect on loan delinquency outcomes in either reform

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

- Estimate regression on matched sample with matched borrower pair FEs $\psi_{(i,j)}$
- Take matched sample of loans around each reform and track performance over the full time sample ($\approx$ 5 years on average), **controlling for maturity**
- For both reforms we find...
  - **No evidence of change in delinquency** (30-day, 30-60 day, 90+ day) or frequency of lenders writing off the loan (charge-offs)
  - No heterogeneity in delinquency within an income bin or by mortgage DTI

No avg. effect on loan delinquency (2014 reform)

2010 reform

	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	$\pm 4\%$	$\pm 4\%$	$\pm 4\%$	$\pm 4\%$	$\pm 4\%$	$\pm 4\%$
Property controls		✓	✓		✓	✓
N	960	922	922	960	922	922

Robustness checks for matched DiD strategy

1. **Alternative LTV bandwidths:** results robust for main outcomes, although standard errors blow up for $\leq \pm 3\%$ windows around 60% LTV Jump

2. **Similar results for prices and loan quantity within a standard loan maturity** Jump
   - 51.9% of mortgages have 20-year amortization period, and 34.5% are 30-year loans
   - Due to power issues check with more expansive bandwidth $> \pm 4\%$

3. **Delinquency results at different horizons and by ex ante bank risk** Jump
   - No average effect on loan delinquency or charge-off rates
   - Use parent bank ROE as a proxy for ex ante riskiness measure (Meiselman, Nagel, Purnanandam 2023)
   - Banks with high *ex ante* ROE have higher systematic tail risk exposure during the crisis
   - Post-reform reduction in ever-delinquent probability if higher bank ROE Jump
   - $\Longrightarrow$ may be some gains on the systemic risk side to spatial targeting

Summary of matched DiD analysis

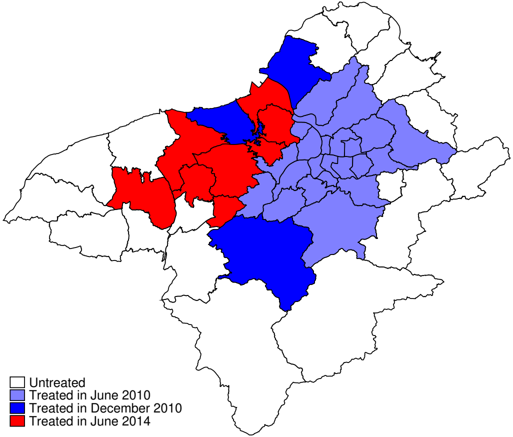
Both policies reduced leverage and house prices for mortgages on investment homes. Loans became *ex ante* less risky ($r \downarrow$), but no change in realized risks to lenders.

- Matched DiD results show us what happened to directly regualted loan contracts around the LTV cutoff
- What about the rest of the market? $\longrightarrow$ **elasticity** between prices and credit provision
- To address this question we use a **border difference-in-discontinuity** to study effects on the overall housing market + quantify spatial spillovers
  - Identification: no other factors influencing house prices changing discontinuously around policy border <u>at the time of reform</u>

Border Diff-in-Disc Approach

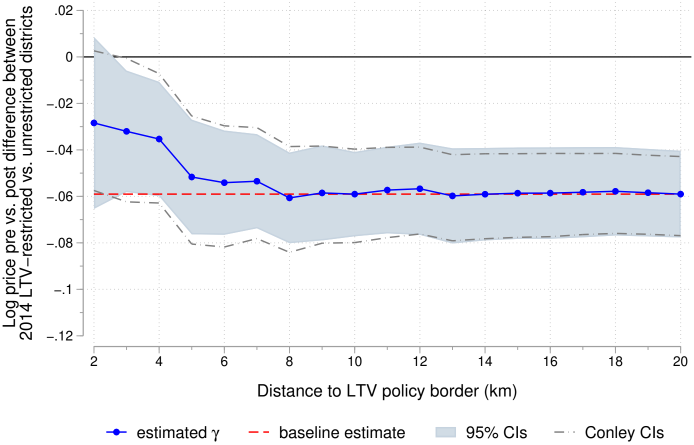
Border "diff-in-disc" design to capture market effects

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



- Compare two properties with same distance + characteristics and compare outcomes pre vs. post LTV tightening
- Extend to border "diff-in-diff-in-disc" comparing the newly treated region to the previously treated one (blue) $\longrightarrow Post_{d,t}$
- Reduced demand from investors for houses in treated regions $\implies \gamma < 0$ on prices
  - Banks might respond by offering more attractive loan terms
  - No evidence of supply-side response from bank-level DiDs

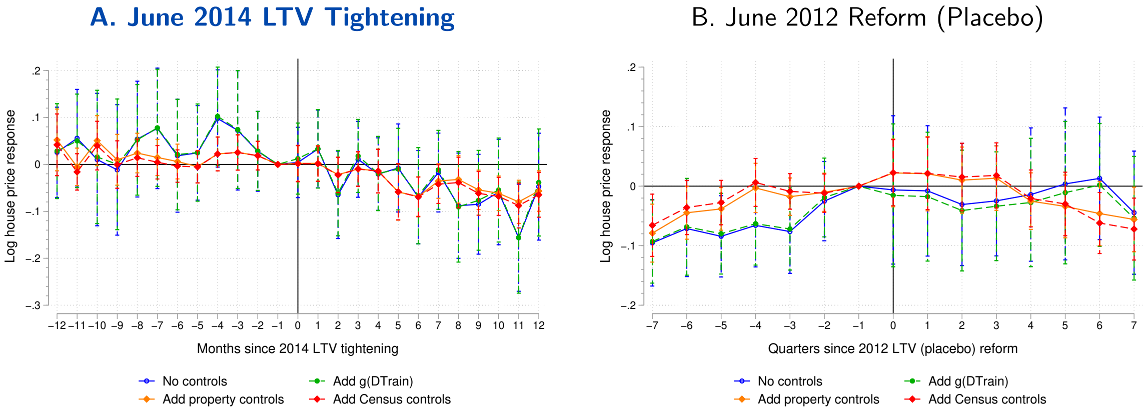
Prices ↓ for properties in leverage-restricted areas



- **5% drop in house prices** as cross into LTV regulation area
- Complements matched DiD results by controlling for prop. + neighborhood characteristics
  - Age, size, building material, commuting costs, high-rise
  - Census demographics + slope/elevation/temperature
- Robust to choice of bandwidth (x-axis) and conservative standard error bands (Conley)

Details 2012 placebo Table

No pre-trends on dynamic diff-in-disc effects



- Select symmetric windows around reform to avoid policy overlap + seasonality effects
- Robust to quadratic lat/lon function 2010 reform 2016 repeal

Separating cross-border spillover effects

- In our baseline specification $\widehat{\gamma}$ captures the sum of two effects:
  1. Direct effect on treated properties in districts subject to LTV policy
  2. Spillover effect due to increased demand for properties in neighboring untreated areas

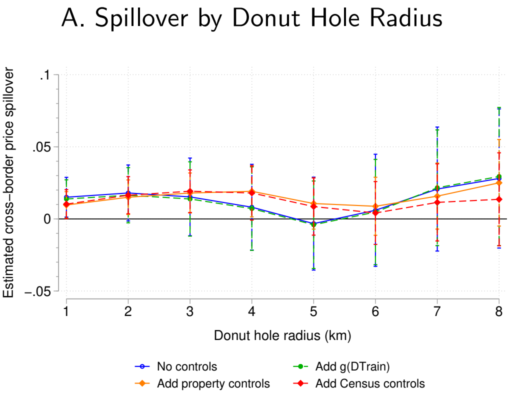
- One idea: augment baseline specification to isolate semi-circle $\mathcal{H}$ of length r

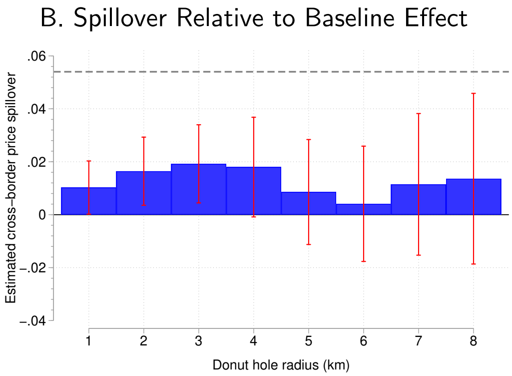
$$Y_{i,d,t} = \gamma \cdot \Big(LTVCap_{i,d} \times Post_{d,t}\Big) + \eta \cdot \Big(\mathbb{1}\{i \in \mathcal{H}(r)\} \times Post_{d,t}\Big) + f(lat_i, lon_i) + \beta' \cdot \mathbf{X}_{i,d,t} + \xi_d + \delta_t + \varepsilon_{i,d,t} \quad (2)$$

- η (or $\gamma - \eta$) difficult to interpret due to GE effects on either side of border
  - Ex: people move to unregulated areas but others move to regulated areas once prices fall

