Politics and the Price of Housing*

Marco Grotteria[1], S. Lakshmi Naaraayanan[1], and Andrea Pagliuca[2]

[1]London Business School and CEPR.
[2]London Business School.

June 1, 2026

Abstract

We study how congressional representation on the House Financial Services Committee (FSC) affects local housing markets. Using the near-universe of U.S. housing transactions from 1990 to 2020 and a stacked difference-in-differences border-discontinuity design, we find that house prices rise by about 5 percent when a district's representative joins the FSC and fall symmetrically upon exit. These effects are driven by increased mortgage origination, greater government-sponsored enterprise purchases, and higher conforming loan limits, with no change in construction activity or affordable housing allocations. FSC representation shapes local housing markets primarily through credit allocation, generating political benefits for incumbent legislators.

Keywords: Political power; congressional committees; financial regulation; housing markets; credit supply; government-sponsored enterprises (GSEs); real estate prices.

JEL Classification: D72, G21, H73, R31.

*For helpful comments, we thank Viral Acharya, Nathan J. Canen, João Cocco, Francisco Gomes, Helios Herrera, Tim McQuade, Anna Pavlova, Elias Papaioannou, Henri Servaes, David Sraer, Thomas Lambert, Francesco Trebbi and Luigi Zingales. We also thank seminar participants at the Adam Smith Juniors Workshop, Alliance Manchester Business School, Bocconi University, BSE Summer Forum (Political Economy), London Business School, LBS PhD Alumni Workshop, EIEF Rome Junior Finance Conference, Spring Finance Workshop, Summer Real Estate Research Symposium, UC Berkeley Haas, and Warwick Business School for helpful comments and suggestions. Corresponding authors: mgrotteria@london.edu (Grotteria), lnaaraayanan@london.edu (Naaraayanan) and apagliuca@london.edu (Pagliuca).

1 Introduction

Three facts characterize homeownership in the United States. (i) Roughly two-thirds of households are owner-occupiers (U.S. Census Bureau, 2025). (ii) For households in the middle and upper-middle of the income distribution, housing accounts for nearly 70% of total wealth (Kuhn, Schularick, and Steins, 2020). (iii) Most households own a single house (Piazzesi, 2025). Together, these facts imply that movements in *local* house prices translate directly into changes in household wealth for a broad segment of voters.[1] This may create incentives for elected officials to use *federal* housing-policy instruments to favor disproportionately their constituents. A natural question, then, is whether legislators actually exploit these instruments— and if so which instruments—in electorally valuable ways.

We study this question by examining how housing and mortgage credit evolve in a representative's home district when the representative enters or leaves the U.S. House Financial Services Committee (FSC). The FSC exercises jurisdiction over banking regulation, mortgage markets, and federal housing policy. Through oversight of the government-sponsored enterprises (GSEs), the Federal Housing Finance Agency (FHFA), the Federal Housing Administration (FHA), and the Department of Housing and Urban Development (HUD), FSC members can influence local availability and terms of mortgage credit and the implementation of major urban development programs. Service on the FSC therefore provides legislators with considerable influence over housing policy and the allocation of housing finance.

We exploit the staggered timing of legislators' entry into—and exit from—the FSC. Following Cohen, Coval, and Malloy (2011) and Bertrand, Bombardini, Fisman, and Trebbi (2020), we treat committee assignments as plausibly exogenous to contemporaneous district-level economic conditions, since appointments are determined largely by seniority and intra-party negotiations rather than congressional district fundamentals. We combine this variation with a border-discontinuity design that compares properties in narrow bands on either side of congressional district boundaries (Card and Krueger, 1994; Dell, 2010). Comparing properties within a tight geographic bandwidth holds local economic conditions constant and controls for the effects of national and state-level housing policies, isolating the change attributable to FSC representation.

[1] Because households typically own a single home, housing risk is largely undiversified (Giacoletti, 2021), 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). More generally, housing also plays a central role in general-equilibrium asset-pricing models (Piazzesi, Schneider, and Tuzel, 2007).

Our empirical analysis proceeds in three steps. First, we construct a property-level panel linking the near-universe of U.S. housing transactions between 1990 and 2020 to congressional district boundaries in each year. Restricting the sample to properties transacted more than once during this 30-year period allows us to estimate *within-property* changes in house prices—which is important given that properties differ along unobservable dimensions (Wallace and Meese, 1997; van Binsbergen, Cocco, Grotteria, and Naaraayanan, 2026).[2] We begin with a two-way fixed effects (TWFE) specification that includes property and state-by-year-month fixed effects, and find that FSC membership raises local house prices.

Second, we sharpen the comparison by narrowing the sample to properties within five kilometers of a congressional district boundary, matching each property to its nearest adjacent district. Specifically, we construct unordered district-pair identifiers at the property-congress level and interact our fixed effects with these identifiers. In this way, the estimation compares price changes for properties just inside a treated district to those just outside. Properties just inside districts represented on the FSC appreciate relative to properties just outside. We show further robustness across alternative distance bands, including narrower (3 km) and wider (10 km) border areas.

Third, 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) design using a two-year window around each entry or exit event, with never-treated districts as controls. Properties in districts whose representative joins the FSC appreciate by approximately 5 percent relative to properties in adjacent control districts; a symmetric decline of similar magnitude accompanies exits. 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.

We begin by assessing the sensitivity to the fixed-effects structure. In particular, we retain property fixed effects without interacting them with district-pair identifiers and replace state-by-year-month fixed effects with year-month fixed effects, comparing a broader sample of properties across geographies. We then exclude properties that are close to the border but far from any developed area on the other side to ensure comparability in local economic conditions near shared congressional district borders. In this way, treated and control properties are drawn

[2]Restricting to repeat transactions over a 30-year window introduces no meaningful selection: properties transact, on average, once per decade.

from spatially contiguous local markets. Finally, in our baseline stacked DiD, the inclusion of property fixed effects in a narrow window of 2 years around the event allows us to absorb time-invariant confounders in the event window, but it may also introduce selection: properties that transact twice within this short window could over-represent units acquired, renovated, and quickly resold. To address this concern, we expand the event window to $[-3, +3]$ and $[-4, +4]$ years around changes in FSC membership. Our results are robust to these alternative specifications.

Having established that FSC representation affects local house prices, we turn to the channels through which these effects operate. Committee membership could affect local housing markets by altering credit supply—what we call the *housing-finance channel*—or by redirecting urban development resources and federal spending toward a member's district—the *housing-supply and development channel*. The evidence strongly supports the former, but not the latter.

The housing-finance channel can operate through two complementary routes. FSC members may directly shape the implementation of federal housing-finance policy, affecting the volume of loans eligible for GSE purchase or securitization and the terms on which these loans are originated—potentially altering the composition of borrowers. Alternatively, even absent formal policy changes, private intermediaries may favor origination and pricing toward FSC-represented districts in anticipation of oversight, regulatory attention, or future policy. We find evidence consistent with both routes.

On the policy side, districts represented on the FSC exhibit discrete changes in GSE purchase activity, securitization volumes, and conforming loan limits—increasing upon entry and declining upon exit from the FSC. These margins matter because GSE purchase and securitization decisions are governed by their charters and by FHFA oversight—GSEs cannot reallocate credit without operational or regulatory justification—so changes that line up with committee transitions point to shifts in the rules governing GSE activity (U.S. Congress, 2026a,b,c; Federal Housing Finance Agency, 2026). Importantly, we find no deterioration in borrower quality as measured by default rates on GSE-purchased mortgages, suggesting that the credit expansion does not come through riskier lending. On the favoritism side, we document a broader credit expansion in politically connected districts, including a significant increase in small-business lending following FSC appointments and a corresponding decline upon exit. Complementing these results, we show banks and mortgage lenders substantially increase campaign contributions to representatives when the latter join the FSC and pull back when they leave.

The housing-supply and development channel can also operate through several routes. FSC members may affect construction activity in their districts or the amount of affordable housing programs such as the Low-Income Housing Tax Credit (LIHTC). We find no evidence they do: building permits do not respond to committee transitions, and LIHTC allocations are similarly unaffected. We also test whether FSC members steer federal procurement contracts and financial assistance—particularly from agencies under their direct oversight—toward their home districts, and again find no support. Together with the credit-supply results, these null findings indicate that legislative power over housing policy operates primarily through a localized housing-finance channel rather than through the reallocation of federal expenditure.

Finally, a natural question is whether elected officials who influence housing policy also benefit politically from the resulting increases in house prices. The answer is unclear. On the one hand, rising prices directly benefit homeowners, whose wealth increases. On the other hand, policies that ease credit constraints and expand access to mortgage lending are likely valued by prospective buyers, who may not benefit from— and may even be harmed by— higher prices. The net electoral effect may thus depend on which channel dominates in the eyes of constituents. We investigate this question by comparing re-election probabilities and individual campaign contributions across districts represented and not represented by FSC members. We show increases in house prices are positively associated with the likelihood of getting re-elected, with higher individual contributions and a larger donor base. The effects are economically and statistically significant for all three outcome variables. The findings suggest that the influence of committee representation over housing policy generates electoral benefits for incumbent legislators.

The remainder of the paper proceeds as follows. Section 2 discusses related literature. Section 3 describes the data, treatment construction, and research design. Section 4 presents the main results and robustness analyses. Section 5 investigates mechanisms. Section 6 discusses legislators' political benefits. Section 7 concludes.

2 Related Literature

Our contribution is to identify whether political influence over federal housing policy affects local housing markets and, if so, through what channels. We show that legislators use federal housing-finance measures to relatively favor their own constituents, thereby raising local house prices. In doing so, we contribute to three strands of the literature.

