Language Frictions in Consumer Credit *

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Abstract

This paper studies how language barriers between lenders and borrowers translate into differences in borrower outcomes in the U.S. mortgage market. I use survey data to infer and machine learning techniques to predict borrowers' English proficiency. I document significant descriptive differences in perceptions of mortgages, application experiences, and mortgage rates between limited English proficient (LEP) and non-LEP borrowers. To measure the causal effects of language frictions, I exploit a Federal Housing Finance Agency policy that provided translated mortgage documents in Spanish to mortgage lenders. After the policy change, LEP Hispanic borrowers had a streamlined application process, contacted more lenders, understood mortgage contracts better, and enjoyed lower borrowing costs. Reducing language frictions also led to expanded access to conventional loans for LEP borrowers. Overall, my findings highlight a cost-effective way to create a responsible inclusion of well-qualified LEP borrowers in the mortgage market.

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Households make financial decisions affected by various frictions in consumer credit markets. For example, costly search in the auto loan market results in higher interest rates, smaller loan sizes, and distorted consumption (Argyle, Nadauld and Palmer, 2023). Households respond slowly to mortgage refinancing opportunities because they are reluctant to incur current time costs for the sake of future benefits (Andersen et al., 2020). Information frictions restrain people from reducing their use of payday lending, as they are not fully aware of the borrowing costs (Bertrand and Morse, 2011).

In this paper, I study a fundamental yet often overlooked type of friction in consumer credit markets: language frictions. Specifically, I investigate the language barriers between financial institutions and consumers with limited English proficiency (LEP). With restricted abilities to read, speak, write, or understand English, LEP individuals may misunderstand financial contracts, have limited access to information, or miss opportunities in consumer credit markets. Importantly, LEP individuals are far from being a negligible group in the U.S. The census data reveal that nearly one in ten working-age U.S. adults falls into this category. A majority of this substantial group, approximately 64 percent, speaks Spanish as their primary language, followed by Chinese, Vietnamese, Korean, and Tagalog. Figure 1 plots the geographic distribution of LEP individuals across the U.S., highlighting large variation in the proportion of LEP individuals, LEP Hispanic, and LEP Chinese people at the county level.

I focus on the U.S. mortgage market because language frictions are particularly salient in this market for three reasons. First, acquiring a mortgage is perhaps the most important financial decision for many households. Given the loan size relative to household budgets and the loan duration relative to household life cycle, even minor misunderstandings can translate into significant financial consequences over time. By the end of 2019, mortgage balances accounted for 68 percent of total household debt, with about 42 percent of families having debt secured by their primary residence (Bhutta et al., 2020). Therefore, beyond individual implications, mortgage decisions also have a pivotal influence on broader economic stability.

Second, language frictions exacerbate the challenges of the already intricate mortgage application process for LEP borrowers. In addition to the lengthy process (Ellie Mae, 2016), mortgage forms and disclosures can be overwhelming even for native English speakers. For instance, disclosures for alternative mortgage products are written at reading levels ranging from 9th to 12th grade, but 43 percent of U.S. adults read below these levels (United States Government Accountability Office, 2006). Recent studies have found that lenders can exploit borrowers' confusion and steer them to certain products (Agarwal, Ben-David and Yao, 2017; Di Maggio, Kermani and Korgaonkar, 2019; Guiso et al., 2022). As mortgage disclosures are generally not available in non-English languages, LEP borrowers encounter more obstacles in learning about and accessing mortgage products and services, comprehending and completing key application documents, and resolving issues with mortgage lenders and servicers.

Finally, compliance risks and uncertainty deter financial institutions from fully serving LEP borrowers. Mortgage lenders may acknowledge the importance of offering products and services to LEP borrowers, but they remain cautious about violating statutes and regulations in practice. For instance, they are concerned about fair lending risks under the Equal Credit Opportunity Act (ECOA) when determining how and in which languages to offer products and services.[1]

Although LEP individuals represent a substantial segment of the U.S. population and have considerable credit needs, there is little empirical research that systematically addresses language frictions in the mortgage market (e.g., Kleimann Communication Group (2017) as a qualitative study). In this paper, I attempt to fill this gap by answering three questions: (1) Do language frictions affect access to mortgage credit? On the intensive margin, I examine how challenging the process can be for LEP borrowers who finally manage to apply for a mortgage. On the extensive margin, I examine whether LEP borrowers can take out mortgages in the first place. (2) How do language frictions affect the price of credit? (3) Does reducing language frictions affect the quality of credit?

To answer these questions, I confront two key empirical challenges. First, researchers rarely observe borrowers' English proficiency in conventional mortgage data sets. To overcome this data challenge, I leverage the newly available National Survey of Mortgage Originations (NSMO). If borrowers believe that it is important for a lender to speak their primary language, which is not English, I classify them as LEP borrowers. Furthermore, I construct a novel loan-level data set from Home Mortgage Disclosure Act (HMDA) data and employ supervised learning to predict borrowers' LEP status. Therefore, I can use this novel data set to complement my analysis using the survey data.

Second, it is difficult to isolate the role of language frictions from confounding fac-

[1] See Recent Requests for Information (RFIs) of ECOA at https://www.consumerfinance.gov/rules-policy/notice-opportunities-comment/archive-closed/request-information-equal-credit-opportunity-act-and-regulation-b/.

tors (e.g., financial literacy and cultural identity), as LEP borrowers often differ from non-LEP borrowers in many observable and unobservable aspects. To overcome this identification challenge, I exploit the implementation of the Federal Housing Finance Agency (FHFA) Language Access Plan in 2018. The government agency provided a centralized collection of translated mortgage documents to assist lenders, servicers, and housing counselors in serving LEP borrowers. This simple intervention addresses three issues: which documents to provide translations for, in which languages to provide translations, and the accuracy of translations. By alleviating lenders' concerns about compliance risks, this policy effectively reduced language barriers for LEP borrowers while having no impact on their creditworthiness.

My analysis begins by using the survey data to document large descriptive differences in perceptions, experiences, and real outcomes between LEP and non-LEP mortgage borrowers. Prior to applying, LEP borrowers often have more concerns about mortgage qualification and possess less product and market knowledge. During the application process, they engage with fewer lenders and are more likely to encounter additional problems, such as resolving credit report errors, having extra home appraisals, and redoing mortgage paperwork. Notably, they pay about 3.2 basis points higher interest rates than risk-equivalent non-LEP counterparts for similar mortgages, but they do not exhibit a higher likelihood of missing mortgage payments. This finding suggests that language frictions may prevent some well-qualified LEP borrowers from obtaining favorable loan terms. After successfully taking out their mortgages, they appear to be less familiar with their own mortgage contracts. For example, a higher proportion of LEP borrowers remain unaware of key mortgage features, such as adjustable interest rates, prepayment penalties, escrow accounts, or balloon payments. These results still hold after accounting for borrower demographic characteristics such as race, ethnicity, income, and education.

To identify the causal effects of language frictions more rigorously, I use a triple-difference strategy, which leverages the fact that the FHFA Language Access Plan was first launched with only Spanish translations. Specifically, I compare changes in outcomes of interest around the policy shock for Hispanic borrowers who are limited English proficient and those who are not, using similar changes among non-Hispanic borrowers as a counterfactual for what would have occurred in the absence of the policy shock.

I first apply the triple-difference strategy to the NSMO sample. I find that LEP Hispanic borrowers had a streamlined application process and a better understanding of their mortgage products following the policy shock. There was a 43% decrease in their likelihood of redoing mortgage paperwork—a decline of 13.7 percentage points from the pre-treatment level. They were 16.4 percentage points less likely to be unaware of whether their mortgages had a balloon payment, representing a 47% improvement relative to the pre-treatment level. Mortgage interest rates decreased by roughly 14.9 basis points for LEP Hispanic borrowers. This reduction was larger for borrowers with less experience transacting with lenders, such as purchase loan borrowers and first-time borrowers. My estimates suggest that increased borrower search could partially explain the price effect: LEP Hispanic borrowers were 16.2 percentage points more likely to seriously consider multiple lenders after the policy shock, indicating a 35% increase from the pre-treatment level. Additionally, the motivation of borrower search changed from seeking approval or learning information to finding better loan terms. On the other hand, mortgage performance, measured by the 90-day delinquency rate, did not change significantly. Most of these findings are evident by only comparing sample means of the treatment and control groups before and after the policy change. Moreover, these results remain consistent when I use different control groups, but they vanish in a series of placebo tests, where I simulate fake policy interventions.

The survey is a sample of current mortgage holders, so it cannot capture the full picture of LEP consumers who might be excluded from even entering the mortgage market due to language frictions. Therefore, I turn to the 2011-19 HMDA data to investigate the effects of reducing language frictions on access to credit on the extensive margin. Since I cannot observe each applicant's LEP status, I use a difference-in-differences framework, which leverages a time-varying treatment intensity measured by the proportion of potentially treated LEP people at the county level. I find that the propensity of LEP people to apply for conventional purchase loans increased by roughly 1.1 percentage points after the policy shock. The policy also reduced LEP borrowers' probability of submitting incomplete applications or being denied by 6.2 and 15.5 percentage points, respectively. If we assume that the fraction of potentially treated LEP people rises by 4 percentage points (i.e., one standard deviation of the LEP fraction), then mortgage originations will increase by approximately 5.4 percent. These results are robust to using a two-way fixed effects estimator with heterogeneous treatment effects and a series of placebo tests where I randomly assign the share of LEP people to each county within each state. Further analysis of the heterogeneous effects shows that the policy had a larger effect in areas where the demand for translated documents was higher.

In the last part of this paper, I use a novel loan-level data set to revisit the estimated effect on the price of credit and the quality of credit. The analysis using the survey data has strongly suggested that mortgage rates decreased and mortgage performance did not worsen. However, the limited sample size and the absence of lender and location information may raise concerns about my findings. To address this concern, I assemble a large and representative loan-level data set named HMDA$^+$. This data set combines demographic, lender, and location information from HMDA data with mortgage origination and performance information from Fannie Mae, Freddie Mac, and Ginnie Mae data. To implement the triple-difference strategy in the HMDA$^+$ sample, I employ machine learning techniques to predict individual-level LEP status. Remarkably, my machine learning model achieves an overall accuracy of nearly 99% in the test sample. Although the application of the machine learning model to the prediction sample inevitably results in misclassified cases, I show that under several assumptions that are likely to hold in my research, I can recover a lower bound of the true treatment effect on the treated (ATT) from the triple-difference regression. In my case, unlike traditional measurement error, I can exploit the performance of machine learning model to quantify how severe the measurement error is.

Using the HMDA$^+$ data set, I show that the interest rates of purchase loans paid by risk-equivalent LEP Hispanic borrowers dropped by at least 4.9 basis points. Again, I find no evidence of a decline in mortgage performance. The granularity of the HMDA$^+$ data set allows me to estimate the policy effect on up-front costs and heterogeneous effects across counties with varying needs for translated documents. First, I find that the decrease in interest rates was not offset by an increase in discount points, implying lower total borrowing costs for LEP borrowers. Second, the decrease in interest rates was larger in counties with a lower level of social capital. This result suggests that the FHFA Language Access Plan was particularly effective on its intended targets, since LEP borrowers in these counties typically receive less community support.

My paper contributes to three strands of literature. First, it is closely related to the literature on various types of frictions in consumer credit markets. Behavioral frictions can affect 401(K) plan participation (Madrian and Shea, 2001), investment in individual stocks (Puri and Robinson, 2007), credit card repayment (Keys and Wang, 2019), and mortgage refinancing (Andersen et al., 2020). Many studies establish the existence of search frictions and explore their role in explaining price dispersion, such as Stango and Zinman (2016) in the credit card market and Argyle, Nadauld and Palmer (2023) in the auto loan market. In the mortgage market in particular, Woodward and Hall (2012), Alexandrov and Koulayev (2018), and Bhutta, Fuster and Hizmo (2021) show that borrowers pay excessive mortgage fees because they do not shop for lower-cost mortgages.

Information frictions between borrowers and lenders are pervasive in consumer credit markets. On one hand, a growing empirical literature documents the presence of adverse selection and moral hazard, arising from information asymmetries where borrowers have private knowledge. Recent examples include Gupta and Hansman (2022) on mortgages, Adams, Einav and Levin (2009) on auto loans, Agarwal, Chomsisengphet and Liu (2010) on credit cards, and Dobbie and Skiba (2013) on payday loans. On the other hand, lenders may possess superior information, capitalizing on the borrowers' lack of understanding of financial products. Several papers reinforce this perspective by arguing that financial sophistication is a key driver of differential outcomes in household finance (Agarwal and Mazumder, 2013; Hastings, Madrian and Skimmyhorn, 2013; Lusardi and Tufano, 2015; Gomes, Haliassos and Ramadorai, 2021). This paper complements this literature by studying the role of important yet less emphasized language frictions in the mortgage market. Language frictions might be a deeper source of borrowers' search and information frictions, because they can influence search behavior and financial literacy but not the other way around. I provide evidence that reducing language frictions can induce LEP mortgage borrowers to search more. A government report finds that a lack of English-language skills hinders LEP individuals' financial literacy, making it difficult to conduct everyday financial affairs (United States Government Accountability Office, 2010).

Second, this paper contributes to the burgeoning body of research that examines the real effects of government interventions in credit markets, especially on disadvantaged households. Recent examples include the Community Reinvestment Act (Bhutta, 2011), bank branch deregulation (Célerier and Matray, 2019), credit card regulation (Agarwal et al., 2015), and broader regulation of consumer protection (Campbell et al., 2011; Posner and Weyl, 2013). Since the Great Recession, a large amount of rules and policies have been proposed and implemented in the mortgage market. DeFusco, Johnson and Mondragon (2020) study how the Dodd–Frank "Ability-to-Repay" rule affected the price and availability of credit. Kielty, Wang and Weng (2021) estimate the effect of the integrated disclosure rule, which streamlines mortgage disclosures. This paper contributes to the literature by examining a mortgage-related policy targeting at a previously overlooked but non-trivial group of people. Unlike many of the above-mentioned regulations, the policy studied in this paper did not impose mandates on financial institutions, did not explicitly control financial product design, and did not involve any cash transfers. Nevertheless, I find a significantly beneficial policy impact on various outcomes throughout the mortgage origination life cycle. My findings support the ongoing efforts by government agencies to make information on mortgages available, comparable, and comprehensible for LEP individuals.[2] Given the close nexus between English proficiency, race, and national origin, my findings also offer important insights into the racial wealth gap and immigrant assimilation.

Finally, this paper contributes to the research on the economic effects of language ability. Previous studies mainly find that workers with limited English skills earn less than comparable workers who are proficient in English (McManus, Gould and Welch, 1983; Tainer, 1988; Chiswick, 1991; Zavodny, 2000; Dustmann and Fabbri, 2003). In addition to the earning effects, Bleakley and Chin (2010) estimate the effect of English proficiency on marriage, fertility, and residential choice. Guven and Islam (2015) study the impact of English proficiency on individuals' health and satisfaction with partners and jobs. My contribution is to extend this stream by examining the effects of English ability in an important consumer loan market.

The remainder of this paper proceeds as follows. Section I introduces the data used in this paper. Section II documents descriptive differences between LEP and non-LEP borrowers throughout the mortgage application process. Section III outlines the policy shock that I use to study the causal effects of reducing language frictions and the triple-difference strategy. Section IV presents my empirical results using the survey data. Section V shows the estimated policy impact on access to credit in a difference-in-differences framework. Section VI extends the triple-difference analysis to a novel loan-level data set and provides a lower bound of the ATT. Section VII concludes.

[2]In early 2021, the Bureau of Consumer Financial Protection (CFPB) issued Statement Regarding the Provision of Financial Products and Services to Consumers with Limited English Proficiency to encourage financial institutions to better serve consumers with limited English proficiency and to provide principles and guidelines to assist financial institutions in complying with applicable laws.

I Data

My loan-level data come from four sources described below. Details of the sample selection are available in Section A of the Online Appendix.

National Survey of Mortgage Originations (NSMO): My main data source is this quarterly mail survey, which draws a nationally representative sample from newly originated closed-end first-lien residential mortgages between 2013 and 2019. Important for my analysis, this data set provides rich information on borrowers' demographic characteristics and unique information on borrowers' experiences getting a mortgage and their perceptions of the mortgage market. This data set also contains common underwriting variables drawn from administrative sources, including credit score, loan-to-value ratio (LTV), loan term, interest rate, loan type (conventional or FHA), loan purpose (purchase or refinance), occupancy (primary residence or investment property), and origination quarter.

Home Mortgage Disclosure Act (HMDA): The HMDA database covers the near universe of mortgage applications in the United States. Supplemented with the HMDA lender file (a.k.a. "the Avery File"), it can provide lender identity, loan type, loan purpose, loan size, and detailed property location for each home mortgage application or purchased loan. Using the 2011-19 HMDA data, I calculate measures of credit access and market concentration by race and county. This database also provides applicants' demographic features (e.g., race, ethnicity, gender, and income), which are useful for me to predict borrowers' LEP status. However, it lacks information on borrowers' credit scores and subsequent loan performance.

Fannie Mae and Freddie Mac Single-Family Loan Level Data (GSE): I combine the Fannie Mae and Freddie Mac loan-level data together. The resulting data set covers the majority of fully amortizing fixed-rate single family mortgages that the Enterprises acquired with origination dates from 2015 to 2019. It contains detailed information on origination characteristics (e.g., origination date, interest rate, FICO score, LTV ratio, and 3-digit ZIP code of the property), mortgage performance, and some large originators' identities.[3]

Ginnie Mae MBS Single-Family Loan Level Disclosure Data: This loan-level data set is similar to the GSE data set in content (similar origination and performance variables), but it covers federally insured or guaranteed mortgages in the pool of Ginnie Mae mortgage backed securities. My final data set only includes

[3]This GSE data set only identifies the originator that sold the loan to the Enterprises in cases where the originator had sufficiently high origination market share in the reporting period.

the most prevalent federally insured mortgage type—FHA loans originated between 2015 and 2019. I clean this data set so that it has the same data structure as the above GSE data set.

Each of the aforementioned mortgage data sets contains important information about some aspects of a mortgage or borrower. However, none of them offers a full set of loan and borrower characteristics, and there is no unique loan identification number to connect these data sets. This problem is especially important in my context, because my research question and identification strategy call for a representative sample with detailed borrower demographic and mortgage contract variables. To overcome this data challenge, I use the above data sources to construct two novel loan-level data sets.[4] A detailed description of the matching procedure is available in Section A of the Online Appendix.

FHA Snapshot$^+$: The most granular property location in the Ginnie Mae data is at the state level, which is too coarse for a precise empirical analysis. To solve this problem, I merge Ginnie Mae data with FHA Snapshot data, which record detailed property location of FHA loans. The final merged data set, named as FHA Snapshot$^+$, covers about 65% of FHA loans sold to Ginnie Mae in HMDA data during 2015-19. The analysis weight in this data set represents how many FHA loans in HMDA data are represented by each observation. I use this data set to calculate average mortgage rates, delinquency rate, and average FICO scores at the 5-digit ZIP code level.

HMDA$^+$: The NSMO data set provides the richest information about the borrower and the mortgage, but it lacks location and lender information. To complement the analysis using the NSMO sample, I assemble a new loan-level data set by merging HMDA data with GSE and FHA Snapshot$^+$ data. The final HMDA$^+$ data set covers nearly half of GSE and FHA loans in the 2015-19 HMDA data, which contains a broad set of borrower, property, mortgage contract, mortgage performance, and lender information. It has approximately 9.5 million mortgages originated between 2015 and 2019, including about 5.1 million purchase loans and 4.4 million refinance loans, or about 8.2 million conventional loans and 1.4 million FHA loans. To make this data set representative, I use the reciprocal of the likelihood of being sampled from HMDA data as the analysis weight for each observation.

Supplemental Data: I use the micro-level 2015–19 ACS to construct a training

[4] Previous literature has used different mortgage data and matching methods to solve this problem (Bartlett et al., 2022; Saadi, 2020). Section A of the Online Appendix discusses the advantages of my matching method compared to existing efforts.

sample for my machine learning algorithm to predict borrowers' LEP status in the HMDA$^+$ data set. For county level information, I collect the share of LEP people from the aggregate ACS files, other population data from the Population and Housing Unit Estimates Tables, income data from the Small Area Income and Poverty Estimates (SAIPE) Program, and single family appraisal data from the Uniform Appraisal Dataset (UAD) Aggregate Statistics.

II LEP Borrowers in the Mortgage Market

This section uses NSMO data to describe LEP borrowers, their mortgage application experiences, and their mortgage results.

A Demographic Characteristics of LEP Borrowers

The survey does not directly ask respondents how well they speak English, so I assign borrowers' LEP status based on their answer to this question: "*Is speaking my primary language, which is not English, important in choosing the mortgage lender or broker?*" Following this definition, about 10% of the respondents in this sample are LEP borrowers.[5] Figure B1 shows an increasing trend in the proportion of LEP mortgage borrowers from 8.5% in 2013 to 11% in 2019.

Panel A of Table 1 reports summary statistics of demographic characteristics for all borrowers, LEP borrowers, and non-LEP borrowers, respectively. In terms of gender, marital status, and age at the time of taking out their current mortgage, borrowers with limited English proficiency are similar to native borrowers. As expected, LEP borrowers typically have lower socioeconomic status. Only 53.4% of LEP borrowers hold a bachelor's degree or higher, compared to 65.8% of non-LEP borrowers. Around 21.8% of LEP borrowers have an annual household income less than \$50,000, compared to 14.3% of non-LEP borrowers. The average FICO scores of LEP borrowers is 722, 11 points lower than that of non-LEP borrowers. This difference is not large, roughly one-sixth of the FICO score standard deviation in the full sample. Figure B2 plots the distribution of educational achievement, age at mortgage origination, income, and credit scores for both LEP and non-LEP borrowers, exhibiting analogous patterns as summarized by the sample means.

[5]For reference, servicers in Dallas averaged having 15% of LEP borrowers as customers (Kleimann Communication Group, 2017).

B Perceptions and Experiences

I next use regression analysis to show that borrowers with limited English proficiency have different mortgage application experiences even conditional on a variety of borrower and mortgage characteristics. In particular, I estimate the following specification:

$$y_{it} = \alpha + \beta LEP_i + \gamma X_i + \delta_t + \epsilon_{it} \tag{1}$$

where y_{it} is an outcome of interest for loan i originated in quarter t. LEP_i is an indicator denoting English proficiency of borrower i. X_i is a vector of borrower demographic characteristics (e.g., gender, race, ethnicity, marital status, age, income, education, and FICO score) and mortgage characteristics (e.g., loan-to-value ratio, loan type, loan purpose, loan term, property type, occupancy type, interest type, and census tract type).[6] Origination quarter fixed effects δ_t account for market conditions across time.

I start with borrowers' anxiety when they begin the application process. Column 1 of Table 2 reports the estimate from a univariate regression, implying that LEP borrowers are more concerned about qualifying for a mortgage than non-LEP borrowers. The remaining columns of this table add a series of control variables that increasingly remove the influence of other confounding factors. Column 2 includes origination quarter and census tract type fixed effects, which control for market conditions. Column 3 directly controls for race, ethnicity, gender, education, and other demographic characteristics. This addresses the concern that the difference in demographic characteristics, instead of the difference in English proficiency, leads to the difference in borrower behavior. This column shows that the effect of English proficiency is comparable to the effect of a college degree. Column 4 further includes borrowers' risk fixed effects, which are the full pairwise interactions between LTV bins and FICO score bins.[7] Finally, column 5 adds predetermined loan characteristics, such as loan purpose, property type, and occupancy status. The coefficient of interest from columns 2 to 5 remains statistically significant at the 1% level. The most demanding specification in the last column demonstrates that LEP borrowers are about 6 percentage points more likely to worry about qualifying for a mortgage,

[6] The NSMO public use file does not provide detailed location information. Instead, it reports 3 types of census tract: metropolitan Community Reinvestment Act (CRA) low- and moderate-income (LMI) tract, metropolitan CRA non-LMI tract, and non-metropolitan tract.

