Politics and the Price of Housing

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Abstract

We examine how congressional representation affects local housing markets through its influence on housing finance. Using the near-universe of U.S. housing transactions from 1990 to 2020 linked to congressional districts, we exploit the staggered entry and exit of representatives from the House Financial Services Committee (FSC) as plausibly exogenous shocks to their influence over housing and mortgage policy. A border design comparing properties within five kilometers of adjacent districts holds local economic conditions constant. House prices increase by about 4 percent when a district's representative joins the FSC and decline by a similar amount when the representative leaves. These effects coincide with higher mortgage origination, greater government-sponsored enterprise (GSE) purchases, and higher conforming loan limits, but no change in construction activity or allocations of Low-Income Housing Tax Credit programs. Representation on the FSC affects local housing markets primarily through credit supply, leading to localized changes in property values.

Keywords: Political power; congressional committees; financial regulation; housing markets; credit supply; government-sponsored enterprises (GSEs); real estate prices.
JEL Classification: D72, G21, H73, R31.

1 Introduction

Housing is the primary asset for most U.S. households, accounting for nearly 70% of total wealth among those in the middle and upper-middle of the wealth distribution (Kuhn, Schularick, and Steins, 2020). Because most households own a single home, they cannot diversify housing risk, making local house price movements a key driver of consumption and employment (Iacoviello, 2005; Campbell and Cocco, 2007; Mian, Rao, and Sufi, 2013; Mian and Sufi, 2014). Access to mortgage credit plays a central role in these dynamics. Favilukis, Ludvigson, and Van Nieuwerburgh (2017) show that easier credit conditions explain much of the 2000s housing boom, while Guren and McQuade (2020) highlight that contractions in credit supply—amplified through foreclosures—intensified the subsequent bust. Greenwald and Guren (2025) further estimate that shifts in credit standards account for roughly one-half of the rise in price-rent ratios during the 2000s boom. Yet credit supply is not solely determined by market forces: it is substantially shaped by politics. Policymakers oversee financial regulation and the activity of lenders and government-sponsored enterprises that channel mortgage funding. If legislators influence these levers, political representation may determine where credit flows and, in turn, local housing values. This paper provides new evidence on how congressional representation shapes both housing prices and mortgage market outcomes.

Our analysis focuses on the U.S. House Financial Services Committee (FSC), which oversees banking and housing, and whose members hold formal authority over banking regulation, credit markets and mortgage policy. To identify the impact of congressional power on local housing outcomes, we exploit the staggered entry and exit of representatives from the FSC as quasi-exogenous shifts in district-level political influence. Following Cohen, Coval, and Malloy (2011), Blanes i Vidal, Draca, and Fons-Rosen (2012), and Bertrand, Bombardini, Fisman, and Trebbi (2020), we treat variation in committee assignments as plausibly exogenous, since such appointments are primarily driven by seniority and internal party negotiations rather than district-level economic fundamentals. We further strengthen identification by exploiting granular spatial discontinuities at congressional district boundaries to compare properties located just inside and just outside districts experiencing FSC transitions. Specifically, we focus on houses within narrow bands—up to a few kilometers—on either side of each border, ensuring that local economic fundamentals are likely comparable. This border-based design isolates localized changes in house prices that arise specifically from political representation on the FSC.

To implement this design empirically, we construct a property-level panel linking the near-universe of U.S. housing transactions between 1990 and 2020 to congressional districts on a Congress-by-Congress basis. A congressional district is classified as *treated* during Congresses in which its representative serves on the FSC. In our motivating analysis, we estimate a two-way fixed effects (TWFE) specification to quantify the average impact of FSC representation on local house prices in the full sample. We restrict the sample to properties that transact at least twice over the 30-year period (*repeat transactions*), which allows us to maintain a consistent composition of properties and enables the inclusion of property fixed effects to identify *within-property* changes in values—which is key given that properties vary across many unobservable dimensions (Wallace and Meese, 1997; van Binsbergen, Cocco, Grotteria, and Naaraayanan, 2025).[1] The granularity of the data further allows us to incorporate state-by-year-month fixed effects, absorbing time-varying local economic conditions that could otherwise confound house prices.

In our main empirical specification, we focus on properties located within five kilometers of a congressional district boundary (*border sample*), matching each property to its nearest neighboring district. The border-based design compares changes in house prices for properties just inside a treated district to those just outside, holding constant local economic conditions that vary smoothly across boundaries. To do this, we construct unordered district pairs linking adjacent districts (*pair-id*) and estimating specifications that interact each fixed effects with *pair-id*. The inclusion of property-by-pair fixed effects ensures that identification is based on within-property variation among houses consistently assigned to the same boundary pair, avoiding properties that might be reclassified due to redistricting or boundary changes including gerrymandering. Further, we interact state-by-year-month fixed effects with *pair-id* to ensure that identification of the estimate comes exclusively from within-boundary-pair differences over time. We show that properties located just inside districts represented on the FSC appreciate relative to those just outside, consistent with localized gains from political representation. These results are robust to using alternative distance bands— a narrower (3 km) and a wider (10 km) distance to the congressional district boundary.

To address concerns about treatment effect heterogeneity arising from the staggered timing of representatives' entry and exit from the FSC (Roth, Sant'Anna, Bilinski, and Poe, 2023; Baker, Larcker, and Wang, 2022), we implement a stacked difference-in-differences (DiD) specification using a two-year window around each event. We define an event at the property level

[1]Restricting the sample to repeat transactions over a 30-year period does not introduce meaningful selection, as the typical property transacts approximately once every ten years.

as the first instance during our sample period in which the property's congressional district experiences a change in FSC representation—either an entry or an exit by its representative. In each event year (*cohort*), properties in districts whose representatives never serve on the FSC during the sample period serve as controls. As before, we include both property and state-by-year-month fixed effects, now interacted with each *pair-id* and *cohort* to account for boundary- and event-specific heterogeneity. In this specification, house prices increase by about 4 percent when a representative joins the FSC and decline by a similar magnitude when they exit. The estimates remain robust across alternative distance bands, including narrower (3 km) and wider (10 km) border windows.

We conduct a battery of robustness exercises to mitigate concerns that our results are driven by sample composition, the choice of control group, and alternative event-windows. First, we modify the set of fixed effects to assess sensitivity of our estimates to sample composition: specifically, we retain property fixed effects without interacting them with district-pair identifiers, which allows for properties to change pair-id. In additional tests, we allow for comparisons across states by replacing state-by-year-month fixed effects with year-month fixed effects. Second, to further ensure comparability in local economic conditions near shared congressional district borders, we exclude properties that are close to the border but far from any developed area on the other side, ensuring that treated and control properties are drawn from spatially contiguous local markets. Third, in our baseline stacked DiD, the inclusion of property fixed effects in the narrow window of 2 years around the event allows us to identify the effect of the FSC entry and exit on house prices while mitigating concerns about the differential effects of confounding factors on house prices in the treated and control group. At the same time, the short event-window used in combination with property fixed effects can introduce selection in the properties included in the sample: it may over-sample transactions by buyers who acquire rundown properties, fix them up, and then sell them in a short period of time. To address this concern, we expand the event window to $[-3, +3]$ and $[-4, +4]$ years around FSC membership.

Having established that congressional representation on the FSC leads to localized increases in house prices, we next examine the channels through which this effect operates. We group potential mechanisms into those related to credit supply and those reflecting fiscal or housing-supply responses. The most direct channel operates through credit markets: committee members may relax local credit constraints by expanding mortgage availability or lowering borrowing costs, potentially altering the composition of borrowers. This can occur either through influence on housing finance policy or because lenders favor the districts of legislators serving on the FSC. We find strong evidence for both.

Consistent with the regulatory influence channel, districts represented on the FSC experience higher GSE purchases and securitization and rising conforming loan limits. Consistent with favoritism, we document a broader credit expansion in politically connected districts and find a significant increase in small-business lending following FSC appointments and a corresponding decline upon exit. Complementing these results, campaign contribution data show that banks and mortgage lenders substantially increase donations to representatives when they join the FSC and reduce them when they leave, suggesting that both credit allocation and financial support reflect a common mechanism of political favoritism.

Beyond mortgage markets, representation may also influence local housing prices through complementary channels under the FSC's jurisdiction. Members can affect the allocation of federal procurement contracts and financial assistance—particularly from agencies under their oversight—expand the reach of affordable housing programs such as the Low-Income Housing Tax Credit (LIHTC), or shape other federal resources supporting housing and community development. These mechanisms could, in principle, stimulate local income, amenities, or construction activity, thereby affecting house prices. However, contrary to these hypotheses, we find no evidence of changes in construction activity—as proxied by building permits—or in the allocation of affordable housing programs, procurement contracts, or financial assistance. Together, the results indicate that legislative power operates primarily through a localized credit-supply channel.

A natural question is why politicians would seek to influence credit allocation in the first place. Political control over mortgage credit can serve both electoral and distributive objectives: legislators may use financial policy to reward constituents or signal responsiveness to local economic conditions. Consistent with this view, we find that higher local house prices are associated with a greater probability of re-election for FSC members. This electoral link complements the findings of McCartney (2021), who shows a positive relation between house prices and voter turnout.

The remainder of the paper proceeds as follows. Section 2 describes the relation to the previous literature. Section 3 describes the data, treatment construction, and empirical strategy. Section 4 presents the main results and robustness analyses. Section 5 investigates the underlying mechanisms and the role of credit policy in driving our findings. Section 6 concludes.

2 Related Literature

This paper provides new empirical evidence that political representation shapes constituents' housing returns. We show that representation influences housing markets through both preferential credit allocation by lenders and changes in mortgage regulation. In doing so, we contribute to the literature on political economy, housing markets, and household finance.

First, our work relates to the extensive literature on the determinants of house prices and the broader effects of housing policy. Seminal contributions emphasize the role of fundamentals—such as inflation, user cost, and income growth—in shaping housing demand and values (Poterba, 1984; Davis and Heathcote, 2007; Glaeser and Gyourko, 2018). More recent studies highlight that financial frictions and credit supply play a central role in driving housing dynamics (Landvoigt, Piazzesi, and Schneider, 2015; Guren and McQuade, 2020; Greenwald and Guren, 2025). Beyond credit markets, housing policy interventions can themselves have sizable general-equilibrium effects. Diamond, McQuade, and Qian (2019) show that the expansion of rent control in San Francisco reduced tenant displacement but led to a persistent contraction in the local rental supply and higher market rents, illustrating how regulation can reallocate housing wealth and exacerbate inequality. Similarly, Diamond and McQuade (2019) find that affordable housing development under the Low-Income Housing Tax Credit (LIHTC) program revitalizes low-income neighborhoods but can depress property values in higher-income areas, implying that the spatial distribution of housing subsidies has heterogeneous welfare effects. Our analysis complements these studies by focusing on political channels—specifically, how representation in financial policymaking affects local housing prices through credit allocation—holding economic fundamentals and policy interventions constant.

Second, we build on research examining political influences in mortgage and housing finance. Prior work shows that political incentives shape the design of bailouts and crisis interventions, and that elected officials respond to foreclosure pressures, constituent preferences, and industry lobbying (Keys, Mukherjee, Seru, and Vig, 2010; Mian, Sufi, and Trebbi, 2010; Igan, Mishra, and Tressel, 2012; Agarwal, Amromin, Ben-David, and Dinc, 2018; McCartney, 2021; McCartney, Orellana-Li, and Zhang, 2024). We extend this literature by documenting how congressional representation provides localized control over mortgage policy. Specifically, we present novel evidence of a politically induced relaxation of GSE credit constraints through the expansion of conforming loan limits. Consistent with this mechanism, we find that FSC representation increases local mortgage supply by roughly 4% and raises the share of GSE-eligible mortgages in treated districts.

Finally, we connect to the broader literature on political power and rents (Bombardini and Trebbi, 2020, 2025). Political influence has been shown to affect firm performance, access to finance, and regulatory outcomes (e.g., Faccio, Masulis, and McConnell, 2006; Zingales, 2017; Hassan, Hollander, van Lent, and Tahoun, 2019; Grotteria, 2023). Despite housing being the largest asset on household balance sheets, little is known about whether political power influences household wealth through its effect on housing prices. We show that political influence over mortgage markets is capitalized into local house prices and, consequently, in the wealth of homeowners. Because housing is the dominant asset for most households, these effects imply that political power can redistribute wealth across communities through credit policy—revealing a distinct and previously overlooked channel linking political representation to the distribution of wealth.

3 Data and empirical strategy

House committee assignments. We obtain data on House of Representatives committee assignments from Harvard Dataverse (Stewart, 2021), which compiles official membership records from the Congressional Research Service and the Inter-university Consortium for Political and Social Research (ICPSR). The dataset covers Congresses 103–116 (1993–2021) and reports, for each legislator, their committee affiliations, party, start and end dates of appointment, and corresponding state and congressional district. We use these variables to construct a congress–member–district panel tracking changes in committee membership over time. Our treatment indicator is equal to one for districts whose representative serves on the House FSC (committee code 113) in a given Congress and zero otherwise. This committee oversees banking, housing, and financial regulation, and its members hold formal authority over mortgage credit markets and housing finance policy.

We merge the committee information with data on legislators characteristics obtained from the Center for Effective Lawmaking. For each member, the data includes information on the Legislative Effectiveness Score (LES) as well as member attributes, including political ideology (DW–NOMINATE scores), seniority, gender, minority status, and whether the legislator is a new entrant to Congress. LES quantifies how successful each member of Congress is at advancing bills through the legislative process, combining information on bill sponsorship, progression through committee, and ultimate enactment (Volden and Wiseman, 2014). These measures allow us to compare FSC members with other representatives across several dimensions.

Table 1 reports summary statistics for members of the FSC (Panel A) and non-members (Panel B) over the period 1993–2021. The two groups exhibit broadly similar distributions in ideology, party composition, rank, seniority, and legislative effectiveness, consistent with the idea that committee assignments are not systematically correlated with observable political attributes that might confound our analysis.