- **Solution:** exclude i in the "donut hole" $\mathcal{C}(r) := \{i| - r \leq x(i) \leq r\}$ and compare $\widehat{\gamma}$

Small cross-border spillover effects of LTV tightening



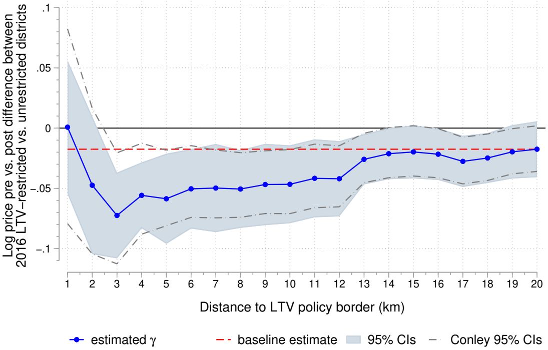


- Spillover effects limited to within $\leq$ 4 km of policy border $\longrightarrow$ commuting costs
- Reject null that entire DiD pricing effect in treated areas is due to spillovers

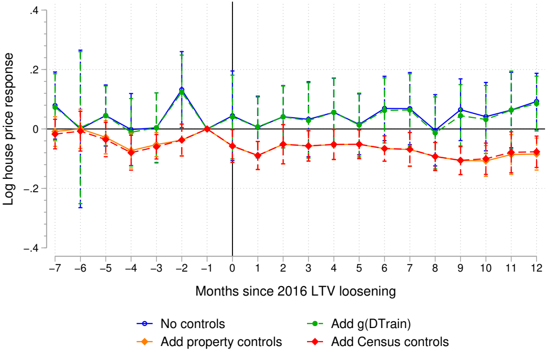
Prices do not recover in regulated areas after repeal

A. Robustness to bandwidth

Go back

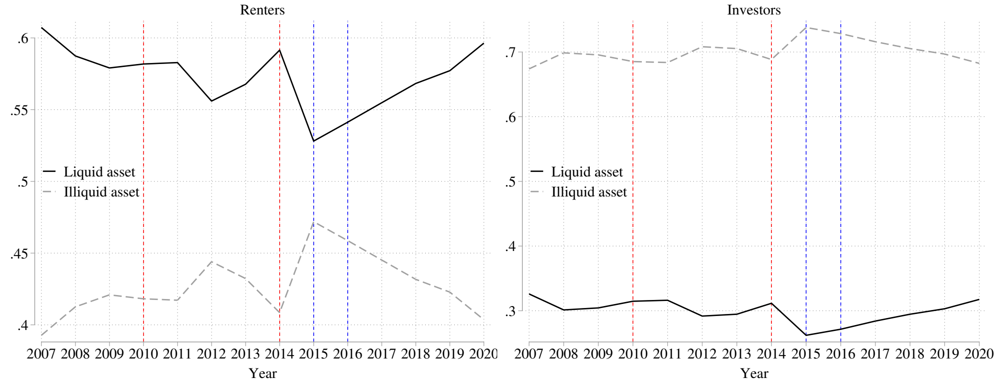


B. Dynamic border diff-in-disc effects



- Prolonged period of leverage constraints neg. altered investors' $\mathbb{E}[\Delta P]$
- Consistent with experimental evidence (Fuster & Zafar 2016, 2021)

No price recovery due to portfolio reallocation



- All homebuyers persistently reallocate money away from real estate
- Consistent with idea that successive LTV policies negatively alter beliefs about future house price growth $\longrightarrow$ Central Bank commitment problem

Additional results for border analysis

1. Results hold for each reform if use faraway never-treated districts as control group Jump

2. Heterogeneous effects on prices by *ex ante* neighborhood average income Jump
   - **Price declines concentrated in higher-income areas subject to LTV limits**
   - Properties in 10% more affluent districts experience 1% greater decline in P

3. Similar effects for district-level sales volume Jump Land vs. building
   - Overall home sales volume declines by 26% in 2014 regulated districts relative to faraway unregulated districts

4. Sharper discontinuity at the border if use city-level boundaries Jump
   - $\implies$ spatial targeting might be improved by defining boundaries according to banks' mortgage markets

Heterogeneous responses of homebuyers and investors

District border pair analysis

- Confidential mortgage data identify the mortgage number (1st/2nd/3rd+), but cannot be geocoded to obtain distances to policy border
- For each policy, pick pairs of districts just next to policy borders
- For first, second, and third mortgages, separately run

$$Y_{i,d(p),t} = \gamma \cdot \left(Treat_{i,d} \times Post_{d,t} \right) + \beta' \cdot \mathbf{X}_{i,d,t} + \xi_d + \delta_t + \varepsilon_{i,d,t} \qquad \text{[full sample]}$$

$$Y_{i,d(p),t} = \gamma \cdot \left(Treat_{i,d} \times Post_{d,t} \right) + \beta' \cdot \mathbf{X}_{i,d,t} + \xi_{p,t} + \varepsilon_{i,d,t} \qquad \text{[border sample]}$$

- Following Dube et al. (2010), Model 1 uses all districts, while Model 2 uses only border districts with pair-time FEs
- $\mathbf{X}_{i,d,t}$ include borrower and property characteristics
- Also consider triple-diff to compare outcome of given mortgage type to others

2014 policy: FTHB affordability improvement

2010 2016

	log(loan \$)				log(price)			
Mortgage type	All	First	Second	Third	All	First	Second	Third
$TreatPost_{i,d,t}$	−0.036*	−0.030	−0.131*	0.185	−0.031*	−0.037**	0.002	−0.062
	(-1.92)	(-1.46)	(-1.70)	(0.94)	(-1.74)	(-2.06)	(-4.33)	(-0.30)
$Treat_{i,d}$	0.297***	0.285***	0.282*	0.312	0.300***	0.289***	0.303***	0.869***
	(6.19)	(5.81)	(3.04)	(1.44)	(6.35)	(6.13)	(3.20)	(3.38)
N	28,738	25,265	2,252	181	28,738	25,265	2,252	181
Adj. R^2	0.41	0.41	0.45	0.64	0.41	0.45	0.45	0.60
$TreatPost_{i,d,t} \times Type_i$		0.042	−0.052	0.030		−0.043*	0.037	0.065
		(1.38)	(-1.63)	(0.41)		(-1.80)	(1.51)	(0.92)
N		28,738	28,738	28,738		28,738	28,738	28,738
Adj. R^2		0.41	0.41	0.41		0.45	0.45	0.45

- Demand for 2nd homes further declined; excess supply made 1st homes affordable
- Uniform policy on 3rd home did not generate spatial differences and price reductions

Spatially-targeted policy vs. uniform policy

- Comparing the two types of policies requires buyers to be similar to avoid selection bias

	Applicant characteristics			Property type		
	Income	Age	Education	Apartment	Single family	W/parking space
2nd homebuyers	879.2	43.5	15.0	0.810	0.040	0.411
3rd homebuyers	1100.2	46	15.1	0.815	0.035	0.381
t-stat	-3.79	-6.90	-1.05	0.32	0.62	1.53

- While 3rd homebuyers subject to the global policy are older and richer, they have similar education and preferences to 2nd homebuyers subject to the spatially targeted policy

Collateral Misreporting Channel

Isolating collateral misreporting

- **Appraisal gap** = (log) difference between bank's appraised collateral value and most recent local property tax appraisal value for house i

$$Gap_{i,b,d,t} = \log(A_{i,b,d,t} - A^*_{i,d,t^*}) \quad (3)$$

  - For land transactions, A^* publicly observable
  - For buildings, compute A^* based on AVM (hedonic) fitted value Details
  - Houses appraised every 3 years for building property tax

- Include appraisal drift function $\mathcal{D}(t, t^*)$: bank may simply move their collateral appraisal in lockstep with reval announced by tax authority

- Using change in loan-to-price (LTP) ratios would overestimate amount of misreporting
  - Since $\Delta P < 0$ due to regulation, then even if no misreporting $\Delta LTP > 0$

Triple diff shows large inc. in collateral misreporting

$$Gap_{i,b,d,t} = \alpha + \gamma_1 \cdot Post_t + \gamma_2 \cdot LTV\_District_{i,d} + \gamma_3 \cdot \left(Post_t \times LTV\_District_{i,d}\right)$$
$$+\gamma_4 \cdot 2nd\_Mrtg_i + \gamma_5 \cdot \left(Post_t \times 2nd\_Mrtg_i\right) + \gamma_6 \cdot \left(LTV\_District_{i,d} \times 2nd\_Mrtg_i\right) \quad (4)$$
$$+\gamma_7 \cdot \left(Post_t \times LTV\_District_{i,d} \times 2nd\_Mrtg_i\right) + \mathcal{D}(t,t^*) + \theta' \cdot \mathbf{X}_{i,t} + \beta' \cdot \mathbf{X}_{b,t-1} + \eta_b + \xi_d + \delta_t + \varepsilon_{i,d,b,t}$$

Transaction types	All transactions		Apartment units	
α	14.19***	14.23***	13.43***	13.11***
	(5.62)	(5.56)	(6.93)	(6.74)
Triple interaction (γ_7)	0.09**	0.13***	0.09*	0.14***
	(2.46)	(5.75)	(1.81)	(3.46)
Drift function	dummy	linear	dummy	linear
Time FEs	✓	✓	✓	✓
District & bank FEs	✓	✓	✓	✓
Bank/property/borrower controls	✓	✓	✓	✓
N	41,015	40,123	29,648	29,283
Adj. R^2	0.56	0.55	0.62	0.61

Note: Full set of interaction terms suppressed for space.