[7] I follow the Loan-Level Price Adjustment Grid of Fannie Mae to group FICO scores and LTV into bins. See https://singlefamily.fanniemae.com/media/33201/display.

and this magnitude is about twice the effect of college education. These results suggest that the effect of English ability cannot be fully captured by race, gender, or education.

Using the specification in column 5, Figure 2 compares the perceptions and experiences of LEP and non-LEP borrowers throughout the mortgage application process. This comparison is visualized through red bars representing the estimated $\hat{\beta}$ in Equation (1).[8] To provide perspective on the magnitude of these differences, I also include green bars to represent the raw differences between LEP and non-LEP borrowers, and blue bars to represent the conditional differences between borrowers with and without college degrees.

Before the application process: LEP borrowers tend to have less knowledge about the mortgage product and market. As Panel A of Figure 2 shows, borrowers with limited English proficiency are less familiar with different types of mortgages available, the down payment needed to qualify for a mortgage, their own credit histories, and the current mortgage rate. To put this into perspective, these knowledge gaps are roughly 60% of the differences between borrowers with and without college degrees. Panel B of this figure illustrates that LEP borrowers value established banking relationships and personal connections a lot when selecting mortgage lenders or brokers. For instance, 39% of non-LEP borrowers, compared to 55% of LEP borrowers, believe that a friend or relative's recommendation is very important. This disparity remains essentially the same even after accounting for borrower and mortgage characteristics. On the other hand, college education does not have as significant an impact on these preferences.

In the application process: Borrowers with limited English proficiency search less compared to native borrowers. In the first two columns of Table 3, I find that LEP borrowers seriously consider and finally apply to fewer lenders. Interestingly, as shown in Table B1, the coefficients of race and ethnicity have the opposite sign to that of LEP status, suggesting that English proficiency, race, and ethnicity have quite different impact on borrower search behavior. In columns 3 to 5 of Table 3, I explore the motivations behind LEP borrowers' shopping behavior. The regression results show that they do shop around not because they try to find better loan contracts, but because they have concerns about qualifying for a loan or they hope to learn information from the Loan Estimate form.[9]

[8] Figure 2 plots the differences in outcomes between LEP and non-LEP borrowers. For level information of each type of borrowers, see Figure B3.

[9] Lender must provide borrowers a Loan Estimate form within three business days of receiving

Panel C of Figure 2 further shows that LEP borrowers face more challenges in the application process. Conditional on relevant borrower and mortgage features, LEP borrowers are about 3-6 percentage points more likely to resolve credit report errors, have more than one home appraisal, and redo their paperwork due to processing delays. On the other hand, borrowers' educational level has a limited effect on the probability of encountering problems in the process of getting a mortgage. One exception is that the probability of answering follow-up requests for more information about income or assets does not significantly differ between LEP and non-LEP borrowers. However, borrowers without a college degree are significantly less likely to do so compared with college graduates.

After the application process: Although each respondent in the survey has taken out a mortgage successfully, LEP borrowers seem less familiar with their own mortgage contracts. Interviews and focus groups with LEP borrowers reveal that they are particularly confused with the meaning of "balloon" as a terminology in mortgage-related documents (Kleimann Communication Group, 2017). Panel D of Figure 2 shows that LEP borrowers are roughly 5 percentage points more likely to report that they do not know if their mortgage has a balloon payment. They also tend to be less familiar with other nonstandard mortgage features such as adjustable interest rate, prepayment penalties, and escrow account. In contrast, the differences between borrowers with and without college degrees are much smaller. Furthermore, despite completing the application process, LEP borrowers struggle to explain some complex mortgage concepts to others. As shown in Figure B4, fewer LEP borrowers can articulate the difference between a fixed- and an adjustable-rate mortgage, explain why payments into an escrow account can change, or describe the process of taking out a mortgage.

The above findings are robust within a specific group of people. For example, Figures B5 and B6 document significant differences in perceptions and experiences across English proficiency among Hispanics and Asians, respectively. Figures B7 and B8 show the disparities between LEP and non-LEP borrowers among both college and non-college graduates. Figures B9 and B10 highlight these differences within high income and low income borrowers. Figures B11 and B12 extend this observation to borrowers who accessed mortgages via brokers and lenders. Taken together, language frictions lead to distinctive mortgage application experiences for LEP borrowers.

the application. This form provides important information, including the estimated interest rate, monthly payment, and total closing costs for the loan.

C Mortgage Outcomes

Finally, I use administrative data in NSMO to compare mortgage outcomes between LEP and non-LEP borrowers. Panel B of Table 1 reports the comparison using the raw data. About 74.2% non-LEP borrowers take out conventional loans, while only 67% of LEP borrowers do the same. LEP borrowers tend to take out smaller mortgages than non-LEP borrowers: the proportion of mortgage amount less than $200,000 is 2.3 percentage points higher. In addition, LEP borrowers have slightly higher loan-to-value ratios by 1.3 percentage points and debt-to-income ratios by 2.5 percentage points relative to non-LEP borrowers. Figure B13 confirms these results by plotting the distribution of loan amount, LTV, and DTI for LEP and non-LEP borrowers, respectively.

Panel B of Table 1 also shows that the average raw interest rate for LEP borrowers is approximately 7 basis points higher than that for non-LEP borrowers. Since interest rate is largely determined by borrowers' credit scores and mortgage choices, I control for these features and estimate Equation (1) for a more precise comparison. In addition to predetermined loan characteristics such as loan purpose, property type, and occupancy status, I also control for loan type, loan term, and interest type. In Panel A of Table 4, column 1 suggests that the interest rate of a similar mortgage product is 3.2 basis points higher for LEP borrowers than risk-equivalent non-LEP borrowers. From columns 2 to 4, I further include race, ethnicity, gender, and education as control variables. The difference in mortgage rate associated with language frictions is still statistically significant. Panel A of Table B2 shows that the price dispersion ranges from 2.7 to 3.8 basis points across different loan purposes or borrowers with different borrowing histories.

On the other hand, LEP borrowers are not ex-post riskier than ex-ante risk-comparable non-LEP borrowers. As can be seen from Panel B of Table 1, the unconditional probability of 90-day delinquency of LEP borrowers is slightly higher than that of non-LEP borrowers. However, the regression results presented in Panel B of Table 4 show that the difference in mortgage performance is insignificant after I control for ex-ante risk factors, mortgage characteristics, and demographic features. Furthermore, Panel B of Table B2 confirms that this result is unrelated to loan purpose or prior borrowing experience.

III Empirical Design

Although I have included a comprehensive list of borrower and mortgage characteristics in the above analysis, it is still possible that the observed differences between LEP and non-LEP borrowers are caused by unobservables, such as cultural background and social relationships. To address this issue, I exploit the implementation of the FHFA Language Access Plan as an exogenous shock to language frictions in the mortgage market. In this section, I introduce the policy shock and outline my main identification strategy.

A The FHFA Language Access Plan

In 2017, FHFA required Fannie Mae and Freddie Mac to identify major obstacles for LEP borrowers in accessing mortgage credit, analyze potential solutions, and develop a multi-year plan. The interviews and focus groups with LEP borrowers and mortgage lenders revealed two key findings (Kleimann Communication Group, 2017). First, accurate translations and standardized mortgage terminology are key to any further action. Second, borrowers and lenders may not be able to find existing language resources and are hesitant to use them. Because these language materials are not centrally collected or uniformly considered reliable, consumers lack trust in them, and lenders are afraid of potential legal issues.

Based on these findings, in May 2018, FHFA and the Enterprises published a multi-year plan to improve the ability of mortgage-ready LEP borrowers to understand and participate in all facets of the mortgage life cycle. The plan lists two key measures:

(1) Disclosure: The mortgage translation disclosure, published in the third quarter of 2018, is used by lenders and servicers to clarify that mortgage transactions are likely to be conducted in English and that not all communications related to a mortgage transaction will be in the borrower's preferred language. It also refers borrowers to the multilingual housing counseling services that may be available from HUD-approved housing counseling agencies. Figure F1 shows the disclosure in both English and Spanish. Lenders and servicers can customize the disclosure with their own logo and formatting. By explicitly stating the limitations of potential language assistance, this disclosure serves as a protection for financial institutions.[10]

[10] A policy expert at FHFA said that the mortgage translation disclosure was designed to alleviate lenders' concerns about compliance risks when serving LEP borrowers.

(2) Clearinghouse: The Mortgage Translation Clearinghouse is an online centralized collection of language resources, which is designed to assist lenders, servicers, and housing counselors in serving LEP borrowers. It provides translated glossaries, forms, disclosures, and other documents and materials from the Enterprises and other government agencies involved in the mortgage process. When the clearinghouse website was initially launched in 2018, it only provided translated mortgage documents in Spanish. Figure F2 shows a snapshot of the clearinghouse website in early 2019. Chinese translations were added to the website in 2019, followed by Vietnamese, Korean, and Tagalog translations in 2020.

I use the implementation of the FHFA Language Access Plan to perturb language frictions faced by LEP borrowers. For borrowers, the government provides translated documents, enhancing their trust in these translations. For lenders, this simple policy intervention alleviates their concerns by addressing three key issues: which documents to provide translations, in which languages to provide translations, and the accuracy of translations. As a result, financial institutions face smaller regulatory uncertainty and compliance risks.

I use Google Trends data to show the usage of FHFA language resources in practice. Figure 3 plots monthly search interest for search term "mortgage translation" and "mortgage" between July 2018 and December 2019.[11] I normalize two series relative to its value in July 2018, separately. Following the addition of a foreign language to the Mortgage Translations Clearinghouse, the Google searches for "mortgage translation" would increase by two to three times. In contrast, the Google searches for "mortgage" did not exhibit a similar change, which suggests that the sharp rise in the social interest in "mortgage translation" was not driven by the overall sentiment in the mortgage market. Moreover, the web traffic data from Semrush implies that the clearinghouse web page had taken up about 5.52% of the total web traffic of the FHFA website by the end of 2019.

B Triple-Difference Model

To identify the causal effect of language frictions, a difference-in-differences model can compare the outcomes of interest around the policy shock between LEP and non-LEP borrowers. Such a comparison could capture the effect of other LEP-specific

[11]The Mortgage Translation Clearinghouse appears at the top of the first result page when the keyword is "mortgage translation", but it is not in the first 20 results when the keyword is "mortgage."

shocks instead of the FHFA Language Access Plan. To address this concern, I exploit the fact that the initial translated mortgage documents were only available in Spanish. I expect the changes in outcomes of interest to be concentrated among LEP Hispanic borrowers, which suggests a triple-difference analysis. Specifically, I compare the double differences (as described above) among Hispanic borrowers to the same double differences among non-Hispanic borrowers. Alternatively, the triple-difference model can be interpreted as comparing changes in outcomes before and after the policy change across Hispanic and non-Hispanic LEP borrowers while using similar changes in non-LEP borrowers as a counterfactual. This specification controls for any unobserved common shocks that affected all LEP people or all Hispanic people in a given year. For example, shocks to the mortgage market that differ across LEP status but not differentially by ethnicity would not lead to bias in this specification, and neither would shocks that differ across ethnicity but not differentially by LEP status.

Figure 4 presents the main idea of the triple-difference strategy. It plots the proportion of different types of borrowers who had to redo mortgage paperwork before and after the policy change.[12] I divide borrowers into four groups based on their ethnicity and LEP status. The treatment group consists of LEP Hispanic borrowers (Panel A). The control group consists of non-LEP Hispanic (Panel B), LEP non-Hispanic (Panel C), and non-LEP non-Hispanic borrowers (Panel D). As can be seen in Panel A, there is a salient drop of the proportion among LEP Hispanic borrowers. We can reject the null hypothesis that the reduction is smaller than 5 percentage points at the 1% level. On the other hand, the proportion in Panels B to D declines by roughly the same amount, and the decline is not significantly smaller than 5 percentage points.[13] Although not conclusive, this figure implies that LEP Hispanic borrowers were about 14 percentage points less likely to redo their mortgage paperwork after the policy shock.

I formally implement this approach by estimating the following regression:

$$\begin{aligned} y_{it} = \alpha + \beta_0 LEP_i + \beta_1 Hispanic_i + \beta_2 LEP_i \times Hispanic_i + \beta_3 LEP_i \times Post_t \\ + \beta_4 Hispanic_i \times Post_t + \beta_5 LEP_i \times Hispanic_i \times Post_t + \gamma X_{it} + \delta_t + \epsilon_{it}. \quad (2) \end{aligned}$$

[12] In the Online Appendix, Figure F3 plots the same triple-difference graphical evidence for other outcomes of interest.

[13] LEP non-Hispanic borrowers have a higher socioeconomic status than LEP Hispanic borrowers, which explains why the pre-policy proportion of redoing paperwork is lower in Panel C than in Panel A.

In this specification, $Hispanic_i$ equals one if borrower i is Hispanic. According to the timeline in the multi-year plan, FHFA and the Enterprises prepared and reviewed the necessary materials in the first and second quarters of 2018. The disclosure and the clearinghouse were finalized and launched in the third and fourth quarters. Thus, $Post_t$ equals one if the mortgage was originated after June 2018. I interact census tract type and mortgage characteristics with $Post_t$ to allow these controls to have time-varying effect on the dependent variable. All other terms are as previously defined in Equation (1). The coefficient of interest is β_5, which measures the difference in the outcome for LEP Hispanic borrowers relative to non-LEP Hispanic borrowers around the policy change, relative to the same difference across LEP and non-LEP borrowers among non-Hispanic borrowers. This triple-dimension comparison is based on a broad set of borrower and mortgage characteristics as well as origination quarter fixed effects. The sign of β_5 depends on which outcome I examine. For instance, when I use the probability of confronting additional problems in the application process as the dependent variable, I expect to find $\beta_5 < 0$. On the other hand, when I use search intensity as the dependent variable, I expect to find $\beta_5 > 0$ if the policy reduced search costs.

Recall that the NSMO sample includes mortgages originated between 2013 and 2019. During this period, Spanish translations were made available online in the second half of 2018, followed by Chinese translations in the second half of 2019. However, I can only observe borrowers' race (e.g., Asian) rather than their primary language (e.g., Chinese or Tagalog), which prevents me from precisely defining the treatment group in 2019. Therefore, for my main results, I drop Asian borrowers to obtain a longer post-policy period, and the triple-difference model identifies the average treatment effect on LEP Hispanic borrowers. As a robustness check, I drop observations after the introduction of Chinese translations and re-estimate Equation (2).

IV Results Using NSMO

In this section, I first use the survey data to estimate the causal effects of language frictions on borrowers' application experience, mortgage rate, and loan performance. I then examine borrower search as one potential channel through which LEP borrowers paid lower interest rates after the policy shock. Finally, I provide robustness tests of my main results.

A Effect on Perceptions and Experiences

Having mortgage documents translated made it less likely that LEP borrowers would have extra problems when they applied for a mortgage. The first two columns in Panel A of Table 5 show that the probabilities of resolving credit report errors and answering follow-up requests on income or asset information both decreased by about 16 percentage points for LEP Hispanic borrowers. They correspond to a 48% and 25% drop relative to the pre-treatment level among LEP Hispanic borrowers, respectively. The most time-consuming part of getting a mortgage is home appraisal, which usually takes between one and two weeks to finish.[14] The estimate in column 3 implies a statistically significant 12.5 percentage points reduction in the probability of having more than one home appraisal following the policy change. Before the implementation of the FHFA Language Access Plan, nearly 22% of LEP Hispanic borrowers had extra home appraisals, while only 8% of non-LEP Hispanic borrowers did. My estimate suggests that the policy change almost eliminated the gap caused by language frictions.[15] The last column shows that LEP Hispanic borrowers were 13.7 percentage points less likely to redo mortgage paperwork subsequent to the policy change, or a 42% decrease from the pre-treatment level. These results demonstrate that translations could help LEP borrowers save time and reduce psychological costs associated with an important and complicated financial transaction. The results also offer suggestive evidence that lenders and servicers indeed provided language help to LEP borrowers following the policy change; otherwise, LEP borrowers would not have experienced a streamlined mortgage application process.

The positive policy impact also existed after the application process. LEP Hispanic borrowers' ex-post knowledge of their own mortgage contracts improved following the policy change. Column 1 in Panel B reveals that LEP Hispanic borrowers were 8 percentage points more likely to know if their mortgage has an adjustable interest rate, implying a 76% improvement relative to the pre-treatment level. Columns 2 and 3 report small impacts on their familiarity with prepayment penalty and escrow account. Column 4 indicates that LEP Hispanic borrowers were about 16 percentage points more likely to know whether their own mortgage had a balloon payment. In the NSMO sample, about 38% of LEP Hispanic borrowers did not know this prior to the policy shock, suggesting an approximately 42% decrease in the unawareness of

[14]See https://pacresmortgage.com/your-loan-timeline-from-offer-through-closing/.

[15]In Section C.1 of the Online Appendix, I use another data source, the Uniform Appraisal Dataset (UAD), to provide suggestive evidence of the positive policy impact on LEP borrowers' experience with property appraisals. Table C1 reports the results.

balloon payment among the treatment population.

B Effect on Mortgage Rate and Performance

The foregoing estimates suggest that providing Spanish mortgage-related documents greatly improved the origination experience for LEP Hispanic borrowers. Next, I explore how language frictions affect mortgage rates and performance. To do this, I first present graphical evidence showing that only LEP Hispanic borrowers paid lower conditional interest rates after the policy shock. The conditional interest rate is the mean of raw interest rate plus the residual after regressing the raw rate on origination quarter fixed effects, census tract type fixed effects, loan type, loan term, loan purpose, property type, occupancy type, and interest type. Figure 5 plots the average conditional interest rates for 8 samples, partitioned by ethnicity, English proficiency, and origination time. In Panel A that focuses on the LEP Hispanic group, there is a noticeable difference in interest rates before and after the policy change at the 5% level. However, Panels B to D show no such distinction. Formally, I cannot reject the null hypothesis that the pre-policy mean is equivalent to the post-policy mean in these control groups at conventional levels.

I then quantify the changes in interest rates demonstrated by Figure 5 with a regression analysis. Panel A of Table 6 shows that risk-equivalent LEP Hispanic borrowers paid lower interest rates for similar mortgages product after the policy shock. The coefficient of interest in column 1 implies a reduction in interest rates by about 14.9 basis points. To put this magnitude in more context, consider a typical LEP Hispanic borrower who took out a 30-year fixed rate mortgage in January 2019 with a loan amount of $249,000 and a mortgage rate of 4.5%.[16] Converting the estimate of the interest rate effect into dollars implies a reduction of $22 per month in mortgage payments. In the remaining columns of Panel A, I find that the drop in mortgage rate was larger for purchase loans compared with refinance loans, and larger for first-time borrowers compared with repeat borrowers. Although the estimates have relatively wide confidence intervals, there is a noticeable difference in both coefficient magnitude and statistical significance. This heterogeneity across loan purpose and borrowing history offers additional validity to my results. Compared to first-time (purchase) borrowers, repeat (refinance) borrowers should have more experience applying for a mortgage, so we would expect them to benefit less

[16] The average loan amount for Hispanic borrowers was $249,000 in 2019. The average rate of a 30-year fixed rate mortgage was 4.5% in January 2019.

from translated mortgage documents.

To capture loan performance, I use an indicator that takes the value of one if the borrower was ever 90 or more days late in making payments, and zero otherwise. As shown in Panel B of Table 6, I do not find a significant effect on mortgage performance across different loan types. The coefficients are all negative but statistically insignificant, suggesting, if anything, a weak positive effect on loan performance. Table C2 reports similar results when I use indicators of 60-day delinquency and default to measure mortgage performance. This is not surprising since borrowers received a better price following the policy change. Consistent with this, Table F1 shows that LEP Hispanic borrowers had lower debt-to-income ratios (DTI) after the policy shock.

To summarize, the results in Tables 5 and 6 provide evidence that language frictions were a substantial barrier for borrowers with limited English proficiency in the mortgage market. Providing translated mortgage documents to these borrowers could greatly improve their welfare without introducing additional risks to the residential mortgage market.

C Mechanism of the Price Effect: Borrower Search

Why did LEP borrowers pay a lower interest rate when they had access to mortgage documents in their primary languages? Using the survey data, I offer suggestive evidence indicating that the reduction in interest rates could be attributed to increased borrower search, rather than an improvement in financial literacy.

Agarwal et al. (2014) find that financially sophisticated households are less likely to pay too high mortgage rates. However, the FHFA Language Access Plan seemed to have a limited effect on LEP borrowers' ex-ante knowledge of the U.S. mortgage market. As shown in Table F2, I do not find a statistically significant effect on LEP borrowers' knowledge of mortgage types available, down payments needed, personal credit history, or market rate when they began the application process. These results suggest that borrowers with limited English proficiency did not improve their financial literacy in advance, which is consistent with how the policy was designed and implemented. Both the disclosure and the clearinghouse website were designed for lenders and servicers, and LEP borrowers were probably unaware of language help until they actually contacted a lender.

Several papers find that obtaining multiple mortgage quotes may help borrowers get a mortgage with better financial terms (Woodward and Hall, 2012; Alexandrov and Koulayev, 2018; McManus, Liu and Yi, 2018).[17] However, borrowers seem to conduct little search in the mortgage market despite the large potential benefit (Cai and Shahdad, 2015). The NSMO sample also suggests that more than half of borrowers did not search before taking out a mortgage. Since LEP borrowers have difficulty processing complicated mortgage documents and negotiating with mortgage lenders, it is not surprising that they search even less compared to non-LEP borrowers (see Table 3). Reducing language frictions for LEP borrowers can effectively reduce their search costs, since translated documents can help them better understand paperwork and compare multiple offers. As a result, they are more likely to take out a mortgage with a lower interest rate.

I first provide graphical evidence that uses unadjusted data and summarizes my triple-difference strategy. Figure 6 plots the pre- and post-policy distributions of borrower search intensity for different types of borrowers. Panel A clearly illustrates that more LEP Hispanic borrowers seriously considered multiple lenders when they faced lower language frictions in the mortgage market. The Kolmogorov-Smirnov test rejects the null hypothesis that the pre- and post-policy distributions are the same at the 5% level. In contrast, we do not observe such a pattern among non-LEP Hispanic borrowers or non-Hispanic people regardless of LEP status. Both theoretical (Chandra and Tappata, 2011) and empirical (Sorensen, 2000) literature suggest that increased search intensity leads to reduced price dispersion, which is confirmed in Figure F5. We observe a reduction in price dispersion among LEP Hispanic borrowers after the policy shock, while other groups show no such pattern. Although not conclusive, the graphical evidence suggests that reducing language frictions can induce LEP borrowers to search more.

Motivated by the above graphical evidence, I estimate Equation (2), which allows me to control for factors that are correlated with both LEP status and search behavior. Table 7 reports the regression results. The dependent variable in column 1 is a dummy variable indicating that the borrower seriously considered multiple lenders. The point estimate in column 1 implies that LEP Hispanic borrowers were 16.2 percentage points more likely to do so following the policy shock. Column 2 uses the number of lenders seriously considered as the dependent variable to reinforce this finding. In columns 3 to 5, I examine the policy impact on the reasons why borrowers search. As noted in

[17] Agarwal et al. (2020) find a non-monotonic relationship between consumer search and realized interest rates due to the fact that ex-ante risky borrowers search for approval. As LEP borrowers tend to have lower creditworthiness, for borrower search as a valid channel, I show that LEP borrowers can get cheaper mortgages when they consider more lenders in Figure F4.

Section II, LEP borrowers shop around mainly because they try to get approval or to gain experience. Although the estimates are imprecise, columns 3 and 4 suggest that more LEP Hispanic borrowers started to shop for better loan terms rather than for application approval. Column 5 shows that LEP Hispanic borrowers were 26.9 percentage points less likely to report learning information as the goal of search after the policy shock. Given that about 60% of pre-treatment LEP Hispanic borrowers applied to multiple lenders due to this reason, the FHFA Language Access Plan had a large impact on borrowers' search motivation.

D Robustness Checks

This subsection summarizes a suite of specification tests using different control groups and a suite of placebo tests exploiting different fake policy shocks. I focus on four outcomes that I have found significant effects in the above analysis: the likelihood of redoing mortgage paperwork, awareness of balloon payments, mortgage rate, and borrower search.