We next compare the socioeconomic conditions of treated and untreated congressional districts by combining the data with annual demographic and economic indicators from the American Community Survey (ACS) 1-Year Estimates spanning 2005–2021. These data provide consistent measures of population, age composition, educational attainment, income, labor market outcomes, and housing characteristics at the congressional district level. Table 2 presents summary statistics for treated and control districts. Panel A reports characteristics of districts represented by legislators serving on the FSC, while Panel B summarizes those for districts whose representatives never served on the committee. By construction, district populations are similar due to the legal requirement of equal apportionment within states. Beyond this, the two groups are similar on observable socioeconomic characteristics: median household incomes average around $62,000, homeownership rates are roughly 63–64 percent, and unemployment and poverty rates are comparable. FSC districts tend to exhibit slightly higher educational attainment and marginally lower poverty, but the overall balance across several observable characteristics supports the interpretation of FSC membership as a plausibly exogenous source of variation in local political influence.

CoreLogic Deed & Tax records. We use micro-level housing transaction data from CoreLogic Deed & Tax Records covering the period 1990–2020. The dataset provides detailed information on sale amounts, and geographic identifiers, including latitude and longitude coordinates. We restrict the sample to single-family residences, residential condominiums, duplexes, and apartments. Because our identification strategy requires precise spatial assignment of properties to congressional districts and house prices, we drop observations with missing block-level latitude or longitude coordinates or missing sale prices.

To link properties to congressional districts, we employ official shapefiles defining congressional boundaries for each Congress. For Congresses 103 through 114, we use the historical shapefiles compiled by Lewis, DeVine, Pitcher, and Martis (2013). For Congresses 115 through 116, we use TIGER/Line congressional district shapefiles from the U.S. Census Bureau. Using these spatial boundaries, each property in the CoreLogic data is assigned to its corresponding congressional district in each Congress. To ensure accurate matching, we link a property to congressional districts only for periods after the property was built, based on its recorded year of construction.

We define as treated those properties located in districts whose representative serves on the House FSC in a given Congress. A property can enter treatment for two distinct reasons: first, when its incumbent representative is newly appointed to the FSC; and second, when congressional redistricting alters district boundaries such that the property becomes part of a district represented by an FSC member. Our spatially matched, longitudinal dataset therefore captures both sources of variation—political entry and geographic reassignment—allowing us to analyze how changes in congressional representation affect local house prices over time.

In our motivating analysis, we estimate a TWFE model using all residential transactions across the contiguous United States between 1990 and 2020. We then implement a border-based design that compares housing transactions in properties located near congressional district boundaries. Specifically, we focus on properties within 10, 5, or 3 km of a boundary, assigning each property to its nearest neighboring district. The procedure is explained in Appendix A. Properties located just inside a district whose representative serves on the FSC are classified as treated, while those just outside in an adjacent non-treated district serve as controls. This border-based approach compares changes in house prices across properties that are geographically proximate and thus likely share similar neighborhood characteristics and local economic fundamentals. To operationalize the border-based design, we construct unordered district pairs linking adjacent congressional districts and estimate specifications that interact all fixed effects with each *pair-id*.

We trim the distribution of sale prices at the top 1 percentiles to mitigate the influence of outliers. Our dependent variable is the natural logarithm of the sale amount. Table 3 reports summary statistics for key property characteristics at the property level—that is, one observation per property— comparing treated and control units within matched district pairs within 5 km. The sample is restricted to district pairs that contain at least one treated property, ensuring that control observations are drawn only from comparable local markets. Panel A summarizes treated properties—those located in districts represented by members of the

FSC—while Panel B presents statistics for control properties in adjacent non-FSC districts within the same boundary pair.

Across both groups, the distribution of observable housing attributes is remarkably similar. Average living area, lot size, and the number of bedrooms and bathrooms are nearly identical, and the age of properties differs by only about two years on average. These similarities suggest that treated and control properties are well balanced on observable characteristics, reinforcing the validity of our within-pair identification strategy. Combined with spatial proximity at the district border, this table supports interpreting subsequent differences in price appreciation as arising from variation in political representation rather than underlying housing quality or neighborhood composition.

Other data. To examine the mechanisms through which representation on the FSC affects local house prices, we combine several complementary datasets capturing variation in credit market conditions, housing supply programs, and federal resource allocation.

We first assemble detailed information on mortgage lending from multiple sources. The Home Mortgage Disclosure Act (HMDA) data provide detailed loan-level records of mortgage origination, including loan amount, type, purpose, lender, and borrower characteristics. We aggregate these to both the congressional district–year and lender–district–year levels. The district–year aggregation captures the total flow of mortgage credit and the composition of loans within each local market, regardless of lender identity, allowing us to measure how overall credit supply and loan type shares evolve following a district's entry into the FSC. In contrast, the lender–district–year panel preserves the lender dimension, enabling us to study how specific financial institutions adjust their lending across markets—whether political representation induces particular lenders to expand lending in treated districts, and through which funding channels (GSE or private). Together, they allow us to distinguish between aggregate market effects and within-lender reallocation of mortgage credit.

To capture changes in secondary-market activity, we use data from the Freddie Mac Single-Family Loan-Level Dataset (Freddie Mac), which reports every fixed-rate, one-to-four-unit mortgage purchased by Freddie Mac since the early 1990s. Each loan record includes the origination balance, borrower FICO score, loan-to-value (LTV) and debt-to-income (DTI) ratios, contract interest rate, and subsequent monthly performance outcomes such as delinquency and default status. We aggregate these observations to the congressional district–year level to track how the composition and performance of loans purchased by Freddie Mac evolve around FSC transitions.

We focus on two broad channels through which political representation may influence local mortgage market conditions: (1) changes in housing finance regulation and program design, and (2) differential treatment of legislator's district by private financial institutions through lending behavior. To assess the regulatory channel, we compile data from the Federal Housing Finance Agency (FHFA) on conforming loan limits for Fannie Mae and Freddie Mac, and from the Federal Housing Administration (FHA) on forward loan limits. These limits determine the maximum loan size eligible for federal backing and are periodically adjusted across metropolitan areas and counties. Variation in these limits over time and across locations allows us to test whether FSC representation is associated with more favorable adjustments to federal housing finance parameters, thereby relaxing local credit constraints.

To examine the broader credit effects of congressional representation, we also use data on small-business lending from the Community Reinvestment Act (CRA) Analytics Data Tables maintained by the Federal Reserve Board. These data provide an annual panel of bank-level small-business and small-farm loans reported under the CRA, aggregated at the bank–county level from 2005 to 2021. The dataset covers both origination and purchases of loans for firms with gross annual revenues below $1 million. It includes information on lending inside and outside each bank's CRA assessment areas.

We aggregate these data to the congressional district–year level to measure changes in small-business lending around House FSC appointments. This allows us to capture whether districts gaining representation experience broader credit expansion beyond mortgage markets. To evaluate the favoritism channel more directly, we complement the lending data with political contribution records from the *Database on Ideology, Money in Politics, and Elections* (DIME) (Bonica, 2024). DIME contains over 850 million itemized contributions to federal, state, and local elections from 1979 to 2024, based on Federal Election Commission filings and state reports. We use these data to examine whether HMDA lenders increase campaign contributions to legislators as they join the FSC, relative to non-lenders, providing suggestive evidence of preferential treatment by financial institutions around legislators gaining influence over financial regulation.

Next, we assess the supply-side responses of the housing market. We use project-level data from the LIHTC Database maintained by the U.S. Department of Housing and Urban

Development (HUD). The LIHTC program is the primary federal mechanism for supporting the construction and rehabilitation of affordable rental housing. The database contains detailed information on more than 54,000 projects (3.7 million units) placed in service between 1987 and 2023, including project location, number of units, credit type, construction type, and other financing sources. We link project locations to congressional districts and evaluate whether representation on the FSC affects the allocation of federally subsidized housing projects.

We complement these data with information from the Building Permits Survey (BPS) conducted by the U.S. Census Bureau, which provides comprehensive national and local statistics on new privately owned residential construction. These datasets are directly relevant because the House FSC exercises formal oversight over federal housing and community development programs, including the Department of Housing and Urban Development (HUD), the Federal Housing Administration (FHA), and the Federal Housing Finance Agency (FHFA). Moreover, the committee's stated priorities across nearly all Congresses emphasize "expanding access to affordable housing," "strengthening community development," and "reforming housing finance," underscoring its central role in shaping both public and private investment in the housing sector. As such, the LIHTC and BPS data capture two complementary channels—federally subsidized and market-based construction—through which congressional representation on the FSC may influence local housing supply.

Finally, we assemble data on federal procurement contracts and financial assistance awards from *USAspending.gov*, which provides transaction-level information on the value, timing, and geographic allocation of federal disbursements. To focus on programs most closely aligned with the Financial Services Committee's policy domain, we restrict both datasets to transactions issued by agencies with financial oversight functions—specifically the Department of the Treasury, Department of Housing and Urban Development, Securities and Exchange Commission, Consumer Financial Protection Bureau, Federal Housing Finance Agency, and National Credit Union Administration. Although the allocation of federal contracts or assistance fall outside the Committee's direct legislative jurisdiction, this restriction isolates federal spending most plausibly influenced by FSC membership, allowing us to test whether FSC membership translates into influence over the agencies it directly supervises.

4 Political Influence and Housing Returns

4.1 Two-Way Fixed Effects

We begin by presenting motivating evidence on whether congressional representation in financial policymaking relates to local housing returns. Our goal in this section is to quantify whether a district's entry into the FSC is reflected in local property values. Because we observe repeated transactions for the same properties over time, we estimate a TWFE model that compares changes in sale prices for the same home before and after its district gains representation on the FSC. This approach isolates within-property price changes and flexibly controls for statewide macroeconomic and policy shocks that vary over time.

Our baseline repeat-sales specification is given by:

$$\log(\text{SaleAmt})_{idt} = \alpha + \beta\ \cdot \text{CumTreat}_{it} + \gamma_i + \lambda_{s\times \text{ym}} + \varepsilon_{idt}, \tag{1}$$

where $\log(\text{SaleAmt})_{idt}$ is the log sale price of property i in district d at time t, and CumTreat_{it} measures the cumulative number of congressional terms during which the property's district has been represented on the FSC. Property fixed effects, γ_i, absorb all time-invariant housing characteristics, while state-by-year-month fixed effects, $\lambda_{s\times ym}$, flexibly controls for state-level time-varying macroeconomic and policy shocks. The coefficient β captures the average within-property change in house prices associated with FSC representation.

For our border-based design, we refine the TWFE identification in two ways. First, we restrict the sample to properties located within a fixed distance (10, 5, or 3 km) of a district boundary and assign each property to its nearest neighboring district pair, denoted by p. Second, we interact all fixed effects with the pair identifier, thus allowing us to flexibly control for differences in time-varying local shocks for each pair of districts. We estimate:

$$\log(\text{SaleAmt})_{idt} = \alpha + \beta\ \cdot \text{CumTreat}_{it} + \gamma_{ip} + \lambda_{s\times \text{ym}\times p} + \varepsilon_{idt}. \tag{2}$$

This specification compares changes in house prices for properties located just inside treated districts with those just outside in neighboring control districts, holding constant common local economic conditions and neighborhood amenities. The inclusion of property-by-pair-id and state-by-month-by-pair-id fixed effects ensures that identification relies exclusively on within-boundary-pair differences over time. Standard errors are clustered at the congressional district-year-month level to account for spatial correlation in unobserved shocks affecting properties in the same year-month.

Table 4 presents the estimated effects of congressional representation on the FSC on local housing prices. Across specifications, the coefficient on cumulative treatment, β, is positive and highly significant. In the full sample of U.S. housing transactions between 1990 and 2020, the estimated elasticity of transaction prices with respect to FSC representation is 0.0015, implying that average home values increase by roughly 0.15 percent with each additional congressional term of representation. The effect becomes stronger as we restrict the sample to properties closer to district boundaries—0.23 percent within 10 km, 0.38 percent within 5 km, and 0.49 percent within 3 km of a border.

To interpret the economic magnitude of this increase, we consider the average congressional tenure of an FSC representative. During our sample period, the average congressional district is represented on the FSC for approximately four consecutive Congresses. Therefore, our estimates imply cumulative housing price gains range between 0.6–2.0 percent relative to comparable neighboring districts, depending on proximity to the border. The evidence therefore that political influence in credit and housing policy—as proxied by membership in the FSC—is reflected in local house prices, consistent with homeowners capitalizing expected economic benefits of representation.

4.2 Stacked DiD

To address potential biases from staggered treatment timing and heterogeneous treatment effects (Baker et al., 2022; Roth et al., 2023), we estimate a stacked DiD design that isolates the effects of districts' *first* entry into and exit from the House FSC (Cohn, Liu, and Wardlaw, 2022; Gormley and Matsa, 2011). For each property, we define an event as the first Congress within our sample period in which the district it is located either gains or loses FSC representation. Properties located in districts whose representatives never serve in the FSC form the control group. We construct event windows around transitions assigning a pseudo event-year for each property in the control group. We estimate:

$$\log(\text{SaleAmt})_{idt} = \sum_{\ell=-L}^{L} \beta_\ell \cdot \mathbf{1}_{t=\tau_d+\ell} + \gamma_{i\times p\times c} + \lambda_{s\times ym(t)\times p\times c} + \varepsilon_{idt}, \tag{3}$$

where τ_d denotes the event year for district d. Property-by-pair-id-by-cohort fixed effects, $\gamma_{i\times p\times c}$, absorb time-invariant differences in housing and location quality and restrict comparisons to properties that remain within the same boundary pair during a given cohort. State-by-year-month-by-pair-id-by-cohort fixed effects, $\lambda_{s\times ym(t)\times p\times c}$, absorb local shocks common to both sides of a boundary pair and allow for distinct time paths across cohorts. Interacting both sets of fixed effects with cohort indicators ensures the our estimations avoid biases from overlapping treatment windows while allowing the effects of regional shocks to vary across different cohorts. As before, standard errors are clustered at the congressional district-year-month level to account for spatial correlation in unobserved shocks affecting properties in the same year-month but now interacted with cohort. The stacked estimator compares treated districts only to those that never receive treatment, ensuring that each treated observation contributes exactly once to a clean pre/post comparison.