Full table

- ATT: $Gap \uparrow$ by $\approx$ 15% ($2.3k) relative to average of $15.5k gap for 2nd+ mortgages under 2014 reform
- $Gap \uparrow$ by $\approx$ 30% under 2010 reform with the loophole
- Since market prices depend on lagged appraisals, collateral inflation creates **persistent mispricing** in regulated neighborhoods

Additional results for appraisal gap analysis

1. **Baseline DiD:** average appraisal gap for entire mortgage market $\uparrow$ 6% Jump

2. **Winsorizing:** drop if Gap outside $\pm 5 \times IQR$, or drop loans with extremely low or high bank appraisals relative to AVM (Demiroglu & James 2018) Jump

3. **Controls:** lagged bank balance sheet variables, branch vs. bank fixed effects, and winsorized property controls Jump

4. **Dropping older properties** to account for increased difficulty in valuing properties with historical amenity value Jump

5. **Alternative scaling** of Gap Jump
   - Baseline definition is $Gap = \log(A - A^*)$
   - Check using $Gap = (A - A^*)/.5(A + A^*)$ (Kruger & Maturana 2021)

Separating Credit Demand vs. Credit Supply Responses

How do spatially targeted LTV limits operate?

Go back

- LTV limits are enforced through banks but target household leverage
  - **Demand channel:** investors lower WTP for properties in regulated areas due to higher downpayment requirements
  - All else equal, higher leverage loans generate higher internal rates of return (IRR) for lenders
  - **Supply channel:** lenders might ration credit in regulated areas, or steer borrowers towards loan contracts which are unregulated or which carry higher IRR
    - ★ Matched DiD: robust decline, 150-300 bps. in IRRs after 2014 reform
- Standard technique to separate supply vs. demand in loan origination is the Amiti & Weinstein (2018) decomposition for corporate loans
  - Problem: relies on identification of bank and borrower fixed effects
  - Very limited number, and very selected sample, of repeat borrowers within time window around each reform

Isolating supply-side responses using regulation exposure

- Idea: tease out credit rationing using measures of how *ex ante* exposed lenders' loan portfolios are to LTV regulation Go back

- Define **exposure** as the dollar share of loans each lender j originated in treated areas within a year before the reform:

$$Exposure_j = \frac{\sum_{i=1}^{N_j} \left(Loan\_amt_{i,j} \times Treated_{i \in d} \right)}{\sum_{i=1}^{N_j} Loan\_amt_{i,j}} \tag{5}$$

- Further decompose into exposure by 1st (unregulated) vs. 2nd+ mortgages (regulated) or parent bank b level vs. branch j level Distribution

- Collateral internalization: $Exposure_j$ also picks up the fact that collateral values may fall due to change in broader housing market demand (Favara & Giannetti 2017)
  - Measure based directly on collateral values would require book-to-market conversion

Drop in loans concentrated among most exposed branches

$$L_{j,b,d,t} = \gamma_1 \cdot Post_t \times Treated_{j \in d} + \gamma_2 \cdot Post_t \times Exposure_j + \gamma_3 \cdot Post_t \times Treated_{j \in d} \times Exposure_j + \eta_j + \theta_{d,t} + \varepsilon_{j,b,d,t} \quad (6)$$

	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 × time FEs	✓	✓	✓	✓
N	28,280	28,280	10,013	10,013
Adj. R^2	0.52	0.61	0.41	0.49

Note: t-stats from standard errors clustered by bank-time in parentheses. t-stats clustered by branch in brackets. $Exposure_j$ measured using 2nd+ mortgages originated on properties located in regulated areas but in the year prior to the 2014 reform.

- Collapse data to branch-month level
- More exposed branches within the same district reduce their 2nd+ mortgage lending by more
- Suggestive of substitution towards unregulated first mortgage borrowers
- Extensive margin is important: stronger results for untransformed 2nd+ mortgage outcomes

Go back

But no evidence of branch network contagion effects

$$\Delta L_{j,b,d,t,t+1} = \alpha + \gamma_1 \cdot Exposure_{j,t-1} + \gamma_2 \cdot Exposure_{j,t-1} \times Treated_{j \in d}$$
$$+\gamma_3 \cdot \sum_{k \neq j}^{N_b} Exposure_{k,t-1} + \gamma_4 \cdot \sum_{k \neq j}^{N_b} Exposure_{k,t-1} \times Treated_{j \in d} + \xi_d + \varepsilon_{j,b,d,t,t+1} \quad (7)$$

- Direct exposure $Exposure_{j,t-1}$ vs. exposure of N_b peer branches within same bank chain $\sum_{k \neq j}^{N_b} Exposure_{k,t-1}$ Definition
- **Hypothesis:** banks more exposed to regulatory risk through LTV limits might smooth out the shock across branches in their network $\implies \gamma_4 > 0$
  - Lower screening standards in unregulated areas or loan segments to make up lost profits
  - If so, spatial limits may have simply exported risk to unregulated areas
- **Our result:** no network effects on either 1st or 2nd+ mortgage issuance Full table

Discussion: How do we choose between macroprudential policy instruments?

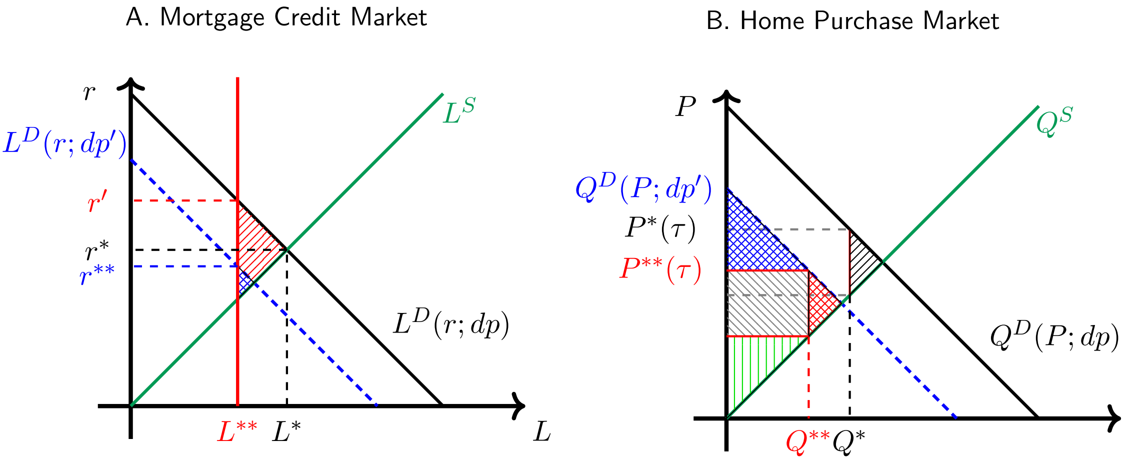
Comparing macroprudential policy elasticities

- Our results yield an elasticity of local house prices w.r.t. local leverage ratios of $\varepsilon \approx 0.75$
  - This number takes into account spillovers across the border to the unregulated control housing markets in the 2014 reform

$$\varepsilon = \frac{\%\Delta P}{\%\Delta LTV} = \frac{\overbrace{-(5\% - 2\%)}^{\text{Border DiD estimate of } \Delta P \text{ net of spatial spillover}}}{\underbrace{(55\% - 60\%)/60\% - (67\% - 70\%)/70\%}_{\text{1st stage effect comparing regulated vs. unregulated areas}}} = 0.75$$

  - Lower bound due to avoidance through collateral appraisal inflation
- Appears consistent with ε estimates from other broad-based LTV policies
  - Local semi-elasticity estimates for strict LTV policies (Armstrong, Sklling, Yao 2019 [NZ] ; de Araujo et al. 2020 [Brazil]) $\implies 0.3 \leq \varepsilon \leq 1$
  - Caveat: estimates not reported in comparable ways across studies (local vs. macro)

Short-run welfare losses from spatial LTV limit



- $\Delta Surplus = \Delta BS + \Delta SS + \tau \cdot \Delta(P \times Q) \propto \Delta C$
- **Reduced-form consumption losses:** $\Delta C = \Delta(P \times Q)$

Place-based policy welfare decomposition

- Anchoring min. downpayments to local HP growth is a type of **place-based policy**
- Decompose losses generated for each type of market actor
  - Small effects on deed (stamp duty) tax of 1% drop in revenues Details
  - Ignore banking sector risk since no avg. effects on delinquency outcomes
  - Analog to decompositions in Busso, Gregory, Kline (2013); Lu, Wang, Zhu (2019)
- Borrowers don't lose much because higher downpayment accompanied by lower rates
  - Housing supply shifts inward only slightly since mortgage lock-in channel is weak
  - Reform also targeted investors, who are more mobile in terms of housing choice
- Most losses driven by lenders who originate **lower IRR loans** and incumbent homeowners who see property values $\downarrow$
  - 12% loss to housing consumption $\longrightarrow$ low relative to transfer taxes Estimates

Takeaways from spatial quantity controls on housing

- We provide new evidence on how conditioning mortgage credit provision to investors on *ex ante* local ΔP can cool housing markets and help **first-time homebuyers**
  - Contrast to local transaction taxes which often result in $P \uparrow$ due to capital lock-in
  - Lower rate on mortgages since banks no longer charge insurance premia
  - No impact on delinquency outcomes $\implies$ not mitigating systemic risks
- **Policy implications** for current system in U.S. of local restrictions based on national rules (FHA, Conforming Loan Limit)
  - Potential macroprudential gains to moving to rule indexed to local ΔP
  - But financial costs of mispricing collateral if incentives to collude are high
  - Small inc. in real commuting costs from moving further out from CBD
- Redistribution: losses borne by (high-income) incumbent homeowners and lenders

THANK YOU!