Choice of control group: In the above analysis, I drop Asian borrowers to avoid the misclassification of borrowers' treatment status. Alternatively, I can drop observations after the addition of Chinese translations to the Mortgage Translations Clearinghouse, so Asian borrowers are also included in the control group. Table C3 presents the triple-difference estimates following this sample selection, which are similar to my main findings.

The second row (Panels C and D) of Figures 4 through 6 suggests that LEP and non-LEP borrowers within the non-Hispanic population would have evolved similarly in the absence of the policy change. Likewise, the second column (Panels B and D) suggests that the parallel trends between Hispanics and non-Hispanics among non-LEP people are likely to hold. This motivates three alternative choices of the control group. First, I use non-LEP Hispanic borrowers as the control group to run difference-in-differences regressions. Second, I use LEP non-Hispanic borrowers as the control group to run difference-in-differences regressions. Finally, I directly compare LEP Hispanic borrowers with all other borrowers in the same sample used for my main results. Table 8 shows that all three specifications lead to similar estimated policy impact as my triple-difference estimation.

To give a sense of the dynamics of the policy impact, Figure 7 plots estimates from a flexible difference-in-differences specification. These estimates, using the same model as Panel C of Table 8, allow the policy impact to vary semiannually, relative to the first half of 2018. Despite some noise due to limited sample size, outcomes of interest diverge from their pre-policy trends following the policy shock. Importantly, the trends are statistically indistinguishable before the policy shock. The policy impact grows over time, which reflects the fact that lenders need time to learn about the policy and adjust their services for LEP borrowers.

Placebo Tests: Given the framework of the triple-difference setting, I intentionally change each item in $LEP_i \times Hispanic_i \times Post_t$ to make up fake policy interventions. As the treatment status is completely misclassified, I do not expect to find a statistically or economically significant effect.

First, I assume that the FHFA Language Access Plan was implemented in July 2016, which is a perturbation to $Post_t$. I drop mortgages originated after June 2018 and re-estimate Equation (2). Panel A of Table C4 reports a small and statistically insignificant effect of the fake policy shock. Second, I assume that the clearinghouse website initially published Asian language translations, which is a perturbation to $Hispanic_i$. I replace $LEP_i \times Hispanic_i \times Post_t$ with $LEP_i \times Asian_i \times Post_t$ when estimating the triple-difference model. Panel B of Table C4 shows that the estimated effect is indeed statistically and economically insignificant. Third, I randomly assign borrowers' LEP status, which is a perturbation to LEP_i.[18] In each iteration, I randomly select a group of people as LEP borrowers. To preserve the market share of LEP borrowers in reality, the simulated sample has the same number of LEP borrowers as the real NSMO sample. I then estimate Equation (2) and get one placebo coefficient, $\hat{\beta}_5$. Figure C1 plots the distribution of this coefficient arising from 1000 iterations. All panels present a bell shaped distribution of the placebo coefficient, which centers close to 0. Furthermore, the vertical red line indicating the true estimated effect lies in the extreme tail of the whole distribution, which leads to a small empirical p-value. Taken together, all robustness checks provide confidence that my empirical results are not statistical artifacts.

V Effect on Access to Mortgage Credit

In this section, I use county code level data and an accompanying difference-in-differences model to estimate the effects of reducing language frictions. This aggre-

[18] Bertrand, Duflo and Mullainathan (2004) recommend using the empirical distribution of estimated placebo effects to evaluate the results from difference-in-differences studies. As a triple-difference model can be transformed into a difference-in-differences model (Olden and Møen, 2022), their argument is also valid in my context.

gate level analysis complements the triple-difference analysis in two ways. First and foremost, it allows me to explore the policy impact on LEP consumers who were discouraged from participating in the mortgage market due to language frictions. As individual-level LEP status is not required in a difference-in-differences model, I can use the county-level HMDA data to study the effect of language frictions on the extensive margin. Second, this empirical design is flexible enough to incorporate the effect of providing Chinese translations in late 2019 by changing the local treatment intensity. In Section C.4 of the Online Appendix, I extend the difference-in-differences analysis to ZIP code level data on mortgage rates and risk. The findings align closely with the loan-level triple-difference estimates discussed earlier.

A Identification Strategy

My difference-in-differences strategy leverages the fact that native speakers do not benefit from the translations of mortgage-related documents into other languages. I compare the changes in outcomes of interest around the policy shock between areas with varying proportions of treated LEP people. Specifically, I estimate the following model:

$$Y_{ct} = \alpha + \beta D_{ct} + \gamma X_{ct} + \delta_c + \delta_{st} + \epsilon_{ct} \tag{3}$$

where

$$\mathrm{D}_{ct} = \begin{cases} 0, \quad \text{if } \ t \leq 2017, \\ \text{Hispanic LEP share}_c, \quad \text{if } \ t = 2018, \\ \text{Hispanic LEP share}_c + \text{Chinese LEP share}_c, \quad \text{if } \ t = 2019. \end{cases}$$

In Equation (3), c, s, and t index county, state, and year respectively. The key independent variable of interest, D_{ct}, which comes from the county-level ACS files, measures the share of potentially treated LEP people in county c in year t. This variable captures the gradual introduction of Spanish translations in 2018 and Chinese translations in 2019. I exclude small counties with a population under 5,000 so that the share of LEP people is relatively precise at the county level. X_{ct} is a vector of county-year level controls, including total population, median household income, the proportion of Hispanics, Asians, and Black people. These control variables account for the racial and ethnic composition and local housing market demand, which may be correlated with both the share of treated LEP borrowers and local mortgage market outcomes. δ_c and δ_{st} are county fixed effects and state-year fixed effects, which absorb county-specific time-invariant characteristics and state-level time-specific shocks.

The standard identification assumption in this framework is that the trends in outcomes in areas with varying fractions of LEP people would have evolved in parallel in the absence of the policy shock. I provide graphical evidence showing the plausibility of this assumption by estimating a more flexible version of the above model as follows:

$$Y_{ct} = \alpha + \sum_{\tau \neq 2017} \beta_\tau \text{LEP Share}_{ct} \times \mathbb{1}(t = \tau) + \gamma X_{ct} + \delta_{st} + \delta_c + \epsilon_{ct} \tag{4}$$

where $\mathbb{1}(t = \tau)$ is an indicator variable taking the value of one if year t is equal to τ. LEP Share$_{ct}$ is the share of LEP Hispanic people before 2019 and the share of LEP Hispanic and LEP Chinese people in 2019. I normalize the coefficient for $\tau = 2017$ to zero, so that all estimates of β_τ can be interpreted as the difference in the outcome of interest between areas with varying shares of treated LEP people relative to the corresponding difference in the year just before the policy shock. If β_τ is statistically insignificant before but significant after the policy change, this provides evidence of the validity of the parallel trends assumption and suggests that the discrete jump in coefficients is induced by the policy shock studied in this paper.

B Results

Since refinance loan borrowers have more prior experience in the mortgage market, I focus on purchase loans in my analysis.[19] Using 2011-19 HMDA data, I follow the borrower funnel in the mortgage market to calculate four outcomes of interest at the county level: the number of applications, the share of incomplete applications, the denial rate, and the number of originations. If some LEP borrowers have a more user-friendly application procedure after the policy shock, they may encourage more people in their communities to apply for mortgages. LEP borrowers are more likely to submit complete and accurate applications if they have access to mortgage documents in their primary language, so I expect a decrease in the share of incomplete applications and the denial rate for treated LEP borrowers after the policy shock. Finally, more applications and a lower denial rate will lead to an increase in mortgage originations.

Panel A of Table 9 shows that the FHFA Language Access Plan had a positive effect on credit access of conventional purchase loans. Column 1 implies that a 4

[19] Table F3 reports the regression results for conventional refinance loans, showing that the policy shock had much smaller effect on refinance loans.

percentage point increase in the proportion of potentially treated LEP borrowers (i.e., one standard deviation of the proportion) could result in 48 more applications after the policy change. The economic magnitude of this effect is sizable relative to the proportion of LEP individuals. Given that the average number of applications in my sample is 905, my estimate implies an increase in mortgage applications by approximately 5.3 percent. As an alternative way to interpret this result, I convert the difference-in-differences coefficient into the implied change in the latent propensity to apply for mortgages among LEP people. Let APP_0 and APP_1 denote the number of applications before and after the policy shock, respectively. If we assume that D_{LEP} percent of LEP and D_{NLEP} percent of non-LEP people would like to apply for a mortgage before the policy shock, then we can express APP_0 and APP_1 as weighted averages of the demand among LEP and non-LEP people:

$$APP_0 = D_{LEP} \times LEP \times POP + D_{NLEP} \times (1 - LEP) \times POP$$

$$APP_1 = (D_{LEP} + \Delta) \times LEP \times POP + D_{NLEP} \times (1 - LEP) \times POP,$$

where LEP and POP denote the share of LEP people and total population, respectively.[20] The second line arises from the assumption that reducing language frictions would boost the propensity by Δ among LEP people but not among non-LEP people. Then the difference-in-differences coefficient actually identifies $\Delta \times POP$. Since the average population is around 113,000 in my sample, the difference-in-differences estimate in column 1 implies that the policy shock increased the propensity to apply for a mortgage among LEP people by approximately 1.1 percentage points.

Consistent with my expectation, column 2 shows that the policy led to a reduction in the probability of submitting an incomplete application by 6.2 percentage points for treated LEP borrowers. Column 3 shows that the denial rate also dropped by roughly 15.5 percentage points. Given these estimated effects on the first three stages of the borrower funnel, it is not surprising that if the share of potentially treated LEP people increased by 4 percentage points, there would be 36 additional originated conventional purchase loans. As the sample mean of mortgage originations is 665, this estimated effect corresponds to a roughly 5.4% increase in mortgage originations.

As shown in Panel B of Table 9, LEP borrowers were also less likely to submit incomplete applications or be denied for FHA loans. However, there was a slight drop in FHA loan applications and originations, which suggests a substitution effect

[20] For simplicity, here I ignore the population growth from pre-policy to post-policy period.

between conventional and FHA loans. Thus, the overall supply of purchase loans might remain largely unchanged. I confirm this point in Table F4, which provides the regression results using all types of purchase loans (conventional, FHA, VA guaranteed, and FSA/RHS loans).[21] Columns 2 and 3 in Table F4 show that the incomplete share and denial rate of all types of purchase loans still decreased significantly. Based on the above results, the following analysis will only use the sample of conventional purchase loans.

To present the dynamics of the policy impact on the extensive margin, I estimate Equation (4) that allows the policy impact to vary by year. Figure 8 plots the series of β_τ along with their 95% confidence intervals. Almost all coefficients prior to the policy shock are close to zero and statistically insignificant, lending support to the validity of the parallel trends assumption required for the identification in a difference-in-differences research design. In contrast, the coefficients after the policy shock are economically large and statistically significant for all outcomes. For example, there is a discrete drop in the application denial rate when the FHFA Language Access Plan began. The magnitude of this drop is roughly 14 percentage points, which is similar to the point estimate in Table 9. The results so far have shown that providing translated mortgage documents addressed the credit access issue faced by borrowers with limited English proficiency.

Heterogeneous Effects: The magnitude of the policy impact should be a function of the extent to which borrowers with limited English proficiency need translated mortgage documents. Therefore, I investigate how the credit access effects vary across counties in this regard.

Section II has demonstrated that LEP borrowers largely rely on their friends and relatives when applying for a mortgage. If LEP borrowers have high trust in their communities, providing translations will hardly change their behavior. Therefore, I expect a stronger policy impact in counties with lower levels of social capital. I split the sample based on a county-level community health index, which measures local civic engagement and the health of associational life.[22] Figure 9 presents the difference-in-differences results by social capital. Consistent with my expectation, counties that had a community health index below the national median experienced a larger increase in the number of applications and originations and a larger decline

[21] Consistent with this result, I do not find significant effect on homeownership.

[22] The county-level community health index combines non-religious non-profits per capita, congregations per capita, and the informal civil society index. See U.S. Congress Joint Economic Committee (2018) for more details.

in application denial rate. The share of incomplete applications in these counties also dropped significantly at the 5% level, though the magnitude is slightly smaller than that in counties with higher levels of social capital.

The demand of LEP borrowers for translated mortgage documents also depends on racial composition and lender competition. If a county has a large fraction of Hispanic and Chinese people, local lenders may already have well-trained bilingual agents and translated documents prior to the policy shock. As shown in Table F5, I find a large and statistically significant policy effect in counties where the proportion of Hispanics and Chinese is below the national median. By contrast, there is little change in counties with a proportion above the national median. Moreover, the effect on mortgage credit access is likely to be smaller in less competitive counties for two reasons. First, if the market for LEP borrowers is concentrated among several lenders, these lenders may have expertise in serving LEP borrowers. Second, lenders with greater market power have less incentive to provide better service for LEP borrowers. I divide the sample by the county-level HHI of conventional purchase loan originations for Hispanic and Asian borrowers in 2017. Table F6 shows that the increase in credit supply and decrease in failed applications concentrated in counties with a more competitive mortgage market for Hispanic and Asian borrowers. Taken together, the FHFA Language Access Plan was more effective in counties where the demand for translations was higher.

Robustness Checks: Recent applied econometrics literature finds that two-way fixed effects (TWFE) estimations of difference-in-differences coefficients can lead to substantial bias when there are heterogeneous treatment effects (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). In my application, Equation (3) is a prototype of this regression with a time-varying continuous treatment status. Following De Chaisemartin and d'Haultfoeuille (2020), I estimate the difference-in-differences model with a TWFE estimator allowing heterogeneous treatment effects.[23] Table C5 shows that my main findings are robust to this approach and actually underestimate the policy impact. The reason why the TWFE estimator behaves this way is that the treatment effect was larger in counties with a smaller share of LEP people.[24] Following the policy change, some counties experienced a smaller increase in the treatment level, but these counties also experienced

[23]This approach can take care of continuous treatment status. In practice, the estimation uses counties whose treatment level changed in absolute value by less than 0.5% as control groups.

[24]Table F7 shows that the treatment effect was larger in counties with a smaller share of LEP people.

a larger change in credit expansion for a given change in the treatment level. The traditional TWFE estimator only accounts for the first effect, so it underestimates the effect of the policy shock (Sun and Shapiro, 2022).

Furthermore, I conduct three falsification tests to ensure that my difference-in-differences results are robust. I first assume that the policy shock happened in 2016 and re-estimate Equation (3) using the 2011-17 HMDA data. As shown in Panel A of Table C6, I find little effect of this fake policy intervention on mortgage credit access. Second, I estimate the effect of providing Spanish translations on Asian borrowers. In the difference-in-differences model, the treatment intensity is determined by the share of LEP Hispanics, but the outcomes are calculated using only Asian borrowers in HMDA data. Panel B of Table C6 reports an insignificant policy effect on Asian borrowers. On the other hand, Table C7 reports an economically and statistically significant effect when the outcomes are calculated using Hispanic borrowers. Finally, I randomly assign the share of LEP Hispanics and LEP Chinese at the county level within each state, and then I use this simulated treatment level and true data of outcomes and control variables to run difference-in-differences regressions. Figure C2 plots the distribution of 1000 placebo estimates following the above steps. The figure shows that the true estimate, denoted by the red vertical line, lies in the extreme tail of the distribution of placebo estimates. The corresponding empirical p-values are smaller than 1% in all panels.

VI Effect on Borrowing Costs and Mortgage Performance Using HMDA$^+$

The NSMO data set offers rich and unique information about borrowers' mortgage market experiences, but its small sample size could restrict the precision of my estimation. Another concern is that the NSMO data set does not contain location or lender information. The direction of the potential bias is not ex-ante obvious. On one hand, if some lenders took advantage of LEP borrowers before and then the predatory lending practices were curbed, the NSMO results would have a negative bias. On the other hand, if some lenders were more productive than others in serving LEP borrowers before and then the cost advantage diminished, the NSMO results would have a positive bias. To address these issues, I use machine learning to predict borrowers' LEP status in the HMDA$^+$ data set and then implement the triple-difference strategy using this data set. Although this approach generates measurement error of

LEP status, I can exploit the misclassification to provide a lower bound of the ATT under several plausible assumptions. The detailed lender and location information in the HMDA$^+$ data set enables me to identify the effect of language frictions in a more refined specification than that used in Section IV, and explore differential effects based on the cross-county heterogeneity. In this section, I introduce how I predict individual-level LEP status, discuss how I recover the lower bound of the ATT, and present the empirical results using this unique data set.

A Use Machine Learning to Predict LEP Status

The HMDA$^+$ data set has included borrowers' ethnicity and mortgage origination time, but the implementation of the triple-difference strategy further requires borrowers' LEP status. Predicting this variable in the HMDA$^+$ data set has three challenges. First, it is difficult to find a large labeled database as the training sample. The training sample should consist of mortgage borrowers and include their LEP status, but few loan-level data sets can meet both requirements simultaneously. Second, as one can expect, education, country of birth, and age at migration are useful predictors for this classification task, but these variables are not available in the HMDA$^+$ data set. Third, traditional classifiers (e.g., the Logit model) are prone to classify all borrowers as non-LEP in an imbalanced classification task. Since the fraction of LEP borrowers is small, this conservative prediction can achieve a decent accuracy rate in nature, but it lacks sufficient variation in treatment status for a triple-difference regression. Below, I briefly introduce how I solve these problems and leave the details of my machine learning model in Section D of the Online Appendix.

I start from the micro-level 2015-19 ACS to construct the training sample, because this data set directly provides individual-level LEP status based on the census definition. To ensure that the training sample mimics the population in the HMDA$^+$ data set, I only keep adult household heads who own their homes with mortgages. In addition, only people who moved into the current residence within a year are selected, so that I use the survey year as their mortgage origination year. Finally, I expand the sample based on individual weights in the ACS. Table D1 reports summary statistics for this sample. Similar to the NSMO sample, LEP borrowers have a lower educational and income level. It is important to note that the training sample tries to represent home buyers between 2015 and 2019, so the prediction sample will only include purchase loans in the HMDA$^+$ data set.

The input variables for the training process should be available in both the training sample and the prediction sample. Therefore, I pick gender, race, ethnicity, household income, and a series of state × year fixed effects as inputs for the classification task. Although this is a short list of features that does not appear to be directly related to borrowers' English proficiency, it turns out that these variables have enough predictive power to produce relatively accurate results. The success relies on the use of a machine learning algorithm that can efficiently learn non-linear decision boundaries.

To solve this imbalanced classification problem, I employ XGBoost, a leading machine learning library for regression, classification, and ranking problems. XGBoost implements machine learning algorithms under the Gradient Boosting framework in a highly efficient, flexible, and portable way. I randomly allocate 80% of the ACS sample for training, while the remaining 20% (i.e., the test sample) is used to evaluate the performance of my model. Specifically, I first use the full training sample to train a classifier C_f, and then I only use Hispanics to train another classifier C_h. Following the convention in machine learning literature, class 1 stands for LEP and class 0 stands for non-LEP. For non-Hispanic people, classifier C_f completely determines the prediction of LEP status. For Hispanic people, if either classifier C_f or classifier C_h predicts a positive class, then they are LEP borrowers. This two-step procedure results in improved performance for the sample of Hispanic borrowers, which is crucial for accurately recovering the ATT through a triple-difference regression.

My XGBoost model outperforms the traditional Logit model. Table D2 reports three useful evaluation metrics for the full sample and the sample of Hispanics in Panels A and B, separately. For each class, precision is the fraction of relevant instances among the retrieved instances, recall is the fraction of relevant instances that are retrieved, and accuracy is the ratio of correct predictions to all instances. As expected, the Logit model predicts almost all people as non-LEP borrowers. Therefore, it only detects 0.5% and 2.3% of LEP people in the full sample and the Hispanic sample. In contrast, my model can find approximately 78.7% and 83.1% of LEP borrowers in two samples. The precision rate of the Logit model in the full sample is only 0.54, which means that nearly half of the predicted LEP people are actually proficient in English. In contrast, my model can achieve a precision rate of 0.89 in the full sample. Overall, the accuracy rate of my model increases by 3.2 percentage points in the full sample and 15.4 percentage points in the Hispanic sample.

I then use this fine-tuned machine learning model to predict purchase loan borrowers' LEP status in the HMDA$^+$ data set. Table D3 compares several key characteristics between LEP and non-LEP borrowers, based on the machine learning prediction.

Consistent with the NSMO sample, a typical LEP borrower has a lower income, credit score, and mortgage amount but a higher DTI, mortgage rate, and delinquency rate. This suggests that the prediction is definitely better than a random guess. About 4.9% of the training sample are LEP borrowers, but the machine learning model predicts that only 3.3% of the HMDA$^+$ sample are LEP borrowers. This is probably because the prediction sample has a smaller share of minorities and low-income Hispanic people than the training sample.[25] Fortunately, as long as the misclassification is not too severe, it can be used to back out a lower bound of the ATT under several assumptions that are likely to hold in my case.

B Recover the Lower Bound of the ATT

Since the machine learning prediction generates measurement error of the key independent variable, it is necessary to gauge the bias of the triple-difference estimation that uses the predicted LEP status. In this subsection, I lay out the assumptions and explain the intuition of how I recover the lower bound of the ATT from a triple-difference model with treatment status misclassification. The formal discussion is presented in Section E of the Online Appendix.

For the sake of exposition, I summarize the empirical framework in the language of potential outcomes. Consider a canonical triple-difference model with a $2 \times 2 \times 2$ setup. Three dummy variables, P, L, H, indicate post-policy period, LEP status, and Hispanic ethnicity in the data, respectively. However, L is a misclassified version of the unobserved true LEP status, L^*. I use a latent variable, ρ, as an indicator for misclassification, so $\rho = 1$ when $L \neq L^*$. The treatment status D is also a dummy variable, which takes the value of one only when $P = 1$, $L^* = 1$, and $H = 1$. Let Y_t and $Y_t(D)$ represent the observed outcome and the potential outcome in period t if the treatment status is D. I am interested in identifying the ATT that can be written as:

$$\text{ATT} = \mathbb{E}[Y_1(1) - Y_1(0) \mid L^* = 1, H = 1].$$

When I run triple-difference regressions using the HMDA$^+$ data set, I implicitly proceed with:

Assumption 1. *Parallel trends between the misclassified treatment status (L).*[26]

[25]See Section D.3 of the Online Appendix for a more detailed discussion.

[26]In the interest of space, the formal expression of this assumption is presented in Section E of the Online Appendix.

If there is no misclassification of L^*, then this assumption helps to identify the ATT (Olden and Møen, 2022). On the other hand, when there exists misclassification, lemma E.1 in the Online Appendix reveals that the triple-difference estimator cannot recover the ATT under this assumption. The triple-difference estimator can be written as a weighted average of the ATT for the correctly classified and misclassified treatment groups, but the sum of two weights is not guaranteed to be one. Thus, the bias direction is ambiguous. However, I then show that a reasonable assumption on the structure of misclassification can help to identify both the direction and magnitude of the bias.

Assumption 2. *Non-differential Misclassification:* $\rho \perp\!\!\!\perp (Y_1(1), Y_1(0)) \mid L^*, H$

This assumption states that misclassification is not correlated to potential outcomes conditional on the true treatment status. It is likely to hold in my context, because the misclassification mechanically comes from a pure statistics exercise. Then the following proposition holds.

Proposition 1. *In a* $2 \times 2 \times 2$ *canonical triple-difference design, if Assumptions 1 and 2 hold, the triple-difference estimator can be written as:*

$$\theta_{DDD} = ATT(\mathbb{P}(\rho = 0 \mid L = 1, H = 1) + \mathbb{P}(\rho = 0 \mid L = 0, H = 1) - 1). \qquad (5)$$

If $1 - \mathbb{P}(\rho = 1 \mid L = 1, H = 1) - \mathbb{P}(\rho = 1 \mid L = 0, H = 1) \in (0, 1]$*, then the triple-differences estimator has attenuation bias.*

Proposition 1 shows that the triple-difference estimate is smaller than the true ATT when the misclassification problem is not too severe. It also links the performance of machine learning models to the bias magnitude. As shown in Equation (5), the relationship between the triple-difference estimate and the ATT depends on the precision metrics (i.e., $\mathbb{P}(\rho = 0)$). Because non-Hispanic people are always untreated, the bias is only determined by the precision in the Hispanic sample. However, the true LEP status is not observable in the prediction sample, so I cannot evaluate the prediction performance in this sample. To convert the triple-difference estimate to the ATT, I impose two additional assumptions that allow me to pin down the range of the prediction precision.