Figure 3 plots the estimated event-time coefficients (semesters) for entry and exit in 2-year window for the border sample of five kilometers. House prices remain essentially flat in the years leading up to FSC transitions, supporting the validity of parallel pre-trends. Following committee *entry*, average property values rise by roughly 4 percent within two years. The pattern reverses almost symmetrically after *exit*, with prices declining by a similar magnitude. Consistent with partial anticipation of committee departures, housing prices begin to fall in the semester preceding the start of the next Congress, likely reflecting November elections when incumbents' future committee assignments plausibly become more predictable.

Table 5 reports stacked DiD estimates for FSC entry. Across distance bands (Panel A), price appreciation at entry is robust: approximately 2 percent within 10 km and 4 percent within 3 km of a treated boundary. Panel B reports stacked DiD estimates for FSC exits. House prices decline by 4–6 percent following the loss of committee representation, with the largest effects for properties proximate to boundaries of treated districts.

Table 6 reports several robustness exercises to the baseline stacked DiD specification. We vary the event-window length, sample restrictions, and fixed-effect structure to ensure that our results are not driven by specification choices or sample composition. In "Alt FE A," we retain property fixed effects without interacting them with district-pair identifiers, which permits broader within-district comparisons across properties in the same cohort. In "Alt FE B," we relax geographic constraints by removing the state component from the time fixed effects, allowing comparisons across adjacent states.

We further verify that our findings are not driven by the spatial composition of properties near district borders or by short-term transaction patterns. First, we exclude properties located near boundaries but distant from any developed area on the opposing side, ensuring that treated and control properties are drawn from comparable local markets. Second, we expand the event window to [-3,+3] and [-4,+4] years around FSC membership to address potential selection arising from combining a narrow event window with property fixed effects: it may over-represent transactions by flippers. Across all robustness checks, the estimated effects of FSC entry and exit remain stable and statistically significant.

Taken together, the entry and exit results demonstrate that representation on the FSC produces economically meaningful and reversible changes in local housing valuations, consistent with shifts in expected access to credit and housing finance for which we provide evidence in the next section.

5 Potential Mechanisms

We next examine the mechanisms through which representation on the FSC may affect local house prices. We group these mechanisms into two broad categories. The first operates through the flow and cost of mortgage credit. Committee members can ease local credit constraints by broadening mortgage access or lowering borrowing costs. Such effects can arise directly through policy influence over mortgage regulation or indirectly as lenders extend preferential terms to districts represented by members sitting on the FSC.

The second category encompasses channels outside direct mortgage origination but still within the Committee's oversight of financial and housing-related agencies. Representation may influence the allocation of federal resources administered by agencies such as the HUD, the FHFA, or the Treasury, including housing development programs, LIHTC allocations, or financial assistance to small businesses. These channels, though not strictly credit-market mechanisms, remain under the FSC's policy domain and can affect local housing demand and prices through fiscal and supply-side responses.

To examine these mechanisms, we estimate a stacked DiD specification that compares changes in outcomes around the first time a district gains FSC representation to those in districts never represented by an FSC member. In contrast to the property-level analysis of house prices, the treatment here is defined at the congressional district level. Specifically, we estimate

$$Y_{dt} = \beta \cdot \mathbf{1}\{\text{Post}_{dt} \times \text{Treated}_d\} + \gamma_{d\times c} + \lambda_{s\times t\times c} + \varepsilon_{dt}, \quad (4)$$

where Post_{dt} is equal to one for years including and after a district's first entry (exit) into (from) the FSC, c denotes the cohort of first entry (exit), and s denotes the state. The coefficient β captures the average post-entry (post-exit) change in the outcome relative to the pre-entry (pre-exit) year. The district-by-cohort fixed effects $\gamma_{d\times c}$ absorb time-invariant heterogeneity across districts within each entry (exit) cohort, while $\lambda_{s(d)\times t\times c}$ control for state-by-year shocks common to districts in the same cohort. Standard errors are clustered at the state-by-year-by-cohort level.

We consider several outcomes and consider appropriate estimation that vary across them. For outcome variables measured in monetary amounts, such as the total value of originated loans, the dependent variable in (4) is defined in logs, and we estimate an Ordinary Least Squares (OLS) specification. For outcome variables measured in levels, such as borrower FICO scores or loan-to-value (LTV) ratios, we estimate the specification in levels using OLS. For count outcomes, such as the number of originated loans, we modify (4) and estimate the model using a Poisson pseudo–maximum likelihood estimator, with the estimated coefficients interpreted as percentage changes.

To explore the mechanisms in greater detail, we also estimate analogous specifications using loan-level data from the HMDA, aggregated to the lender–district–year level. The granularity of HMDA data is that it allows us to separate extensive and intensive margins of credit supply, examine heterogeneity across lending institutions, and identify the specific channels through which representation may affect mortgage-market activity. In these tests, we modify (4) by interacting the district-by-cohort fixed effects by the identifier of the lender ultimate parent.

5.1 Credit Supply through GSEs

Mortgage origination volumes and GSE purchases. Table 7 presents district-level estimates from the HMDA data around the first entry (panel A) and exit (panel B) of a representative into the FSC. Following FSC entry, the total value of originated mortgages increases modestly (3.8 percent). More strikingly, the volume of loans subsequently purchased by the GSEs increases by about 5.3 percent. Both the estimates are statistically significant.[2] By contrast, loan originations by the FHA, Banks and Credit Unions (BCU), and Private institutions, are all statistically indistinguishable from zero. Finally, Figure B.3a examines changes in GSE

[2]On the extensive margin, unreported results show that the number of GSE-purchased loans increases by approximately 5.5 percent, while their share of total originations rises by about 1.5 percentage points.

purchases in event-time around FSC transitions, and shows that the *average* amount for GSE loans increases and remains higher in the years following the entry.

The fact that GSE-purchased lending rises more strongly than total origination, suggests a compositional shift in mortgage flows rather than a broad expansion of credit. Because Fannie Mae and Freddie Mac commit to purchase conforming loans meeting specific criteria, lenders originate such loans in the primary market with the expectation of resale into the GSE pipeline. The increase in GSE purchases following FSC entry, therefore, reflects greater alignment of local lending with GSE purchase programs, consistent with improved access to or anticipation of secondary-market liquidity tied to political representation.

When representatives exit the committee, these patterns reverse. Both total and GSE-purchased origination amounts fall significantly, by –8.4 and –7.4 percent, respectively. Moreover, unlike entry, we do see a decline across other institutions. The symmetric entry and exit effects reinforce the interpretation that local GSE activity responds contemporaneously to changes in political representation, reflecting preferential access to secondary-market liquidity rather than shifts in aggregate demand for mortgages.

To probe the mechanisms further, we exploit the granularity of the HMDA data and estimate analogous specifications at the lender–district–year level (Table B.1). Consistent with the district-level results, when a district's representative joins the FSC, we observe increases in total lending volume purchased by the GSEs and, to a lesser extent, the FHA. In contrast, we find no corresponding changes for loans financed through private-label securitization. We see symmetric effect on exits, with economically larger contractions in lending.

Importantly, the specification includes a rich set of fixed effects—district × cohort and state × year × cohort interactions with lender fixed effects, which absorb both cross-sectional differences in lender composition and time-varying shocks common to particular lender types or geographies. The specification isolates within-lender variation across treated and control districts within the same state and year, strengthening the interpretation that lenders adjust behavior in response to changes in their district's political influence rather than to local demand conditions.

Overall, the evidence indicates a supply-side response to increased congressional influence over housing finance. Committee membership appears to relax frictions or improve access to GSE funding for lenders in the member's district, increasing the volume of origination ultimately purchased by the GSEs.

Securitization intensity and borrower composition. We next examine secondary-market data from the Freddie Mac single-family loan-level dataset, which records loans purchased and securitized by Freddie Mac. We focus only on mortgages purchased for single-family properties. Table 8 shows that after FSC entry, the total balance of loans purchased increases by about 10.5 percent, and average debt-to-income ratio increases by 0.2, consistent with a local relaxation of credit constraints or expanded loan eligibility. Given that the average district accounts for less than 0.5 percent of Freddie Mac's annual national purchases, a 10 percent increase reflects a meaningful local reallocation of GSE financing towards congressional district that saw first time entry into the FSC by a representative—large enough to affect mortgage liquidity and pricing in the local market, but not of a scale that would alter credit at the national-level.

Importantly, from the table, we do not see a change in borrower characteristics around the entry. Average FICO scores and LTV ratios show no economically or statistically meaningful shifts. This absence of change in observable risk measures suggests that GSE expansion does not come from underwriting riskier borrowers but rather from a greater rate in securitization of loans.

When representation ends, in Panel B, we do not see concomitant changes in loan volumes and amounts. Coefficients on these outcomes are economically zero and statistically indistinguishable from zero. However, we do see a economically small reductions in LTV (–0.3 percentage points) and an increase in the FICO score (about 2 points), consistent with a reversion toward more conservative financing once the district loses political influence.

We next test whether the increase in GSE purchases associated with FSC representation is followed by a weaker loan performance. Table 9 reports results for a battery of delinquency and default outcomes. Following committee entry, loans originated in treated districts show no deterioration in repayment behavior. If anything, short-term delinquency rates decline slightly and cross all other measures, including 30+, 60+, 90+, and 180+ day delinquencies and ultimate defaults. The effects are economically small and statistically indistinguishable from zero. Around exits, similar to entry, we do not see any systematic improvement or deterioration.

The symmetry across entry and exit, combined with the unchanged borrower characteristics documented above, implies that the additional loans purchased during periods of committee representation are not riskier. The marginal borrower financed through this politically-influenced credit expansion resembles the average borrower in both observable and realized credit risk.

GSE eligibility: Conforming loan limits. Another possible mechanism may operate through the regulation of mortgage eligibility, particularly the determination of conforming loan limits that govern access to GSE and FHA support. The political economy of these limits has repeatedly reflected the influence of committee members from high-cost districts. During the 2008 negotiations over the *Housing and Economic Recovery Act*, members of the House FSC—most prominently Representative Gary Miller of California—argued that their constituents were unfairly excluded from federally supported mortgages under the existing caps. Their efforts succeeded: the limit for Fannie Mae, Freddie Mac, and FHA loans was raised to $729,750 and proportionally for FHA programs, covering precisely the most expensive housing markets in their home states.[3] A similar episode unfolded in 2013, when the Federal Housing Finance Agency (FHFA) considered lowering the conforming ceiling. Representative Carolyn Maloney of New York led a bipartisan coalition opposing the change. The FHFA ultimately retained the existing caps and expanded the set of "high-cost" counties.[4] These episodes illustrate how committee membership can translate into district-level regulatory benefits, raising the question of whether such influence leaves measurable traces in the data.

Table 10 reports stacked DiD estimates of the effect of FSC representation on district-level conforming loan limits for both GSE and FHA programs. The coefficient on the interaction term, Treated×Post, captures the average within-district change in limits following a representative's first committee appointment relative to control districts.

In Panel A, conforming loan limits rise sharply following FSC entry. The GSE limit increases by roughly $7,250 and the FHA limit by $6,600 (although this latter is not statistically significant) relative to control districts. Symmetrically, in Panel B we show that at exit, limits sharply decrease by roughly $4,400 for the GSE and $16,200 for the FHA (both statistically and economically significant). The effects are economically big: a $10,000 increase represents a 1.5–2 percent rise in the national conforming cap, implying a 1–2 percent increase in effective purchasing power for borrowers near the binding threshold (given typical loan-to-value ratios around 80 percent). Moreover, these baseline estimates are evaluated for single-family (1-unit) properties. The FHFA also sets these limits for 2-, 3-, and 4-unit properties, using (almost) fixed multipliers, with 4-unit properties have limits approximately twice as large as those for

[3] U.S. House Committee on Financial Services, "Raising the Conforming Loan Limit," hearing transcript, May 22, 2008.

[4] FHFA Press Release, "FHFA Announces 2014 Conforming Loan Limits," November 26, 2013; Congressional Letter from Rep. Carolyn Maloney et al. to FHFA Acting Director Edward DeMarco, November 20, 2013.

1-unit properties.

These estimates provide direct evidence that committee representation affects the regulations governing credit access for constituents. Combined with the 10 percent increase in GSE purchases documented above, the results imply a politically mediated credit channel: higher conforming limits expand the set of loans eligible for GSE purchase, while greater securitization deepens secondary-market liquidity. Together, these mechanisms amplify mortgage availability and housing demand in politically connected districts without compromising credit quality, manifesting in higher localized house prices.

Small-business lending. We next examine whether congressional representation on the FSC affects the broader supply of credit beyond mortgage markets. Using CRA reporting data, we estimate district-level changes in the total value of small-business loans around FSC appointments. The results, shown in Table 11, indicate that small-business lending rises significantly following FSC entry and falls after exit. Specifically, loan volumes increase by about 6 percent when a representative joins the Committee and decline by roughly 10 percent when the member leaves. Both effects are statistically significant and consistent with broader credit expansion in politically connected districts. The fact that we observe sizable and opposite responses at entry and exit points to a common underlying mechanism—political influence over credit supply—and is harder to reconcile with shifts in local credit demand or business conditions.

Political contributions. To shed light on whether the increase in mortgage credit reflects political favoritism toward local lenders, we examine campaign contributions from financial institutions. Specifically, we compare changes in contributions from HMDA-linked corporate entities relative to non-HMDA ones around a district's first FSC entry or exit, estimating a triple-difference specification that interacts the stacked DiD treatment with an HMDA affiliation indicator. The results, reported in Table 12, indicate that contributions from financial institutions rise significantly following FSC entry and decline after exit. In particular, the triple-difference estimates show that contributions from HMDA-linked corporations increase by roughly 37 percent relative to non-HMDA entities after a representative joins the Committee, whereas they fall by about 32 percent when the member leaves. These effects are economically large and statistically significant, consistent with the view that financial-sector donors strategically adjust their political giving in response to changes in congressional influence over financial regulation. The symmetry of the entry and exit effects further supports an interpretation based on changes in political influence rather than ideological alignment.