First, our work relates to a large literature on the determinants of house prices (Piazzesi and Schneider, 2016). Classic work highlights fundamentals—such as inflation, user cost, and income growth—as key drivers of housing demand and values (Poterba, 1984; Davis and Heathcote, 2007; Glaeser and Gyourko, 2018). More recent research emphasizes financial frictions and credit supply, showing that credit expansions fuel booms and contractions amplify busts (Landvoigt, Piazzesi, and Schneider, 2015; Guren and McQuade, 2020; Adelino, Schoar, and Severino, 2025; Greenwald and Guren, 2025). A separate strand studies the effects of rent controls and LIHTC policies: Diamond, McQuade, and Qian (2019) shows rent control reduced rental supply and raised market rents, while Diamond and McQuade (2019) finds LIHTC can revitalize low-income neighborhoods but depress values in higher-income area. While this literature establishes that credit and affordability policies matter for housing outcomes, it is largely silent on what determines how these policies are allocated across places. Our results identify that political influence of House representatives operates as a place-based determinant of credit availability and house prices, whereas we find little evidence of comparable effects through policies targeting housing affordability.

Second, we relate to research on 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.[3] 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 districts whose representatives join the FSC.

Finally, our paper connects 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; Akey, 2015; Akey and Lewellen, 2017; Zingales, 2017; Hassan, Hollander, van Lent, and Tahoun,

[3] A cross-country view is that policymakers may have incentives to accommodate credit expansions, even when doing so increases fragility Herrera, Ordoñez, and Trebesch (2020).

2019; Akey, Heimer, and Lewellen, 2021; Grotteria, 2023; Akey, Gupta, and Lewellen, 2025). 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 federal 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 the Harvard Dataverse (Stewart, 2021), which compiles official membership records from the Congressional Research Service and the Inter-university Consortium for Political and Social Research. We focus on Congresses 103–116 (1993–2021) and, for each legislator, we observe 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. We identify representatives who served on the House FSC (committee code 113) in a given Congress.

This committee oversees banking, housing, and financial regulation, and its members hold formal authority over mortgage credit markets and housing finance policy. For instance, the authority of the committee encompasses oversight of conforming loan limits that determine eligibility for GSE backing, approval and review of changes to GSE pricing frameworks (e.g., loan-level price adjustments), and supervision of FHA mortgage insurance programs that determine access to credit for minority and low-income populations, and for first-time homebuyers. Further, the committee holds hearings and advances legislation affecting mortgage underwriting standards, secondary-market risk retention, foreclosure mitigation, and community development block grants. Through oversight hearings, statutory reforms, and regulatory scrutiny, FSC membership provides legislators with formal authority over the supply and pricing decisions, which are consequential for the allocation of mortgage credit.

We then 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 that quantifies how successful each member of Congress is at advancing bills through the legislative process, combining information on bill sponsorship, progression through the committees, and ultimate enactment (Volden and Wiseman, 2014). The data also includes member attributes, including political ideology (DW–NOMINATE scores), seniority, gender, minority status, and whether the legislator is a new entrant to Congress. 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 congressional districts whose representatives served on the FSC in a given Congress with those who never served on the FSC. To do so, we use data on annual demographic and economic indicators from the American Community Survey 1-Year Estimates that are available only from 2005. 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 these 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 income averages around \$62,000, homeownership rates are roughly 64 percent, and unemployment and poverty rates are comparable at 6% and 11%, respectively. Overall, the balance across several observable characteristics supports the interpretation of FSC membership as orthogonal to local socioeconomic conditions.

CoreLogic Deed & Tax records. We use the near-universe of housing transactions 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 winsorize the distribution of sale prices at the 1 percentiles to mitigate the influence of outliers. We exclude intra-family transactions, as these often occur at non-market prices. We also remove properties involved in foreclosures, as well as those with unusually high transaction frequency (more than one transaction per day or more than five per year). Finally, we restrict the sample to single-family residences, condominiums, duplexes, and apartments. Because our identification strategy requires both 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 use 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) while for Congresses 115 through 116, we use TIGER/Line congressional district shapefiles from the U.S. Census Bureau. Using these spatial boundaries, we assign each property in the CoreLogic data to its corresponding congressional district in each Congress. We link a property to congressional districts only for periods after the property was built, based on its recorded year of construction.

We define *treated* those properties located in districts whose representative serves on the House FSC in a given Congress. The properties can be treated for two distinct reasons: first, when its incumbent representative is 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. Therefore, our spatially matched longitudinal dataset captures both sources of variation.

We begin by estimating a TWFE specification that uses all residential transactions in our sample across the contiguous U.S.. We sharpen the comparison by narrowing the sample to properties within 10, 5, or 3 km of a congressional district boundary, matching each property to its nearest adjacent district. The procedure is explained in Appendix A. 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 (*pair-id*) linking adjacent congressional districts.

Our dependent variable is the natural logarithm of the sale amount. Table 3 reports summary statistics for key characteristics at the property level, comparing treated and control units within 5 km of matched district pairs. 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 similar. Average living area, lot size, and the number of bedrooms and bathrooms are nearly identical, and the average age of properties differs by only about two years. These similarities reinforce the validity of our within-pair identification strategy. Together with the spatial border design, these balance tests support interpreting subsequent price differences as holding constant underlying observable housing quality and 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.[4]

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 around the appointment of a district's representative to the FSC. We further exploit the lender dimension by constructing a lender–district–year panel. This analysis enables us to shed light on whether and how financial institutions adjust their lending across markets in response to changes in political representation.

To capture changes in secondary-market activity, we use data from the Freddie Mac Single-Family Loan-Level Dataset, 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 then examine whether the increase in mortgage origination and securitization is driven

[4] In Appendix A, we detail the procedure used to map each data source geographically to congressional districts.

by (i) changes in housing finance regulation and program design, and/or (ii) favoritism of legislator's district by private financial institutions. To assess changes in housing finance regulation, we compile data from the FHFA on conforming loan limits for Fannie Mae and Freddie Mac and from the 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 translates into more generous limit adjustments in the member's home district.

To examine favoritism by lenders toward members' home districts, we go beyond mortgages and study both broader credit supply and political giving by private financial institutions. Specifically, we use data on small-business lending from the Community Reinvestment Act (CRA) Analytics Data Tables maintained by the Federal Reserve Board from 2005 to 2021. The dataset covers both origination and purchases of loans for firms with gross annual revenues below $1 million. We aggregate these data to the congressional district–year level and test how CRA lending changes around FSC transitions. 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 change their campaign contributions to legislators as they enter and exit the FSC, relative to non-lenders.

Next, we study changes in housing-supply and development policy around FSC transitions. We use project-level data from the LIHTC Database maintained by the 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 ones implemented by the HUD, and the FHA. 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.[5] 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. We restrict both datasets to transactions issued by agencies under the FSC's oversight.[6] Although the allocation of federal contracts or assistance fall outside the Committee's direct legislative jurisdiction, this restriction helps isolate federal spending most plausibly influenced by FSC membership.

4 Political Influence and Housing Returns

4.1 Two-Way Fixed Effects

In this section, we estimate a TWFE specification that compares changes in house prices for the same home around FSC transitions. Our repeat-sales specification is given by:

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

where $\log(\text{SaleAmt})_{idt}$ is the log sale price of property i in district d on date t, and Treat_{it} is an indicator variable equal to one in a year 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-discontinuity design, we refine the TWFE specification in two ways. First, we restrict the sample to properties located within a fixed distance (10, 5, or 3 km) of a congressional district boundary and match each property to its nearest adjacent district. Let p

[5] See, U.S. House Committee on Financial Services (2019) and U.S. House Committee on Financial Services (2017) for examples of the authorization and oversight plan of the FSC.

[6] Specifically, we consider the Department of the Treasury, the HUD, Securities and Exchange Commission, Consumer Financial Protection Bureau, the FHFA, and National Credit Union Administration.

denote the *unordered* pair formed by the property's district and its nearest neighboring district. Second, we interact all fixed effects with p, allowing time-varying shocks to differ flexibly across each boundary pair. We estimate:

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

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-year-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 level to account for spatial correlation in unobserved shocks affecting properties in the same district-year.

Table 4 presents the estimated effects of congressional representation on the FSC on local house prices. Across specifications, the coefficient β is positive and statistically significant. In the full sample of U.S. housing transactions between 1990 and 2020, the estimated semi-elasticity of transaction prices with respect to FSC representation is 0.01. This estimate implies that the average home values increase by roughly 1 percent when the property's district gains representation on the FSC. The effect is stable as we restrict the sample to properties closer to district boundaries—1.8 percent within 10 km, 1.5 percent within 5 km or 3 km of a border. The results provide evidence that FSC membership is reflected in local house prices.

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 implement a stacked DiD design (Cohn, Liu, and Wardlaw, 2022; Gormley and Matsa, 2011) which we combine to the border-discontinuity approach. We estimate separate specifications for representatives' *first* entry into and *first* exit from the House FSC. For each property, we define the treatment as the first Congress within our sample in which the district the property is located either gains or loses FSC representation. Properties in districts whose representatives never serve on the FSC form the control group. For each entry or exit event, we construct symmetric event windows, assigning a pseudo event-year to each control property. The stacked estimator, therefore, compares treated properties only to never-treated ones, ensuring that each treated observation contributes exactly once to a clean pre/post comparison. Specifically, we estimate:

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

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 in a given cohort. State-by-year-month-by-pair-id-by-cohort fixed effects, $\lambda_{s\times ym\times p\times c}$, absorb local shocks common to both sides of a boundary pair and allow for time-varying differences 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 level but now interacted with cohort.

Figure 2 plots the estimated event-time coefficients (semesters) for entry and exit in a 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 5 percent relative to first semester after the election. 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, albeit statistically insignificant.[7]

Table 5 reports stacked DiD estimates for FSC entry (Panel A) and exit (Panel B). Across distance bands, price appreciation at entry is robust: approximately 7 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 average effects for properties closer to the 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. First, we expand the event window to [-3,+3] and [-4,+4] years around FSC transitions to address potential selection

[7]This drop likely reflects November elections when incumbents' future committee assignments plausibly become more predictable.

arising from combining a narrow event window with property fixed effects: it may over-represent transactions by flippers. Second, we exclude properties distant from any developed area on the opposing side, ensuring that treated and control properties are drawn from comparable local markets. Third, 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, which allows for comparisons across adjacent states. 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 significant changes in local house prices.