Assumption 3. *The fraction of LEP people among Hispanic borrowers in the prediction sample is lower than that in the training sample.*

Assumption 4. *The machine learning model does not perform better in the prediction sample than in the test sample.*

Figure D1 provides supporting evidence for Assumption 3, as it shows that the training sample has a larger fraction of low-income Hispanic households than the prediction sample. Then Assumption 4 also stands because the prediction sample does not represent the test sample perfectly. This assumption implies that the precision and recall metrics in the prediction sample should be smaller than those in the test sample.

Under these two assumptions, I can calculate a lower bound of the ATT. Table E1 presents the confusion matrix of Hispanic borrowers in the HMDA$^+$ sample. Each element of this matrix, as well as the precision and recall metrics, can be expressed by two unknowns: the number of true positive instances (denoted as x) and the number of LEP Hispanic borrowers in reality (denoted as y). Finding the lower bound of the ATT is equivalent to solving a constrained optimization problem: maximize the sum of prediction precision for two classes (i.e., $\mathbb{P}(\rho = 0 \mid L = 1, H = 1) + \mathbb{P}(\rho = 0 \mid L = 0, H = 1)$) while subject to a system of constraints implied by Assumptions 3 and 4. Solving this problem, I obtain the maximum value of 1.72 for the sum of prediction precision for two classes.[27] Therefore, according to Proposition 1, the ATT in this case is the triple-difference coefficient multiplied by 1.39. Notice that this is the lower bound of the ATT. Given the same triple-difference coefficient, any deviation of the machine learning performance (x) or the actual number of LEP Hispanic borrowers (y) from the value that achieves the maximum will generate a larger ATT.

C Results

The results from the previous two parts allow me to implement the triple-difference strategy in the HMDA$^+$ sample and calculate a lower bound of the ATT. For the same reason, I drop Asian borrowers as I did in the NSMO sample. Since the HMDA$^+$ data set includes precise property location and lender information, I can further add post policy-county fixed effects and post policy-lender fixed effects in Equation (2). Table 10 reports the triple-difference results and the implied lower bound of the ATT.

First, I confirm that the policy decreased interest rate across different types of purchase loans. After converting the coefficient to the ATT, the point estimate in column 1 of Panel A implies a decrease in interest rate by at least 4.9 basis points

[27] See Section E.2 of the Online Appendix for the calculation.

for risk-equivalent LEP Hispanic borrowers after the policy shock.[28] Bartlett et al. (2022) report that the average lender's profit per mortgage is 10 basis points of the interest rate, so the effect I document corresponds to nearly half of the average lender's profit. The economic magnitude is also comparable to other research evaluating the effect of different shocks on the mortgage interest rate. For example, Allen, Clark and Houde (2014) find a 6-bps increase in interest rates after a merger between two lenders in Canada. Kielty, Wang and Weng (2021) find that simplifying mortgage disclosures led to an interest rate reduction of 1.8 bps. The heterogeneous effects presented in columns 2 and 3 are consistent with the findings using the survey data: first-time borrowers benefited more from the language help. Columns 4 and 5 show that the price impact on mortgages originated through retailers was roughly 78% larger than that on mortgages originated through brokers. LEP borrowers probably will choose brokers who can speak their primary languages, so the effect of translated documents is largely substituted by mortgage brokers. Using this unique loan-level data set, I also find that the FHFA Language Access Plan was more effective in counties where translated documents were more needed. Figure 10 plots the results from estimating the triple-difference model in two groups of counties with different levels of social capital. The effect of reducing language frictions on interest rate was larger in counties with a level of social capital below the national median, which is consistent with the heterogeneity in aggregate quantity effects.

Second, I show that the drop in interest rates was not offset by an increase in up-front costs. Bhutta and Hizmo (2020) find that Black and Hispanic borrowers pay higher interest rates but lower up-front costs, so interest rate gaps alone do not reflect the whole picture of borrower costs. To address this concern, I estimate Equation (2) using the discount points scaled by mortgage amount as the dependent variable. The sample only includes the mortgages originated in 2018 and 2019, as HMDA started to disclose discount points in 2018. As shown in Panel B of Table 10, there was little effect of reducing language frictions on the up-front discount points, suggesting that the decline in interest rates cannot be attributable to borrowers purchasing discount points to get a cheaper interest rate. Using the results in column 2 of Panels A and B, I conduct a back-of-the-envelope calculation of total borrowing costs. The discount points paid by a first time home buyer insignificantly increased by only 0.049% of the mortgage principal. This would decrease the interest rate by 1.2 bps, since each

[28] In Section E.2 of the Online Appendix, I provide a reasonable bundle of values of x and y so that the magnitude of the ATT in the HMDA$^+$ sample is comparable to the triple-difference estimates in the NSMO sample.

point costs 1 percent of the loan amount and can reduce interest rate by about 25 basis points.[29] However, I find a 7.2 bps reduction in interest rate, suggesting lower overall borrowing costs for first time LEP Hispanic borrowers.

Third, I do not find a significant effect on mortgage performance, which is consistent with previous results using NSMO and the ZIP code level data. Panel C of Table 10 reports the triple-difference estimates using the 90-day delinquency incidence as the dependent variable. If anything, providing Spanish mortgage documents to LEP Hispanic borrowers slightly improved their mortgage performance.

Finally, I provide indirect evidence of the borrower search mechanism behind the price effect. As the competing channel here is the reaction of lenders to the policy change, I show that the price effect still remains when I block the competing channel. Specifically, I add Post $\times$ Lender $\times$ County fixed effects in Equation (2), so the resulting specification absorbs how lenders reacted to the policy shock differently across their operating markets. Table F8 reports the results from estimating this modified specification. The magnitude of most coefficients decreases, but they are still statistically significant at the 1% level. This means that lenders' response to the policy shock cannot fully explain the decrease in interest rate, suggesting the existence of the borrower search channel.

Overall, my findings using the HMDA$^+$ sample are consistent with those using the survey data. The triple-difference estimates in this section, which provide the lower bound of the ATT, suggest that the FHFA Language Access Plan reduced borrowing costs for borrowers with limited English proficiency. At the same time, the policy did not worsen mortgage performance conditional on ex-ante risk factors.

VII Conclusion

Many consumers with limited English proficiency have difficulty accessing the financial market, despite making up a significant portion of the U.S. population. This paper quantifies the language frictions faced by LEP borrowers in the U.S. mortgage market. I document that LEP borrowers encounter more challenges in the application process and pay higher interest rates for similar mortgages than similar non-LEP borrowers. To estimate the causal effects of language frictions, I exploit the implementation of the FHFA Language Access Plan in 2018. Using a triple-difference design, I find that LEP Hispanic borrowers had a better mortgage application experience and

[29] See, for example, https://bettermoneyhabits.bankofamerica.com/en/home-ownership/buying-mortgage-points-lower-rate.

gained a better understanding of mortgage contracts after the policy shock. They paid lower interest rates, and one possible explanation is that they searched more after they had mortgage documents in their primary language. Using a difference-in-differences design, I find that the credit supply of conventional purchase loans increased and the average conditional interest rate decreased in counties with a larger fraction of potentially treated LEP people after the policy shock. At the same time, the more inclusive credit access did not introduce extra risks, as measured by both ex-ante and ex-post indicators.

This paper offers clear policy implications. Providing translations can better equip borrowers with limited English proficiency to navigate the mortgage application process, thereby fostering a more inclusive and equitable mortgage market. Compared to direct fiscal transfers, this policy is a much more cost-effective way to help LEP borrowers. Moreover, the heterogeneous effects that I find suggest that the policy has the greatest positive effect on those LEP borrowers who need translations the most. Broadly speaking, lowering language barriers is an effort to reduce information asymmetry in the mortgage market. Borrowers engage in mortgage transactions only a few times in their lives, so they have an information disadvantage relative to lenders. Since each transaction involves a large portion of borrowers' budget, addressing asymmetric information problems can significantly improve borrowers' decision making and generate substantial welfare improvements.

As English proficiency is closely related to immigration and race, my results also highlight that language help in the mortgage market could be beneficial to address three important social issues. First, many creditworthy LEP people are financially underserved, so reducing language frictions creates an effective and responsible integration of LEP consumers into the financial marketplace. Second, many LEP borrowers are immigrants, so reducing language frictions facilitates their assimilation into society. Finally, many LEP borrowers are minorities, so reducing language frictions offers opportunities for them to build their financial capabilities and potentially close the racial wealth gap.

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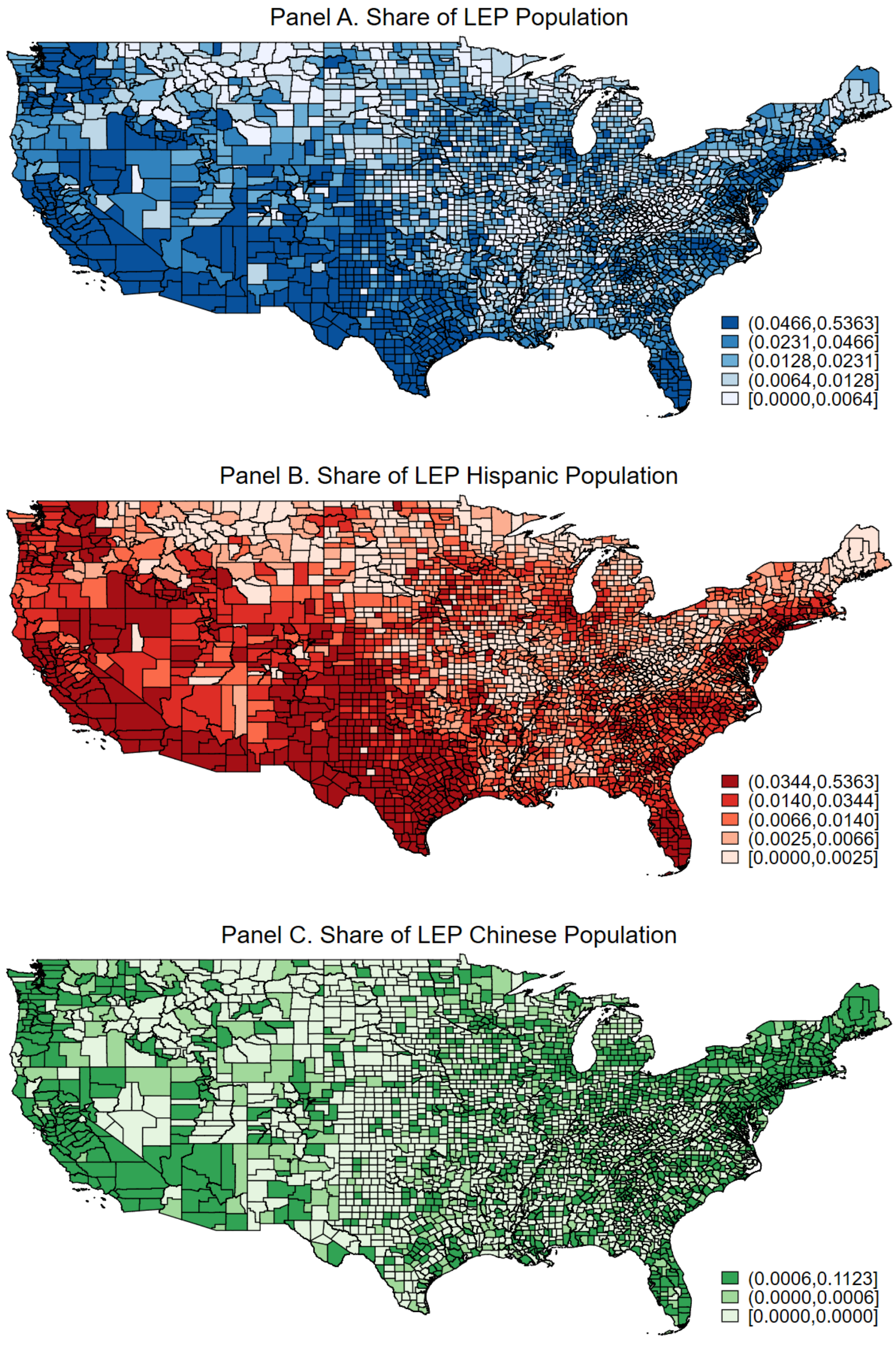
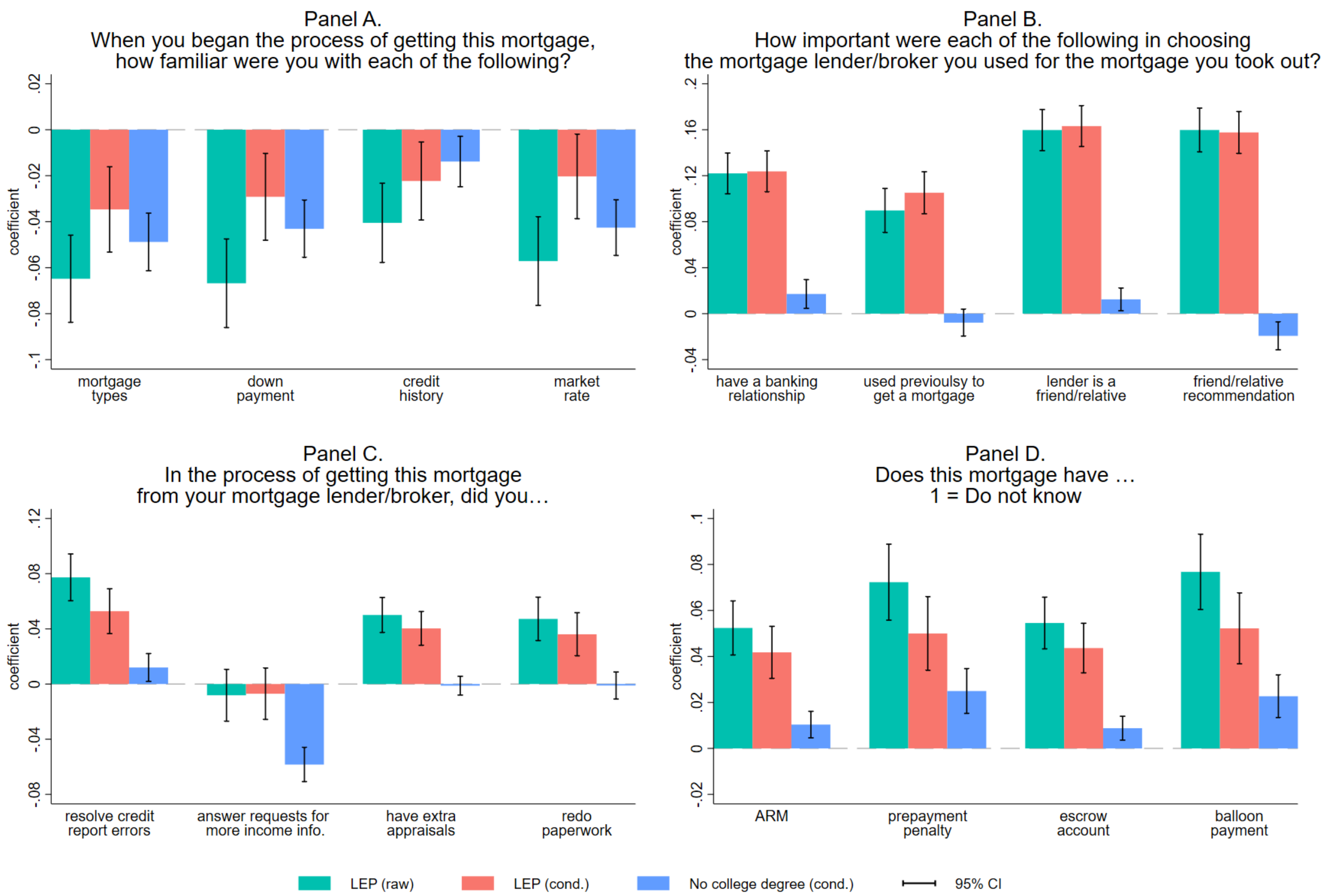
Figure 1. Share of LEP Population in the U.S.



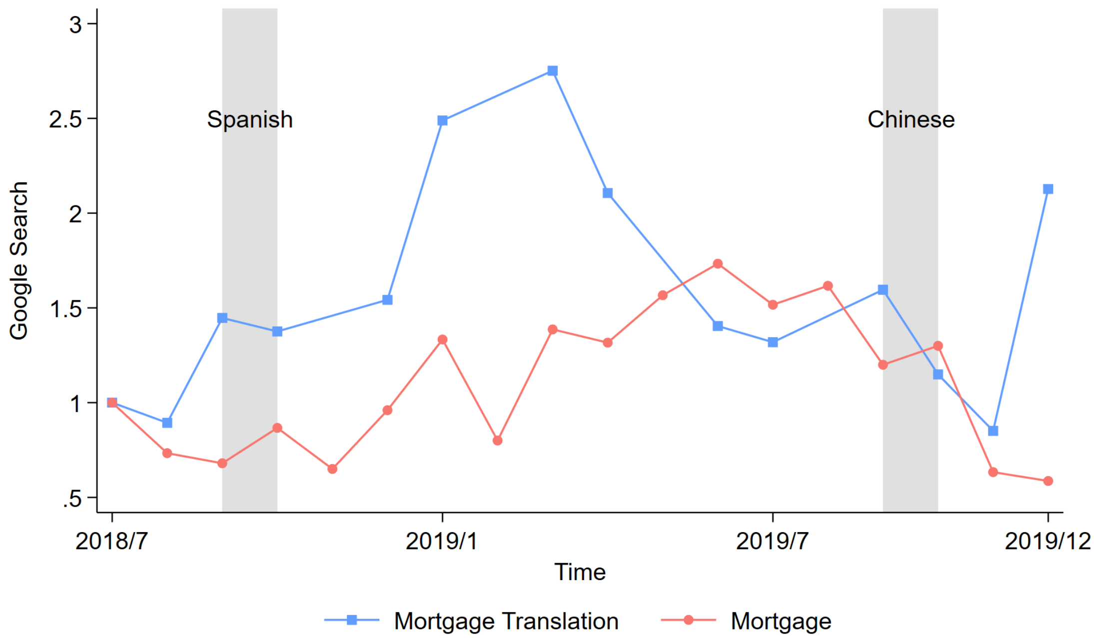
Notes: This figure plots the share of LEP people, the share of LEP Hispanics, and the share of LEP Chinese at the county level. The statistics come from the American Community Survey 2015-2019.

Figure 2. Estimated Differences between LEP and non-LEP Borrowers



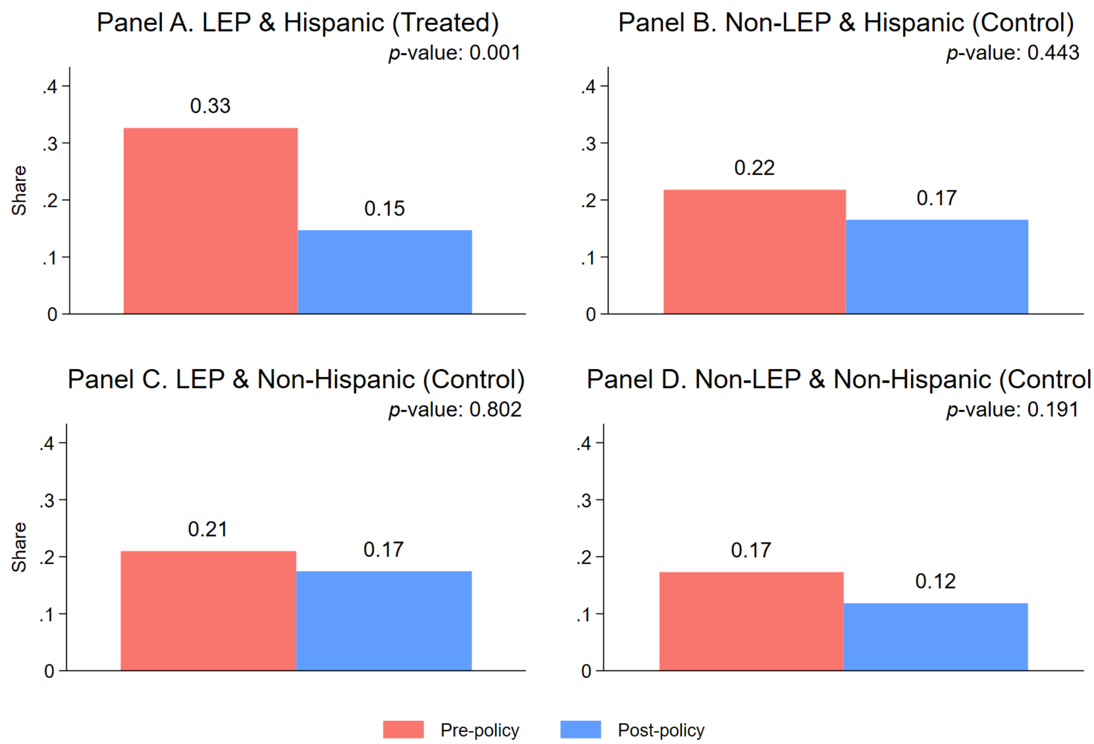
Notes: This figure plots the estimate of β in Equation (1) and its 95% confidence interval. The outcomes are indicators for whether the borrowers was familiar with four things about the mortgage market in panel A, indicators for whether the borrower thought four factors were important when choosing the mortgage lender in panel B, indicators for whether the borrower had four problems in the process of getting the mortgage in panel C, and indicators for whether the borrower knew about four alternative features of the mortgage in panel D. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. The 95% confidence intervals are based on robust standard errors.

Figure 3. Google Trends



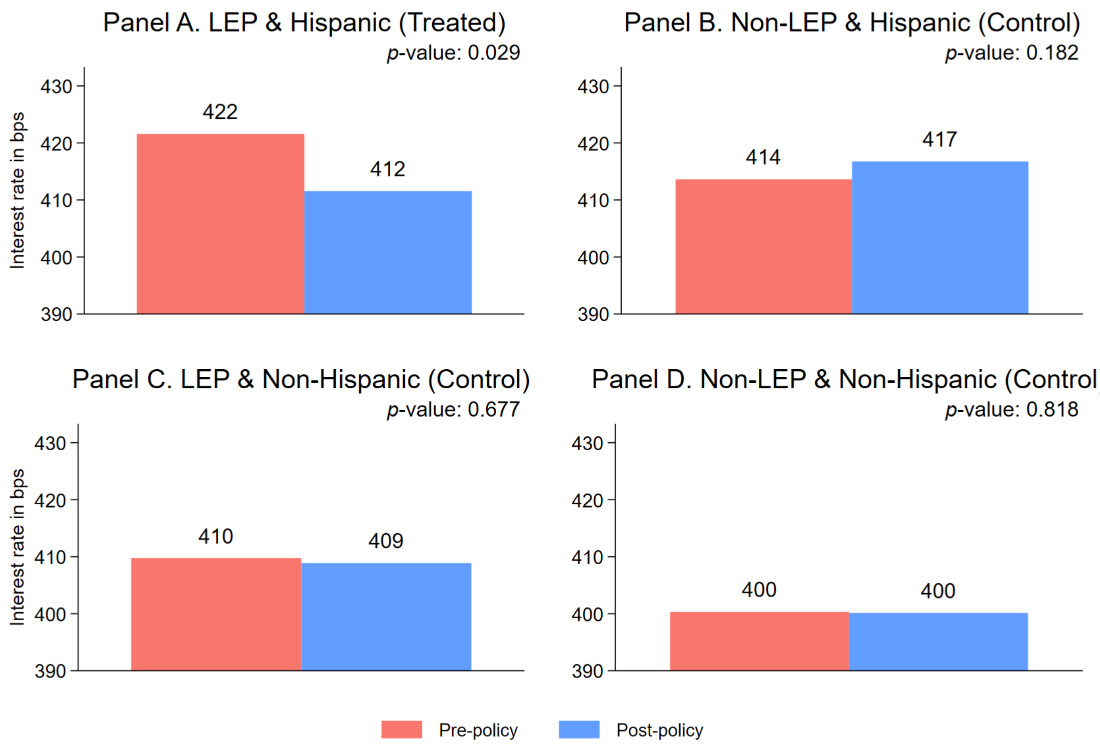
Notes: This figure plots monthly Google Trends data for "mortgage" and "mortgage translation" from July 2018 to December 2019. Each series is separately normalized relative to its value in July 2018. The shaded areas represent the approximate time of adding Spanish translations and Chinese translations to the Mortgage Translations Clearinghouse.