5.2 Other Mechanisms

Finally, we examine potential channels outside of credit markets, including new construction activity, and affordable housing programs and federal procurement.

Construction activity. Table 13 reports stacked DiD estimates for building permits at the district–year level. Permit value, number of buildings, and number of units all decline by about 5 percent around both FSC entry and exit. These results indicate that representation on the FSC does not increase short-run construction activity. The inelastic response of permitting reinforces our main finding that changes in house prices following FSC entry and exit cannot be attributed to shifts in new construction.

Affordable housing. Table 14 reports stacked DiD estimates for the allocation of Low-Income Housing Tax Credit (LIHTC) projects at the district–year level. Around FSC entry, point estimates for total allocation amounts and units are small and statistically insignificant. Around exits, we find moderate increases of 15 to 32 percent in total and low-income units. Although the exit effects are statistically significant, they are not large enough to explain a district-level aggregate effect. The LIHTC program adds roughly 100,000 units nationally per year—equivalent to about 200 units per congressional district—so the magnitudes involved are too small to significantly influence house prices within the two-year event window (HUD, 2023). Furthermore, the similar direction of coefficients across entry and exit does not align with the opposite-signed house price responses observed at those events. Taken together, the results provide evidence against the hypothesis that changes in LIHTC allocations play a major role in explaining the relationship between FSC representation and local housing prices.

Federal contracts and assistance. Table 15 presents stacked DiD estimates examining whether FSC representation is associated with changes in federal resource flows—specifically through procurement contracts (FPDS) and financial assistance awards (FABS).

In Panel A (Entry), the coefficient on *Treated* × *Post* is positive for both outcomes, but not statistically significant. Panel B (Exit) shows the opposite pattern for contracts, a statistically significant reduction of 33 percentage, and small non-significant coefficient for assistance. These patterns suggest that new FSC members may be better positioned to channel procurement activity—rather than grants or aid—toward their districts upon entry. The magnitudes of the effects reinforce the interpretation that FSC representation primarily affects procurement relationships rather than broader assistance programs.

Taken together, the evidence in Table 15 provides little indication that FSC representation materially alters the flow of federal resources to represented districts. The estimated effects on both contracts and assistance are small in magnitude and statistically insignificant in most specifications. If anything, the modest and imprecise positive estimates at entry and the mild declines at exit suggest, at best, transitory or economically negligible reallocations. Overall, there is no systematic evidence that FSC membership affects the distribution of federal contracts or financial assistance.

Overall, across non-credit mechanisms, we find no evidence of a contemporaneous construction surge, only limited and programmatic movements in LIHTC flows, and moderate increases in school revenues. These patterns are internally consistent with a short-run, politically mediated credit-supply channel: representation raises conforming caps and intensifies GSE purchases without deteriorating borrower quality, and the resulting increase in purchasing capacity capitalizes into house prices when supply is inelastic.

5.3 Discussion on Electoral Incentives

A natural question is whether the house price effects we document operate through electoral incentives that influence credit allocation. Table 16 examines how representation on the FSC affects incumbents' re-election probabilities. In column (1), the coefficient on the treated indicator is small and statistically insignificant, indicating that gaining FSC representation is not mechanically correlated with higher incumbents' re-election chances. In column (2), we interact treatment with the residualized change in house prices prior to each election and find a positive and statistically significant interaction term, suggesting that incumbents benefit electorally when house prices appreciate more strongly within their districts. Column (3) shows that this relationship is concentrated in the top quartile of the house price distribution, with no detectable effects elsewhere. Overall, these results indicate that representation on the FSC may amplify the electoral benefits associated with local house price growth. Our findings complement those of McCartney (2021), who show that house price declines reduce voter participation. Taken together, the evidence points to a potential electoral incentive underlying the link between political representation, mortgage credit allocation and local house price growth.

6 Conclusion

Our findings show that political representation on the House FSC has sizable effects on local housing markets. Exploiting the staggered timing of FSC membership and a border design, we find that house prices in newly represented districts rise by about 4 percent and mortgage volumes increase by roughly 7 percent. These effects are driven by greater securitization and greater purchases by government-sponsored enterprises, rather than by changes in construction activity or other local fundamentals. Congressional influence over credit allocation is thus directly capitalized into housing values, translating political power into short-run gains for homeowners.

Because housing is the primary asset for most households, our results reveal a previously overlooked channel through which political power redistributes wealth across communities. More broadly, the evidence shows that political representation—independent of macroeconomic or local fundamentals—can shape local housing markets and influence the distribution of wealth.

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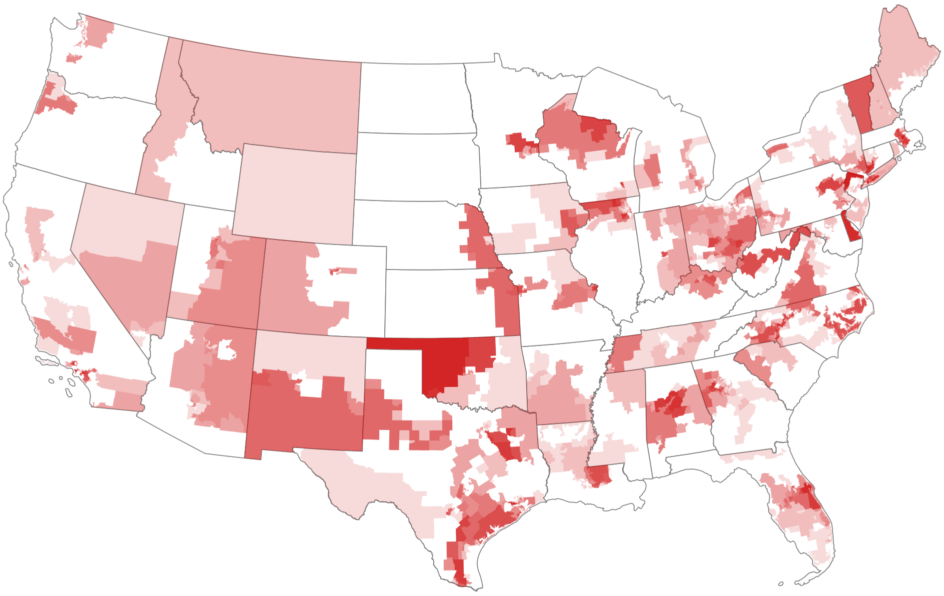


Figure 1: Cumulative exposure of U.S. congressional districts to the House Financial Services Committee, Congresses 103–116. *Notes*: The map shows all contiguous U.S. districts that were represented by a legislator serving on the Financial Services Committee in any Congress between 1993 and 2020. Each treated district is shaded in translucent red; deeper shades indicate districts treated in multiple Congresses. State borders are drawn from the 116th Congress shapefile, with Alaska and Hawaii excluded for scale consistency. An equal-area projection (EPSG:2163) is used to preserve relative area across the contiguous states.

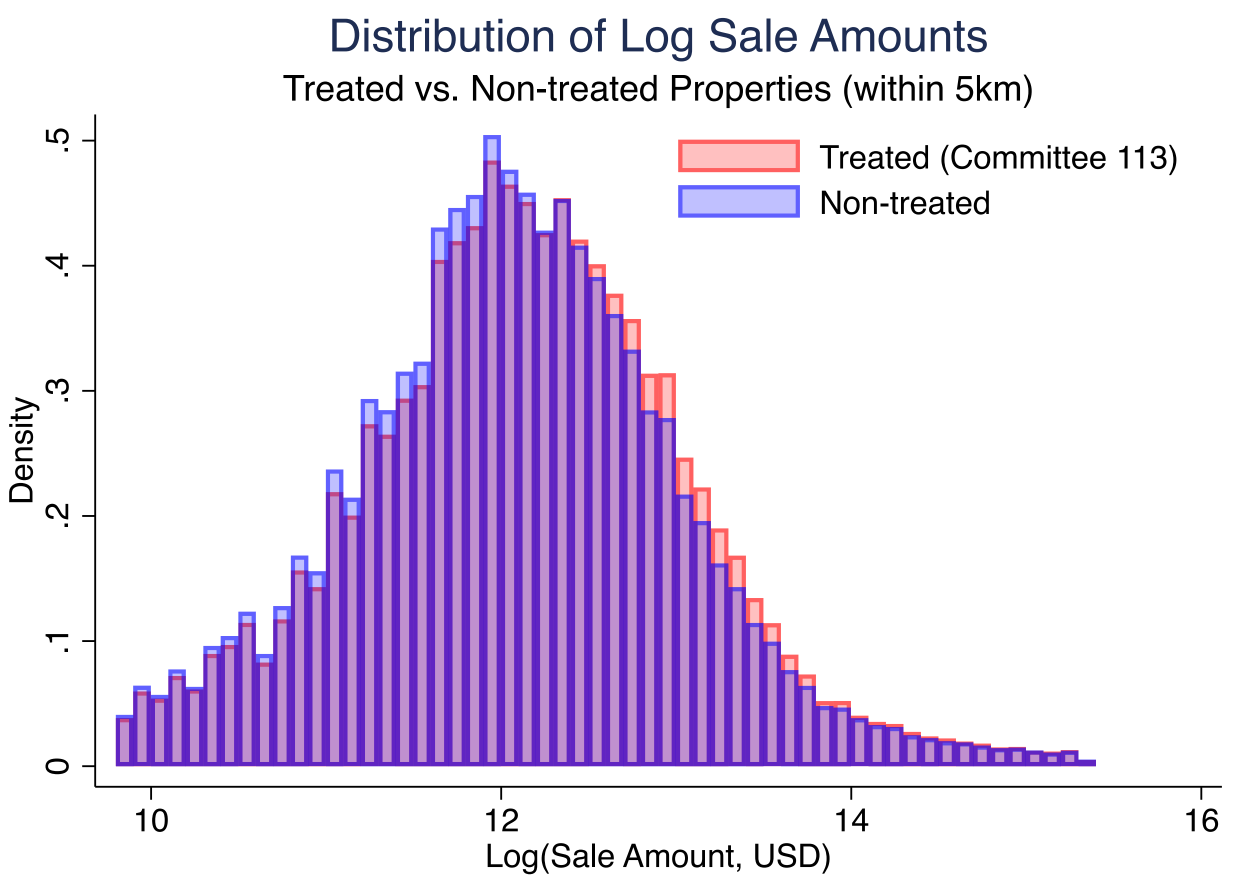
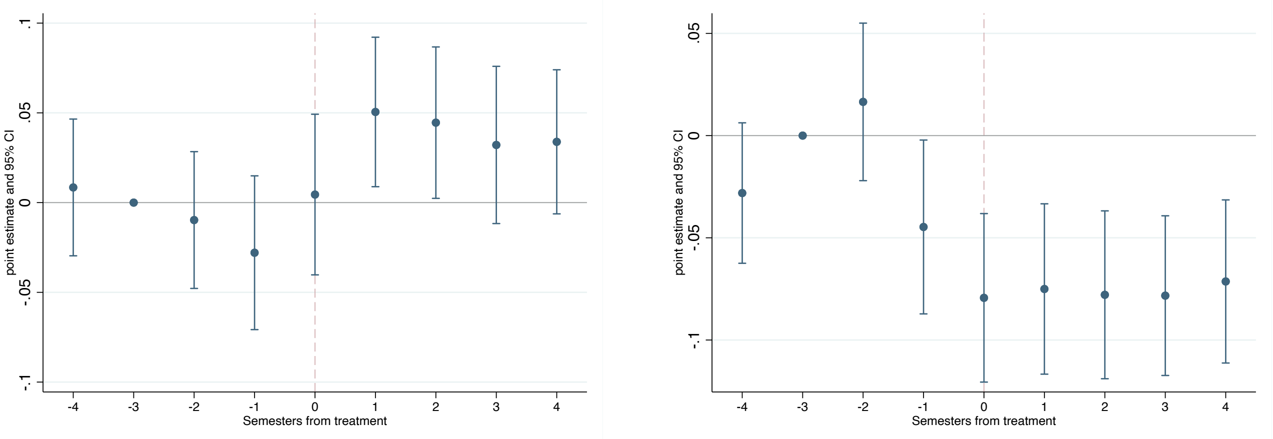


Figure 2: *Notes:* This figure plots the distribution of log-transformed sale prices for properties located within 5 kilometers of a congressional district border. The figure distinguishes between properties in congressional districts associated with members in the House Financial Services Committee (FSC) (in red) and properties in congressional districts associated with legislators who never sat on the FSC (in blue).



(a) Entry into the House FSC

(b) Exit from the House FSC

Figure 3: *Notes*: The figure reports coefficient estimates and corresponding 95% confidence intervals from the stacked DiD specification in Equation (3). The sample is restricted for properties within 5 km of congressional district boundaries. Panel (a) presents the response of house prices around entry into the House Financial Services Committee (FSC) while Panel (b) presents the response around exit from the FSC, in a 2-year window around these events. The specifications include Property-by-pair-id-by-cohort and State-by-year-month-by-pair-id-by-cohort fixed effects. The standard errors are clustered at the congressional district-year-month level and interacted with cohort, to account for spatial correlation in unobserved shocks affecting properties in the same year-month within each cohort.

Table 1: Summary Statistics, Legislators

Notes: The table presents the descriptive statistics for members of the House Financial Services Committee (FSC) (panel A) and those not in FSC committee (panel B) between congresses 103 and 116. For each Congress, we include the following characteristics: *Democrat* an indicator variable to identify party affiliation as the Democratic party, *Female* an indicator variable to identify female legislators, *Minority* to capture whether the legislator is African-American or Hispanic, *DW-NOMINATE 1* and *DW-NOMINATE 2* capture legislator ideologies, *Vote* is the percent of votes received in an election, *Rank in party* is the rank within the party, *New Entrant* is an indicator to capture whether the legislator is an entrant or incumbent in within the Congress, *Seniority* is the seniority of the legislator, *Majority* captures whether the legislator is a member of the party in control of the Senate, *Legislative Effectiveness Score* is the lawmaking effectiveness of the legislator. Data are from the Center for Effective Lawmaking: see Volden and Wiseman (2014, 2018) for further details.