5 Potential Mechanisms

We next examine the mechanisms through which representation on the FSC may affect local house prices. To do so, we estimate a stacked DiD specification that compares changes in outcomes around the first time a district gains or loses FSC representation to those in districts never represented by an FSC member, within the same state. As the outcomes are measured at the district-year level, we define treatment at the congressional-district level as well. Specifically, we estimate:

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

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 $\lambda_{s\times t\times c}$ 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 appropriate estimation that vary across the several outcomes object of our study. 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 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, which is a consistent estimator for count variables as argued by Silva and Tenreyro (2006) and Cohn et al. (2022).[8]

5.1 Housing finance channel

5.1.1 Changes in loan volumes and loan composition

Increase in 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 and from the FSC. Following FSC entry, the total value of originated mortgages increases by 3.7 percent. More strikingly, the volume of loans purchased by the GSEs at origination increases by about 5 percent. Both the estimates are statistically and economically significant. By contrast, the estimates for loan originations by Banks and Credit Unions (BCU) or Private institutions as well as for loans insured by the FHA are all statistically indistinguishable from zero. Finally, Figure B.1a examines changes in GSE 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 toward GSE-eligible origination rather than a uniform expansion of credit.[9]

When representatives exit the FSC, these patterns reverse. Both total and GSE-purchased origination amounts fall significantly, by -8.1 and -7.2 percent, respectively. The symmetric entry and exit effects reinforce the interpretation that local GSE activity responds to changes in political representation. Unlike entry, we do see a decline across the mortgages purchased at origination by other institutions as well.

We exploit the granularity of the HMDA data to further distinguish between changes at the extensive and intensive margins in our results on the composition of lending. We estimate

[8] Most of our datasets begin in 1990. For those that are only available after the start of our analysis period, we use the same stacked DiD specification, constructing cohorts from the first year available in each dataset.

[9] The GSEs are not legally required to purchase every conforming loan. Instead, they routinely acquire mortgages via contractual commitments (forward/advance commitments) in which sellers commit to deliver specified loans or Mortgage Backed Securities (MBS) once originated. See, for example, the description of advance commitments in Fieldhouse and Mertens (2017) and Fieldhouse, Mertens, and Ravn (2018). In mortgage and MBS' markets, undelivered commitments are commonly resolved through close-out ("pair-off") and related settlement conventions, which specify how parties' net positions and determine compensation when delivery does not occur as contracted; see the Securities Industry and Financial Markets Association (SIFMA) (2023).

analogous specifications at the lender–district–year level (Table B.1). The specification includes a rich set of fixed effects—lender × district × cohort and lender × state × year × cohort, which allows to estimate changes in the intensive margin of mortgage origination (within-lender-geography changes).

Consistent with the district-level results, when a district's representative joins the FSC, we observe within-lender-geography increases in lending volume purchased by the GSEs and, to a lesser extent, insured by the FHA. Notably, the effect size at the intensive margin is smaller compared to the district-level results, suggesting that much of the effect documented in Table 7 is driven by changes at the extensive margin. We still find no corresponding increase for loans financed through private-label securitization, and we now find that the largest within-lender increase is in loans sponsored by the BCUs. As before, we see a symmetric effect on exits. These results suggest that the compositional shift is mostly driven by the extensive margin.

Overall, the evidence indicates a supply-side response to increased congressional influence over housing policies. Committee membership appears to improve access to GSE funding for lenders in the member's district, increasing the total volume of origination as well as the loan amount ultimately purchased by the GSEs.

No change in borrower composition. We next examine secondary-market data from the Freddie Mac single-family loan-level dataset, which records mortgages purchased by Freddie Mac, including loans that were not securitized at origination. We restrict attention to mortgages purchased for single-family properties and average the variables of interest at the district-year level. As shown in Table 8, FSC entry is not associated with meaningful changes in borrower or loan characteristics: the estimated coefficients are economically small and statistically indistinguishable from zero. Specifically, we do not find a change in FICO scores, LTV ratios, DTI ratios, and origination interest rates. Taken together, our estimates suggest that GSE expansion is not driven by the underwriting of riskier borrowers.

When representation ends, as before, we do not see economically meaningful changes in borrower or loan characteristics. Only for the LTV ratios and the interest rates we observe statistically significant but economically small changes. The average LTV ratio increases by 0.25 percentage points, which represents an increase of 0.3% relative to a median of 80.1. Similarly, the average interest rate at origination increases by 0.004 which represents an increase of 0.08% relative to a median rate of 5.

Finally, we test whether the increase in GSE purchases associated with FSC representation is followed by a change in loan performance. Table 9 reports results for a battery of delinquency and default outcomes. Following FSC entry, loans originated in treated districts show no deterioration in repayment behavior. If anything, short-term delinquency rates decline slightly and across all measures, including 30+, 60+, 90+, and 180+ day delinquencies and ultimate defaults. Around exits, we do not see any systematic improvement or deterioration.

The unchanged borrower characteristics and loan performance 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.

5.1.2 Evidence of local increases in conforming loan limits

One mechanism that can explain the documented increase in GSE purchase volumes operates through eligibility rules— most directly, conforming loan limits that determine which mortgages qualify for purchase by the GSEs and, separately, for FHA insurance. County-level data on these limits are publicly available from 2009 onward, allowing us to test whether FSC representation affects this specific margin.[10]

There is strong anecdotal evidence that it does. During the 2008 negotiations over the *Housing and Economic Recovery Act*, FSC members—most prominently Representative Gary Miller of California—argued that their constituents were unfairly excluded from federally supported mortgages under the existing caps. The resulting legislation raised the conforming limit from $417,000 to $729,750 for Fannie Mae and Freddie Mac, and proportionally for FHA programs, covering precisely the most expensive housing markets in their home states.[11] A similar episode unfolded in 2013, when 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 even expanded the limits for some counties.[12] These episodes

[10]FHA limits were set at the local level even before 2009. However, we found the data only for a few states in few years (see for instance Single Family FHA Home Mortgage Loan Limits Massachusetts - 2000). Therefore, in Table B.3, we broaden the sample and repeat the analysis using realized limits from 1990. Realized limits are computed as the maximum mortgage amount issued in a congressional-district and year. The results are quantitatively similar and statistically significant.

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

[12]FHFA Press Release, "FHFA Announces 2014 Conforming Loan Limits," November 26, 2013; Congressional

illustrate how committee membership can translate into local regulatory benefits.

Table 10 reports stacked DiD estimates of the effect of FSC representation on district-level conforming loan limits for both GSE and FHA programs. In Panel A, conforming loan limits rise sharply following FSC entry. The GSE limit increases by roughly \$6,050 and the FHA limit by \$4,900, although this latter is not statistically significant. Symmetrically, in Panel B we show that at exit, limits sharply decrease by roughly \$3,450 for the GSE and \$15,770 for the FHA (both statistically and economically significant). The effects are economically large implying a 2.5 percent increase in effective purchasing power for borrowers, 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 having limits approximately twice as large as those for 1-unit properties.

These estimates provide direct evidence that committee representation affects the regulation on credit access for constituents. Combined with the significant increase in GSE purchases documented above, the results imply that changes in conforming limits may be a plausible channel behind the expansion of GSE securitization activity and the subsequent loan purchases.

5.1.3 Favoritism by financial institutions

Increase in 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 transitions. 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 7 percent when a representative joins the Committee and decline by roughly 4 percent when the member leaves, with the latter being statistically insignificant. These results are consistent with broader credit expansion in districts represented by FSC members.

Increase in political contributions by financial institutions. To shed light on whether the increase in mortgage credit reflects favoritism by local lenders, we examine campaign contributions from financial institutions. Specifically, we estimate a triple-differences empirical specification that compares changes in contributions from HMDA-linked corporate entities rel-

---

Letter from Rep. Carolyn Maloney et al. to FHFA Acting Director Edward DeMarco, November 20, 2013.

ative to non-HMDA ones around a district's first FSC entry or exit. The results, reported in Table 12, indicate that contributions from financial institutions rise significantly following FSC entry and decline after exit. In particular, contributions from HMDA-linked corporations increase by roughly 18 percent relative to non-HMDA entities after a representative joins the Committee, whereas they fall by about 23 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.[13] The symmetry of the entry and exit effects reinforces the interpretation based on favoritism by financial institutions.

5.2 No change in housing-supply and development policies

Finally, we examine channels outside mortgage credit but still plausibly within the remit of FSC. Specifically, we study new construction activity, the allocation of affordable housing programs, and federal procurement contracts.

No change in construction activity. Table 13 reports stacked DiD estimates for building permits at the district–year level. The number of buildings, and the number of units decline by roughly 7 percent around FSC entry and and increase by 1 percent around exit. However, results are statistically significant only for the number of units around entry but not exit. At the same time, we find no change in the value of building permits around entry or exit.

These results suggest that committee representation does not affect short-run residential construction in the member's district. Indeed, key determinants of new construction—zoning, permitting rules, and land-use regulation—are set primarily at the state and, especially, local level, and are unlikely to mechanically respond to shifts in federal committee influence. The muted supply response reinforces our main conclusion that the house-price effects of FSC entry and exit are unlikely to reflect changes in new construction.

No change in affordable housing. Table 14 reports stacked DiD estimates for the allocation of 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

[13] Table B.4 presents a robustness check using employees' contributions, distinguishing between those working for bank-related and non-bank-related institutions.

moderate increases of 15 to 32 percent in total and low-income units. Although the exit effects are statistically significant, the economic magnitudes are not large enough to explain a district-level 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 influence house prices within the two-year event window (HUD, 2023). Furthermore, the similar direction of coefficients across entry and exit cannot explain the opposite-signed house price responses observed at those events. Taken together, changes in housing supply cannot explain the relationship between FSC representation and local house price changes.