Figure 4. Triple-Differences Raw Comparison: Redo Paperwork



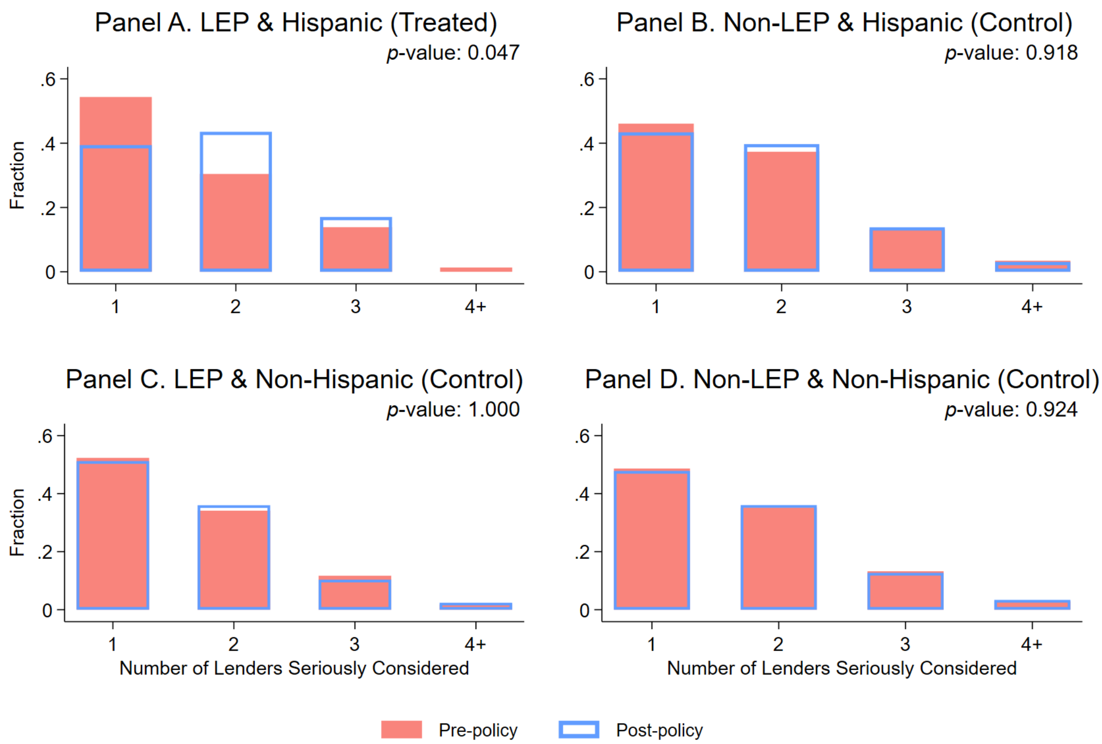
Notes: This figure plots the pre-policy (in red) and post-policy (in blue) proportion of borrowers who redid mortgage paperwork for 4 types of borrowers: LEP and Hispanic in panel A, non-LEP and Hispanic in panel B, LEP and non-Hispanic in panel C, and non-LEP and non-Hispanic in panel D. The number in the bar represents the proportion (e.g., 33% of LEP Hispanic borrowers redid their paperwork before the policy change). The p-values are associated with the null hypothesis that the post-pre difference is larger than -0.05.

Figure 5. Triple-Differences Raw Comparison: Interest Rate



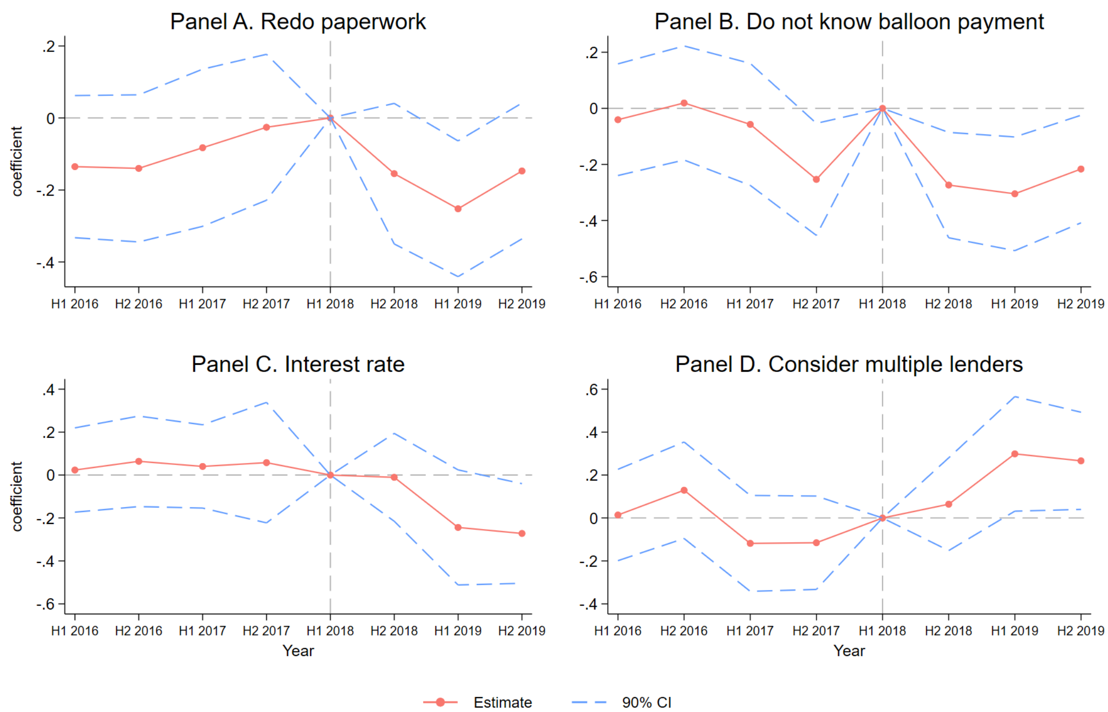
Notes: This figure plots the pre-policy (in red) and post-policy (in blue) average conditional interest rate in basis points for 4 types of borrowers: LEP and Hispanic in panel A, non-LEP and Hispanic in panel B, LEP and non-Hispanic in panel C, and non-LEP and non-Hispanic in panel D. Conditional interest rate is the mean of raw interest rate plus the residual after regressing raw interest rate on origination quarter fixed effects, census tract type fixed effects, loan type, loan term, loan purpose, property tpye, occupancy type, and interest type. The p-values are associated with the null hypothesis that the pre-policy mean is equal to the post-policy mean.

Figure 6. Triple-Differences Raw Comparison: Number of Lenders Seriously Considered



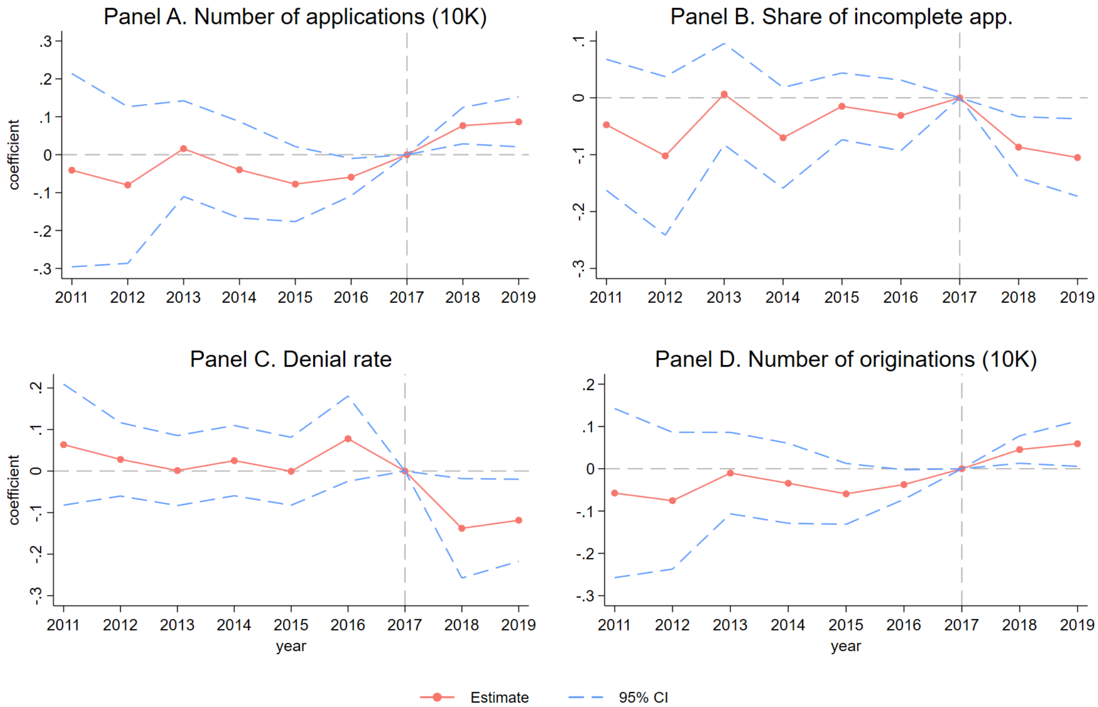
Notes: This figure plots the distribution of the number of lenders seriously considered for 4 types of borrowers: LEP and Hispanic in panel A, non-LEP and Hispanic in panel B, LEP and non-Hispanic in panel C, and non-LEP and non-Hispanic in panel D. The pre-policy distribution is represented by red bars, and the post-policy distribution is represented by blue frames. The number of lenders seriously considered is capped at 4. The p-values are associated with a Kolmogorov-Smirnov test of the equality of the pre-policy and post-policy distribution.

Figure 7. Flexible Difference-in-Differences Estimates of the Effect on LEP Hispanic Borrowers



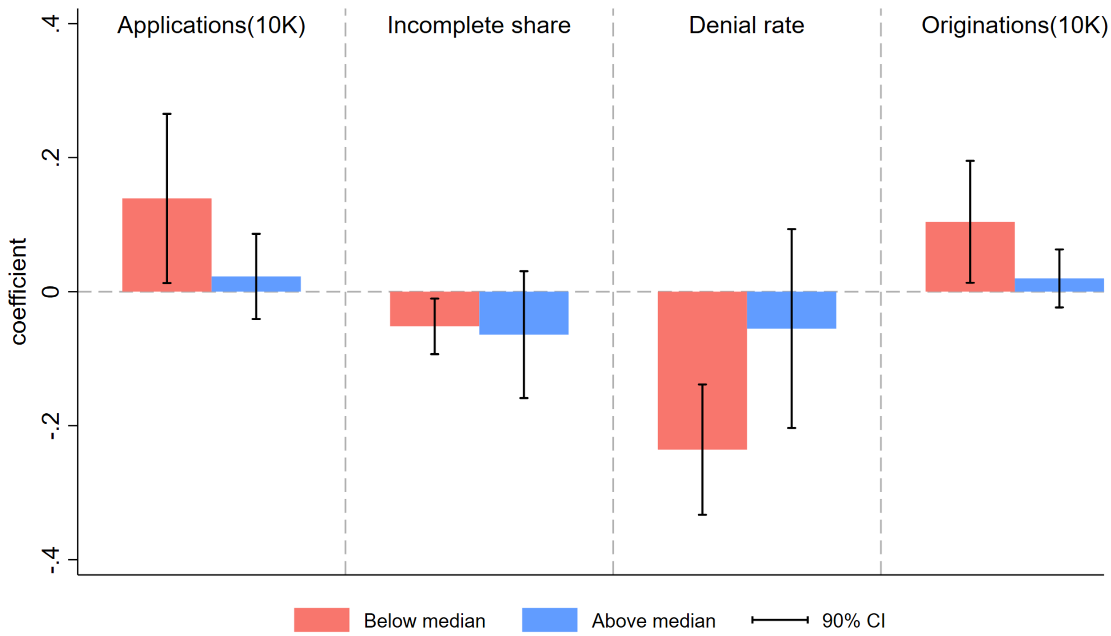
Notes: This figure plots the dynamic policy impact estimated from a flexible difference-in-differences regression where I compare LEP Hispanic borrowers with other borrowers (excluding Asian borrowers). The policy impact can vary every six months, and the coefficient for the first half of 2018 is normalized to zero. For outcomes and control variables, column 1 is same as column 4 in panel B of Table 5, column 2 is same as column 4 in panel C of Table 5, column 3 is same as column 1 in panel A of Table 6, and column 4 is same as column 1 in Table 7. The 90% confidence intervals are based on robust standard error.

Figure 8. Flexible Difference-in-Differences Estimates of the Effect on Mortgage Credit Access of Conventional Purchase Loans



Notes: This figure plots the estimates of β_τ in Equation (4). The dependent variables in panels A to D are the number of mortgage applications (in ten thousands), the share of incomplete applications, the application denial rate, and the number of originations (in ten thousands), respectively. I use the sample of conventional purchase loans in HMDA to calculate these outcomes. The coefficient for 2017 is normalized to zero, so that all estimates can be interpreted as the change in the outcome relative to the year prior to when the policy change went into effect, which is marked by the vertically dashed gray line. All regressions include total population, median household income, the proportion of black people, Asians, and Hispanics, county fixed effects, and state-year fixed effects. The 95% confidence intervals are based on standard errors that are clustered at the state level.

Figure 9. Heterogeneous Effect on Mortgage Credit Access by Social Capital



Notes: This figure plots the difference-in-differences estimates of the policy effect on mortgage credit access of conventional purchase loans and the corresponding 90% confidence intervals. I split the sample based on a county-level community health index (U.S. Congress Joint Economic Committee, 2018). The dependent variables in panels A to D are the number of mortgage applications (in ten thousands), the share of incomplete applications, the application denial rate, and the number of originations (in ten thousands), respectively. The coefficients are obtained by regressing the specifications in Panel A of Table 9.

Figure 10. Heterogeneous Effect on Interest Rate by Social Capital

Notes: This figure plots the triple-difference estimates of the policy effect on interest rate and the corresponding 95% confidence intervals. I split the sample based on a county-level community health index (U.S. Congress Joint Economic Committee, 2018). The dependent variables in panels A to D are the number of mortgage applications (in thousands), the share of incomplete applications, the application denial rate, and the number of originations, respectively. The coefficients are obtained by regressing the specifications in Panel A of Table 9.

Table 1. Summary Statistics of NSMO

Sample	All borrowers (1)	LEP (2)	Non-LEP (3)
Panel A. Demographic characteristics			
Female	0.435	0.454	0.432
	(0.496)	(0.498)	(0.495)
Married	0.666	0.644	0.669
	(0.472)	(0.479)	(0.471)
Age	46.214	46.487	46.182
	(13.854)	(13.817)	(13.858)
College education	0.645	0.534	0.658
	(0.479)	(0.499)	(0.475)
Income<$50K	0.151	0.218	0.143
	(0.358)	(0.413)	(0.350)
FICO score	732.164	722.015	733.330
	(65.924)	(66.552)	(65.752)
Panel B. Mortgage characteristics			
Conventional loan	0.735	0.670	0.742
	(0.441)	(0.470)	(0.437)
Loan amount<$200K	0.510	0.530	0.507
	(0.500)	(0.499)	(0.500)
Loan to value ratio	78.070	79.230	77.937
	(19.462)	(19.285)	(19.478)
Debt to income ratio	36.193	38.396	35.940
	(12.273)	(12.952)	(12.167)
Interest rate	4.029	4.090	4.022
	(0.678)	(0.669)	(0.678)
90-day delinquency	0.015	0.020	0.014
	(0.121)	(0.141)	(0.119)
Observations	37,720	3,793	33,927

Notes: This table reports summary statistics of demographic characteristics (panel A) and mortgage characteristics (panel B) in NSMO. All table entries represent sample means and standard deviations in parentheses, weighted by the analysis weight in NSMO. Summary statistics are presented for all observations in column 1 as well as separately for LEP (column 2) and non-LEP borrowers (column 3).

Table 2. LEP Status and Concern about Qualifying for a Mortgage

Dependent variable	$\mathbb{1}$(concern about qualifying for a mortgage)				
	(1)	(2)	(3)	(4)	(5)
LEP	0.102***	0.100***	0.064***	0.058***	0.059***
	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)
No college degree			0.062***	0.029***	0.029***
			(0.005)	(0.005)	(0.005)
D.V. mean (LEP)			0.243		
D.V. mean (non-LEP)			0.141		
Observations	37,720	37,720	37,720	37,720	37,720
Quarter FEs	No	Yes	Yes	Yes	Yes
Tract type FEs	No	Yes	Yes	Yes	Yes
Race and ethnicity	No	No	Yes	Yes	Yes
Gender	No	No	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes
Additional controls	No	No	Yes	Yes	Yes
Risk FEs	No	No	No	Yes	Yes
Loan controls	No	No	No	No	Yes

Notes: This table reports the relationship between LEP status and an indicator for whether the borrower was concerned about qualifying for a mortgage. There are three types of census tracts: metropolitan CRA LMI tract, metropolitan CRA non-LMI tract, and non-metropolitan tract. Additional controls include age and its squared, marital status, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. Robust standard errors are reported in parentheses. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table 3. LEP Status and Search Behavior

Dependent variable	Number of lenders		Why apply to multiple lenders?		
	seriously considered	applied to	find better loan terms	concern over qualification	learn information
	(1)	(2)	(3)	(4)	(5)
LEP	-0.065***	-0.024**	0.016	0.105***	0.075***
	(0.015)	(0.012)	(0.017)	(0.020)	(0.021)
D.V. mean (LEP)	1.643	1.296	0.821	0.407	0.425
D.V. mean (non-LEP)	1.719	1.303	0.822	0.270	0.319
Observations	37,720	37,720	8,569	8,569	8,569
Quarter FEs	Yes	Yes	Yes	Yes	Yes
Tract type FEs	Yes	Yes	Yes	Yes	Yes
Race and ethnicity	Yes	Yes	Yes	Yes	Yes
Gender	Yes	Yes	Yes	Yes	Yes
Education	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes
Risk FEs	Yes	Yes	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the relationship between LEP status and borrowers' search behavior. The dependent variables in the first two columns are the number of lenders people seriously considered and the number of lenders people finally applied to. The dependent variables in columns 3 to 5 are three dummy variables indicating the reason of applying to multiple lenders. All regressions control for race, ethnicity, gender, and education, as well as origination quarter fixed effects and census tract type fixed effects. Additional controls include age and its squared, marital status, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. Robust standard errors are reported in parentheses. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table 4. LEP Status, Interest Rate, and 90-Day Delinquency

	(1)	(2)	(3)	(4)
	Panel A. Interest rate			
LEP	0.032***	0.029***	0.029***	0.021**
	(0.010)	(0.010)	(0.010)	(0.010)
	Panel B. 90-Day delinquency			
LEP	0.003	0.002	0.002	0.002
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	37,720	37,720	37,720	37,720
Quarter FEs	Yes	Yes	Yes	Yes
Tract type FEs	Yes	Yes	Yes	Yes
Risk FEs	Yes	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes	Yes
Race and ethnicity	No	Yes	Yes	Yes
Gender	No	No	Yes	Yes
Education	No	No	No	Yes

Notes: This table reports the relationship between LEP status and interest rate (panel A) and 90-day mortgage delinquency (panel B). Interest rate is winsorized at 1% and 99% level. Column 1 uses the full sample. All regressions include origination quarter fixed effects and census tract type fixed effects. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan type, loan purpose, loan term, interest type, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. Robust standard errors are reported in parentheses. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table 5. Effect on Perceptions and Experiences

	(1)	(2)	(3)	(4)
Panel A. Dependent variable: $\mathbb{1}$(encounter ... in the application process)				
	Resolve credit report error	Request more income info.	Have more appraisals	Redo paperwork
LEP × Hispanic × Post	-0.163***	-0.162**	-0.125***	-0.137**
	(0.060)	(0.071)	(0.048)	(0.054)
Pre-policy treated mean	0.339	0.642	0.218	0.326
Panel B. Dependent variable: $\mathbb{1}$(do not know if my own mortgage has ...)				
	Adjustable rate	Prepayment penalty	Escrow account	Balloon payment
LEP × Hispanic × Post	-0.083*	0.025	-0.069	-0.164***
	(0.047)	(0.063)	(0.048)	(0.057)
Pre-policy treated mean	0.109	0.296	0.206	0.380
Observations	35,553	35,553	35,553	35,553
Quarter FEs	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Post × Tract type FEs	Yes	Yes	Yes	Yes
Post × Risk FEs	Yes	Yes	Yes	Yes
Post × Loan controls	Yes	Yes	Yes	Yes

Notes: This table reports the triple-difference estimates of the policy effect on perceptions and experiences in the mortgage market. The dependent variables in panel A are indicators for whether the borrower encountered additional problems in the process of getting the mortgage. The dependent variables in panel B are indicators for whether the borrower knew about alternative features of the mortgage. *Post* equals one if the mortgage was originated after June 2018. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, occupancy status, and loan type. All regressions are weighted by the analysis weight in NSMO. Robust standard errors are reported in parentheses. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table 6. Effect on Interest Rate and 90-Day Delinquency

Sample	All	Purchase	Refinance	First-time borrowers	Repeat borrowers
	(1)	(2)	(3)	(4)	(5)
	Panel A. Interest rate				
LEP × Hispanic × Post	-0.149**	-0.165*	-0.082	-0.221*	-0.145
	(0.074)	(0.096)	(0.121)	(0.125)	(0.093)
	Panel B. 90-Day delinquency				
LEP × Hispanic × Post	-0.016	-0.022	-0.022	-0.009	-0.012
	(0.015)	(0.020)	(0.024)	(0.029)	(0.017)
Observations	35,553	18,118	15,977	6,739	28,807
Quarter FEs	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes
Post × Tract type FEs	Yes	Yes	Yes	Yes	Yes
Post × Risk FEs	Yes	Yes	Yes	Yes	Yes
Post × Loan controls	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the triple-difference estimates of the policy effect on interest rate (winsorized at 1% and 99% level) and 90-day delinquency. Column 1 uses the full sample. Columns 2 and 3 use the sample of purchase and refinance loans, respectively. Columns 4 and 5 use the sample of loans borrowed by first-time borrowers and repeat borrowers, respectively. *Post* equals one if the mortgage was originated after June 2018. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan type, loan purpose, loan term, interest rate type, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. Robust standard errors are reported in parentheses. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table 7. Effect on Search Behavior

Dependent variable	Search intensity		Why apply to multiple lenders?		
	$\mathbb{1}$(consider multi. lenders)	# lenders considered	find better loan terms	concern over qualification	learn information
	(1)	(2)	(3)	(4)	(5)
LEP × Hispanic × Post	0.162**	0.202*	0.058	-0.154	-0.269**
	(0.073)	(0.112)	(0.097)	(0.125)	(0.135)
Observations	35,553	35,553	8,001	8,001	8,001
Quarter FEs	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes
Post × Tract type FEs	Yes	Yes	Yes	Yes	Yes
Post × Risk FEs	Yes	Yes	Yes	Yes	Yes
Post × Loan controls	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the triple-difference estimates of the policy effect on search behavior. The dependent variable in column 1 is a dummy variable, indicating that the borrower seriously considered more than one lender. The dependent variable in column 2 is the number of lenders seriously considered, capped at 4. In columns 3 to 5, the dependent variable is an indicator for each of the three reasons why the borrower applied to multiple lenders. *Post* equals one if the mortgage was originated after June 2018. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. Robust standard errors are reported in parentheses. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table 8. Robustness Tests: Difference-in-Differences

Dependent variable	Redo paperwork (1)	Balloon payment (2)	Interest rate (3)	Consider multi. lenders (4)
	Panel A. Sample of Hispanic borrowers			
LEP × Post	-0.117**	-0.133**	-0.106*	0.128*
	(0.054)	(0.054)	(0.064)	(0.070)
Observations	2,933	2,933	2,933	2,933
	Panel B. Sample of LEP borrowers			
Hispanic × Post	-0.157***	-0.135***	-0.095	0.174***
	(0.051)	(0.051)	(0.066)	(0.066)
Observations	3,485	3,485	3,484	3,485
	Panel C. Sample of non-Asian borrowers			
Treated × Post	-0.124***	-0.164***	-0.142**	0.175***
	(0.045)	(0.048)	(0.061)	(0.062)
Observations	35,553	35,553	35,553	35,553
Quarter FEs	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Post × Tract type FEs	Yes	Yes	Yes	Yes
Post × Risk FEs	Yes	Yes	Yes	Yes
Post × Loan controls	Yes	Yes	Yes	Yes

Notes: This table reports three difference-in-differences estimates using NSMO. In panel A, the sample only includes Hispanic borrowers, so the triple-difference model degenerates to a difference-in-differences model where treatment status is determined by LEP status. In panel B, the sample only includes LEP borrowers, so the triple-difference model degenerates to a difference-in-differences model where treatment status is determined by ethnicity. In panel C, the sample excludes Asian borrowers, and I compare LEP Hispanic borrowers with other borrowers. For outcomes and control variables, column 1 is same as column 4 in panel B of Table 5, column 2 is same as column 4 in panel C of Table 5, column 3 is same as column 1 in panel A of Table 6, and column 4 is same as column 1 in Table 7.