	N	Mean	StDev	P1	P25	P50	P75	P99
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: In FSC								
Democrat (1=Yes)	903	0.5	0.5	0.0	0.0	0.0	1.0	1.0
Female (1=Yes)	903	0.2	0.4	0.0	0.0	0.0	0.0	1.0
Minority (1=Yes)	903	0.2	0.4	0.0	0.0	0.0	0.0	1.0
Vote (%)	883	66	14	47	55	63	72	100
DW-NOMINATE 1	903	0.0	0.4	-0.7	-0.4	0.1	0.4	0.8
DW-NOMINATE 2	903	-0.1	0.3	-0.9	-0.3	-0.1	0.1	0.7
Rank in party	903	112	63	3	63	111	163	242
New Entrant	903	0.2	0.4	0.0	0.0	0.0	0.0	1.0
Seniority	903	4	3	1	2	3	6	14
Legislative Effectiveness Score (LES)	903	1	1	0	0	1	1	6
Panel B: Not in FSC								
Democrat (1=Yes)	6,012	0.5	0.5	0.0	0.0	0.0	1.0	1.0
Female (1=Yes)	6,012	0.2	0.4	0.0	0.0	0.0	0.0	1.0
Minority (1=Yes)	6,012	0.2	0.4	0.0	0.0	0.0	0.0	1.0
Vote (%)	5,896	67	13	48	58	65	73	100
DW-NOMINATE 1	6,012	0.0	0.4	-0.7	-0.4	0.1	0.4	0.7
DW-NOMINATE 2	6,012	0.0	0.3	-0.7	-0.2	-0.0	0.2	0.8
Rank in party	6,012	114	66	3	57	113	169	243
New Entrant	6,012	0.2	0.4	0.0	0.0	0.0	0.0	1.0
Seniority	6,012	5	4	1	2	4	8	18
Legislative Effectiveness Score (LES)	6,012	1	1	0	0	1	1	7

Table 2: Summary Statistics, Congressional Districts

Notes: The table presents descriptive statistics for key characteristics of U.S. congressional districts over the period 2005–2021. Panel A reports statistics for treated districts, identified as those whose representative sits in the House Financial Services Committee (FSC), while Panel B reports statistics for control districts. For each district, we include the following characteristics: *Population* measures total district population; *Median Age* captures the median age of residents; *Share Under 18* denotes the percentage of residents under age 18; *Bachelor's Degree or Higher* represents the share of adults aged 25 or older with at least a bachelor's degree; *Median Household Income* and *Per Capita Income* are reported in 2021 U.S. dollars; *Unemployment Rate* and *Labor Force Participation* measure district-level labor market outcomes; *Homeownership Rate* captures the percentage of households that own their home; and *Poverty Rate* indicates the share of individuals living below the federal poverty line. For each variable, the table reports the number of observations (N), mean, standard deviation (SD), and selected percentiles (P1, P25, P50, P75, P99). Data are derived from the U.S. Census Bureau's American Community Survey (ACS) 1-Year Estimates.

Variable	N	Mean	SD	P1	P25	P50	P75	P99
Panel A: Treated Districts								
Population	679	736,559	51,253	621,341	709,129	734,532	764,944	910,031
Median Age	175	38.3	3.5	31.1	36.1	37.9	40.6	48.4
Share Under 18 (%)	175	22.4	2.9	12.2	21.2	22.2	23.8	30.6
Bachelor's Degree or Higher (%)	114	35.2	12.0	12.4	27.8	32.8	42.4	70.4
Median Household Income ($)	484	62,754	18,137	35,333	49,523	57,596	74,064	112,224
Unemployment Rate (%)	356	5.5	2.0	2.8	4.2	5.0	6.2	12.0
Labor Force Participation (%)	484	64.3	4.6	53.8	61.1	64.3	67.5	73.3
Per Capita Income ($)	679	31,303	10,903	16,727	24,088	29,146	35,710	78,815
Homeownership Rate (%)	356	63.4	13.4	23.2	57.3	67.1	72.1	82.1
Poverty Rate (%)	679	10.7	5.2	3.1	6.8	9.9	13.4	24.7
Panel B: Control Districts								
Population	4,092	735,554	59,591	578,799	707,642	729,968	762,509	929,144
Median Age	1,128	38.9	3.6	31.0	36.7	38.9	40.8	48.6
Share Under 18 (%)	1,128	22.2	2.7	15.7	20.4	22.2	23.7	29.8
Bachelor's Degree or Higher (%)	754	33.4	10.9	14.2	25.3	31.7	40.5	64.1
Median Household Income ($)	2,994	61,867	17,841	34,226	49,548	58,427	70,742	118,989
Unemployment Rate (%)	2,252	5.6	1.8	2.8	4.3	5.3	6.5	11.6
Labor Force Participation (%)	2,994	63.0	5.0	47.8	60.2	63.5	66.3	73.3
Per Capita Income ($)	4,092	30,604	9,108	15,938	24,418	28,818	34,872	60,964
Homeownership Rate (%)	2,252	63.9	10.7	29.3	59.6	66.1	71.2	79.7
Poverty Rate (%)	4,092	10.6	4.9	3.4	7.2	9.6	12.9	26.4

Table 3: Summary Statistics, Property Level: Treated vs. Control

Notes: The table presents descriptive statistics computed at the property level, where each observation corresponds to a unique residential property. The sample is restricted to matched pairs at 5 km distance from a congressional district border; the control panel includes only properties belonging to those treated pairs. Panel A reports statistics for treated properties, while Panel B reports statistics for control properties. Variables include key structural and physical characteristics obtained from the 2020 property characteristics file. Specifically, *Living area* measures the interior finished square footage of the property; *Land area* is the total lot size in square feet; *Bedrooms* and *Bathrooms* represent the number of rooms designated for sleeping and bathrooms, respectively; and *Age* denotes the number of years since the property's construction year. For each variable, the table reports the number of observations (N), mean, standard deviation (StDev), and selected percentiles (P1, P25, P50, P75, P99). Data are from CoreLogic Deeds and Tax Assessment records.

	N	Mean	StDev	P1	P25	P50	P75	P99
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Treated properties								
Living area (Sq.ft)	7,834,086	1,971	1,137	700	1,254	1,680	2,334	6,515
Land area (Sq.ft)	8,382,510	27,158	71,061	871	5,600	8,712	16,444	418,612
Bedrooms (count)	5,879,056	3.2	1.0	1.0	3.0	3.0	4.0	6.0
Bathrooms (count)	6,871,559	2.4	1.0	1.0	2.0	2.0	3.0	5.0
Age (years)	7,671,508	34.8	29.4	0.0	8.8	29.0	53.1	110.7
Panel B: Control properties								
Living area (Sq.ft)	18,877,586	1,983	1,129	703	1,262	1,692	2,360	6,429
Land area (Sq.ft)	20,630,185	29,245	76,006	896	5,580	8,655	17,351	435,600
Bedrooms (count)	13,652,109	3.2	1.0	1.0	3.0	3.0	4.0	6.0
Bathrooms (count)	16,653,646	2.3	1.0	1.0	2.0	2.0	3.0	5.0
Age (years)	18,192,124	33.1	29.1	0.0	7.3	27.0	52.0	110.0

Table 4: Two-way Fixed Effects: House Financial Services Committee (FSC) Representation and House Prices

Notes: The table reports two-way fixed-effects (TWFE) estimates of the effect of representation on the House Financial Services Committee (FSC) on log house prices. The dependent variable is $\log(\text{SaleAmt})_{idt}$, the natural logarithm of the transaction sale amount for property i in congressional district d at time t. The key independent variable, *CumTreat*, measures the cumulative number of congressional terms during which the property's district has been represented on the FSC). All regressions include property or property-by-pair fixed effects (γ_i or γ_{ip}) and either state-by-year-month or year-month-by-pair fixed effects ($\lambda_{s\times ym}$ or $\lambda_{s\times ym\times p}$), as indicated in the table. The coefficient on *CumTreat* captures within-property changes in sale prices following the districts' representative's entry into the FSC. Column (1) includes all districts while Columns (2)–(4) correspond to border-based samples restricted to properties located within 10, 5, or 3 kilometers of a congressional district boundary, respectively. Standard errors are clustered at the congressional district–year–month level to account for spatial correlation in unobserved shocks. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	All Districts	Within 10 km	Within 5 km	Within 3 km
	(1)	(2)	(3)	(4)
CumTreat	0.00153***	0.00227***	0.00378***	0.00486***
	(0.00035)	(0.00064)	(0.00065)	(0.00068)
R^2	0.705	0.722	0.727	0.730
Observations	123,780,307	66,957,912	48,527,391	34,924,042
Fixed Effects:				
Property	Yes			
Property × Pair		Yes	Yes	Yes
State × Year–Month	Yes			
Year–Month × Pair		Yes	Yes	Yes

Table 5: Stacked Difference-in-Differences: FSC Representation and Effects on Housing Prices

Notes: The table reports stacked difference-in-differences (DiD) estimates of the effect of congressional representation on the House Financial Services Committee (FSC) on housing prices. Panel A presents estimates for districts' representative's *first entry* into the FSC, while Panel B reports estimates for *first exit*. Each column corresponds to a separate stacked DiD specification that isolates transitions in FSC representation by constructing event windows around the first Congress in which a district's representative gains or loses a committee seat. Properties located in districts whose representatives never serve on the FSC form the control group. The dependent variable is $\log(\text{SaleAmt})_{idt}$, the natural logarithm of property sale prices. All regressions include property-by-pair-by-cohort fixed effects ($\gamma_{i\times p\times c}$) and state-by-year-month-by-pair-by-cohort fixed effects ($\lambda_{s\times ym\times p\times c}$), which flexibly control for differences in housing characteristics, local shocks, and regional trends within cohort district-pairs. Columns (1)–(3) correspond to border-based samples restricted to properties located within 10, 5, or 3 kilometers of a congressional district boundary, respectively. The coefficient on *Treated*$\times$*Post* captures the average post-entry (or post-exit) change in house prices relative to before around FSC transitions. Standard errors are clustered at the congressional district–year–month-by-cohort level to account for spatial correlation in unobserved shocks. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A. Entry

	10 km	5 km	3 km
	(1)	(2)	(3)
Treated × Post	0.023**	0.047***	0.036**
	(0.011)	(0.012)	(0.014)
R^2	0.702	0.703	0.705
Observations	35,784,569	25,286,322	17,950,381
Fixed Effects			
Property × pair-id × cohort	Yes	Yes	Yes
State × year × month × pair-id × cohort	Yes	Yes	Yes

Panel B. Exit

	10 km	5 km	3 km
	(1)	(2)	(3)
Treated × Post	-0.051***	-0.056***	-0.046***
	(0.010)	(0.011)	(0.013)
R^2	0.699	0.700	0.702
Observations	34,825,755	24,583,106	17,457,055
Fixed Effects			
Property × pair-id × cohort	Yes	Yes	Yes
State × year × month × pair-id × cohort	Yes	Yes	Yes

Table 6: Robustness, Stacked DiD: FSC Representation and Housing Prices

Notes: The table reports stacked difference-in-differences (DiD) robustness checks examining the effect of congressional representation on the House Financial Services Committee (FSC) on housing prices. Panel A reports robustness tests for districts' *first entry* into the FSC, and Panel B reports analogous checks for *first exit*, both using the 5 km border sample. Each column corresponds to a variant of the stacked DiD specification with alternative event windows or fixed-effect structures. Columns (1)–(2) restrict the event window to three or four years before and after the FSC transition; Column (3) limits the sample to properties in districts represented by incumbents with at least four consecutive years of service; Column (4) reweights properties by proximity to the district boundary; and Columns (5)–(6) report results under alternative fixed-effect specifications. "Alt FE A" excludes property × pair-id × cohort fixed effects, while "Alt FE B" excludes state × year × month × pair-id × cohort fixed effects. Standard errors are clustered at the congressional district–year–month-by-cohort level to account for spatial correlation in unobserved shocks. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A. Entry, Robustness Checks (5 km Sample)

	Window $[-3, +3]$	Window $[-4, +4]$	4y incumbents	Prop. dist.	5km (Alt FE A)	5km (Alt FE B)
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	0.049***	0.040***	0.038***	0.046***	0.050***	0.039***
	(0.010)	(0.008)	(0.014)	(0.012)	(0.012)	(0.013)
R^2	0.692	0.686	0.704	0.703	0.703	0.703
Observations	41,359,511	58,296,146	24,535,337	25,036,259	26,283,912	25,297,724
Fixed Effects						
Property × pair-id × cohort	Yes	Yes	Yes	Yes	No	Yes
State × year × month × pair-id × cohort	Yes	Yes	Yes	Yes	Yes	No
Property × cohort	No	No	No	No	Yes	No
Year × month × pair-id × cohort	No	No	No	No	No	Yes

Panel B. Exit, Robustness Checks (5 km Sample)

	Window $[-3, +3]$	Window $[-4, +4]$	4y incumbents	Prop. dist.	5km (Alt FE A)	5km (Alt FE B)
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	-0.039***	-0.027***	-0.085***	-0.057***	-0.054***	-0.060***
	(0.009)	(0.007)	(0.014)	(0.011)	(0.011)	(0.011)
R^2	0.688	0.686	0.700	0.700	0.700	0.700
Observations	40,081,321	56,176,406	23,596,526	24,339,572	25,490,531	24,594,859
Fixed Effects						
Property × pair-id × cohort	Yes	Yes	Yes	Yes	No	Yes
State × year × month × pair-id × cohort	Yes	Yes	Yes	Yes	Yes	No
Property × cohort	No	No	No	No	Yes	No
Year × month × pair-id × cohort	No	No	No	No	No	Yes