Appendix

COVID-era LTV restrictions in Taiwan

Go back

Effective date	Type	Property target	Region	Buyers	Maximum LTV
December 8, 2020	T	Third (mortgaged) homes	All regions	Individuals	60% of $min(price, collateral\ value)$
		First (mortgaged) homes	All regions	Institutions	60% of $min(price, collateral\ value)$
		Second (mortgaged) homes	All regions	Institutions	50% of $min(price, collateral\ value)$
		High-end properties	All regions	Individuals and institutions	60% of $min(price, collateral\ value)$
		Land	All regions	Individuals and institutions	65% of $min(price, collateral\ value)$
March 19, 2021	T	Third (mortgaged) homes	All regions	Individuals	55% of $min(price, collateral\ value)$
		Fourth (mortgaged) homes	All regions	Individuals	50% of $min(price, collateral\ value)$
		First and second high-end properties	All regions	Individuals	55% of $min(price, collateral\ value)$
		Third high-end properties	All regions	Individuals	40% of $min(price, collateral\ value)$
		Residential properties	All regions	Institutions	40% of $min(price, collateral\ value)$
September 24, 2021	T	Second (mortgaged) homes	All regions	Individuals	Interest-Only mortgages not available
		Land	All regions	Individuals and institutions	60% of $min(price, collateral\ value)$
December 17, 2021	T	Second (mortgaged) homes	8 major cities	Individuals	40% of $min(price, collateral\ value)$
		Third (mortgaged) homes	All regions	Individuals	40% of $min(price, collateral\ value)$
		High-end properties	All regions	Individuals	40% of $min(price, collateral\ value)$
		Land	All regions	Individuals and institutions	50% of $min(price, collateral\ value)$

Govt. picked high (resid.) price growth districts

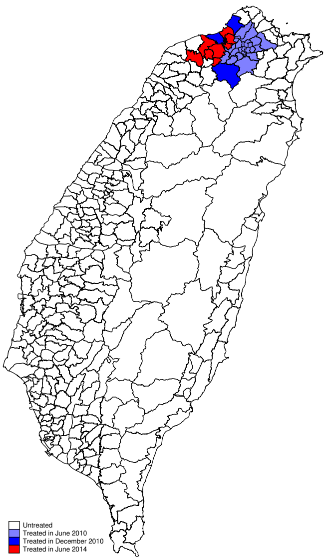
Go back

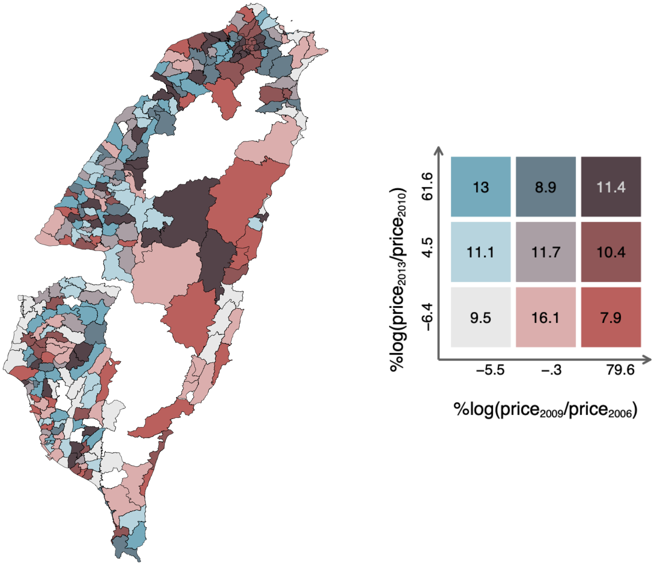
$$\log p_{i\in g,q} = \delta_q^g + \gamma_b^g + \beta^{g\prime} \cdot \mathbf{X_{i\in g,t}} + \varepsilon_{i\in g,q} \implies \Delta \widetilde{P}^g_{q,q+1} = \exp(\widehat{\delta}^g_{q+1}) / \exp(\widehat{\delta}^g_q) - 1$$

	$\%\Delta\widetilde{P}_{08Q1-10Q1}$		$\%\Delta\widetilde{P}_{10Q2-12Q2}$		$\%\Delta\widetilde{P}_{12Q2-14Q2}$		$\%\Delta\widetilde{P}_{14Q3-16Q3}$		$\%\Delta\widetilde{P}_{16Q4-18Q4}$	
	(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		✓		✓		✓		✓		✓