No change in federal contracts and assistance. Table 15 presents stacked DiD estimates of whether FSC representation is associated with changes in federal resource allocation, distinguishing procurement contracts (FPDS) from financial assistance awards (FABS). In Panel A (Entry), the estimates are positive for both outcomes but statistically indistinguishable from zero. Panel B (Exit) also shows no statistically significant change for contract obligations or assistance. Taken together, we find little support for non-credit mechanisms as explanations for the house-price effects of FSC representation.

6 Political Benefits

Finally, a natural question is whether elected officials who influence housing policy are themselves rewarded electorally for the associated increase in house prices. This is unclear ex ante. On the one hand, higher prices raise the wealth of incumbent homeowners. On the other hand, they reduce affordability for prospective buyers, while policies that relax credit constraints and expand mortgage access may nonetheless appeal to those same voters. The net electoral effect thus depends on which channel dominates.

To assess whether FSC members benefit electorally from local house price growth, we examine the relationship between changes in house prices and political outcomes, comparing congressional districts whose representatives serve on the FSC to those that do not have a representative on the FSC. We measure political benefits through three outcomes: (i) the incumbent's probability of re-election, (ii) the number of individual contributors, or (iii) the volume of political contributions made by individual constituents. The focus on contributions made only by individuals complements earlier evidence on increased political contribution by financial institutions.

We construct a measure of local house price growth by residualizing log transaction prices with respect to property fixed effects and year-by-month fixed effects, and then averaging the resulting residuals at the congressional district–Congress level. Let $\widetilde{\log P}_{dt}$ denote this residualized average house price in district d over the election cycle of two years (one Congress) preceding the election at time t. By removing property-level heterogeneity and aggregate time effects, this measure is plausibly orthogonal to national trends and differences in the composition of the housing stock.

Our main specification interacts FSC representation with residualized house prices:

$$y_{dt} = \beta_0 \, \text{Treated}_{dt} + \beta_1 \widetilde{\log P}_{dt} + \beta_2 \left(\text{Treated}_{dt} \times \widetilde{\log P}_{dt} \right) + \alpha_d + \gamma_{st} + \varepsilon_{dt}, \tag{5}$$

where y_{dt} denotes the electoral outcome of interest. Depending on the specification, y_{dt} is either an indicator equal to one if the incumbent is re-elected, the log of total individual contributions, or the number of individual contributors. The variable Treated_{dt} is an indicator equal to one if the district's representative serves on the FSC in Congress t. District fixed effects, α_d, absorb time-invariant characteristics of each congressional district, while state-by-congress fixed effects, γ_{st}, control flexibly for state-level political and economic conditions within each election cycle.

The coefficient of interest is β_2, which captures the differential electoral benefits associated with changes in house prices in FSC-represented districts (treated districts) relative to non-FSC districts. We estimate Equation (5) by OLS when the dependent variable is the re-election indicator or log individual contributions, and by Poisson pseudo-maximum likelihood when the dependent variable is the number of individual contributors.

We further consider a specification focusing on episodes of strong local house price growth. Rather than interacting FSC representation with the continuous residualized price measure, we interact it with an indicator for districts experiencing the highest price growth within their state and year. We define HighPrice_{dt} to be an indicator for the top 25% of the average residualized log house prices within state s over the two years (one Congress) preceding the election at time t. The specification is:

$$y_{dt} = \beta_0 \, \text{Treated}_{dt} + \beta_1 \, \text{HighPrice}_{dt} + \beta_2 \, (\text{Treated}_{dt} \times \text{HighPrice}_{dt}) + \alpha_d + \gamma_{st} + \varepsilon_{dt}, \tag{6}$$

where all other variables are defined as above. The coefficient β_2 now captures whether FSC incumbents disproportionately receive greater electoral support during periods in which their districts experience unusually high house price growth relative to other districts in the same state.

Table 16 reports the results. Across all specifications, we find consistent evidence that house price growth, when combined with FSC representation, leads to improved political outcomes for incumbent politicians. Consider first the continuous interaction specification in Equation (5). The interaction between treatment and residualized house prices is positive and statistically significant across all three outcomes. In Panel A, we see that in control districts, the estimates imply that a one percent increase in residualized house prices is associated with an approximately 0.15 percentage point higher re-election probability. This effect increases to 0.45 for districts represented by FSC incumbents, i.e., an additional 0.3 percentage point relative non-FSC members. Similarly, in Panel B, the same price increase is associated with an additional 2.6 percent increase in the number of individual contributors for districts represented by FSC incumbents. Lastly, in Panel C, the corresponding estimate implies an additional 0.5 percent increase in the total amount of political contributions made by individuals. These results are even more pronounced when focusing on districts that experience house price growth in the top quartile.

Overall, the evidence indicates that FSC members benefit electorally when local housing markets perform strongly. These findings are consistent with the hypothesis that policy-driven house price increases translate into tangible political support, providing a channel through which the influence of the FSC generates electoral benefits for its members.

7 Conclusion

Our findings show that political representation on the FSC has sizable effects on local housing markets. Exploiting the staggered timing of FSC membership and a stacked DiD border-discontinuity design, we find that house prices rise by about 5 percent when a district's representative joins the FSC and fall symmetrically upon exit. These effects are driven by greater mortgage origination and securitization by the GSEs, rather than by changes in construction activity or other local fundamentals.

Our results highlight that local housing values are shaped not only by macroeconomic conditions and fundamentals, but also by differential access of elected representatives to federal policymaking authority. Congressional influence over credit allocation is thus directly capitalized into local housing values, translating political power into gains for homeowners. Understanding how political power shapes the geographic allocation of credit is therefore central to understanding the distribution of gains from homeownership itself. So, our findings have broader implications for the study of wealth inequality. If representation on powerful committees affects the value of housing— the principal asset held by most households— then political institutions can redistribute wealth through changes in housing prices, operating as a channel in addition to taxes and transfers.

References

Adelino, M., A. Schoar, and F. Severino (2025). Credit supply and house prices: Evidence from mortgage market segmentation. *Journal of Financial Economics 163*, 103958.

Agarwal, S., G. Amromin, I. Ben-David, and S. Dinc (2018). The politics of foreclosures. *Journal of Finance 73*(6), 2677–2717.

Akey, P. (2015). Valuing changes in political networks: Evidence from campaign contributions to close congressional elections. *Review of Financial Studies 28*(11), 3188–3223.

Akey, P., N. Gupta, and S. Lewellen (2025). Politics and finance. *European Corporate Governance Institute–Finance Working Paper*.

Akey, P., R. Z. Heimer, and S. Lewellen (2021). Politicizing consumer credit. *Journal of Financial Economics 139*(2), 627–655.

Akey, P. and S. Lewellen (2017). Policy uncertainty, political capital, and firm risk-taking. *Working Paper*.

Baker, A. C., D. F. Larcker, and C. C. Wang (2022). How much should we trust staggered difference-in-differences estimates? *Journal of Financial Economics 144*(2), 370–395.

Bertrand, M., M. Bombardini, R. Fisman, and F. Trebbi (2020). Tax-exempt lobbying: Corporate philanthropy as a tool for political influence. *American Economic Review 110*(7), 2065–2102.

Bombardini, M. and F. Trebbi (2020). Empirical models of lobbying. *Annual Review of Economics 12*, 391–413.

Bombardini, M. and F. Trebbi (2025). The political power of firms in modern democracies. *Handbook of Political Economy*.

Bonica, A. (2024). Database on ideology, money in politics, and elections: Public version 4.0. Computer file.

Campbell, J. Y. and J. F. Cocco (2007). How do house prices affect consumption? evidence from micro data. *Journal of Monetary Economics 54*(3), 591–621.

Card, D. and A. Krueger (1994). Minimum wages and employment: A case study of the fast-food industry in new jersey and pennsylvania. *American Economic Review 84*(4), 772–93.

Cohen, L., J. Coval, and C. Malloy (2011). Do powerful politicians cause corporate downsizing? *Journal of Political Economy 119*(6), 1015–1060.

Cohn, J. B., Z. Liu, and M. I. Wardlaw (2022). Count (and count-like) data in finance. *Journal of Financial Economics 146*(2), 529–551.

Davis, M. A. and J. Heathcote (2007). The price and quantity of residential land in the united states. *Journal of Monetary Economics 54*(8), 2595–2620.

Dell, M. (2010). The persistent effects of peru's mining mita. *Econometrica 78*(6), 1863–1903.

Diamond, R. and T. McQuade (2019). Who wants affordable housing in their backyard? an equilibrium analysis of low-income property development. *Journal of Political Economy 127*(3), 1063–1117.

Diamond, R., T. McQuade, and F. Qian (2019). The effects of rent control expansion on tenants, landlords, and inequality: Evidence from san francisco. *American Economic Review 109*(9), 3365–94.

Faccio, M., R. W. Masulis, and J. J. McConnell (2006). Political connections and corporate bailouts. *Journal of Finance 61*(6), 2597–2635.

Federal Housing Finance Agency (2026). Fannie mae & freddie mac: Supervision. Accessed February 11, 2026.

Fieldhouse, A. J. and K. Mertens (2017). A narrative analysis of mortgage asset purchases by federal agencies. Technical report, National Bureau of Economic Research.

Fieldhouse, A. J., K. Mertens, and M. O. Ravn (2018). The macroeconomic effects of government asset purchases: Evidence from postwar us housing credit policy. *Quarterly Journal of Economics 133*(3), 1503–1560.

Giacoletti, M. (2021, 03). Idiosyncratic risk in housing markets. *The Review of Financial Studies 34*(8), 3695–3741.

Glaeser, E. and J. Gyourko (2018). The economic implications of housing supply. *Journal of Economic Perspectives 32*(1), 3–30.

Gormley, T. A. and D. A. Matsa (2011). Growing out of trouble? corporate responses to liability risk. *Review of Financial Studies 24*(8), 2781–2821.