Table 9. Effect on Mortgage Credit Access

Dependent variable	# Applications (10K) (1)	Share of incomplete app. (2)	Denial rate (3)	# Originations (10K) (4)
	Panel A. Conventional purchase loans			
LEP share × Post	0.121**	-0.062***	-0.155***	0.089**
	(0.060)	(0.022)	(0.041)	(0.044)
Observations	25,225	25,225	25,225	25,225
	Panel B. FHA purchase loans			
LEP share × Post	-0.088*	-0.113***	-0.088**	-0.048
	(0.048)	(0.021)	(0.035)	(0.030)
Observations	25,059	25,059	25,059	25,059
County FEs	Yes	Yes	Yes	Yes
Year × State FEs	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes

Notes: This table reports the difference-in-differences estimates of the policy effect on mortgage credit access of conventional purchase loans (panel A) and FHA purchase loans (panel B). The dependent variables in columns 1 to 4 are the number of mortgage applications (in ten thousands), the share of incomplete applications, the application denial rate, and the number of originations (in ten thousands), respectively. *LEP share* is the share of LEP Hispanic people before 2019, and the share of LEP Hispanics and Chinese in 2019. *Post* equals one after 2017. All specifications include county fixed effects, state-year fixed effects, and additional controls, which include median household income, total population, and the proportion of Hispanics, Asians, and black people. Standard errors are reported in parentheses and are clustered at the state level. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table 10. Effect on Interest Rate, Discount Points, and Delinquency (HMDA^{+})

Sample	Purchase (1)	First-time borrowers (2)	Repeat borrowers (3)	Channel: retail (4)	Channel: broker (5)
	Panel A. Interest rate				
LEP × Hispanic × Post	-0.035***	-0.052***	-0.004	-0.041***	-0.023*
	(0.009)	(0.011)	(0.012)	(0.011)	(0.013)
Implied lower bound	-0.049	-0.072	-0.006	-0.057	-0.032
Observations	3,877,813	1,680,325	2,196,946	2,513,026	1,364,024
	Panel B. Discount points (% of loan amount)				
LEP × Hispanic × Post	0.006	0.035	-0.052*	0.004	0.035
	(0.018)	(0.023)	(0.031)	(0.025)	(0.025)
Implied lower bound	0.008	0.049	-0.072	0.006	0.049
Observations	1,713,458	780,230	932,503	1,095,149	617,429
	Panel C. 90-Day delinquency				
LEP × Hispanic × Post	-0.013	-0.013	-0.008	-0.014	-0.012
	(0.008)	(0.012)	(0.011)	(0.011)	(0.012)
Implied lower bound	-0.018	-0.018	-0.011	-0.019	-0.017
Observations	3,877,813	1,680,325	2,196,946	2,513,026	1,364,024
Month FEs	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes
Post × County FEs	Yes	Yes	Yes	Yes	Yes
Post × Risk FEs	Yes	Yes	Yes	Yes	Yes
Post × Lender FEs	Yes	Yes	Yes	Yes	Yes
Post × Loan controls	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the triple-differences estimates and the implied lower bound of the effect of language help on the interest rate (panel A), discount points (panel B), and 90-day delinquency (panel C). *Post* equals one if the mortgage was originated after June 2018. Column 1 uses all purchase loans in the HMDA^{+} sample. Columns 2 and 3 use the sample of loans borrowed by first-time borrowers and repeat borrowers, respectively. Columns 4 and 5 use the sample of loans originated through retail lenders and brokers, respectively. All regressions include origination month fixed effects, *Post*-county fixed effects, and *Post*-lender fixed effects. Risk fixed effects are the full pairwise interaction between LTV bins and FICO score bins. LTV bins and FICO score bins follow the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan type, loan term, property type, and occupancy status. All regressions are weighted by the analysis weight in the HMDA^{+} sample. Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Language Frictions in Consumer Credit
Online Appendix

A Data

1 NSMO

I clean the National Survey of Mortgage Originations (NSMO) using a number of steps:

1. Drop survey respondents who are not the mortgage borrowers.
2. Drop survey respondents who have negative FICO scores.
3. Drop properties that are units in a partly commercial structure or land only.
4. Drop properties that are used as homes for other relatives.
5. Keep mortgages with a loan term of 5, 10, 15, 20, and 30 years.
6. Winsorize mortgage rate at the 1st and 99th percentile.
7. Winsorize the number of lenders seriously considered and the number of lenders applied to more than 4.
8. Generate risk fixed effects that are the full pairwise interaction of LTV bins and FICO score bins, following the Loan-Level Price Adjustment (LLPA) Grid of Fannie Mae.[1]

I use the data set following these steps to conduct the descriptive analysis in Section II. Summary statistics of this sample are reported in Table 1. I further drop Asian borrowers in this sample for the causal analysis in Section IV. This is because I can only observe borrowers' race (e.g., Asian) rather than borrowers' primary language (e.g., Chinese and Tagalog), which makes it difficult to precisely define the treatment group after the introduction of Chinese translations in 2019. Summary statistics of this subsample are reported in Table A1.

2 FHA Snapshot^{+}

The most granular geographical level in Ginnie Mae data is the state level, so I match FHA Snapshot data with Ginnie Mae data to supplement detailed location variables.

[1] See https://singlefamily.fanniemae.com/media/9391/display.

I first select fixed rate, purchase or refinance FHA loans that were originated after 2015 from Ginnie Mae data. I merge two data sets based on a set of overlapping variables: state, loan purpose, origination year and month, lender, loan size, and interest rate. Specifically, I allow a 2% difference in loan amount and a 3-month difference in origination month in the first round of fuzzy matching. For each one-to-many match, I keep the match with the smallest size and time difference. To ensure the highest-quality match, I exclude all matches with duplicate observations. The origination time in FHA Snapshot is systematically later than that in Ginnie Mae data. Therefore, I select unmatched mortgages that were originated in the fourth quarter after the first round of matching in Ginnie Mae data, and then I merge them with next year's FHA Snapshot data. I further drop mortgages with missing FICO scores or LTV ratios, and only keep mortgages with a loan term of 120 months, 180 months, 240 months, or 360 months. Following the above steps, the final data set contains 1,783,367 FHA loans originated from 2015 to 2019. Figure A1 shows that the FHA Snapshot$^+$ data set covers about 32% of all FHA loans and 65% of FHA loans sold to Ginnie Mae in HMDA data.

To make this sample representative, I use the reciprocal of the likelihood of being sampled from HMDA data as the analysis weight. This assumes that each mortgage in the FHA Snapshot$^+$ data set is randomly sampled from the corresponding stratum in the HMDA data of FHA loans. I separate all FHA loans in HMDA into different strata based on property county, loan size, loan purpose, and origination year. In other words, the analysis weight indicates how many FHA loans of a certain type in the mortgage market are represented by a given loan in the FHA Snapshot$^+$ sample. Table A2 shows that the mortgage characteristics in the FHA Snapshot$^+$ are very similar as those in Ginnie Mae data.

3 HMDA$^+$

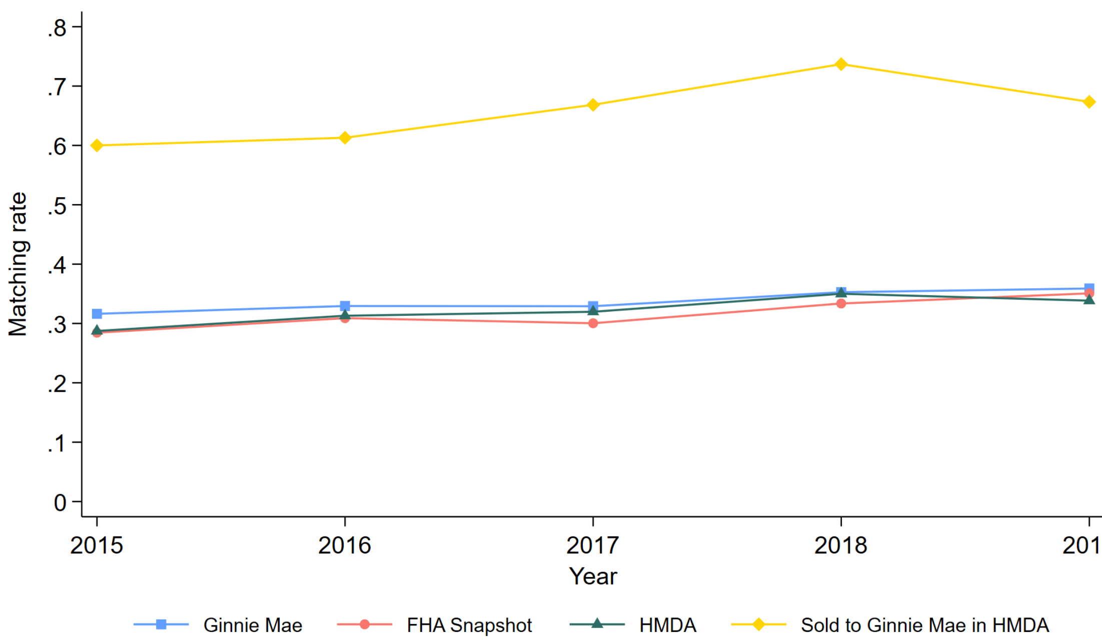
The NSMO data offer rich and unique information about borrowers' mortgage market experiences, but its small sample size could restrict the precision of my estimation. Another concern is that the NSMO data set does not contain location or lender information. To address these concerns, I construct a novel loan-level data set, HMDA$^+$, which contains the most detailed loan-level information of borrowers, lenders, mortgages, and properties. Compared to existing efforts (Saadi, 2020; Bartlett et al., 2022), my HMDA$^+$ data set has three advantages. First, I only use publicly available mortgage data to assemble the final sample. Second, I exploit lenders' names in the matching step to achieve a higher matching rate. Third, analysis weights are developed to represent the population of originated mortgages in HMDA data.

To construct this data set, I first merge Fannie Mae data with HMDA data. I pick one to four-family mortgages sold to Fannie Mae in HMDA data for match. I merge these loans with Fannie Mae data based on loan amount, MSA, 3-digit ZIP code, state, lender, loan purpose, occupancy status, and the number of borrowers. As HMDA started to disclose mortgage rate after 2018, I also use this variable as a match key. To accommodate for rounding differences, I allow a 2% difference in loan amount. For each one-to-many match, I keep the matched pair with the smallest loan size difference. To ensure the highest-quality match, I further exclude all matches with duplicate observations. Because only large lenders are identified in Fannie Mae data, a large amount of Fannie Mae loans will not be matched after the above steps. Therefore, I match the leftover mortgages in both data sets without lender identification. Similarly, I repeat the above steps for mortgages sold to Freddie Mac in HMDA. The final data set contains 8,154,065 GSE loans originated from 2015 to 2019, which covers about 59% of GSE loans in HMDA data. Figure A2 shows that the matching rate increases when interest rate is used for matching.

The second step is to merge FHA Snapshot$^+$ with HMDA. I select one to four-family FHA loans in HMDA for match. I merge two data sets based on county, 5-digit ZIP code, loan purpose, origination year, loan amount, and lender. Interest rate is used as an additional match key after 2018. To accommodate for rounding differences between the two data sets, I allow loan amount to differ by 2% and interest rate to differ by 2 basis point. For each one-to-many match, I keep the match with the smallest size and interest rate difference. To ensure the highest-quality match, I further exclude all matches with duplicate observations. Using this approach, the final data set contains 1,376,241 FHA loans originated from 2015 to 2019, which covers roughly 25% of FHA loans in HMDA data during the same period. The matching rate is lower than that for GSE loans because the baseline data set, FHA Snapshot$^+$, only covers about 32% of all FHA loans. Overall, the HMDA$^+$ data set includes nearly half of GSE and FHA loans in the 2015-19 HMDA data.

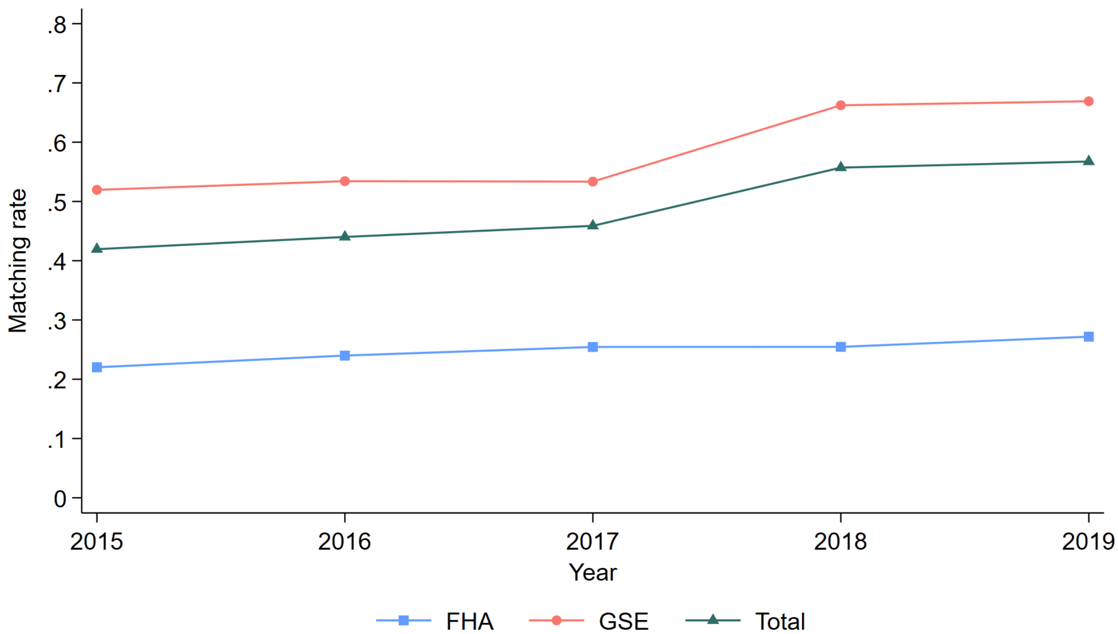
To make the HMDA$^+$ sample representative, I add an analysis weight for each observation, using the reciprocal of the likelihood of being sampled from the HMDA data. The sampling procedure assumes a random sampling within the corresponding stratum. I separate all originated loans in HMDA data into different strata based on property county, loan type, loan size, loan purpose, and origination year. As shown in Table A3, the HMDA^{+} data set represents the whole mortgage market quite well.

Figure A1. FHA Snapshot^{+} Matching Rate



Notes: This figure plots the matching rates between the FHA Snapshot^{+} dataset and Ginnie Mae data, FHA Snapshot data, FHA loans in HMDA, and FHA loans sold to Ginnie Mae in HMDA.

Figure A2. HMDA$^+$ Matching Rate



Notes: This figure plots the matching rates between the HMDA$^+$ dataset and Ginnie Mae data, FHA Snapshot data, FHA loans in HMDA, and FHA loans sold to Ginnie Mae in HMDA.

Table A1. Summary Statistics of NSMO (excluding Asian borrowers)

Sample	All Borrowers (1)	LEP (2)	Non-LEP (3)
Panel A. Demographic Characteristics			
Female	0.439	0.459	0.437
	(0.496)	(0.498)	(0.496)
Married	0.660	0.635	0.663
	(0.474)	(0.481)	(0.473)
Age	46.445	46.711	46.416
	(13.970)	(13.965)	(13.971)
College education	0.632	0.519	0.645
	(0.482)	(0.500)	(0.479)
Income<$50K	0.155	0.222	0.147
	(0.362)	(0.415)	(0.354)
FICO score	730.823	719.668	732.079
	(66.294)	(66.562)	(66.148)
Panel B. Mortgage Characteristics			
Conventional loan	0.725	0.653	0.733
	(0.447)	(0.476)	(0.442)
Loan amount<$200K	0.524	0.551	0.521
	(0.499)	(0.497)	(0.500)
Loan to value ratio	78.419	79.899	78.252
	(19.474)	(19.243)	(19.493)
Debt to income ratio	36.213	38.452	35.961
	(12.323)	(13.008)	(12.218)
Interest rate	4.040	4.110	4.032
	(0.676)	(0.665)	(0.676)
90-Day delinquency	0.016	0.021	0.015
	(0.124)	(0.145)	(0.121)
Observations	35,553	3,489	32,064

Notes: This table reports summary statistics of demographic characteristics (panel A) and mortgage characteristics (panel B) in NSMO excluding Asian borrowers. All table entries represent sample means and standard deviations in parentheses, weighted by the analysis weight in NSMO. Summary statistics are presented for all observations in column 1 as well as separately for LEP (column 2) and non-LEP borrowers (column 3).

Table A2. Summary Statistics of the FHA Snapshot^{+} Sample

Sample	FHA Snapshot^{+} (1)	Ginnie Mae (2)
Purchase	0.753	0.745
	(0.431)	(0.436)
Interest rate	4.159	4.176
	(0.586)	(0.590)
Loan amount($1K)	209.186	205.926
	(104.120)	(103.770)
LTV	92.956	93.100
	(9.370)	(9.150)
DTI	42.016	41.962
	(9.417)	(9.288)
FICO scores	671.276	676.082
	(50.038)	(49.031)
Observations	1,783,367	5,298,341

Notes: This table reports descriptive statistics of the FHA Snapshot^{+} dataset (column 1) and the Ginnie Mae dataset (column 2). All table entries represent sample means and standard deviations in parentheses. Summary statistics in column 1 are weighted by the the reciprocal of the likelihood of being sampled from the HMDA data of FHA loans.

Table A3. Summary Statistics of the HMDA$^+$ Sample

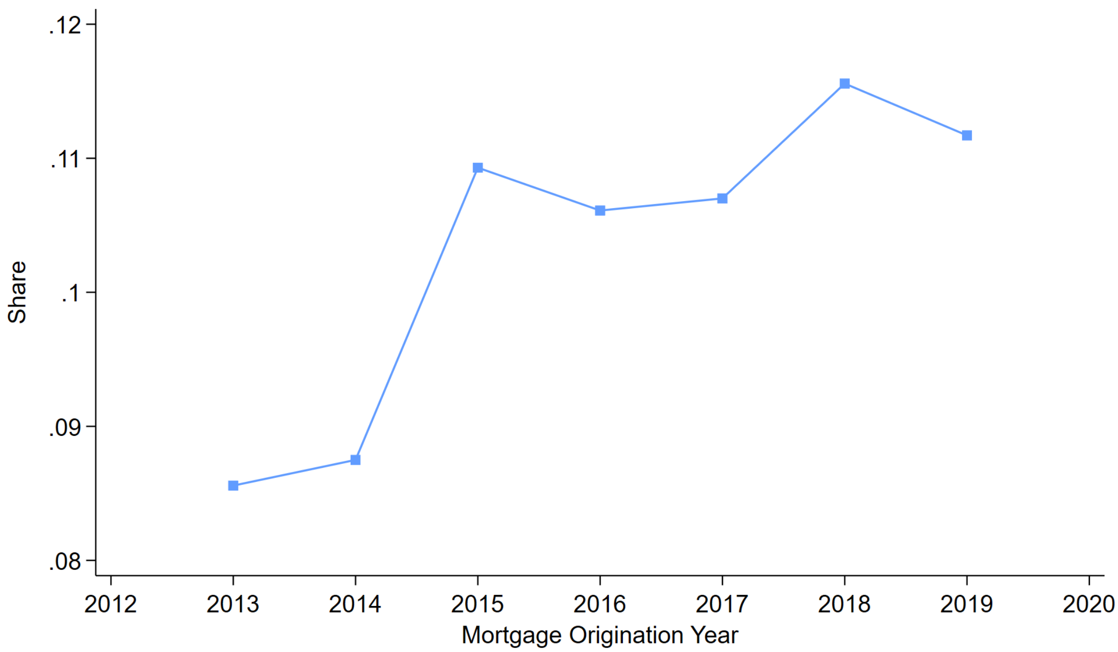
Sample	HMDA$^+$ (1)	HMDA (2)
GSE loans	0.726	0.714
	(0.446)	(0.452)
Purchase loans	0.581	0.581
	(0.493)	(0.493)
Owner occupied	0.905	0.914
	(0.293)	(0.281)
Loan amount	230.351	228.073
	(118.585)	(135.285)
Female	0.339	0.340
	(0.473)	(0.474)
Black	0.073	0.072
	(0.261)	(0.259)
Asian	0.070	0.067
	(0.255)	(0.249)
Hispanic	0.122	0.126
	(0.328)	(0.331)
Income	101.351	100.722
	(85.129)	(444.782)
Observations	9,530,306	19,449,814

Notes: This table reports descriptive statistics of the HMDA$^+$ dataset (column 1) and the HMDA dataset (column 2). All table entries represent sample means and standard deviations in parentheses. Summary statistics in column 1 are weighted by the the reciprocal of the likelihood of being sampled from HMDA data.