Govt. picked high (resid.) price growth districts

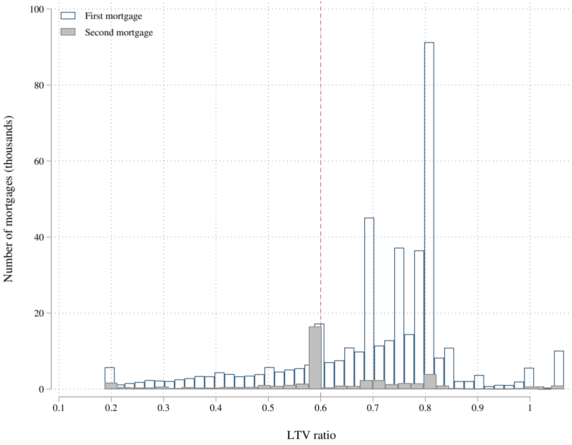
Go back



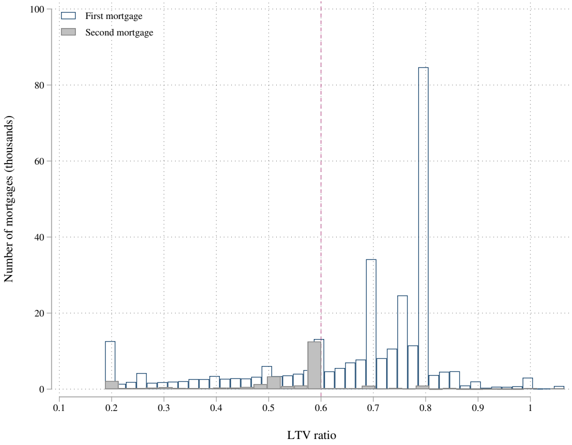


LTV of 2nd home mortgages bunch in treated region

Go back



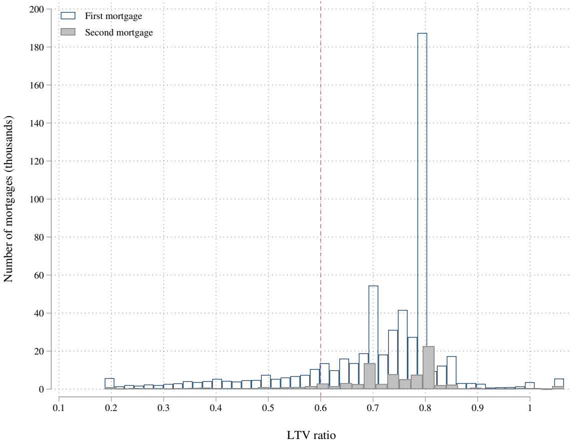
2010 policy regime



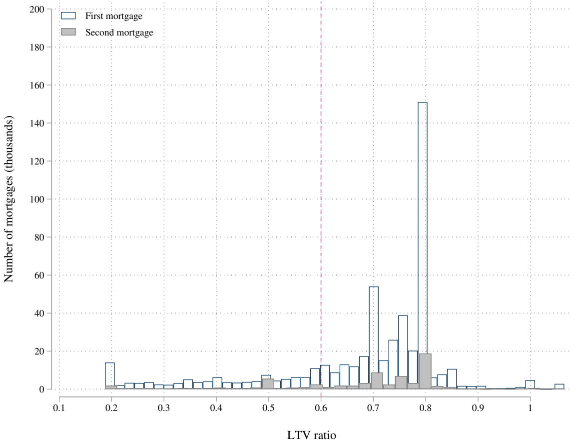
2014 policy regime

Mortgages in untreated region as a placebo

Go back



2010 policy regime

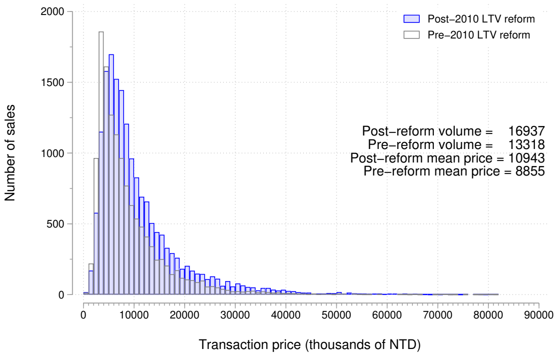


2014 policy regime

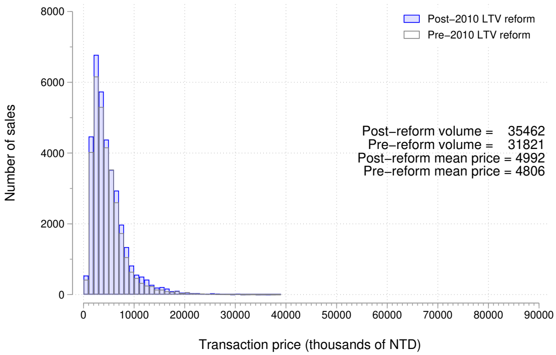
Due to avoidance, 2010 reform had no impact on volume

A. Treated districts

Go back



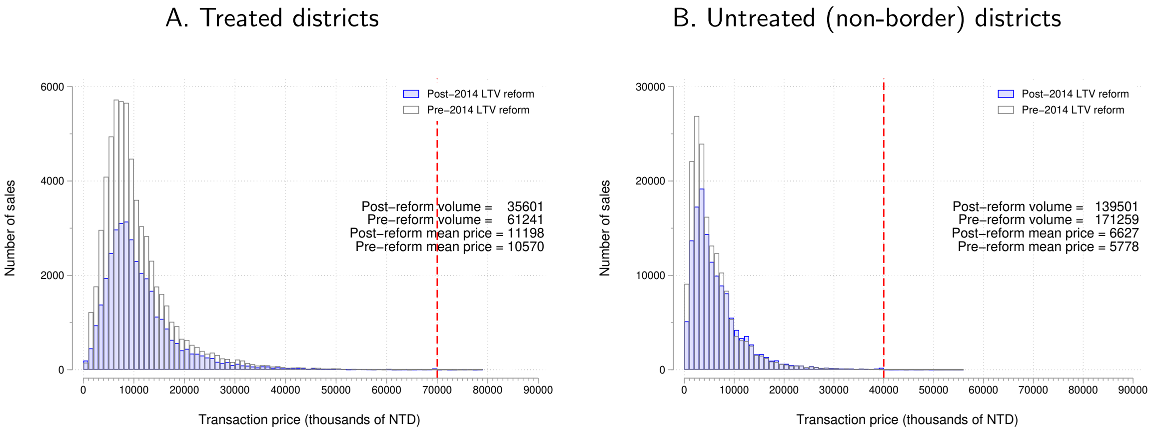
B. Untreated (non-border) districts



- Naive DiD in means: $(16,937/13,318) - (35,462/31,821) = 15.73\% \uparrow$ in volume suggests no deterrence of investment buying due to collateral loophole

Clear drop in sales volume after 2014 LTV limits

Go back



Additional Matched DiD Results

Borrower characteristics before vs. after matching

Go back

A. December 2010 reform

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

B. June 2014 reform

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

2010 MATCHED DIFF-IN-DIFF $\Longrightarrow$ SMALLER, SHORTER LOANS, $P\downarrow$

	log(loan \$)		log(psm)		interest rate (%)	
ATT	-0.130^{***}	-0.128^{***}	-0.092^{*}	-0.104^{**}	-0.029	-0.033
	(0.044)	(0.048)	(0.049)	(0.045)	(0.031)	(0.033)
Matched variables:						
District & bank	✓	✓	✓	✓	✓	✓
Salary income	✓	✓	✓	✓	✓	✓
Age	✓	✓	✓	✓	✓	✓
Education	✓	✓	✓	✓	✓	✓
LTV bandwidth	$\pm4\%$	$\pm4\%$	$\pm4\%$	$\pm4\%$	$\pm4\%$	$\pm4\%$
Property controls		✓		✓		✓
N	4,052	3,742	3,962	3,656	4,052	3,742

- Compared to 2014 reform, much weaker effects on prices and interest rates Go back
- Similar point estimates if use tighter range, but wide CIs
- Some neg. effect on maturity: substitution to shorter loans to lock in teaser rates

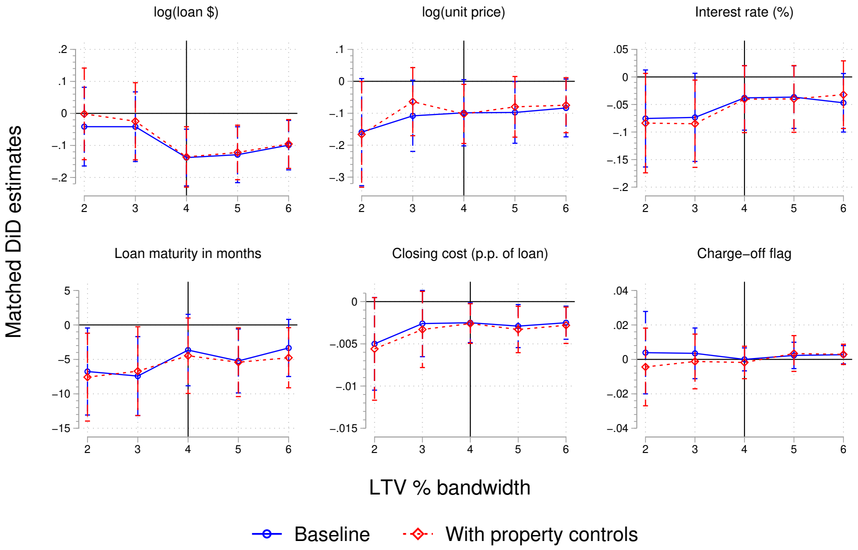
No avg. effect on loan delinquency (2010 reform)

Go back

	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	$\pm 4\%$	$\pm 4\%$	$\pm 4\%$	$\pm 4\%$	$\pm 4\%$	$\pm 4\%$
Property controls		✓	✓		✓	✓
N	4,052	3,742	3,742	4,052	3,742	3,742

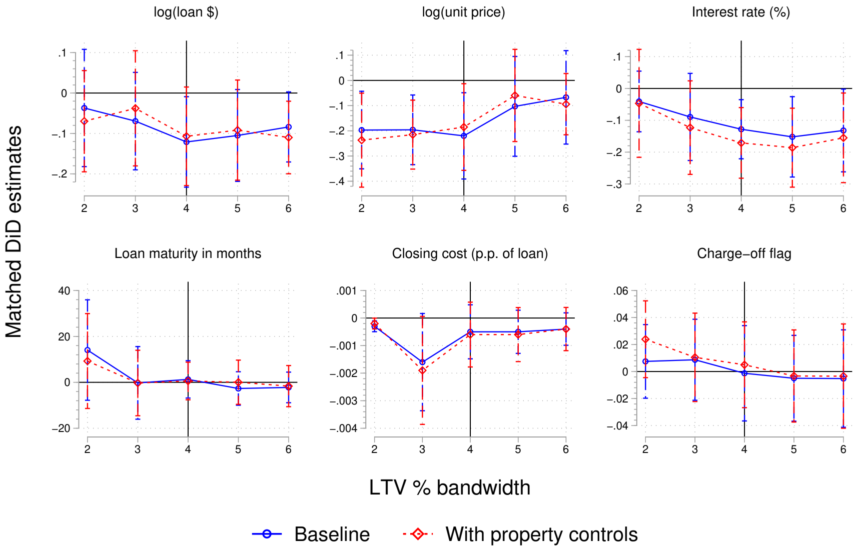
2010 reform matched DiD estimates by bandwidth

Go back



2014 reform matched DiD estimates by bandwidth

Go back



Hetero. delinquency effects by *ex ante* bank ROE

Go back

$Post_t$	-1.401^{***}	-1.430^{***}	-0.676	-0.669
	(0.483)	(0.481)	(0.425)	(0.426)
$Post_t \times \mathbb{1}\{LTV > 60\%\}_j$	1.175^{**}	1.217^{**}	1.140^{**}	1.117^{**}
	(0.515)	(0.516)	(0.502)	(0.496)
$ROE_{i,b} \times Post_t$	0.037^{***}	0.038^{***}	0.0176	0.0173
	(0.013)	(0.013)	(0.011)	(0.011)
$ROE_{i,b} \times \mathbb{1}\{LTV > 60\%\}_j$	0.001	0.001	0.001	0.001
	(0.004)	(0.004)	(0.004)	(0.004)
$ROE_{i,b} \times Post_t \times \mathbb{1}\{LTV > 60\%\}_j$	-0.031^{**}	-0.032^{**}	-0.030^{**}	-0.029^{**}
	(0.013)	(0.013)	(0.013)	(0.013)
LTV bandwidth	$\pm 5\%$	$\pm 5\%$	$\pm 6\%$	$\pm 6\%$
Property controls		✓		✓
N	426	426	484	484