Greenwald, D. L. and A. Guren (2025). Do credit conditions move house prices? *American Economic Review 115*(10), 3559–3596.

Grotteria, M. (2023). Follow the money. *The Review of Economic Studies 91*(2), 1122–1161.

Guren, A. M. and T. J. McQuade (2020). How do foreclosures exacerbate housing downturns? *The Review of Economic Studies 87*(3), 1331–1364.

Hassan, T. A., S. Hollander, L. van Lent, and A. Tahoun (2019). Firm-level political risk: Measurement and effects. *The Quarterly Journal of Economics 134*(4), 2135–2202.

Herrera, H., G. Ordoñez, and C. Trebesch (2020). Political booms, financial crises. *Journal of Political Economy 128*(2), 507–543.

HUD (2023). Low-income housing tax credit (lihtc) database. Accessed November 16, 2025.

Iacoviello, M. (2005). House prices, borrowing constraints, and monetary policy in the business cycle. *American Economic Review 95*(3), 739–764.

Igan, D., P. Mishra, and T. Tressel (2012). A fistful of dollars: Lobbying and the financial crisis. *NBER Macroeconomics Annual 26*(1), 195–230.

Keys, B. J., T. Mukherjee, A. Seru, and V. Vig (2010). Did securitization lead to lax screening? evidence from subprime loans. *The Quarterly journal of economics 125*(1), 307–362.

Kuhn, M., M. Schularick, and U. I. Steins (2020). Income and wealth inequality in america, 1949–2016. *Journal of Political Economy 128*(9), 3469–3519.

Landvoigt, T., M. Piazzesi, and M. Schneider (2015). The housing market(s) of san diego. *American Economic Review 105*(4), 1371–1407.

Lewis, J. B., B. DeVine, L. Pitcher, and K. C. Martis (2013). Digital boundary definitions of united states congressional districts, 1789–2012. Data file and codebook. Retrieved from `https://cdmaps.polisci.ucla.edu` on September 27, 2025.

McCartney, W. B. (2021). Does household finance affect the political process? evidence from voter turnout during a housing crisis. *Review of Financial Studies 34*(2), 949–984.

McCartney, W. B., J. Orellana-Li, and C. Zhang (2024). Political polarization affects households' financial decisions: evidence from home sales. *Journal of Finance 79*(2), 795–841.

Mian, A., K. Rao, and A. Sufi (2013). Household balance sheets, consumption, and the economic slump. *The Quarterly Journal of Economics 128*(4), 1687–1726.

Mian, A. and A. Sufi (2014). What explains the 2007–2009 drop in employment? *Econometrica 82*(6), 2197–2223.

Mian, A., A. Sufi, and F. Trebbi (2010). The political economy of the us mortgage default crisis. *American Economic Review 100*(5), 1967–1998.

Piazzesi, M. (2025). Presidential address: Housing betas. *The Journal of Finance 80*(6), 3103–3136.

Piazzesi, M. and M. Schneider (2016). Housing and macroeconomics. *Handbook of macroeconomics 2*, 1547–1640.

Piazzesi, M., M. Schneider, and S. Tuzel (2007). Housing, consumption and asset pricing. *Journal of Financial Economics 83*(3), 531–569.

Poterba, J. M. (1984). Tax subsidies to owner-occupied housing: An asset-market approach. *The Quarterly Journal of Economics 99*(4), 729–752.

Roth, J., P. H. Sant'Anna, A. Bilinski, and J. Poe (2023). What's trending in difference-in-differences? a synthesis of the recent econometrics literature. *Journal of Econometrics 235*(2), 2218–2244.

Securities Industry and Financial Markets Association (SIFMA) (2023). *Uniform Practices for the Clearance and Settlement of Mortgage-Backed Securities and Other Related Securities* (Chapter PDF ed.). Securities Industry and Financial Markets Association (SIFMA). Includes TBA/settlement conventions (see also other chapters in the Uniform Practices Manual); accessed 2026-02-12.

Silva, J. S. and S. Tenreyro (2006). The log of gravity. *Review of Economics and statistics*, 641–658.

Stewart, C. (2017). Committee assignments. Harvard Dataverse.

Stewart, C. (2021). House Standing Committee Assignments, 103rd - 117th Congress.

U.S. Census Bureau (2025). Housing vacancies and homeownership (current release). Housing Vacancy Survey (HVS). Accessed February 10, 2026.

U.S. Congress (2026a). 12 u.s.c. § 1454(a)(2): Limitations on mortgage purchases (freddie mac conforming loan limits). United States Code (via Legal Information Institute). Accessed February 11, 2026.

U.S. Congress (2026b). 12 u.s.c. § 1716: Declaration of purposes (fannie mae charter act). United States Code (via Legal Information Institute). Accessed February 11, 2026.

U.S. Congress (2026c). 12 u.s.c. § 1717(b)(2): Limitations on maximum original principal obligation (fannie mae conforming loan limits). United States Code (via Legal Information Institute). Accessed February 11, 2026.

U.S. House Committee on Financial Services (2017). Authorization and oversight plan of the committee on financial services: 115th congress. Committee report/document (PDF).

U.S. House Committee on Financial Services (2019). Oversight plan of the committee on financial services for the 116th congress. PDF. Accessed 2026-02-11.

van Binsbergen, J., J. F. Cocco, M. Grotteria, and S. L. Naaraayanan (2026, 01). The impact of carcinogenic risk exposure on housing values: Estimates from chemical reclassifications. *Review of Financial Studies*, hhag006.

Volden, C. and A. E. Wiseman (2014). *Legislative Effectiveness in the United States Congress: The Lawmakers.* Cambridge University Press.

Volden, C. and A. E. Wiseman (2018). Legislative effectiveness in the united states senate. *Journal of Politics 80*(2), 731–735.

Wallace, N. E. and R. A. Meese (1997). The construction of residential housing price indices: a comparison of repeat-sales, hedonic-regression, and hybrid approaches. *Journal of Real Estate Finance and Economics 14*(1), 51–73.

Zingales, L. (2017). Towards a political theory of the firm. *Journal of Economic Perspectives 31*(3), 113–30.

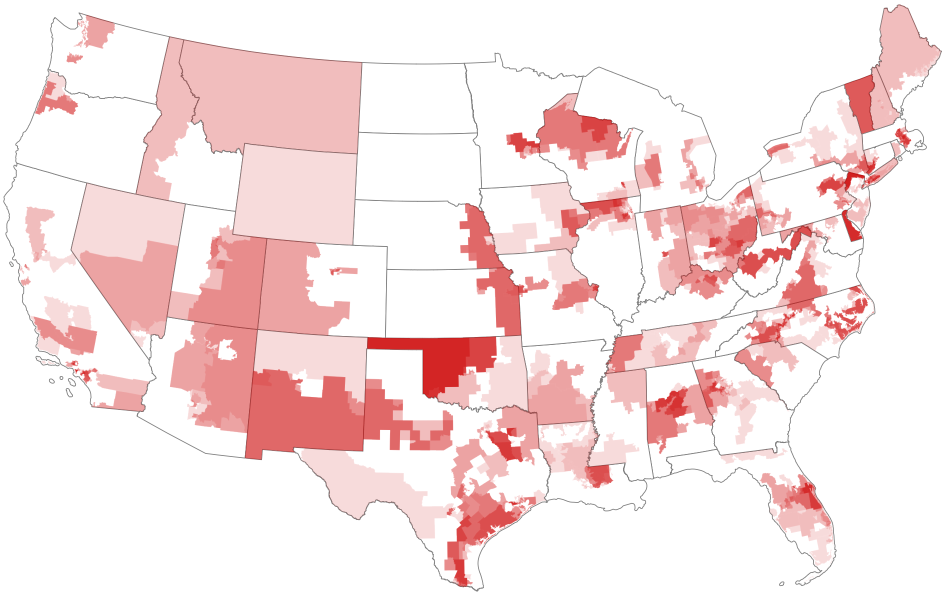
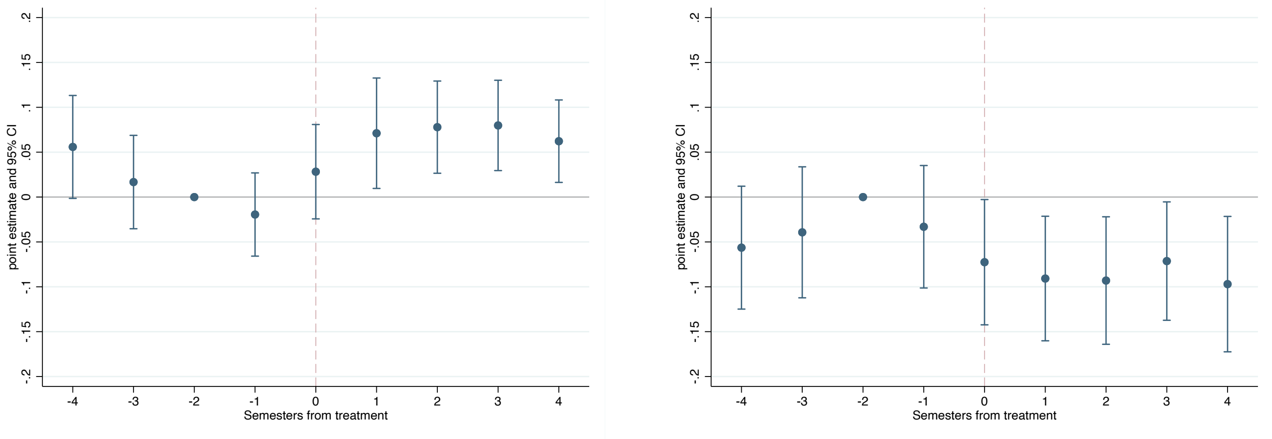


Figure 1: Cumulative exposure of U.S. congressional districts to the House Financial Services Committee, Congresses 103–116. *Notes*: The map shows all contiguous U.S. districts that were represented by a legislator serving on the Financial Services Committee in any Congress between 1993 and 2020. Each treated district is shaded in translucent red; deeper shades indicate districts treated in multiple Congresses. State borders are drawn from the 116$^{\text{th}}$ 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.