B LEP Borrowers in the Mortgage Market

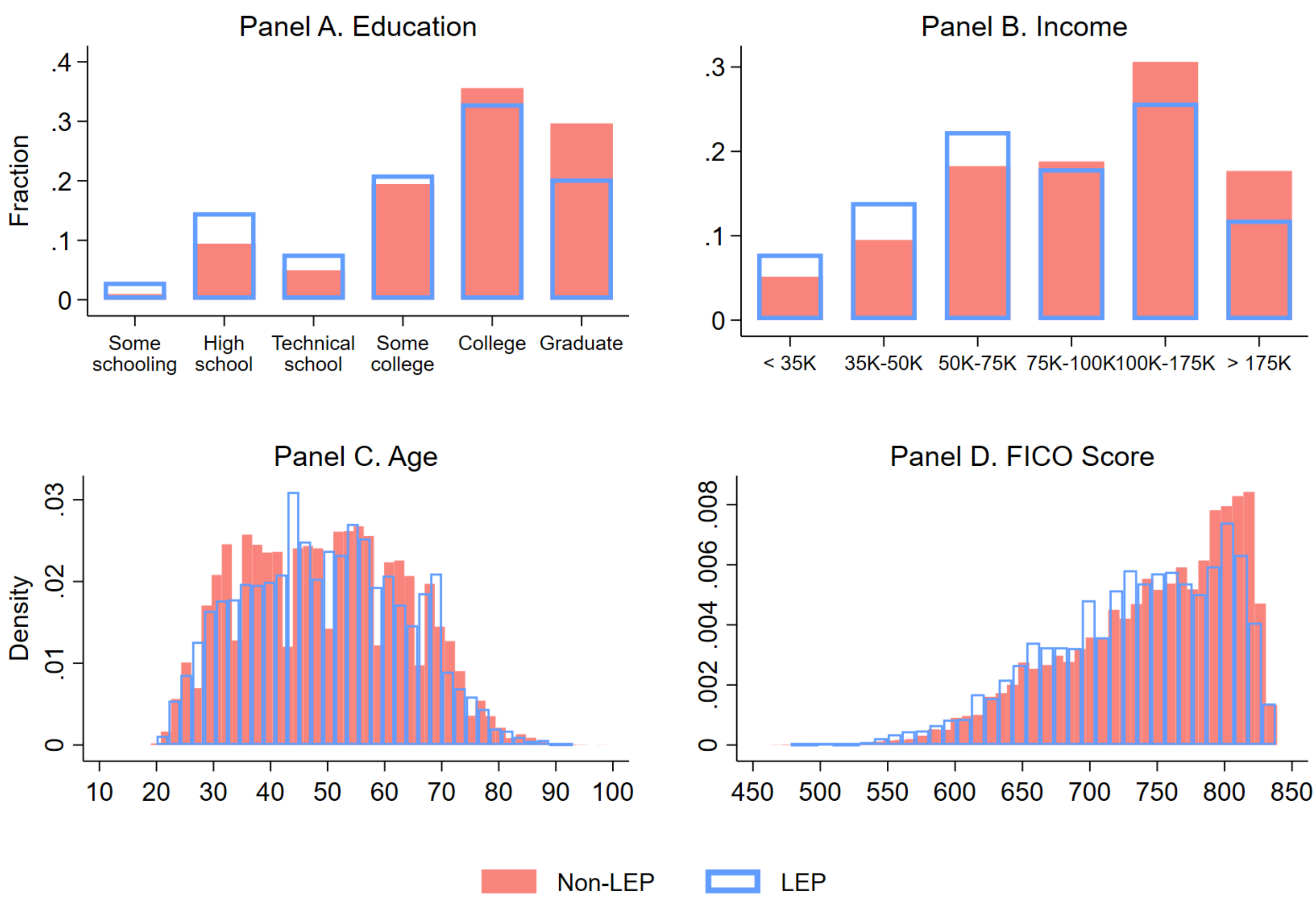
1 Additional Figures

Figure B1. Share of LEP Mortgage Borrowers



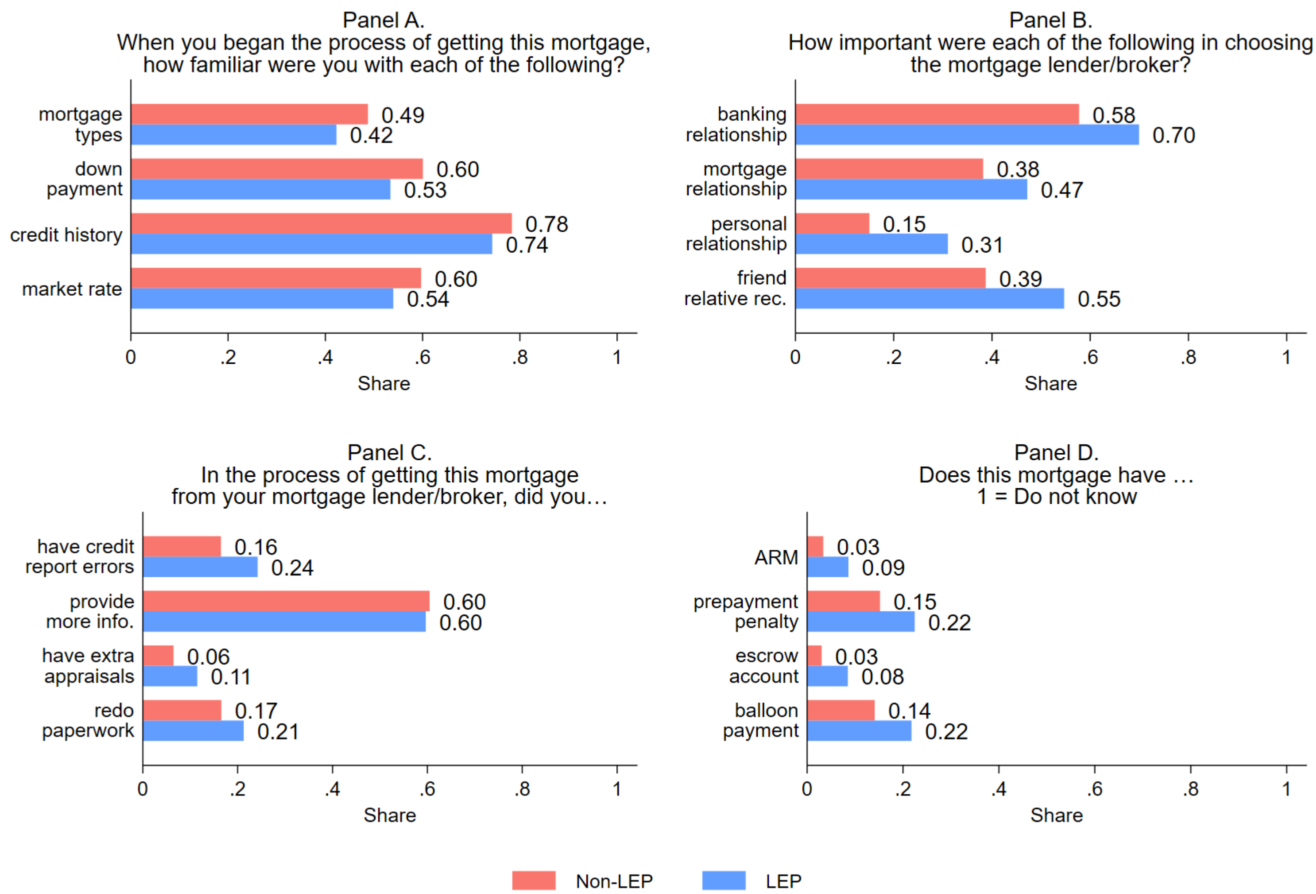
Notes: This figure plots the share of LEP mortgage borrowers from 2013 to 2019 in the NSMO sample.

Figure B2. Comparison of Demographic Characteristics



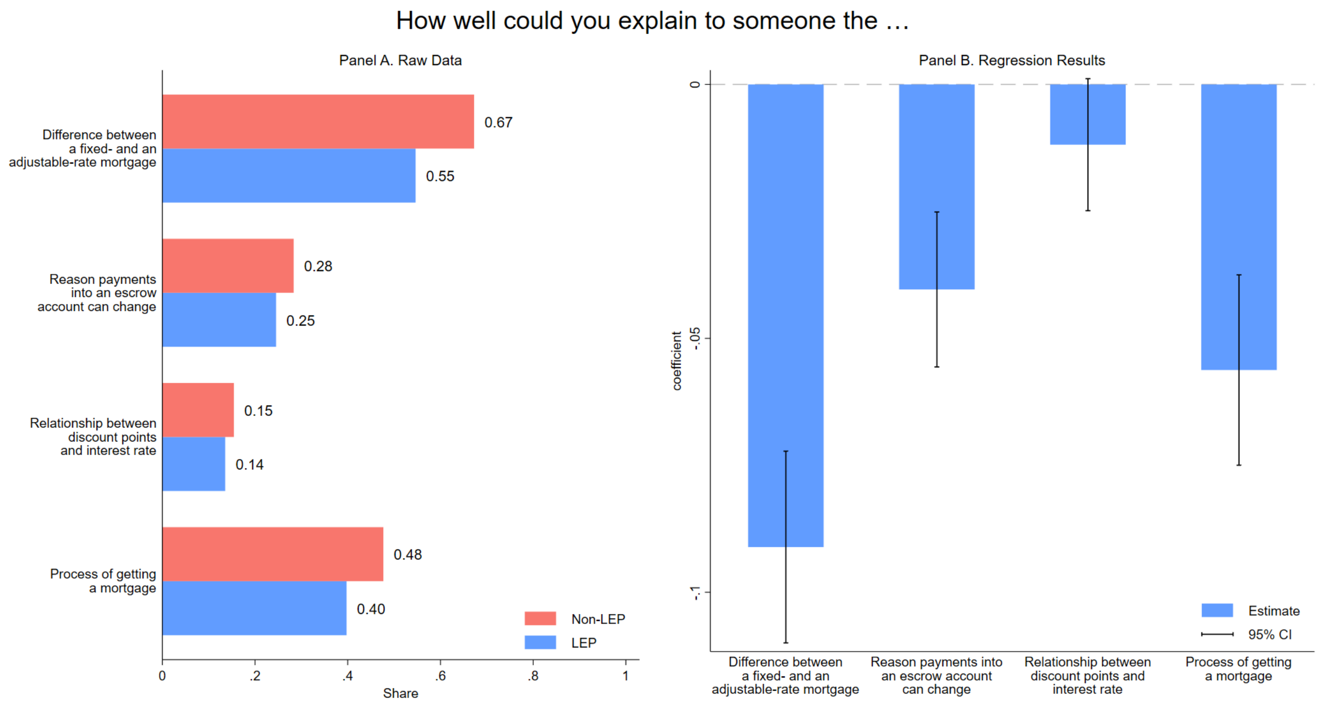
Notes: This figure plots the distribution of selected demographic characteristics for LEP and non-LEP borrowers in the NSMO sample. The demographic characteristics are educational level (panel A), the age of taking out the current mortgage (panel B), annual household income (panel C), and borrower's FICO score (panel D).

Figure B3. Perceptions and Experiences of LEP and non-LEP Borrowers



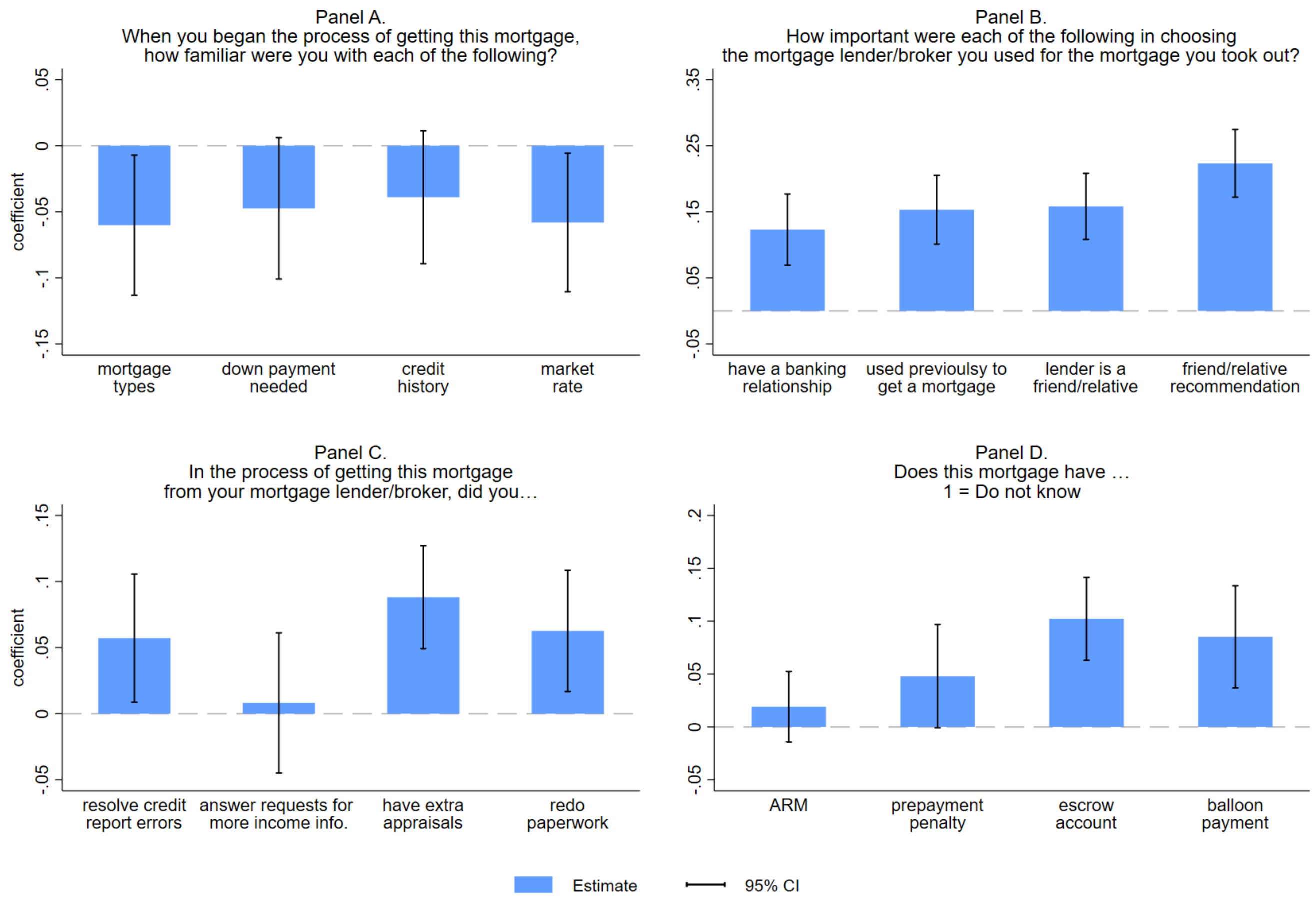
Notes: Panel A of this figure plots the proportion of LEP and non-LEP borrowers who was familiar with each of four things about the mortgage market. Panel B plots the proportion of LEP and non-LEP borrowers who thought each of four factors were important when choosing the mortgage lender. Panel C plots the proportion of LEP and non-LEP borrowers who had each of four problems in the process of getting the mortgage. Panel D plots the proportion of LEP and non-LEP borrowers who knew about each of four alternative features of the mortgage. All statistics are weighted by the analysis weight in NSMO.

Figure B4. LEP Status and Ability to Explain Concepts about Mortgages



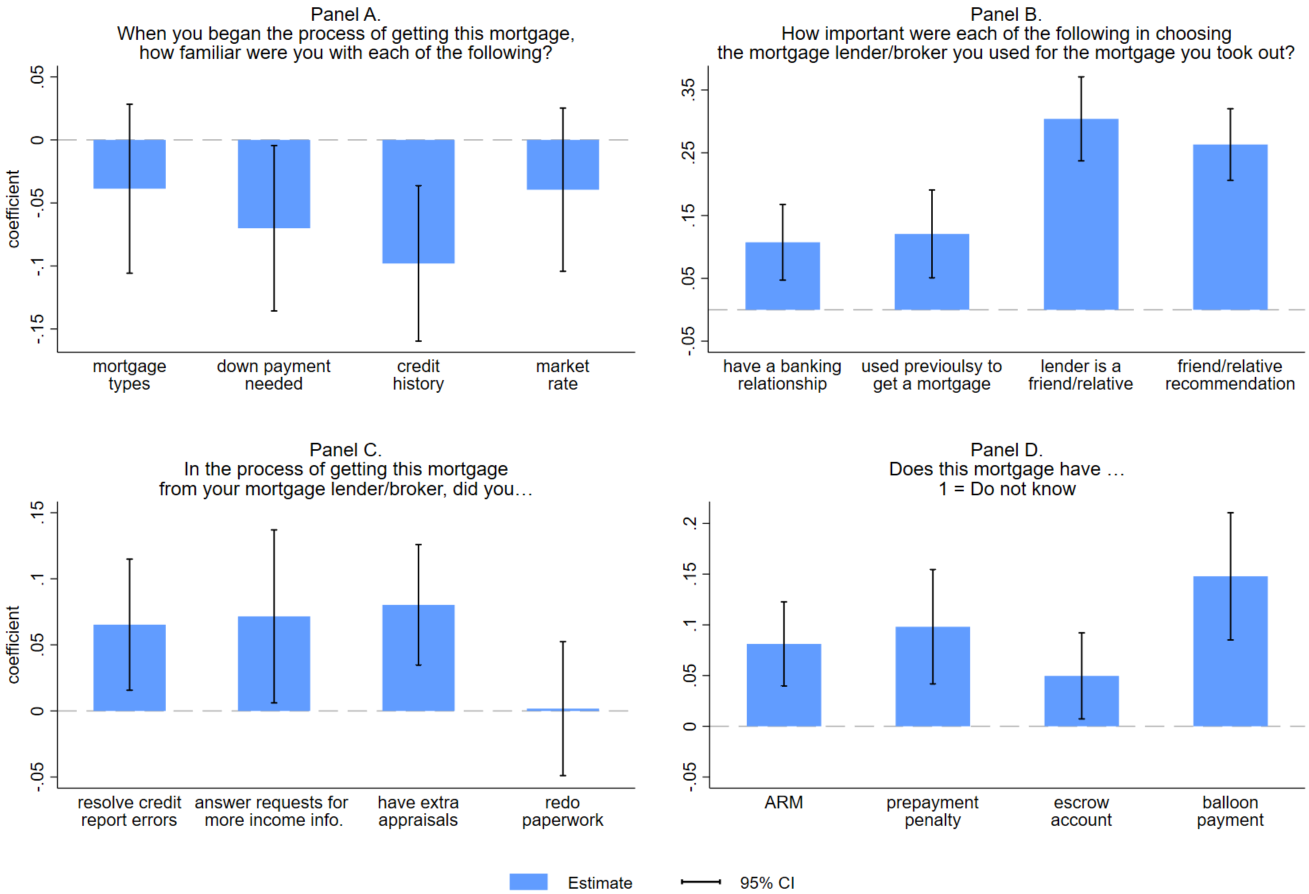
Notes: Panel A of this figure plots the proportion of LEP and non-LEP borrowers who can each explain four subtle concepts about mortgage to others very well. Panel B plots the corresponding estimate of β in Equation (1) and its 95% confidence interval. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. The 95% confidence intervals are based on robust standard errors.

Figure B5. Estimated Differences between LEP and non-LEP Borrowers (Hispanic Borrowers)



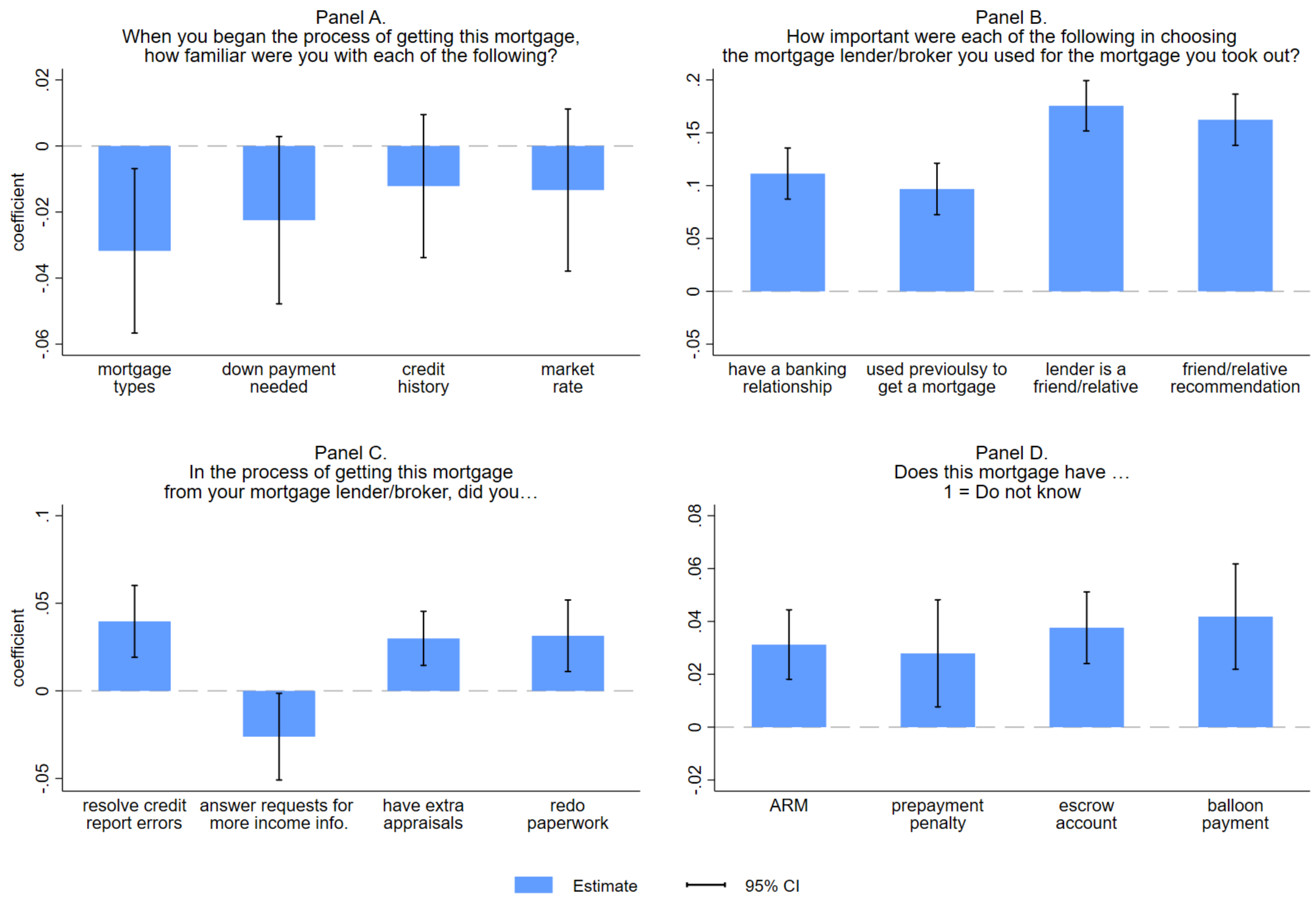
Notes: This figure plots the estimate of β in Equation (1) and its 95% confidence interval, using the sample of Hispanic borrowers. The outcomes are indicators for whether the borrowers was familiar with four things about the mortgage market in panel A, indicators for whether the borrower thought four factors were important when choosing the mortgage lender in panel B, indicators for whether the borrower had four problems in the process of getting the mortgage in panel C, and indicators for whether the borrower knew about four alternative features of the mortgage in panel D. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. The 95% confidence intervals are based on robust standard errors.

Figure B6. Estimated Differences between LEP and non-LEP Borrowers (Asian Borrowers)



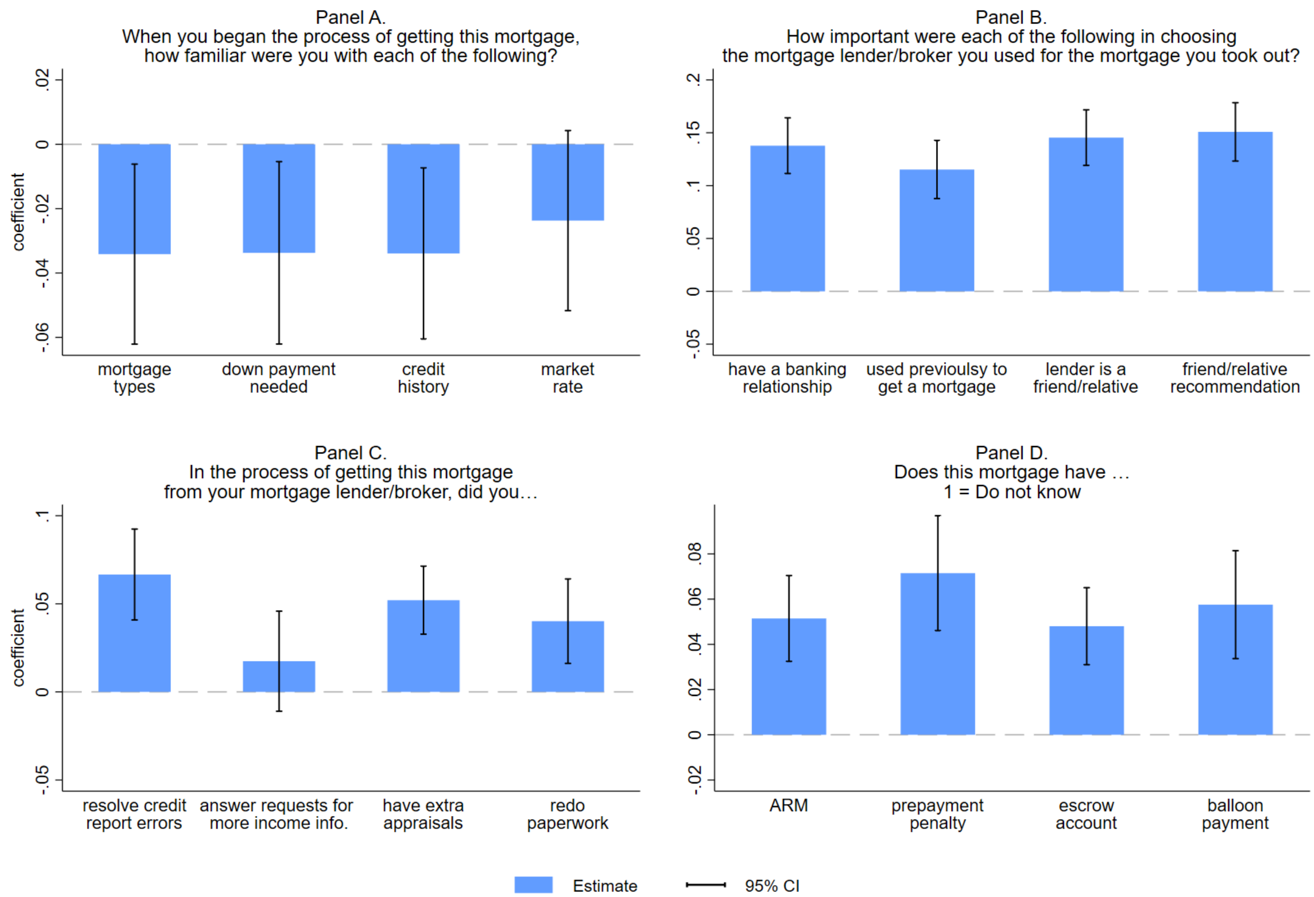
Notes: This figure plots the estimate of β in Equation (1) and its 95% confidence interval, using the sample of Asian borrowers. The outcomes are indicators for whether the borrowers was familiar with four things about the mortgage market in panel A, indicators for whether the borrower thought four factors were important when choosing the mortgage lender in panel B, indicators for whether the borrower had four problems in the process of getting the mortgage in panel C, and indicators for whether the borrower knew about four alternative features of the mortgage in panel D. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. The 95% confidence intervals are based on robust standard errors.