- Results for ever-delinquent flag in 2014
- Caveat: low N since bank balance sheets (TEJ+) required to compute ROE

Calculating internal rates of return on mortgages

Go back

- Internal rate of return (IRR) sets equal the value of the loan L_0 less closing costs C_0 equal to discounted monthly payments PMT

$$L_0 = C_0 + \sum_{n=1}^{T} \frac{PMT}{(1+IRR)^n} + \frac{L_T}{(1+IRR)^T} \tag{8}$$

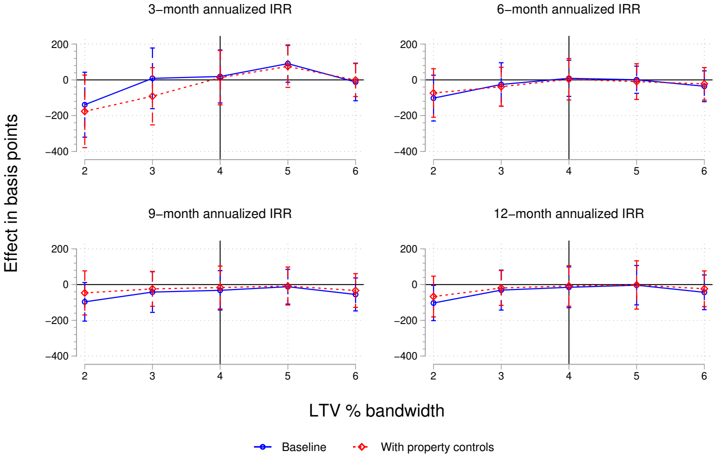
- Since all loans are ARMs in our case, PMT is constant within a reset period $n \leq T$
- Feed in the observed set of interest rates to recalculate PMT within each reset period
- Expected IRRs determined by expected future loan balances:

$$\mathbb{E}_t[L_{t+1}] = \mathbb{E}_t L_0 \prod_{s=1}^{t+1} \Big(1 + i_s/12\Big) - \mathbb{E}_t[TotalPaymentMade_{t+1}] \tag{9}$$

  - Assume lenders forecast i_t via 12-month lagged moving average

2010 reform matched DiD: realized IRR estimates

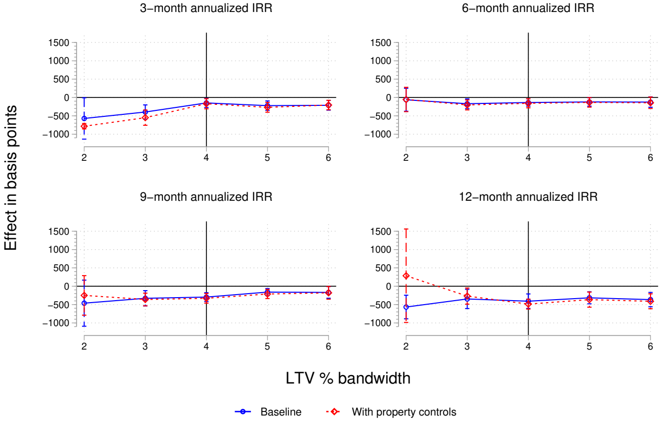
Go back



- No statistically or economically significant effects on realized IRRs with LTV loophole

2014 reform matched DiD: realized IRR estimates

Go back



- Robust, large 150-300 bps. decline in IRRs after 2014 reform
- Pass through of **lower mortgage insurance costs** borne by lenders

Realized vs. excess IRRs around 2014 LTV reform

Go back

	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	$\pm 4\%$	$\pm 4\%$	$\pm 5\%$	$\pm 5\%$	$\pm 4\%$	$\pm 4\%$	$\pm 5\%$	$\pm 5\%$
Property controls		✓		✓		✓		✓
N	172	164	180	176	162	152	172	172

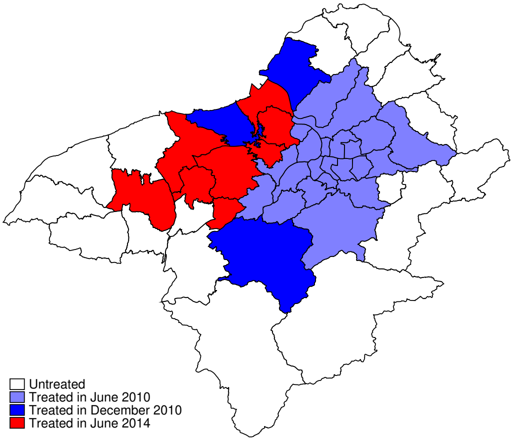
- Excess IRR at horizon t months into loan is gap between expected and realized IRR
- Expectations became re-anchored to realized returns after closing of loophole

Additional Border Diff-in-Disc Results

Implementation of border "diff-in-disc" design

Go back

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



- Bandwidth x: restrict to obs. within distance $\leq x$ to border
- $f(\cdot)$ local linear function in lat/lon
- $g(\cdot)$ linear spline in distance to nearest commuter rail
- Border segment ϕ^s or neighborhood FEs
- Standard errors either (i) clustered by district, or (ii) Conley correction
  - Use Conley distance cutoff which maximizes standard errors
  - Search over range from 2 km to max district distance to border (49 km)

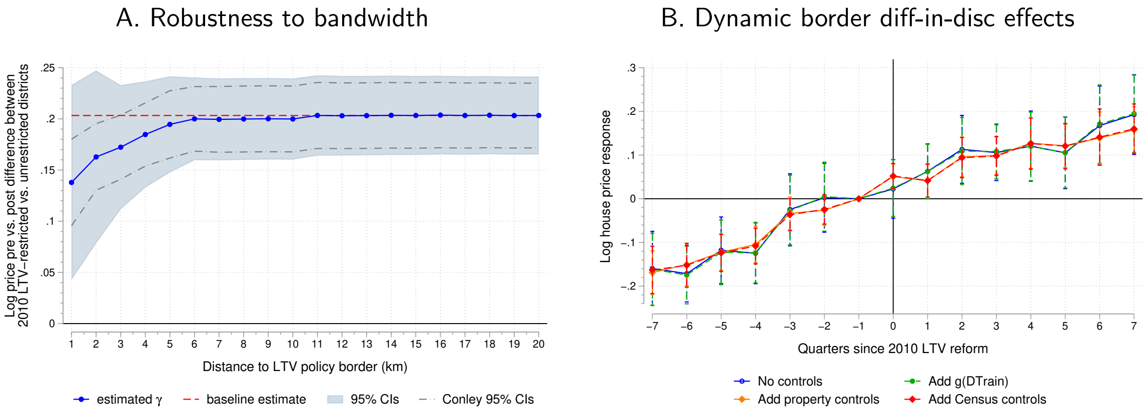
Pooled border diff-in-disc (2014 reform)

Go back

	(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: Conley standard errors estimated with a maximal spatial correlation distance cutoff parameter of 2 km appear in brackets.

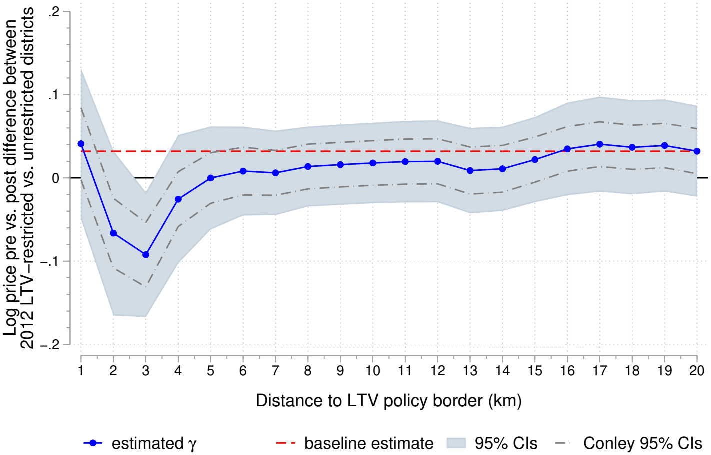
Due to loophole, no impact of 2010 reform local prices



- House prices in treated border neighborhoods continue to grow on trend Go back
  - 2010 treated group of districts also more positively selected based on ΔP path
- Contrast to ATT (matched DiD) estimates which restrict to regulated 2nd mortgages

Placebo: 2012 LTV reform to very high-end homes

Go back



Notes: Shaded confidence intervals obtained by clustering standard errors at the district level. Conley standard error bands in green dashed lines obtained with spatial cutoff of 20 km. Baseline point estimate indicated by red dashed line obtained with a bandwidth of $x = 20$ km.