Table 7: Stacked DiD: Effects on HMDA Originations Around FSC Transitions

Notes: The table reports stacked DiD estimates of the effects of congressional representation on the House Financial Services Committee (FSC) on mortgage-market outcomes, using data disclosed under Home Mortgage Disclosure Act (HMDA) aggregated to the congressional district level. Panel A presents estimates around districts' representative's *first entry* into the FSC, and Panel B reports estimates around *first exit*. The specification follows Equation (4), where the dependent variable Y_{dt} is the natural logarithm of the district-level total origination value and broken down by Government Sponsored Enterprise (GSE), Federal Housing Administration (FHA), Banks and Credit Union (BCU), and Private financial institutions. All regressions include district-by-cohort fixed effects ($\gamma_{d\times c}$) and state-by-year-by-cohort fixed effects ($\lambda_{s\times t\times c}$), which flexibly absorb time-invariant district heterogeneity and state-level shocks common to districts within each cohort. The coefficient on *Treated*×*Post* captures the average post-entry (or post-exit) change in the specified outcome relative to the pre-entry (or pre-exit) year. Standard errors are clustered at the state–year–cohort level to account for correlated shocks within states over time. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	Total	GSE	FHA	BCU	Private
Treated × Post	0.038**	0.053**	0.007	0.029	0.023
	(0.019)	(0.021)	(0.028)	(0.040)	(0.045)
R^2	0.951	0.941	0.954	0.968	0.913
Observations	6,682	6,677	6,644	6,625	6,609
Fixed Effects					
District × Cohort	Yes	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes	Yes

Panel B: Exit

	Total	GSE	FHA	BCU	Private
Treated × Post	−0.084***	−0.074***	−0.085***	−0.112***	−0.073
	(0.016)	(0.017)	(0.023)	(0.035)	(0.051)
R^2	0.935	0.927	0.949	0.967	0.914
Observations	6,200	6,198	6,167	6,176	6,126
Fixed Effects					
District × Cohort	Yes	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes	Yes

Table 8: Stacked DiD: Freddie Mac Purchases Around FSC Transitions

Notes: The table reports stacked DiD estimates of the effects of congressional representation on the House Financial Services Committee (FSC) on Freddie Mac's mortgage purchase activity at the congressional district level. Panel A presents estimates around districts' representatives *first entry* into the FSC, and Panel B reports estimates around *first exit*. The specification follows Equation (4), where the dependent variables include measures of Freddie Mac purchases aggregated to the district–year level: log total balance purchased, average interest rate, average borrower FICO score, and loan-to-value (LTV) and debt-to-income (DTI) ratios at origination and the original combined loan-to-value (OC). The original combined LTV represents the ratio of the combined amount of all liens on the property at origination (the first mortgage plus any disclosed secondary financing) to the lesser of the property's appraised value or purchase price. All regressions include district-by-cohort fixed effects ($\gamma_{d\times c}$) and state-by-year-by-cohort fixed effects ($\lambda_{s\times t\times c}$), which absorb time-invariant district-specific characteristics and common time-varying shocks within states and cohorts. The coefficient on *Treated*×*Post* captures the average post-entry (or post-exit) change in Freddie Mac purchase outcomes relative to the pre-entry (or pre-exit) year. Standard errors are clustered at the state–year–cohort level to account for correlated shocks within states over time. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	Amount Balance	Average Rate (%)	Average FICO	Average LTV (Orig)	Average LTV (OC)	Average DTI (Orig)
Treated × Post	0.105*	0.007	−0.289	−0.109	−0.118	0.198*
	(0.060)	(0.006)	(3.085)	(0.159)	(0.168)	(0.115)
R^2	0.909	0.999	0.667	0.951	0.945	0.888
Observations	3,698	3,698	3,698	3,698	3,698	3,698
Fixed Effects						
District × Cohort	Yes	Yes	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Exit

	Amount Balance	Average Rate (%)	Average FICO	Average LTV (Orig)	Average LTV (OC)	Average DTI (Orig)
Treated × Post	0.015	0.003	2.078	−0.248	−0.305*	−0.041
	(0.036)	(0.004)	(1.871)	(0.159)	(0.161)	(0.077)
R^2	0.918	0.999	0.705	0.952	0.946	0.896
Observations	4,041	4,041	4,041	4,041	4,041	4,041
Fixed Effects						
District × Cohort	Yes	Yes	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes	Yes	Yes

Table 9: Stacked DiD: Freddie Mac Loan Performance Around FSC Transitions

Notes: The table reports stacked DiD estimates of the effect of congressional representation on the House Financial Services Committee (FSC) on the early performance of Freddie Mac–purchased loans. The dependent variables measure performance over the first three years following origination, computed using months since first payment date from Freddie Mac loan-level performance data. Outcomes include the maximum delinquency reached within one, two, and three years after origination (*Max del. (1y–3y)*); indicators for ever being 30, 60, 90, or 180 days delinquent within three years; and whether the loan defaulted within three years. Each regression follows Equation (4), where the treatment indicator equals one in all years after a district first gains (or loses) FSC representation. All regressions include district-by-cohort fixed effects ($\gamma_{d\times c}$) and state-by-year-by-cohort fixed effects ($\lambda_{s\times t\times c}$), which absorb time-invariant district heterogeneity and time-varying shocks common to states and cohorts. The coefficient on *Treated*×*Post* captures the average post-entry (or post-exit) change relative to the pre-transition year. Standard errors are clustered at the state–year–cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	Max del. (1y)	Max del. (2y)	Max del. (3y)	Ever 30+ del.	Ever 60+ del.	Ever 90+ del.	Ever 180+ del.	Defaulted
Treated × Post	−0.031	−0.027	−0.060	−0.012	0.002	−0.032	−0.066	−0.058
	(0.028)	(0.032)	(0.038)	(0.016)	(0.029)	(0.036)	(0.045)	(0.062)
R^2	0.055	0.132	0.216	0.038	0.076	0.106	0.133	0.109
Observations	3,702	3,697	3,700	3,702	3,697	3,697	3,671	3,487
Fixed Effects								
District × Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Exit

	Max del. (1y)	Max del. (2y)	Max del. (3y)	Ever 30+ del.	Ever 60+ del.	Ever 90+ del.	Ever 180+ del.	Defaulted
Treated × Post	0.016	−0.004	−0.012	0.005	0.026	0.014	−0.013	0.038
	(0.033)	(0.025)	(0.034)	(0.014)	(0.027)	(0.032)	(0.036)	(0.056)
R^2	0.060	0.143	0.228	0.041	0.082	0.113	0.139	0.122
Observations	4,046	4,038	4,046	4,046	4,038	4,038	4,010	3,756
Fixed Effects								
District × Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 10: Stacked DiD: Mortgage Loan Limits Around FSC Transitions

Notes: The table reports stacked DiD estimates of the effect of congressional representation on the House Financial Services Committee (FSC) on federally determined mortgage loan limits. The dependent variables measure the statutory maximum loan size eligible for government or government-sponsored enterprise (GSE) guarantees. Across both panels, the left-hand columns report results for the *conforming loan limit*, which defines the maximum mortgage amount eligible for purchase or securitization by Fannie Mae and Freddie Mac. The right-hand columns report results for the *Federal Housing Administration (FHA) loan limit*, which establishes the maximum principal balance eligible for FHA-insured single-family mortgages under Title II of the National Housing Act. FHA limits vary by county and year and are intended to reflect median home prices within statutory "floor" and "ceiling" constraints. Conforming limits are set annually by the Federal Housing Finance Agency (FHFA) under the Housing and Economic Recovery Act (HERA) of 2008, with higher limits ("high-cost area limits") for designated counties. Each regression follows Equation (4), where the treatment indicator equals one in all years after a district first gains (or loses) FSC representation. All regressions include district-by-cohort fixed effects ($\gamma_{d\times c}$) and state-by-year-by-cohort fixed effects ($\lambda_{s\times t\times c}$), which absorb time-invariant district heterogeneity and time-varying shocks common to states and cohorts. Standard errors are clustered at the state–year–cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	GSE limit	FHA limit
Treated × Post	7251.860**	6585.873
	(3578.897)	(4675.699)
R^2	0.970	0.966
Observations	1,606	1,606
Fixed Effects		
District × Cohort	Yes	Yes
State × Year × Cohort	Yes	Yes

Panel B: Exit

	GSE limit	FHA limit
Treated × Post	−4406.529**	−16187.963***
	(1904.019)	(5195.296)
R^2	0.980	0.974
Observations	1,718	1,718
Fixed Effects		
District × Cohort	Yes	Yes
State × Year × Cohort	Yes	Yes

Table 11: Stacked DiD: Small Business Lending Around FSC Transitions

Notes: The table reports stacked DiD estimates of the effect of congressional representation on the House Financial Services Committee (FSC) on district-level small-business lending activity. The dependent variable measures the log of the total value of small-business loans originated within each congressional district and year, based on Community Reinvestment Act (CRA) reporting data. Each regression follows Equation (4). The left-hand column reports estimates for FSC *entry* events, while the right-hand column reports estimates for *exit* events. All regressions include district-by-cohort fixed effects ($\gamma_{d\times c}$) and state-by-year-by-cohort fixed effects ($\lambda_{s\times t\times c}$), which absorb time-invariant district heterogeneity and time-varying shocks common to states and cohorts. The coefficient on *Treated*×*Post* captures the average post-entry (or post-exit) change relative to the pre-transition year. Standard errors are clustered at the state–year–cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	Entry	**Exit**
Treated × Post	0.058*	−0.101***
	(0.032)	(0.032)
R^2	0.972	0.982
Observations	2,236	2,838
Fixed Effects:		
District × Cohort	Yes	Yes
State × Year × Cohort	Yes	Yes

Table 12: Stacked DiD: Political Contributions by Lenders Around FSC Transitions

Notes: The table reports stacked DiD estimates of the effect of congressional representation on the House Financial Services Committee (FSC) on campaign contributions. The dependent variable is the natural logarithm of the total contribution amount, from Bonica (2024), that the incumbent House Representative receives. The first column subsets contributions made by lenders reported in the HMDA originations data while the second column reports subsets the contributions made by other lenders not in reporting in the HMDA originations data (non-HMDA). The last column tests for the significance of the differential effects between HMDA and non-HMDA lenders. All regressions include district-by-cohort fixed effects ($\gamma_{d\times c}$) and state-by-year-by-cohort fixed effects ($\lambda_{s\times t\times c}$), which absorb time-invariant district heterogeneity and time-varying shocks common to states and cohorts. In the last column, we interact the fixed effects with *Group*, that identifies whether the lender from HMDA data. Standard errors are clustered at the state–year–cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	HMDA only	Non-HMDA only	All
Treated × Post	0.375*	0.00675	0.00675
	(0.197)	(0.0703)	(0.0712)
Treated × Post × HMDA			0.368***
			(0.135)
R^2	0.850	0.290	0.412
Observations	952	15,850	16,802
Fixed Effects			
District × Cohort	Yes	Yes	
State × Year × Cohort	Yes	Yes	
District × Cohort × Group			Yes
State × Year × Cohort × Group			Yes

Panel B: Exit

	HMDA only	Non-HMDA only	All
Treated × Post	−0.691***	−0.372***	−0.372***
	(0.200)	(0.0567)	(0.0576)
Treated × Post × HMDA			−0.319**
			(0.142)
R^2	0.847	0.268	0.406
Observations	1,006	15,129	16,135
Fixed Effects			
District × Cohort	Yes	Yes	
State × Year × Cohort	Yes	Yes	
District × Cohort × Group			Yes
State × Year × Cohort × Group			Yes

Table 13: Stacked DiD: Building Permits Around FSC Transitions

Notes: The table reports stacked DiD estimates of the effect of congressional representation on the House Financial Services Committee (FSC) on local residential construction activity, measured using data from the U.S. Census Bureau's Building Permits Survey (BPS). The dependent variables are based on annual permit-level aggregates for all permit-issuing jurisdictions, covering the universe of residential construction authorizations in the United States. Outcomes include the total number of residential buildings authorized (*Total buildings*), the total number of housing units authorized (*Total units*), and the total reported construction value (*Total value*) in thousands of dollars. "Total buildings" counts each distinct building for which a new residential permit was issued, regardless of the number of housing units in the structure. "Total units" counts the total number of dwelling units authorized by these permits (e.g., a single permit for a 50-unit apartment building contributes 1 to *Total buildings* and 50 to *Total units*). "Total value" reports the aggregate dollar valuation of all new residential construction authorized during the period, as recorded on the building permit application, and reflects the intended construction cost rather than observed expenditures. Each regression follows Equation (4). All regressions include district-by-cohort fixed effects ($\gamma_{d\times c}$) and state-by-year-by-cohort fixed effects ($\lambda_{s\times t\times c}$). Standard errors are clustered at the state–year–cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	Value	Buildings	Units
Treated × Post	−0.012	−0.040*	−0.067**
	(0.026)	(0.024)	(0.027)
R^2	0.949	0.967	0.951
Observations	4,864	4,868	4,868
Fixed Effects			
District × Cohort	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes

Panel B: Exit

	Value	Buildings	Units
Treated × Post	−0.059**	−0.052***	−0.044**
	(0.025)	(0.018)	(0.021)
R^2	0.955	0.971	0.957
Observations	4,586	4,591	4,591
Fixed Effects			
District × Cohort	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes

Table 14: Stacked DiD: Low-income Housing Tax Credit Allocations Around FSC Transitions

Notes: The table reports stacked DiD estimates of the effect of congressional representation on the House Financial Services Committee (FSC) on the allocation of federally subsidized housing projects under the Low-Income Housing Tax Credit (LIHTC) program. The LIHTC program is the primary federal mechanism for supporting the construction and rehabilitation of affordable rental housing. Project-level data are from the U.S. Department of Housing and Urban Development (HUD) LIHTC Database, which includes more than 54,000 projects (3.7 million units) placed in service between 1987 and 2023. Each observation corresponds to a distinct project receiving federal tax credits, linked to its congressional district of location. The dependent variables include the total annual dollar amount of tax credits allocated (*Amount*), the total number of housing units in the project (*Units*), the number of units subject to rent ceilings below the elected rent/income threshold (*Rent-controlled*), and the number of units designated for low-income tenants (*Low-income*). Each regression follows Equation (4). All regressions include district-by-cohort fixed effects ($\gamma_{d\times c}$) and state-by-year-by-cohort fixed effects ($\lambda_{s\times t\times c}$), which absorb time-invariant district heterogeneity and time-varying shocks common to states and cohorts. Standard errors are clustered at the state–year–cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	Amount	Units	Rent-controlled	Low-income
Treated × Post	0.107	0.107	0.052	0.047
	(0.094)	(0.151)	(0.068)	(0.070)
R^2	0.670	0.726	0.669	0.654
Observations	3,894	2,123	5,312	5,312
Fixed Effects				
District × Cohort	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes

Panel B: Exit

	Amount	Units	Rent-controlled	Low-income
Treated × Post	0.145	0.317**	0.165**	0.146**
	(0.117)	(0.151)	(0.066)	(0.067)
R^2	0.656	0.737	0.671	0.653
Observations	3,603	2,239	4,843	4,831
Fixed Effects				
District × Cohort	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes

Table 15: Stacked DiD: Federal Contracts and Assistance Around FSC Transitions

Notes: The table reports stacked DiD estimates of the effect of a district's representation on the House Financial Services Committee (FSC) on federal *contracts* (left column) and federal *assistance* (right column). Outcomes are the logarithm of the *sum of obligations* aggregated to the congressional-district-by-year level. We restrict to HFSC-relevant awarding toptier agencies: Consumer Financial Protection Bureau (1158), Department of Housing and Urban Development (882), Treasury (456), Federal Housing Finance Agency (1153), and Securities and Exchange Commission (680). All regressions are estimated following Equation (4) and include both district-by-cohort ($\gamma_{d\times c}$) and state-by-year-by-cohort fixed effects ($\lambda_{s\times t\times c}$). Standard errors clustered at the state–year–cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	Contracts	Assistance
Treated × Post	0.184	0.081
	(0.188)	(0.091)
R^2	0.808	0.942
Observations	3,058	2,250
Fixed Effects		
District × Cohort	Yes	Yes
State × Year × Cohort	Yes	Yes

Panel B: Exit

	Contracts	Assistance
Treated × Post	-0.329^{*}	0.014
	(0.190)	(0.111)
R^2	0.788	0.936
Observations	3,424	2,790
Fixed Effects		
District × Cohort	Yes	Yes
State × Year × Cohort	Yes	Yes

Table 16: Re-election Probability of Incumbent as a Function of House Prices

Notes: The table reports TWFE estimates of how local house price appreciation affects electoral outcomes of congressional incumbents. We use an indicator for re-election as the dependent variable. The key regressor, *Treated*, is equal to one for districts whose representative serves on the House Financial Services Committee (FSC). Interactions with pre-election house price growth capture heterogeneous electoral effects of local housing markets. House price residuals are computed from property-level transaction data by estimating a specification that includes property and year-month fixed effects, thereafter aggregated to the congressional-district–year level. All regressions include Congressional-district and Congress fixed effects. In the last column, we interact Congress with residual quartile that absorb the main effects. Standard errors clustered at the state-year-cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Re-election Probability

	Baseline (1)	Residual × Treated (2)	Residual quartiles (3)
Treated	-0.028	-0.035*	-0.064*
	(0.021)	(0.020)	(0.033)
House Price Residual		0.013	
		(0.011)	
Treated × House Price Residual		0.060**	
		(0.029)	
Treated × Residual Q1			0.055
			(0.040)
Treated × Residual Q3			0.043
			(0.039)
Treated × Residual Q4			0.094**
			(0.040)
R^2	0.214	0.214	0.219
Observations	5,607	5,506	5,506
Fixed Effects			
Congress	Yes	Yes	No
Congress × residual quartile	No	No	Yes
District	Yes	Yes	Yes

Online Appendix

Table of Contents

A Data construction

This Appendix describes the construction of the dataset used in our analysis. The dataset combines multiple administrative and political data sources covering the period from 1989 to 2022, with observations at both the congressional district–year and district–month levels.

Committee Assignment and Legislator Characteristics.—Information on congressional committee membership and legislators' characteristics is compiled from two complementary sources: the official House Committee Assignment records (Congresses 103–117) published by by Stewart (2017), and member-level data from the Center for Effective Lawmaking (CEL). We merge these sources using standardized ICPSR member identification codes to ensure consistent tracking of individuals across congresses. Committee codes are harmonized using the official Congressional codebook, and membership years are mapped to calendar years according to the standard congressional calendar. After merging, the combined dataset includes approximately 9050 member–committee-congress observations. Consistency of member names across sources was verified via fuzzy matching, yielding an average similarity score of 99.5 percent.

Housing and Mortgage Data.—We link data on U.S. representatives and congressional districts to detailed information on local housing markets and credit conditions. Property transaction data from 1990 to 2020 are drawn from the CoreLogic Deeds Database, which records property sales across all U.S. states. Each transaction includes the sale price and precise geographic coordinates. Property characteristics are merged from a 2020 snapshot of CoreLogic's property records, providing a consistent cross-sectional measure of housing attributes.

We geocode all transactions to congressional districts using official digital boundary definitions. For congressional sessions up to the 114th Congress, we use shapefiles from Lewis et al. (2013), retrieved from `https://cdmaps.polisci.ucla.edu`. For districts after the 114th Congress, corresponding to redistricting following the 2010 Census, we use boundary shapefiles provided by the U.S. Census Bureau (TIGER/Line). We match each property to its own congressional district as well as to the nearest congressional district boundary using standard polygon distance computations. However, for properties located near coastlines, lakes, or irregular district shapes, the nearest boundary may not correspond to another valid congressional district. To ensure every property has a well-defined opposite district, we implement a fallback method illustrated in Figure A.1.

In the first step, we identify the point on the boundary of the property's own congressional district that minimizes the distance to the property (shown as the blue dot). We then verify whether another congressional boundary exists directly across that direction. If such a boundary is found, the adjacent district is recorded as the neighboring district. If not—such as in coastal or noncontiguous areas—the algorithm searches for the closest boundary belonging to any other congressional district (purple diamond). This ensures that every property is associated with the most geographically proximate district, even when the nearest edge of its own district borders a body of water or lies at the national boundary. In total, we geolocate 185 million transactions in our sample, corresponding to 85 million distinct properties across the country. Restricting the sample to properties within 10 km of the border reduces it to 130 million transactions, within 5 km to 93 million, and within 3 km to 68 million.

We supplement these data with information from the Home Mortgage Disclosure Act (HMDA) dataset, maintained by the Federal Financial Institutions Examination Council (FFIEC), which provides detailed records of mortgage applications between 1990 and 2019. We process the four major HMDA reporting regimes to harmonize variable definitions over time. Using tract-to-district crosswalks that account for decennial redistricting, we aggregate application-level data to the district–year level. Specifically, we calculate the centroid of each census tract using tract shapefiles and assign it to the corresponding congressional district for each Congress based on historical district boundaries.

We construct measures of total loans originated, total loan amounts, and borrower characteristics such as average income and FHA versus conventional loan shares. The final HMDA panel contains approximately 286 million loan originations, of whose approximately 121 million were for house purchase. Among these, 41.4 million are GSE purchased and 19.6 million are FHA loans.

To further evaluate the loan-level characteristics and loan delinquency, we focus on Freddie Mac's Single-Family Loan-Level Dataset (Standard). The dataset provides detailed information on mortgage origination and monthly loan performance. We combine origination and performance files from 1999 onward, geolocating loans using property-level ZIP codes. For each loan, we construct measures of borrower and loan characteristics at origination—such as credit score, loan-to-value ratio, and debt-to-income ratio—and track outcomes including delinquency, prepayment, default, and modification over the first three years after origination. We then aggregate these loan-level measures to the congressional district–year level using outstanding unpaid principal balance as weights.

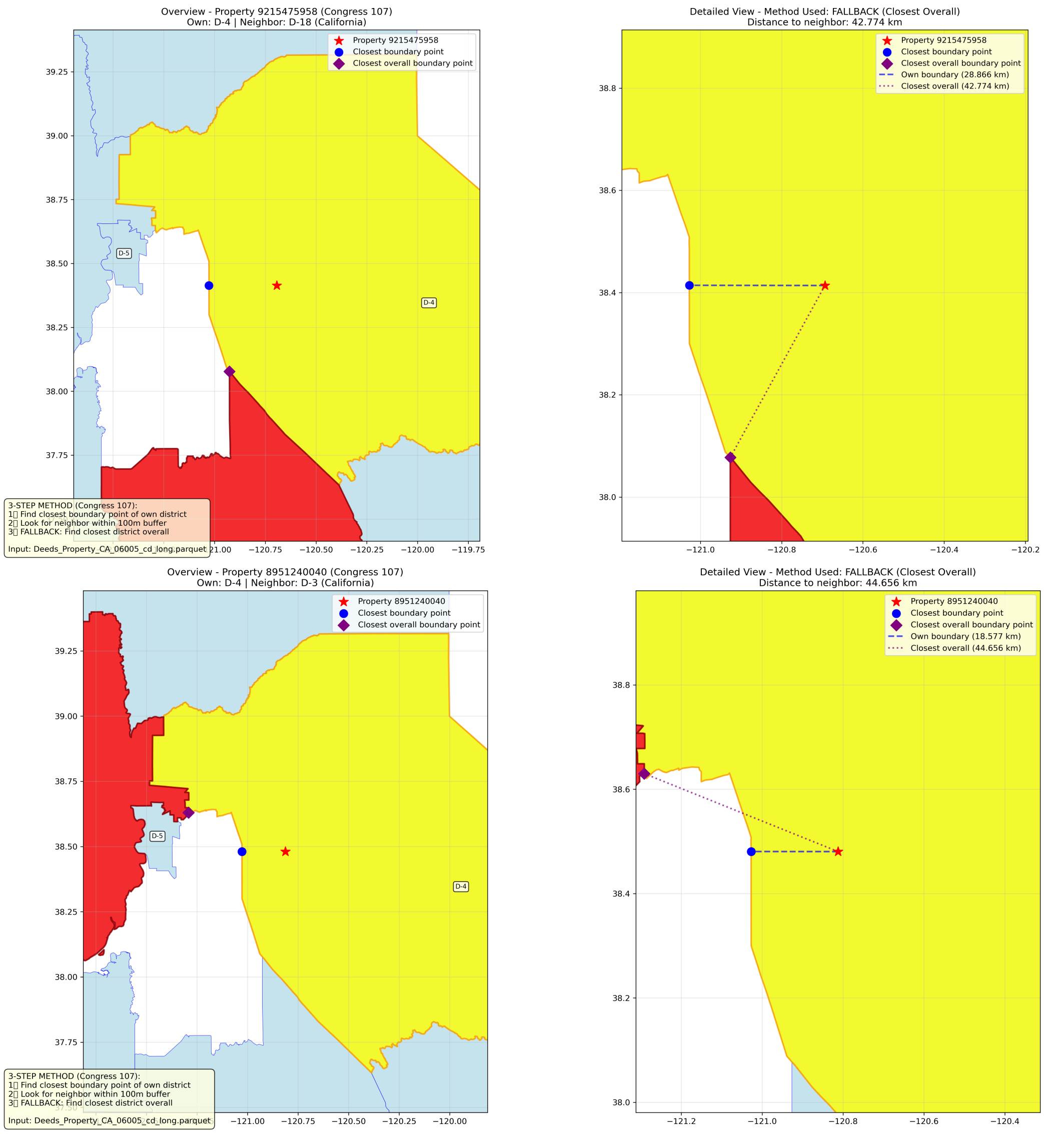


Figure A.1: Fallback method for assigning dual-border congressional districts. In this approach, we first identify for each property the location on the boundary of its own congressional district that is minimally distant from the property (blue dot). We then verify whether a congressional boundary exists on the opposite side of that direction. If not, the algorithm searches for the closest boundary of any other congressional district (purple diamond). This ensures that each property is associated with the most adjacent district even in regions bounded by lakes or sea.

Mortgage Market Regulation.— We incorporate variation in federal mortgage limits by merging county-level data from the Federal Housing Finance Agency (FHFA) on GSE conforming loan limits, available annually from 1995 through 2022. We similarly obtain county-level FHA loan limits from the Department of Housing and Urban Development (HUD). Both series are aggregated to congressional districts using population-weighted averages, with adjustments for boundary changes across redistricting cycles. These variables allow us to capture annual changes in the effective size of the mortgage market in each district.

Housing supply and LIHTC.— We complement these data with information from the Building Permits Survey (BPS) conducted by the U.S. Census Bureau and the Low-Income Housing Tax Credit (LIHTC) database maintained by the U.S. Department of Housing and Urban Development (HUD). The BPS provides comprehensive monthly and annual data on new privately owned residential construction authorized by building permits. We aggregate the establishment-level BPS data to the congressional district–year level, constructing measures of total permitted units, number of buildings, and total permit valuation as indicators of local construction activity. The LIHTC dataset contains detailed project-level information on federally subsidized affordable housing developments, including the number of total and low-income units, placed-in-service year, and location identifiers. We aggregate these data to the district–year level to capture the scale and timing of federally supported housing investment.

Government Contracting and Federal Spending.— We obtain data on federal procurement activity from the Federal Procurement Data System (FPDS), accessed through USAspending.gov. The data include all contract actions above the micro-purchase threshold between 2000 and 2022. We assign each contract both to the congressional district of the contractor's headquarters and to the district where the work was performed. All dollar amounts are converted to constant 2020 dollars using the CPI-U, and contracts with implausible or negative obligations are excluded. We then aggregate to the district–year level, computing total obligations, the number of contract actions, and the share of spending going to prime versus subcontractors.

Political contributions.— Using the dataset constructed by Bonica (2024), which contains all itemized political contributions reported to the Federal Election Commission, we identify donations made by corporate entities operating as financial institutions in the mortgage market. We restrict the DIME contributor database to observations classified as corporations and link these donors to lenders appearing in the HMDA dataset. To establish the linkage, we implement a fuzzy-matching algorithm that compares contributor names in DIME with lender names in HMDA using cosine and partial-token similarity metrics; matches with similarity scores above

90 are classified as HMDA institutions. This mapping allows us to test whether financial firms adjust their political contributions in response to their district representative's membership on the House Financial Services Committee (FSC). In the appendix, we replicate the analysis using contributions from individual employees of HMDA lenders, matched to their employers using the same fuzzy-matching procedure, to verify that the results are not driven by the choice of donor type.