(a) Entry into the House FSC

(b) Exit from the House FSC

Figure 2: *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 level and interacted with cohort, to account for spatial correlation in unobserved shocks affecting properties in the same district-year 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. *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								
House price (US$)	6,201,659	286,250	298,439	9,800	119,780	200,000	342,825	1,940,000
Living area (Sq.ft)	5,637,466	1,920	939	622	1,264	1,691	2,326	5,600
Land area (Sq.ft)	5,730,252	24,566	60,335	580	5,569	8,640	15,523	440,398
Bedrooms (count)	4,292,252	3.2	1.0	1.0	3.0	3.0	4.0	6.0
Bathrooms (count)	4,971,585	2.4	1.0	1.0	2.0	2.0	3.0	6.0
Age (years)	5,554,950	34.4	30.0	0.0	8.0	28.5	52.8	117.5
Panel B: Control properties								
House price (US$)	16,878,033	257,326	280,195	5,200	102,250	176,500	307,500	1,700,000
Living area (Sq.ft)	15,455,001	1,922	933	630	1,263	1,693	2,336	5,600
Land area (Sq.ft)	15,856,728	26,006	63,257	671	5,600	8,600	16,117	445,619
Bedrooms (count)	11,480,443	3.2	1.0	1.0	3.0	3.0	4.0	6.0
Bathrooms (count)	13,773,472	2.4	1.0	1.0	2.0	2.0	3.0	6.0
Age (years)	15,219,406	32.0	29.5	0.0	6.0	25.3	50.0	117.0

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. We use *Treated*, an indicator for whether the property's district is represented on the FSC at time t. All regressions include property or property-by-pair fixed effects (γ_i or γ_{ip}) and either state-by-year-month or state-by-year-month-by-pair fixed effects ($\lambda_{s\times ym}$ or $\lambda_{ym\times p}$), as indicated in the table. 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 level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	All properties	Within 10 km	Within 5 km	Within 3 km
	(1)	(2)	(3)	(4)
Treated	0.0101***	0.0180***	0.0148***	0.0148***
	(0.00327)	(0.00474)	(0.00428)	(0.00451)
R^2	0.740	0.747	0.752	0.755
Observations	83,835,089	40,192,221	28,498,244	20,227,873
Fixed Effects:				
Property	Yes			
Property × Pair		Yes	Yes	Yes
State × Year–Month	Yes			
State × Year–Month × Pair		Yes	Yes	Yes

Table 5: Stacked Difference-in-Differences: FSC Representation and Effects on Housing Prices (Robustness Windows)

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 using alternative event windows and distance cutoffs. 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 stacked DiD specification with an event window of $[-2, 2]$ Congresses and border-based samples restricted to properties located within 10, 5, or 3 kilometers of a congressional district boundary, respectively. The dependent variable is $\log(\text{SaleAmt})_{idt}$. 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 ($\lambda_{s\times ym\times p\times c}$). The coefficient on *Treated*$\times$*Post* captures the average change in house prices after entry (or exit) relative to the pre-transition period. Standard errors are clustered at the congressional district–year–by-cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A. Entry

	10 km	5 km	3 km
	(1)	(2)	(3)
Treated × Post	0.066***	0.054***	0.042***
	(0.014)	(0.013)	(0.013)
R^2	0.790	0.798	0.803
Observations	2,344,854	1,613,360	1,127,920
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	-0.042***	-0.051***	-0.056***
	(0.016)	(0.017)	(0.018)
R^2	0.790	0.795	0.799
Observations	996,172	702,006	489,908
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) vary the event window; Column (3) excludes properties located within 5 km of the border but more than 10 km away from any other property; and Columns (4)–(5) 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 (and instead includes year-month × pair-id × cohort fixed effects). The coefficient on $Treated \times Post$ captures the average change in house prices after entry (or exit) relative to the pre-transition period. Standard errors are clustered at the congressional district–year–by-cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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

	Window $[-3, +3]$	Window $[-4, +4]$	Prop. dist.	5km (Alt FE A)	5km (Alt FE B)
	(1)	(2)	(3)	(4)	(5)
Treated × Post	0.048***	0.044***	0.054***	0.054***	0.066***
	(0.010)	(0.009)	(0.013)	(0.013)	(0.013)
R^2	0.789	0.782	0.798	0.798	0.795
Observations	2,630,323	3,618,677	1,608,239	1,613,406	1,616,557
Fixed Effects					
Property × pair-id × cohort	Yes	Yes	Yes	No	Yes
State × year × month × pair-id × cohort	Yes	Yes	Yes	Yes	No
Property × cohort	No	No	No	Yes	No
Year × month × pair-id × cohort	No	No	No	No	Yes

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

	Window $[-3, +3]$	Window $[-4, +4]$	Prop. dist.	5km (Alt FE A)	5km (Alt FE B)
	(1)	(2)	(3)	(4)	(5)
Treated × Post	-0.047***	-0.035***	-0.051***	-0.051***	-0.054***
	(0.014)	(0.011)	(0.017)	(0.017)	(0.016)
R^2	0.783	0.776	0.795	0.795	0.794
Observations	1,190,498	1,673,676	698,495	702,014	703,653
Fixed Effects					
Property × pair-id × cohort	Yes	Yes	Yes	No	Yes
State × year × month × pair-id × cohort	Yes	Yes	Yes	Yes	No
Property × cohort	No	No	No	Yes	No
Year × month × pair-id × cohort	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 $Treated \times Post$ captures the average change in house prices after entry (or exit) relative to the pre-transition period. Standard errors are clustered at the state-by–year–by-cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	Total	GSE	FHA	BCU	Private
Treated × Post	0.037**	0.050***	0.003	0.029	0.013
	(0.018)	(0.019)	(0.026)	(0.039)	(0.045)
R^2	0.958	0.947	0.955	0.967	0.912
Observations	10,070	10,066	10,017	9,982	9,981
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	−0.081***	−0.072***	−0.072***	−0.089***	−0.100**
	(0.016)	(0.017)	(0.022)	(0.031)	(0.045)
R^2	0.930	0.924	0.942	0.968	0.905
Observations	9,547	9,545	9,490	9,518	9,440
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' representative's *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: average borrower FICO score, and loan-to-value (LTV) and debt-to-income (DTI) ratios at origination, as well as 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 flexibly absorb time-invariant district heterogeneity and state-level shocks common to districts within each cohort. The coefficient on *Treated*×*Post* captures the average change in Freddie Mac purchase outcomes after entry (or exit) relative to the pre-transition period. Standard errors are clustered at the state-by–year–by-cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	Average FICO	Average LTV (Orig)	Average LTV (OC)	Average DTI (Orig)
Treated × Post	1.535	0.012	0.011	−0.023
	(1.339)	(0.113)	(0.113)	(0.067)
R^2	0.731	0.964	0.961	0.928
Observations	8,905	8,905	8,905	8,905
Fixed Effects				
District × Cohort	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes

Panel B: Exit

	Average FICO	Average LTV (Orig)	Average LTV (OC)	Average DTI (Orig)
Treated × Post	0.029	0.252***	0.194**	−0.065
	(1.073)	(0.083)	(0.080)	(0.047)
R^2	0.745	0.961	0.959	0.928
Observations	8,302	8,302	8,302	8,302
Fixed Effects				
District × Cohort	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes

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

Notes: The table reports stacked DiD estimates of the effects of congressional representation on the House Financial Services Committee (FSC) on the early performance of Freddie Mac–purchased loans at 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 variables measure loan 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. 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 change in early loan performance after entry (or exit) relative to the pre-transition period. Standard errors are clustered at the state-by–year–by-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	0.007	−0.009	−0.047**	−0.021***	−0.037**	−0.050***	−0.046**	−0.001
	(0.016)	(0.019)	(0.023)	(0.008)	(0.015)	(0.018)	(0.023)	(0.035)
R^2	0.064	0.196	0.324	0.045	0.100	0.148	0.190	0.133
Observations	8,905	8,905	8,905	8,905	8,905	8,905	8,905	8,834
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	0.015	0.019	0.007	0.004	0.022	0.017	0.003	0.024
	(0.017)	(0.019)	(0.022)	(0.008)	(0.016)	(0.019)	(0.023)	(0.033)
R^2	0.078	0.203	0.300	0.050	0.110	0.144	0.175	0.133
Observations	8,302	8,302	8,302	8,302	8,302	8,302	8,302	8,230
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 effects of congressional representation on the House Financial Services Committee (FSC) on federally determined mortgage loan limits at 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 variables measure statutory maximum loan sizes eligible for government or government-sponsored enterprise (GSE) guarantees, including the FHA loan limit and the conforming loan limit. The FHA loan limit establishes the maximum principal balance eligible for FHA-insured single-family mortgages under Title II of the National Housing Act, while the conforming loan limit defines the maximum mortgage amount eligible for purchase or securitization by Fannie Mae and Freddie Mac. 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. 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*$\times$*Post* captures the average change in loan limits after entry (or exit) relative to the pre-transition period. Standard errors are clustered at the state-by–year–by-cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	GSE limit	FHA limit
Treated × Post	6047.917**	4867.838
	(3020.660)	(4050.262)
R^2	0.939	0.949
Observations	4,518	4,518
Fixed Effects		
District × Cohort	Yes	Yes
State × Year × Cohort	Yes	Yes