- 2012 reform left 2010 regime intact but added new restriction on loans for properties with $P >$ 80 mil. NTD ($\approx$ 2.5 mil. USD)
- We drop obs. below 1st or above 99th pct., so such sales are not included
- No significant effect on prices for all bandwidth choices
- $\Longrightarrow$ border discontinuity not simply picking up differential neighborhood price trends

Robust to using faraway never-treated as control

Go back

	(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

- Nearly identical point estimates if compare properties in regulated districts to those in non-border, never-treated districts

Price declines concentrated in high-income districts

Go back

	(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)
$f(lat, lon)$	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

Large drop in sales volume in regulated districts Go back

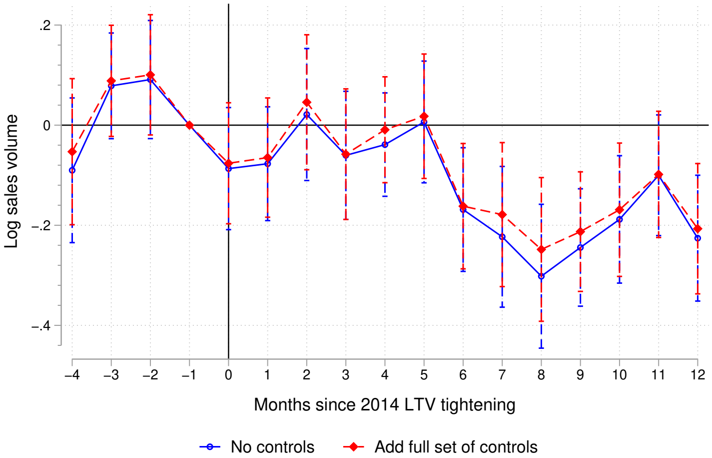
$$\log(Volume_{d,t}) = \gamma \cdot \left(LTVCap_{d,t} \times Post_{d,t}\right) + g(\overline{DTrain}_{d,t}) + \beta' \cdot \mathbf{X}_{c,t} + \xi_d + \delta_t + \varepsilon_{d,t}$$

	(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

- Control group: never-treated, non-border districts $\Longrightarrow$ unlikely to be driven by sorting

Dynamic DiD: sales volume around 2014 LTV reform

Go back



- Timing of drop in sales volume matches timing of price decline

Misreporting test: land vs. buildings in 2010 regime

Go back

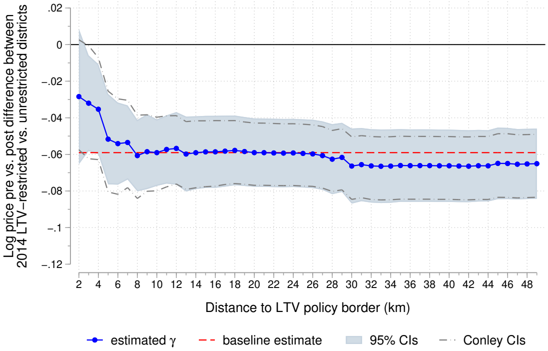
$$\log(Volume_{d,t}) = \gamma \cdot \left(LTVCap_{d,t} \times Post_{d,t}\right) + g(\overline{DTrain}_{d,t}) + \beta' \cdot \mathbf{X}_{c,t} + \xi_d + \delta_t + \varepsilon_{d,t}$$

	(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

- Sign flips if restrict to land sales subject to the $65\% \times \min\{appraisal, price\}$ rule

Sharper drop in prices around city policy boundary

Go back



- Zoom out and redraw boundary to include cities with treated districts
- Similar ΔP away from border, but clearer discontinuity than at district-level
- **Idea:** most lending done by multi-branch banks which reroute customers to nearest branch within city limits
- Suggests that targeting at the neighborhood level may be too fine
- Can improve spatial targeting by defining boundaries as span of *mortgage market*

2010 policy: demand shifting from 2nd to 1st properties

	log(loan $)				log(price)			
Mortgage type	All	First	Second	Third	All	First	Second	Third
$TreatPost_{i,d,t}$	0.306	0.066**	−0.274*	−0.106	0.071***	0.087***	−0.086*	0.193
	(1.22)	(3.28)	(-1.59)	(-0.56)	(2.85)	(2.48)	(-4.33)	(1.40)
$Treat_{i,d}$	0.341***	0.334***	0.357***	0.397***	0.328***	0.324***	0.376***	0.168**
	(13.70)	(13.75)	(6.71)	(3.71)	(14.07)	(13.97)	(8.36)	(2.56)
N	47,381	40,885	4,692	689	47,381	40,885	4,692	689
Adj. R^2	0.38	0.40	0.36	0.66	0.41	0.41	0.41	0.74
$TreatPost_{i,d,t} \times Type_i$		0.289**	−0.329***	−0.0728*		0.044**	−0.072***	0.081**
		(9.97)	(-11.40)	(-1.69)		(2.53)	(-3.98)	(2.15)
N		47,381	47,381	47,381		47,381	47,381	47,381
Adj. R^2		0.39	0.39	0.38		0.41	0.41	0.41

- Despite strong pre-trend, LTV policy successfully reduced speculative demand Go back
- No significant effects on mortgages for third properties which are not spatially regulated

2016 LTV loosening: only investment demand reverted

	log(loan \$)				log(price)			
Mortgage type	All	First	Second	Third	All	First	Second	Third
$TreatPost_{i,d,t}$	-0.017	-0.035	0.263		-0.014	-0.0262	-0.052	
	(-0.71)	(-1.47)	(1.46)		(-0.48)	(-0.94)	(-0.43)	
$Treat_{i,d}$	0.379^{***}	0.405^{***}	-0.006		0.373^{***}	0.386^{***}	0.332^{*}	
	(11.06)	(12.87)	(-0.02)		(11.42)	(12.71)	(1.83)	
N	7,132	6,218	385		7,132	6,218	385	
Adj. R^2	0.37	0.38	0.67		0.41	0.42	0.69	
$TreatPost_{i,d,t} \times Type_i$		-0.185^{***}	0.207^{***}	0.003		-0.096^{**}	0.115^{**}	-0.052
		(-3.18)	(3.40)	(0.02)		(-2.05)	(2.29)	(-0.35)
N		7,132	7,132	7,132		7,132	7,132	7,132
Adj. R^2		0.38	0.38	0.37		0.41	0.41	0.41

- No overall recovery, but demand for 2nd properties reverted Go back
- Removing LTV cap did not reduce affordability for first homebuyers!

Additional Appraisal Gap Results

Steps to compute the appraisal gap

Go back

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

- A^* is the official appraisal for tax purposes
- A is the collateral value reported by the lender at origination
- To obtain A^* we distinguish between land only, building + land, and building transactions
  - Land portion of appraised value observed directly in year t^*, inflate using our index $\Delta\widetilde{P}^d_{t*,t}$
  - For buildings appraised every 3 years in t^*, we use **known local valuation formula:**

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

- $standard\_value$, depreciation factor (δ) and road adjustment factor (ζ) depends on property type, updated by district in each year
- $A > A^*$ in 99.2% of cases, so log transform does not censor the data

DiD evidence of misreporting after 2014

Go back

	All transactions		Apartment units	
α	15.37***	15.05***	14.08***	13.33***
	(5.52)	(5.40)	(7.24)	(7.18)
$Post_t$	0.01	0.00	−0.01	−0.02
	(0.50)	(0.10)	(0.53)	(1.04)
$LTVCap_{i,d}$	−0.05	−0.06*	−0.06**	−0.07***
	(1.31)	(1.85)	(1.96)	(3.12)
$LTVCap_{i,d} \times Post_t$	0.03	0.04**	0.05**	**0.06*** **
	(1.42)	(2.03)	(2.05)	(3.10)
$\mathcal{D}(t, t^*)$	−0.06***	−0.00***	−0.08***	−0.00***
	(2.69)	(4.65)	(3.40)	(4.45)
Drift function	dummy	linear	dummy	linear
Time FEs	✓	✓	✓	✓
District & bank FEs	✓	✓	✓	✓
Bank controls	✓	✓	✓	✓
Property controls	✓	✓	✓	✓
Borrower controls	✓	✓	✓	✓
N	41,015	40,123	29,648	29,283
Adj. R^2	0.54	0.54	0.60	0.60

- *Gap* ↑ by ≈ 6% ($1k) relative to average of $20k gap under the 2010 regime with the loophole
  - Estimation sample: all mortgages (ITT effect)
  - Extend to triple diff to get ATT
- Recall that 2010 reform defined limit as 60% of collateral value
  - Loophole: not a function of the price until 2014!
- Lenders not required to use official appraisers plus no restrictions on valuation model 2010 reform

Appraisal gap triple diff: full results table

Go back

Transaction types	All transactions		Apartment units	
α	14.19***	14.23***	13.43***	13.11***
	(5.62)	(5.56)	(6.93)	(6.74)
$Post_t$	0.08***	0.08***	0.08	0.06
	(3.62)	(3.19)	(1.70)	(1.52)
$LTV\_District_{i,d}$	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***	−0.13**	−0.18***
	(3.06)	(4.94)	(2.18)	(3.42)
$Post_t \times LTV\_District_{i,d} \times 2nd\_Mrtg_i$	0.09**	0.13***	0.09*	0.14***
	(2.46)	(5.75)	(1.81)	(3.46)
$\mathcal{D}(t,t^*)$	−0.05**	−0.00	−0.06**	−0.00***
	(2.45)	(1.38)	(2.85)	(3.14)
Drift function	dummy	linear	dummy	linear
Time FEs	✓	✓	✓	✓
District & bank FEs	✓	✓	✓	✓
Bank/property/borrower controls	✓	✓	✓	✓
N	41,015	40,123	29,648	29,283
Adj. R^2	0.56	0.55	0.62	0.61