Computational Infrastructure.— All data cleaning and analysis were conducted using Stata 18 and Python 3.11.

B Additional Results

Table B.1: Stacked DiD: HMDA Originations, Lender-Level Analysis

Notes: The table reports stacked DiD estimates of the effect of congressional representation on the House Financial Services Committee (FSC) on mortgage origination activity at the lender level. The analysis uses Home Mortgage Disclosure Act (HMDA) data aggregated to the lender–district–year level, following the specification in Equation (4) extended to include lender fixed effects. Panel A presents estimates around districts' representative's *first entry* into the FSC, and Panel B reports estimates around *first exit*. Each regression compares changes in lending behavior for the same lender across treated and never treated districts within the same state and year. All regressions include district-by-lender-by-cohort fixed effects ($\gamma_{d\times\ell\times c}$) and state-by-year-by-cohort-by-lender fixed effects ($\lambda_{s\times t\times c\times\ell}$), which absorb time-invariant differences across district-lenders, and time-varying shocks common to specific lender types or geographies. The coefficient on *Treated*×*Post* captures the average post-entry (or post-exit) change in the outcome relative to the year prior to the FSC transition. Standard errors are clustered at the state–year–cohort level to account for correlated shocks within states over time. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	Orig	GSE	FHA	BCU	Private
Treated × Post	0.028***	0.026**	0.023*	0.060***	0.001
	(0.007)	(0.011)	(0.013)	(0.017)	(0.038)
R^2	0.852	0.841	0.806	0.789	0.784
Observations	1,238,895	418,216	357,500	244,594	49,627
Fixed Effects					
District × Lender × Cohort	Yes	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes	Yes

Panel B: Exit

Amount	Orig	GSE	FHA	BCU	Private
Treated × Post	−0.070***	−0.048***	−0.053***	−0.081***	−0.119***
	(0.006)	(0.010)	(0.010)	(0.013)	(0.039)
R^2	0.848	0.837	0.801	0.784	0.781
Observations	1,210,688	411,325	375,325	280,205	44,928
Fixed Effects					
District × Lender × Cohort	Yes	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes	Yes

Table B.2: Stacked DiD: Freddie Mac Purchases Around FSC Transitions (Special Specification)

Notes: The table reports stacked DiD estimates of the effects of congressional representation on the House Financial Services Committee (FSC) on Freddie Mac's mortgage purchase activity at the congressional district level. Panel A presents estimates around districts' representatives' *first entry* into the FSC, and Panel B reports estimates around *first exit*. The specification follows Equation (4), with dependent variables including total balance purchased and *volume-weighted* borrower characteristics (interest rate, FICO, LTV, and DTI). The sample is restricted to loans associated with Freddie Mac's special eligibility or refinance programs: Home Possible (`program_indicator = "H"`), HFA Advantage (`"F"`), Refi Possible (`"R"`), or HARP loans (`harp_indicator = "Y"`). These programs target affordable or relief refinance mortgages and exclude standard conforming or super-conforming loans. All regressions include district-by-cohort and state-by-year-by-cohort fixed effects, and standard errors are clustered at the state–year–cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	Amount Balance	Average Rate (%)	Average FICO	Average LTV (Orig)	Average LTV (OC)	Average DTI (Orig)
Treated × Post	0.250**	-0.009	-1.371	0.888	0.784	-0.176
	(0.099)	(0.010)	(2.650)	(0.871)	(0.858)	(0.299)
R^2	0.930	0.986	0.782	0.841	0.843	0.988
Observations	1,636	1,636	1,636	1,636	1,636	1,636
Fixed Effects						
District × Cohort	Yes	Yes	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Exit

	Amount Balance	Average Rate (%)	Average FICO	Average LTV (Orig)	Average LTV (OC)	Average DTI (Orig)
Treated × Post	0.019	-0.001	-0.448	-3.466***	-3.640***	0.094
	(0.043)	(0.009)	(1.660)	(0.973)	(0.988)	(0.194)
R^2	0.937	0.983	0.486	0.833	0.832	0.988
Observations	2,195	2,195	2,195	2,195	2,195	2,195
Fixed Effects						
District × Cohort	Yes	Yes	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes	Yes	Yes

Table B.3: Stacked DiD: Campaign contributions by employees Around FSC Transitions

Notes: The table reports stacked DiD estimates of the effect of congressional representation on the House Financial Services Committee (FSC) on campaign contributions. The dependent variables measure the log contribution amount that the incumbent House Representative receives. The first column shows the effect for employees of HMDA companies only, the second column for non-HMDA companies, and the last regression include all the panel and the triple interaction term, showing the differential effect in the stacked DiD between HMDA and non-HMDA political donations. All regressions include district-by-cohort fixed effects ($\gamma_{d\times c}$) and state-by-year-by-cohort fixed effects ($\lambda_{s\times t\times c}$), which absorb time-invariant district heterogeneity and time-varying shocks common to states and cohorts. Standard errors are clustered at the state–year–cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

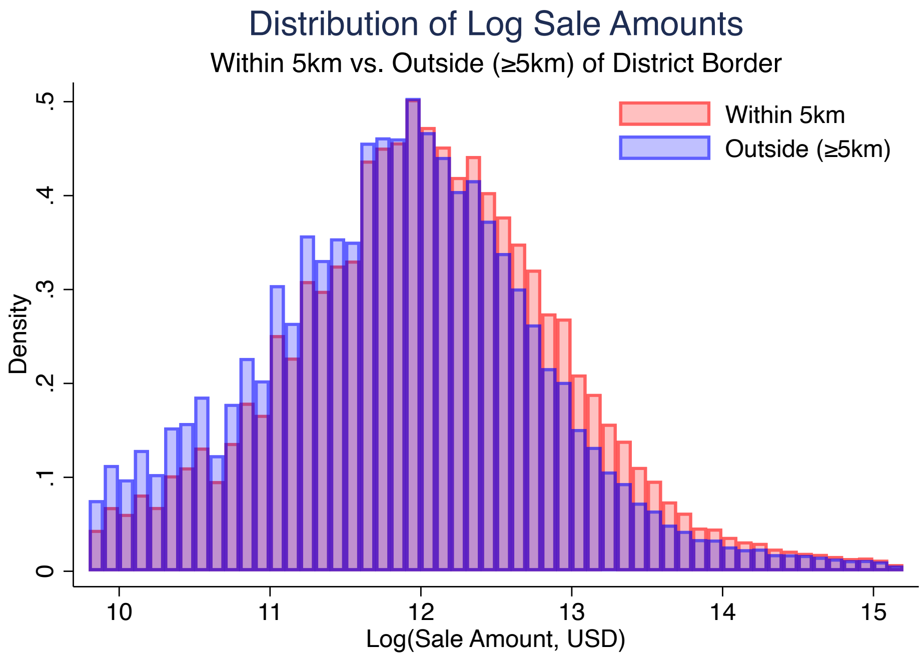
Panel A: Entry

	HMDA only	Non-HMDA only	All
Treated × Post	0.252	0.107	0.107
	(0.164)	(0.0839)	(0.0857)
Treated × Post × HMDA			0.145*
			(0.0750)
R^2	0.816	0.240	0.372
Observations	1,644	16,026	17,670
Fixed Effects			
District × Cohort	Yes	Yes	
State × Year × Cohort	Yes	Yes	
District × Cohort × Group			Yes
State × Year × Cohort × Group			Yes

Panel B: Exit

	HMDA only	Non-HMDA only	All
Treated × Post	−0.371***	−0.208***	−0.208***
	(0.111)	(0.0651)	(0.0666)
Treated × Post × HMDA			−0.163**
			(0.0584)
R^2	0.820	0.219	0.371
Observations	1,708	15,239	16,947
Fixed Effects			
District × Cohort	Yes	Yes	
State × Year × Cohort	Yes	Yes	
District × Cohort × Group			Yes
State × Year × Cohort × Group			Yes

Figure B.1: Distribution of Log House Sale Prices: Within 5km vs. Outside 5km of District Borders



Notes: This figure compares the distribution of log-transformed house sale prices for properties located within five kilometers of a congressional district border (in red) and those located farther away (in blue). The sample is drawn from the complete transactions dataset. Sale prices are trimmed at \$18000 and above the 99% level (about \$4.7 mln).

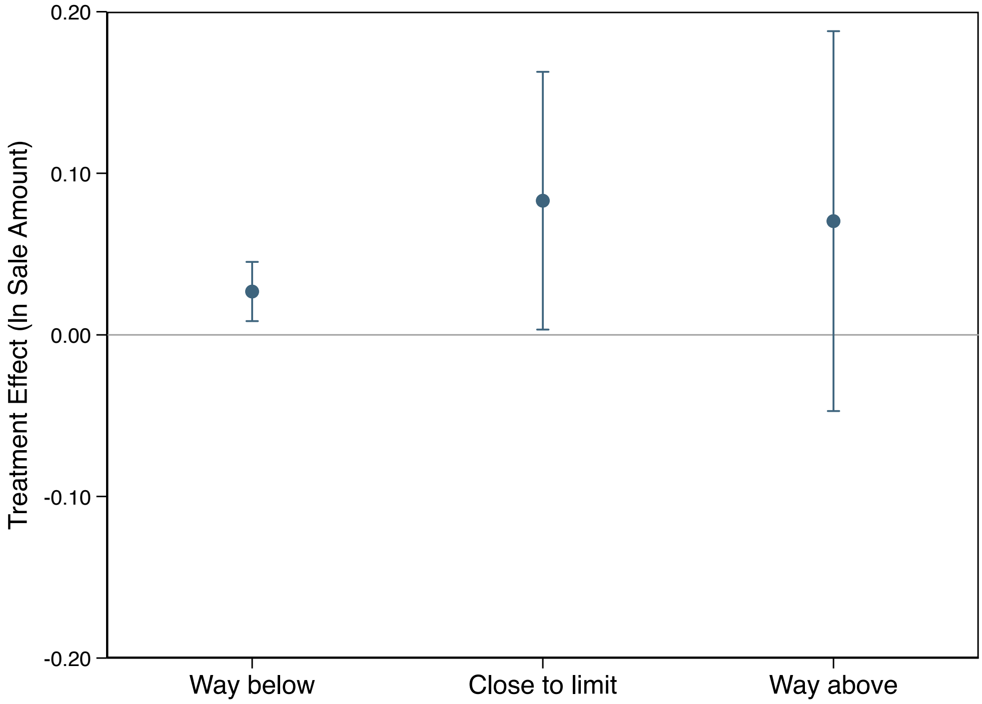
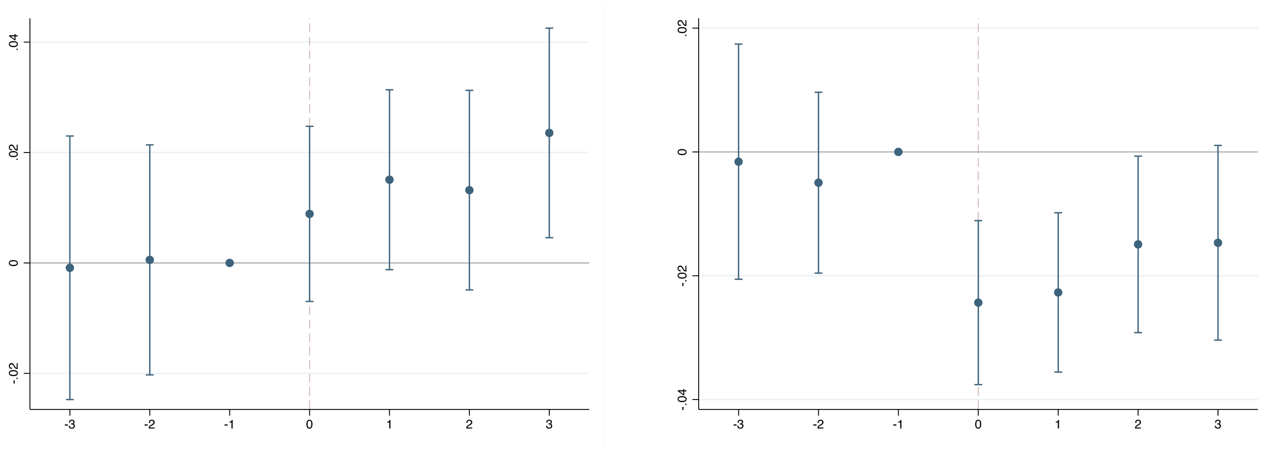


Figure B.2: Estimated treatment effects by proximity to the loan limit. *Notes*: The figure plots DiD estimates of the treatment effect (Treated × Post) for properties categorized by their proximity to the conforming loan limit: Way below, Close to, and Way above. The Close to group includes properties with mortgage amounts within 20 percent of the conforming loan limit in the year of origination, while the Way below and Way above groups include all other properties. Each point represents the estimated coefficient from a separate regression, with vertical lines denoting 95% confidence intervals clustered at the state–district–year–month level. The specification is the same as the baseline stacked DiD.



(a) Entry into the House FSC

(b) Exit from the House FSC

Figure B.3: *Notes*: The figure reports coefficient estimates and corresponding 95% confidence intervals from the stacked DiD specification in Equation (4). The dependent variable is the logarithm of the average GSE loan amount. Panel (a) presents the response of house prices around entry into the House Financial Services Committee (FSC) while Panel (b) presents the response around exit from the FSC, in a 4-year window around these events. The omitted category is the year prior to FSC entry or exit. All regressions include district-by-cohort fixed effects ($\gamma_{d\times c}$) and state-by-year-by-cohort fixed effects ($\lambda_{s\times t\times c}$), which absorb time-invariant district heterogeneity and time-varying shocks common to states and cohorts. Standard errors are clustered at the state–year–cohort level.