Panel B: Exit

	GSE limit	FHA limit
Treated × Post	−3435.053**	−15768.464***
	(1331.188)	(3647.242)
R^2	0.958	0.958
Observations	3,351	3,351
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 effects of congressional representation on the House Financial Services Committee (FSC) on district-level small-business lending activity. 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 is the natural logarithm of the total value of small-business loans originated within each congressional district and year, based on Community Reinvestment Act (CRA) reporting data. 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 $Treated \times Post$ captures the average change in small-business lending after entry (or exit) relative to the pre-transition period. Standard errors are clustered at the state-by–year–by-cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	Entry	**Exit**
Treated × Post	0.070**	−0.042
	(0.031)	(0.028)
R^2	0.949	0.917
Observations	7,424	6,497
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 effects of congressional representation on the House Financial Services Committee (FSC) on campaign contributions received by incumbent House representatives. 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 is the natural logarithm of the total contribution amount from Bonica (2024). The first column reports contributions made by lenders appearing in HMDA originations data, the second column reports contributions made by lenders not reported in HMDA originations data (non-HMDA), and the last column tests for differences between the two groups. 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 change in campaign contributions after entry (or exit) relative to the pre-transition period. In the last column, the specification interacts all fixed effects with *Group*, an indicator for whether the lender appears in HMDA data. Standard errors are clustered at the state-by–year–by-cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	HMDA only	Non-HMDA only	All
Treated × Post	0.327***	0.153**	0.153**
	(0.0896)	(0.0619)	(0.0630)
Treated × Post × HMDA			0.175***
			(0.0646)
R^2	0.742	0.198	0.286
Observations	6,449	45,504	51,953
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	−0.481***	−0.256***	−0.256***
	(0.0747)	(0.0419)	(0.0426)
Treated × Post × HMDA			−0.225***
			(0.0501)
R^2	0.696	0.182	0.285
Observations	6,376	44,129	50,505
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 effects 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). 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 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. 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 change in residential construction activity after entry (or exit) relative to the pre-transition period. Standard errors are clustered at the state-by–year–by-cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	Value	Buildings	Units
Treated × Post	0.016	−0.052	−0.069**
	(0.026)	(0.035)	(0.031)
R^2	0.943	0.971	0.957
Observations	8,905	8,905	8,905
Fixed Effects			
District × Cohort	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes

Panel B: Exit

	Value	Buildings	Units
Treated × Post	0.017	0.015	0.013
	(0.020)	(0.025)	(0.026)
R^2	0.917	0.967	0.943
Observations	8,302	8,302	8,302
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 effects 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. 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 variables are constructed from project-level data 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. Outcomes 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*). 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 change in LIHTC allocations after entry (or exit) relative to the pre-transition period. Standard errors are clustered at the state-by–year–by-cohort level. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	Amount	Units	Rent-controlled	Low-income
Treated × Post	0.112	0.083	0.022	0.015
	(0.088)	(0.135)	(0.064)	(0.066)
R^2	0.663	0.709	0.696	0.675
Observations	6,228	2,982	8,404	8,404
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	0.181*	0.286**	0.082	0.056
	(0.101)	(0.134)	(0.061)	(0.062)
R^2	0.668	0.711	0.709	0.688
Observations	6,288	4,186	8,076	8,069
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 effects of congressional representation on the House Financial Services Committee (FSC) on federal *contracts* (left column) and federal *assistance* (right column). 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 variables are the logarithm of the sum of obligations aggregated to the congressional district–year level. The sample is restricted to HFSC-relevant awarding top-tier 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 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 change in federal contracts and assistance after entry (or exit) relative to the pre-transition period. Standard errors are clustered at the state-by–year–by-cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	Contracts	Assistance
Treated × Post	0.110	0.194
	(0.097)	(0.210)
R^2	0.937	0.782
Observations	2,884	4,121
Fixed Effects		
District × Cohort	Yes	Yes
State × Year × Cohort	Yes	Yes

Panel B: Exit

	Contracts	Assistance
Treated × Post	0.059	−0.173
	(0.096)	(0.186)
R^2	0.927	0.758
Observations	4,803	5,614
Fixed Effects		
District × Cohort	Yes	Yes
State × Year × Cohort	Yes	Yes

Table 16: Electoral Outcomes and Local House Prices

Notes: The table reports two-way fixed effects estimates of the relationship between local house price dynamics and electoral outcomes of congressional incumbents. Panel A reports results for an indicator equal to one if the incumbent is re-elected, while Panels B and C report results for the total number of individual contributors and the total amount of individual contributions, respectively. The main independent variable, *Treated*, is an indicator equal to one for districts whose representative serves on the House Financial Services Committee (FSC). We study heterogeneous effects by interacting *Treated* with residualized log house prices, $\widetilde{\log P}_{dt}$, and with an indicator for high-price growth districts, $HighPrice_{dt}$, defined as districts in the top quartile of house price growth within each Congress. Residualized house prices are obtained from property-level transaction data by estimating log prices with property and year-by-month fixed effects, and then aggregating the residuals to the congressional district–year level. All specifications include congressional district fixed effects and state-by-year fixed effects. Standard errors are clustered at the state–year level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	Continuous-price interaction (1)	High-price interaction (2)
Panel A. Re-election probability		
Treated	-0.00136	-0.0145
	(0.0198)	(0.0219)
$\widetilde{\log P}_{dt}$	0.152**	
	(0.0681)	
Treated $\times \widetilde{\log P}_{dt}$	0.286*	
	(0.152)	
HighPrice$_{dt}$		0.000877
		(0.0154)
Treated × HighPrice$_{dt}$		0.0826**
		(0.0411)
R^2	0.308	0.307
Observations	5,519	5,519
Panel B. Total number of individual donors		
Treated	0.254**	0.0957
	(0.101)	(0.115)
$\widetilde{\log P}_{dt}$	-1.403***	
	(0.385)	
Treated $\times \widetilde{\log P}_{dt}$	2.586**	
	(1.041)	
HighPrice$_{dt}$		-0.117
		(0.0841)
Treated × HighPrice$_{dt}$		0.766***
		(0.289)
Pseudo-R^2	0.788	0.788
Observations	5,772	5,772
Panel C. Total amount of individual contributions		
Treated	0.123***	0.115**
	(0.0458)	(0.0516)
$\widetilde{\log P}_{dt}$	-0.181	
	(0.134)	
Treated $\times \widetilde{\log P}_{dt}$	0.519*	
	(0.289)	
HighPrice$_{dt}$		-0.0234
		(0.0370)
Treated × HighPrice$_{dt}$		0.0636
		(0.0935)
R^2	0.442	0.442
Observations	5,626	5,626
Fixed Effects		
Congressional district	Yes	Yes
State-by-year	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 Stewart (2017), and member-level data from the Center for Effective Lawmaking. 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 cross). 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 (star). 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, 14 million are bought by banks or credit unions, and almost 14 million are not securitized. Median loan amount is $130,000 for all HMDA mortgages, $119,000 for GSE and $137,000 for FHA.

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. Using geographic overlaying between zip3 shapefiles and congressional district, we assign a congressional district to each observation. 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.

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 2008 through 2022. We similarly obtain county-level FHA loan limits from the Department of Housing and Urban Development (HUD). To derive a congressional district–level measure of these limits, we compute a weighted average of county-level limits, where each county's contribution to a district is determined by the extent of its geographic overlap with that district. Using historical shapefiles for both counties and congressional districts, we calculate these overlaps year by year to account for redistricting and boundary changes over time. This approach allows us to capture annual variation in the effective size of the mortgage market within each congressional 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 to the congressional 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, and total financial assistance.

Small business lending—We use county-level yearly data on bank-level small business lending from the Federal Reserve's Community Banking and Small Business Lending data. These data provide detailed information on the volume and distribution of small business loans originated by commercial banks, as reported in regulatory filings such as the Call Reports and the Community Reinvestment Act (CRA) data. Small business loans are defined as business loans with original amounts of $1 million or less, encompassing both term loans and lines of credit extended to small firms. We aggregate the data to the congressional district–year level to measure the intensity of local bank lending to small businesses, and study how this is related with political representation in the FSC. The average amount of small-business lending at congressional district-year level is 3 billion dollars, aggregated across all banks in our sample.

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 made to Congress candidates 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. According to this classification, we have identified almost 2,000 HMDA firms and 97,000 non-HMDA firms donating. Average contribution per electoral cycle to politicians running for Congress are approximately $29,000 for HMDA firms and $20,000 for non-financial.

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 Table B.4, 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.