- Reference group: first mortgages in untreated districts in the pre-reform period $\longrightarrow \alpha$
- Add the coefficients on $2nd\_Mrtg_i$ and $LTV\_District_{i,d}$ to α to get the pre-existing average appraisal gap in the treatment group
- Drift function $\mathcal{D}(t,t^*)$ loads negatively on the gap in all specifications, reflecting benign gap from delays between official appraisals

APPRAISAL GAP TRIPLE DIFF: $Gap = (A - A^*)/.5(A + A^*)$

GO BACK

Transaction types	All transactions		Apartment units	
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
Time FEs	✓	✓	✓	✓
District & bank FEs	✓	✓	✓	✓
Bank/property/borrower controls	✓	✓	✓	✓
N	41,178	40,112	29,797	29,268
Adj. R^2	0.66	0.73	0.70	0.77

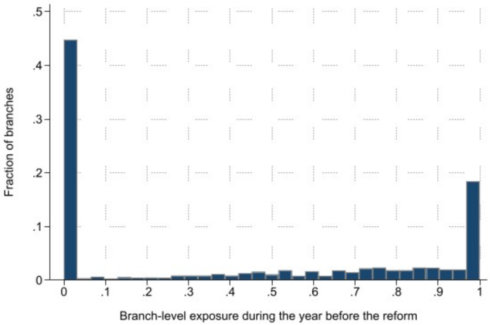
- Alternative measure proposed by Kruger & Maturana (2021)
- Gap centered at zero if A and A^* symmetrically distributed around same mean (not true here)
  - Here, there is a large existing pre-existing gap (α), since typically $A^* << A$
- 2-3 p.p. increase in gap for 2nd+ mortgages relative to average valuation $\overline{A} = .5(A + A^*)$
- Similar result for $Gap = (A - A^*)/A^*$

Additional Bank Branch Exposure Results

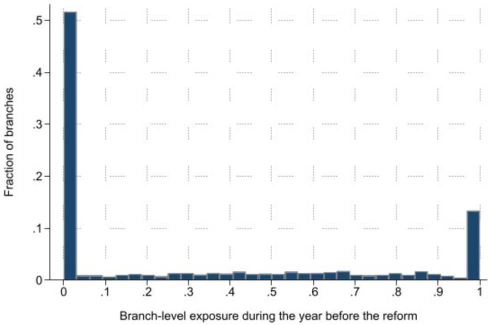
Distribution of branch exposure measure

Go back

Exposure before 2010 LTV reform



Exposure before 2014 LTV reform



Note: $Exposure_j$ defined in terms of 2nd+ mortgage loan amounts

- Share of unexposed branches rises by 6 p.p. after initial 2010 reform
- Use balanced panel of branches b/c some stop originating 2nd+ mortgages altogether

Within-bank branch network exposure measure

Go back

- We define the indirect branch network exposure of a branch j of parent bank b as:

$$\sum_{k \neq j}^{N_b} Exposure_{k,t-1} = \sum_{k \neq j}^{N_b} \left(\frac{\sum_{i=1}^{N_k} \left(Loan\_amt_{i,k} \times Treated_{i \in d} \right)}{\sum_{i=1}^{N(b)} Loan\_amt_{i,b}} \right) \tag{10}$$

  - N_b is the # of branches within bank b
  - N_k is the # of loans originated within branch k
  - $N(b)$ is the # of loans originated within bank b

- **Interpretation:** this measure captures how much the branch peers contribute to the overall regulation exposure of the parent bank's mortgage portfolio

Null results from branch network regressions

Go back

$$\Delta L_{j,b,d,t,t+1} = \alpha + \gamma_1 \cdot Exposure_{j,t-1} + \gamma_2 \cdot Exposure_{j,t-1} \times Treated_{j\in d}$$

$$+\gamma_3 \cdot \sum_{k\neq j}^{N_b} Exposure_{k,t-1} + \gamma_4 \cdot \sum_{k\neq j}^{N_b} Exposure_{k,t-1} \times Treated_{j\in d} + \xi_d + \varepsilon_{j,b,d,t,t+1}$$

	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

Note: Column headings indicate which subsample (first mortgages vs. mortgages on a second property) of loans are included in the lending growth outcome measure. $Exposure$ measured using 2nd+ mortgages originated on properties located in regulated areas but in the year prior to the 2014 reform. $Exposure$ rescaled in terms of 10 p.p. increments. t-stats from standard errors clustered at the branch level in parentheses.

- Collapse data to branch-year level
- Results here use 2nd+ mortgages to construct exposure measures, but also null if use all loans
- ξ_d compare two branches located within same district but which have differential network exposure through peer branches of same parent bank
- Still null effects without the district FEs
- Banks don't ration credit in more exposed branches

Welfare Decomposition

$\approx 1\%$ drop in deed tax revenues at district level

Go back

Property: $DeedTax_{i\in d,t} = \alpha + \beta \cdot LTVCap_{i\in d} \times Post_t + \eta_i + \gamma_t + \epsilon_{i,t}$

District: $\sum_{i\in(d,t)} DeedTax_{i\in(d,t)} = \alpha + \beta \cdot LTVCap_d \times Post_t + \xi_d + \gamma_t + \epsilon_{d,t}$

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

- $DeedTax_{i\in d,t} = 6\% \times A^*_{i,d,t^*} \times \mathbb{1}\{sale_{i,t}\}$
- Since sales volume $V = \sum_i \mathbb{1}\{sale_{i,t}\}$ falls, revenues drop at the district level
- A^* is sticky because reval only occurs once every 3 years
- Repeat sales regression $(\eta_i) \implies$ policy is revenue neutral at individual property level
- Total decline is 1% of annual revenue raised in regulated districts

Housing consumption losses ≈ 11-12% for spatial LTV limit

	$\Delta \log P$	$\Delta \log Q$	$\%\Delta C$	$\%\Delta C \cdot \omega_{2009}$	$\%\Delta C \cdot \omega_{2013}$
Estimate Set #1: (baseline)	-0.050^{***} (0.010)	-0.258^{***} (0.023)	26.5%	11.0%	11.4%
Estimate Set #2: (never-treated districts)	-0.056^{***} (0.008)	-0.258^{***} (0.023)	26.9%	11.1%	11.8%
Estimate Set #3: (including land sales)	-0.066^{***} (0.014)	-0.262^{***} (0.029)	28.0%	11.6%	12.3%

- Feed in border diff-in-disc estimates for prices; DiD estimates for sales volume
- Scale down total $\%\Delta C$ by *ex ante* share ω of transactions in regulated districts

Go back

Our companion paper: PR ratio targeting via taxes

Go back

- Common alternative to leverage-based MPPs is to tax housing transactions

- **Idea:** sellers pass along costs of the tax to buyers, which acts like an increased downpayment requirement

- **Reality:** taxes create an **inventory crunch**, as investors hold onto properties for longer to subdivide the fixed cost over a longer holding period

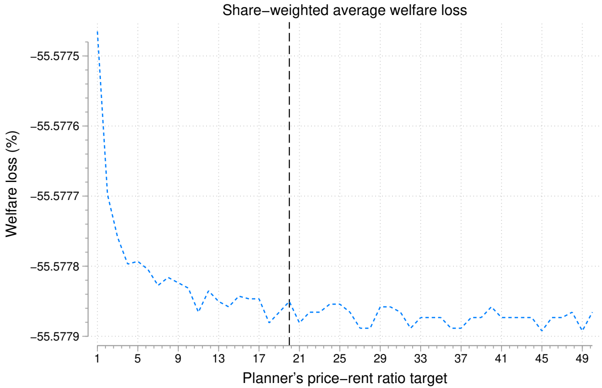
- In our companion paper, we show in a structural model that such flip taxes...

  1. Increase house prices for most tax rates, but help achieve price-rent (PR) ratio targets by pushing more people into rentership
  2. Renters on margin of homeownership gain, but aggregate welfare losses are large and equal to $\approx 56\%$ of housing consumption Details

- Targeting buyers directly using spatially targeted LTV limits helps improve affordability *in price levels* without large welfare losses

Transfer taxes generate large welfare losses

Go back



- Model from Chi, LaPoint, Lin (2023)
  - Investors with heterogeneous beliefs about house prices and rents
  - Government taxes housing sales to bring PR ratio down
  - Idea is that investors are noise traders, so tax moves their beliefs more in line with fundamental value
- **Result: aggregate welfare loss is almost invariant to PR ratio target**
  - Calibrate to 2011-16 transfer tax
  - Loss is roughly 56% of aggregate housing-based consumption