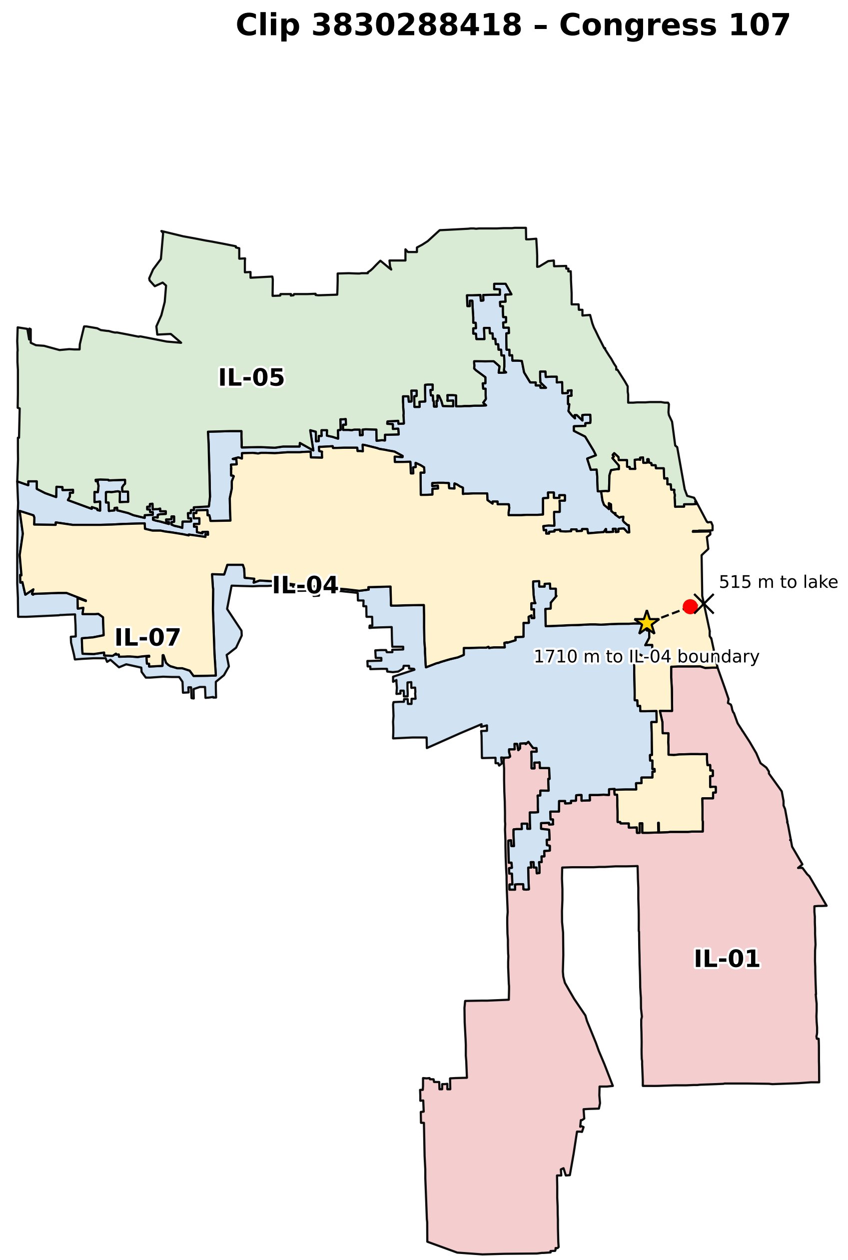


Figure A.1: Fallback method for assigning dual-border congressional districts. In this approach, we first identify for each property the location on the boundary of its own congressional district that is minimally distant from the property (cross). We then verify whether a congressional boundary exists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

B Additional Results

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

Notes: The table reports stacked DiD estimates of the effects of congressional representation on the House Financial Services Committee (FSC) on mortgage origination activity at the lender 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 variables are constructed using Home Mortgage Disclosure Act (HMDA) data aggregated to the lender–district–year level. 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 flexibly absorb time-invariant district-lender heterogeneity and state-level shocks common to districts within each cohort and lender. The coefficient on *Treated*$\times$*Post* captures the average change in mortgage origination activity after entry (or exit) relative to the pre-transition period. Standard errors are clustered at the state-by–year–by-cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	Orig	GSE	FHA	BCU	Private
Treated × Post	0.022***	0.023**	0.019*	0.061***	-0.004
	(0.006)	(0.010)	(0.010)	(0.015)	(0.036)
R^2	0.849	0.828	0.800	0.778	0.773
Observations	1,823,981	574,431	509,973	337,866	74,531
Fixed Effects					
District × Lender × Cohort	Yes	Yes	Yes	Yes	Yes
State × Year × Lender × Cohort	Yes	Yes	Yes	Yes	Yes

Panel B: Exit

	Orig	GSE	FHA	BCU	Private
Treated × Post	−0.054***	−0.035***	−0.046***	−0.064***	−0.097***
	(0.005)	(0.009)	(0.008)	(0.011)	(0.035)
R^2	0.844	0.823	0.794	0.776	0.766
Observations	1,877,036	593,507	595,714	482,580	68,437
Fixed Effects					
District × Lender × Cohort	Yes	Yes	Yes	Yes	Yes
State × Year × Lender × 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' representative's *first entry* into the FSC, and Panel B reports estimates around *first exit*. The specification follows Equation (4), where the dependent variables include total balance purchased and volume-weighted borrower characteristics, including interest rate, FICO score, loan-to-value (LTV), and debt-to-income (DTI) ratios. The sample is restricted to loans associated with Freddie Mac's special eligibility or refinance programs. 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 change in Freddie Mac purchase outcomes after entry (or exit) relative to the pre-transition period. Standard errors are clustered at the state-by–year–by-cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	Average Rate (%)	Average FICO	Average LTV (Orig)	Average LTV (OC)	Average DTI (Orig)
Treated × Post	0.021	-110.830	0.528	-0.653	-0.000
	(0.025)	(88.391)	(0.521)	(0.588)	(0.446)
R^2	0.984	0.582	0.961	0.932	0.799
Observations	4,541	4,541	4,541	4,541	4,541
Fixed Effects					
District × Cohort	Yes	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes	Yes

Panel B: Exit

	Average Rate (%)	Average FICO	Average LTV (Orig)	Average LTV (OC)	Average DTI (Orig)
Treated × Post	-0.012	9.017	0.355	-0.798	-0.637**
	(0.018)	(50.833)	(0.471)	(0.523)	(0.303)
R^2	0.980	0.515	0.919	0.918	0.770
Observations	6,045	6,045	6,045	6,045	6,045
Fixed Effects					
District × Cohort	Yes	Yes	Yes	Yes	Yes
State × Year × Cohort	Yes	Yes	Yes	Yes	Yes

Table B.3: Stacked DiD: Mortgage Realized Loan Limits Around FSC Transitions

Notes: The table reports stacked DiD estimates of the effects of congressional representation on the House Financial Services Committee (FSC) on realized GSE and FHA loan limits at 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 variables measure statutory maximum loan sizes eligible for government or government-sponsored enterprise (GSE) guarantees. Limits are obtained by selecting the maximum loan amount of GSE-purchased and FHA-insured loans at the congressional district level, after trimming at the 95th percentile to remove outliers. The dependent variables are the logarithms of these limits. 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*$\times$*Post* captures the average growth in loan limits after entry (or exit) relative to the pre-transition period. Standard errors are clustered at the state-by–year–by-cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

	GSE limit	FHA limit
Treated × Post	0.009*	0.015**
	(0.005)	(0.007)
R^2	0.976	0.974
Observations	10,067	10,063
Fixed Effects		
District × Cohort	Yes	Yes
State × Year × Cohort	Yes	Yes

Panel B: Exit

	GSE limit	FHA limit
Treated × Post	−0.010**	−0.011**
	(0.004)	(0.005)
R^2	0.966	0.970
Observations	9,546	9,542
Fixed Effects		
District × Cohort	Yes	Yes
State × Year × Cohort	Yes	Yes

Table B.4: Stacked DiD: Campaign contributions by employees Around FSC Transitions

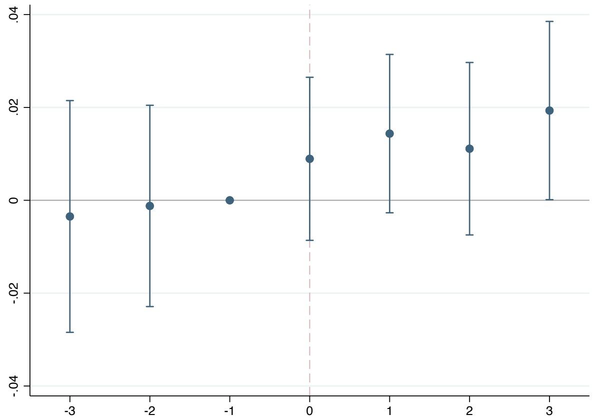
Notes: The table reports stacked DiD estimates of the effects of congressional representation on the House Financial Services Committee (FSC) on campaign contributions received by incumbent House representatives. Panel A presents estimates around districts' representative's *first entry* into the FSC, and Panel B reports estimates around *first exit*. The specification follows Equation (4), where the dependent variables measure the logarithm of contribution amounts received by the incumbent. The first column reports contributions from employees of firms appearing in HMDA data, the second column reports contributions from employees of non-HMDA firms, and the last column pools both groups and includes a triple interaction to estimate the differential effect between HMDA and non-HMDA contributions. 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 change in campaign contributions after entry (or exit) relative to the pre-transition period. Standard errors are clustered at the state-by–year–by-cohort level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Entry

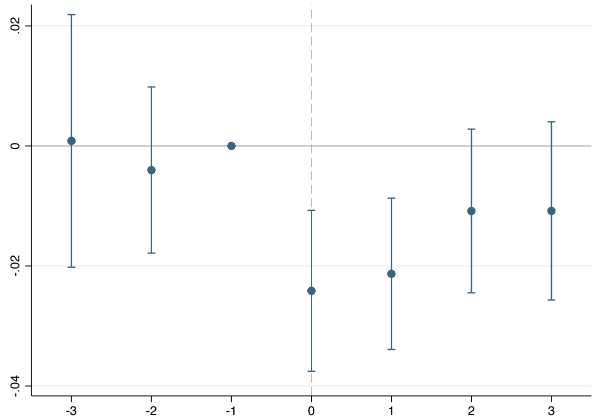
	HMDA only	Non-HMDA only	All
Treated × Post	0.425***	0.207***	0.207***
	(0.105)	(0.0627)	(0.0638)
Treated × Post × HMDA			0.218***
			(0.0478)
R^2	0.729	0.177	0.294
Observations	6,450	47,579	54,029
Fixed Effects			
District × Cohort	Yes	Yes	
State × Year × Cohort	Yes	Yes	
District × Cohort × Group			Yes
State × Year × Cohort × Group			Yes

Panel B: Exit

	HMDA only	Non-HMDA only	All
Treated × Post	−0.335***	−0.186***	−0.186***
	(0.0744)	(0.0473)	(0.0481)
Treated × Post × HMDA			−0.149***
			(0.0400)
R^2	0.697	0.163	0.288
Observations	6,422	46,072	52,494
Fixed Effects			
District × Cohort	Yes	Yes	
State × Year × Cohort	Yes	Yes	
District × Cohort × Group			Yes
State × Year × Cohort × Group			Yes



(a) Entry into the House FSC



(b) Exit from the House FSC

Figure B.1: *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 loan amounts around entry into the House Financial Services Committee (FSC), while Panel (b) presents the response around exit from the FSC, over a three-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 flexibly absorb time-invariant district heterogeneity and state-level shocks common to districts within each cohort. Standard errors are clustered at the state-by–year–by-cohort level.