Figure B7. Estimated Differences between LEP and non-LEP Borrowers (College Graduates)



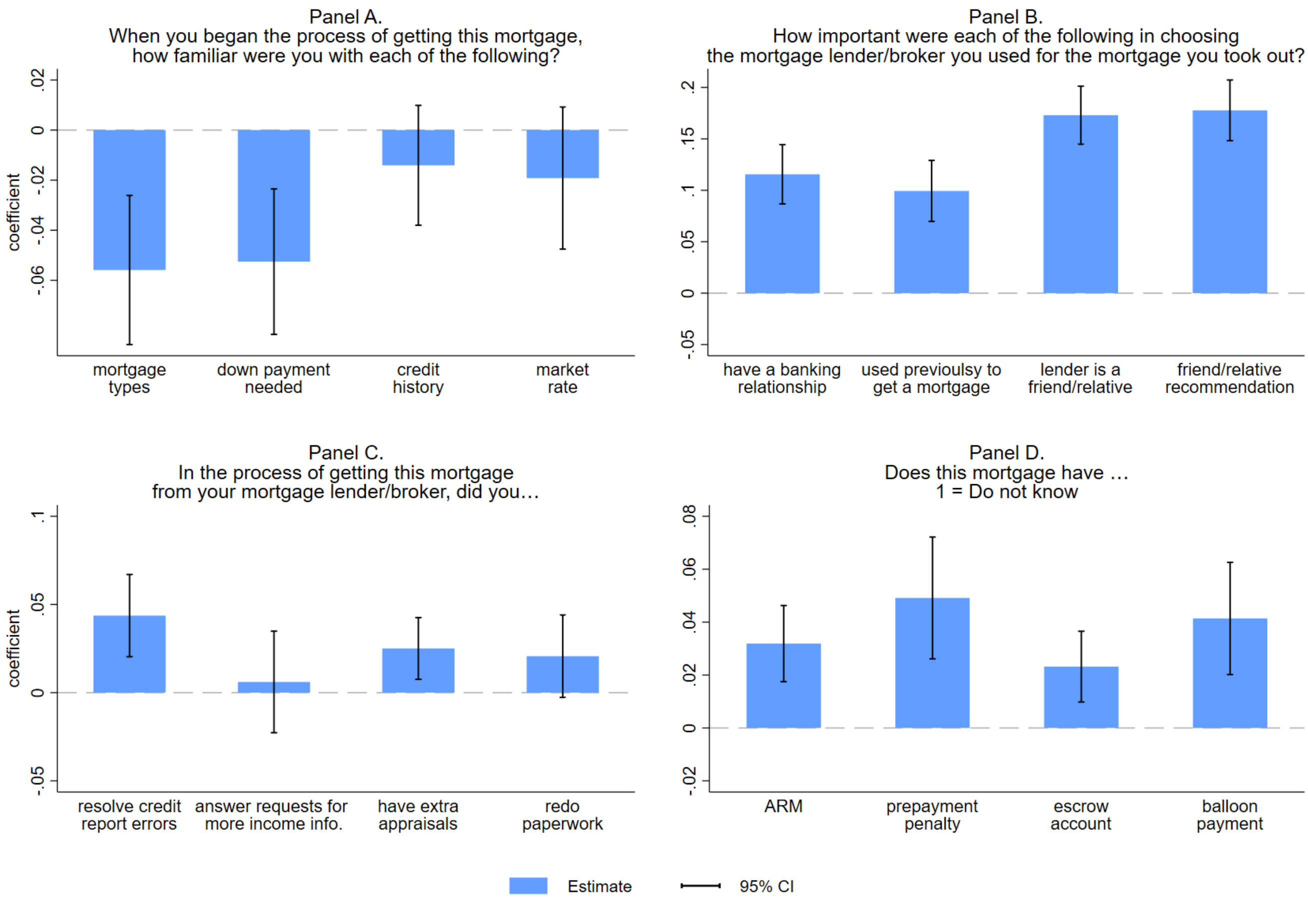
Notes: This figure plots the estimate of β in Equation (1) and its 95% confidence interval, using the sample of college graduates. The outcomes are indicators for whether the borrowers was familiar with four things about the mortgage market in panel A, indicators for whether the borrower thought four factors were important when choosing the mortgage lender in panel B, indicators for whether the borrower had four problems in the process of getting the mortgage in panel C, and indicators for whether the borrower knew about four alternative features of the mortgage in panel D. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. The 95% confidence intervals are based on robust standard errors.

Figure B8. Estimated Differences between LEP and non-LEP Borrowers (Non-College Graduates)



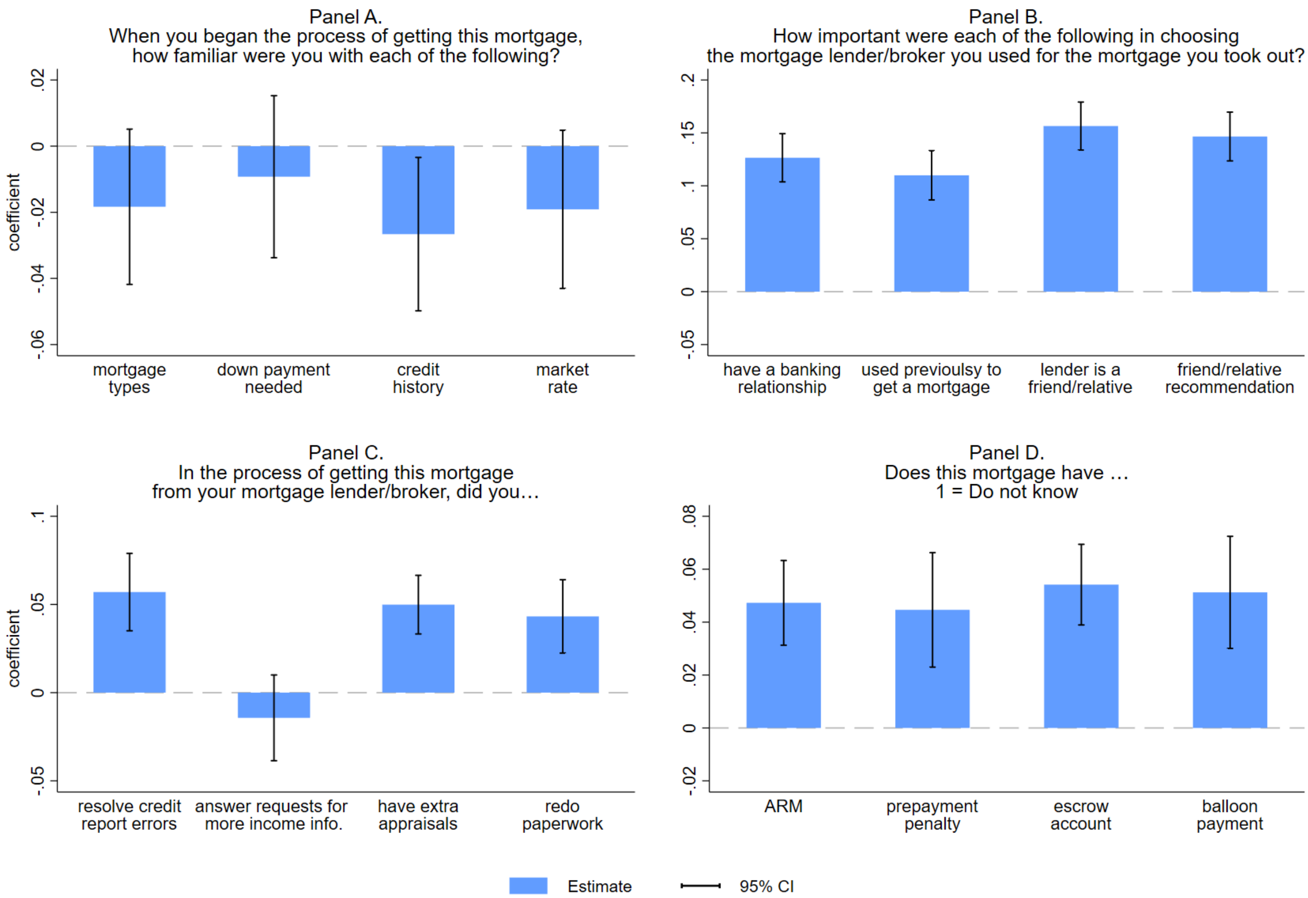
Notes: This figure plots the estimate of β in Equation (1) and its 95% confidence interval, using the sample of borrowers without a bachelor's degree or higher. The outcomes are indicators for whether the borrowers was familiar with four things about the mortgage market in panel A, indicators for whether the borrower thought four factors were important when choosing the mortgage lender in panel B, indicators for whether the borrower had four problems in the process of getting the mortgage in panel C, and indicators for whether the borrower knew about four alternative features of the mortgage in panel D. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. The 95% confidence intervals are based on robust standard errors.

Figure B9. Estimated Differences between LEP and non-LEP Borrowers (High Income)



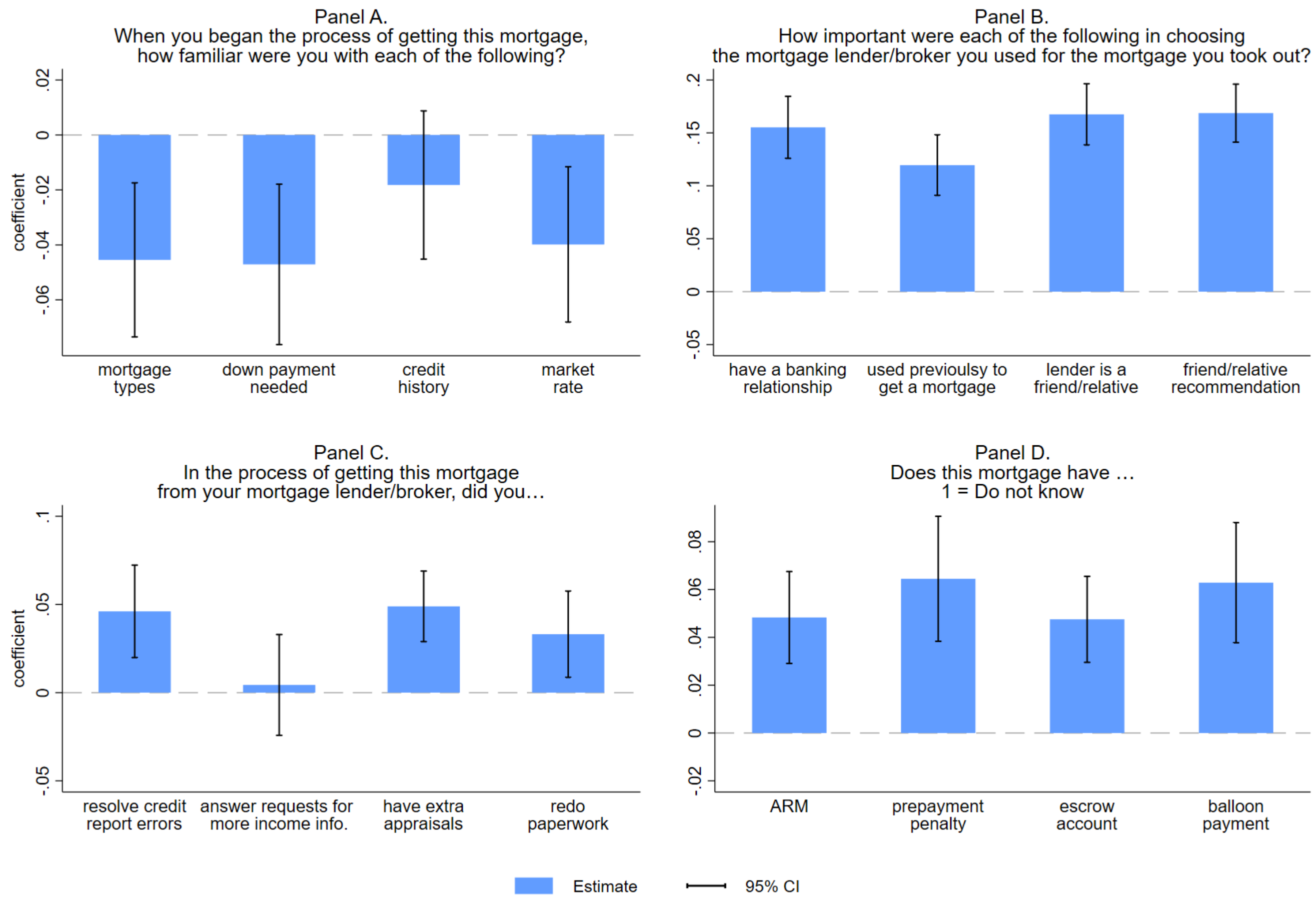
Notes: This figure plots the estimate of β in Equation (1) and its 95% confidence interval, using the sample of borrowers with household incomes exceeding \$100,000. The outcomes are indicators for whether the borrowers was familiar with four things about the mortgage market in panel A, indicators for whether the borrower thought four factors were important when choosing the mortgage lender in panel B, indicators for whether the borrower had four problems in the process of getting the mortgage in panel C, and indicators for whether the borrower knew about four alternative features of the mortgage in panel D. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. The 95% confidence intervals are based on robust standard errors.

Figure B10. Estimated Differences between LEP and non-LEP Borrowers (Low Income)



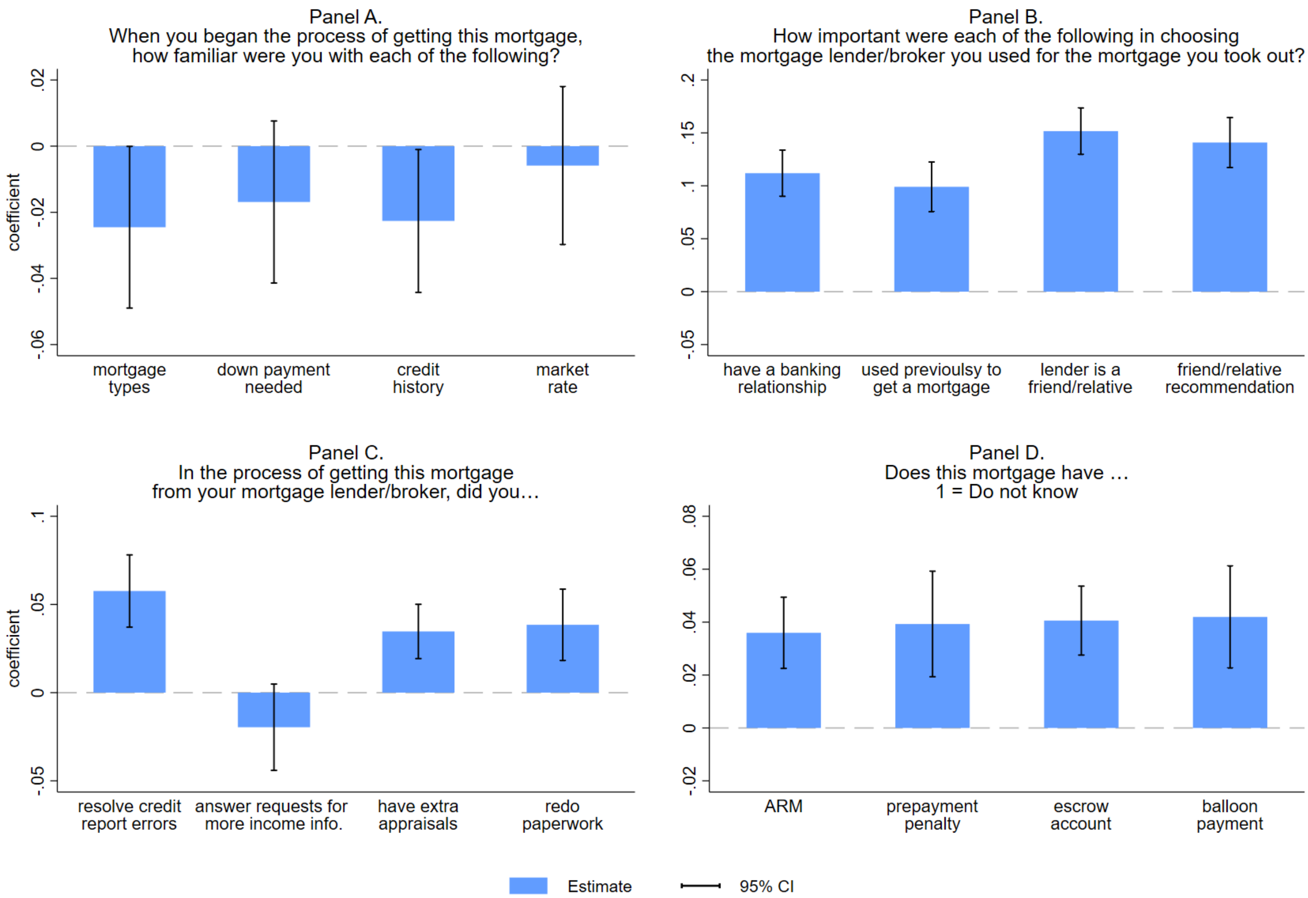
Notes: This figure plots the estimate of β in Equation (1) and its 95% confidence interval, using the sample of borrowers with household incomes less than \$100,000. The outcomes are indicators for whether the borrowers was familiar with four things about the mortgage market in panel A, indicators for whether the borrower thought four factors were important when choosing the mortgage lender in panel B, indicators for whether the borrower had four problems in the process of getting the mortgage in panel C, and indicators for whether the borrower knew about four alternative features of the mortgage in panel D. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. The 95% confidence intervals are based on robust standard errors.

Figure B11. Estimated Differences between LEP and non-LEP Borrowers (Through Brokers)



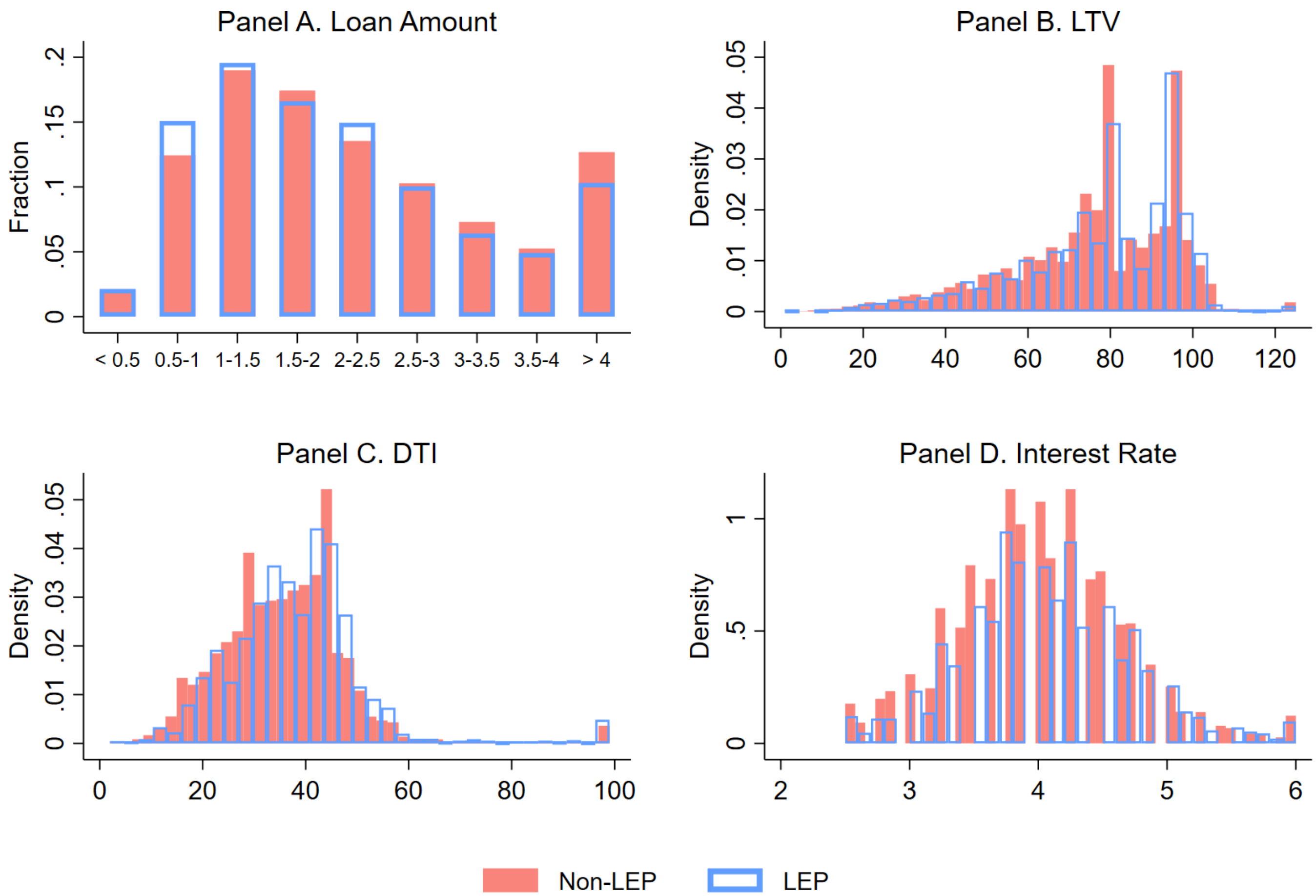
Notes: This figure plots the estimate of β in Equation (1) and its 95% confidence interval, using the sample of borrowers who applied through mortgage brokers. The outcomes are indicators for whether the borrowers was familiar with four things about the mortgage market in panel A, indicators for whether the borrower thought four factors were important when choosing the mortgage lender in panel B, indicators for whether the borrower had four problems in the process of getting the mortgage in panel C, and indicators for whether the borrower knew about four alternative features of the mortgage in panel D. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. The 95% confidence intervals are based on robust standard errors.

Figure B12. Estimated Differences between LEP and non-LEP Borrowers (Through Lenders)



Notes: This figure plots the estimate of β in Equation (1) and its 95% confidence interval, using the sample of borrowers who applied directly to lenders. The outcomes are indicators for whether the borrowers was familiar with four things about the mortgage market in panel A, indicators for whether the borrower thought four factors were important when choosing the mortgage lender in panel B, indicators for whether the borrower had four problems in the process of getting the mortgage in panel C, and indicators for whether the borrower knew about four alternative features of the mortgage in panel D. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. The 95% confidence intervals are based on robust standard errors.

Figure B13. Comparison of Mortgage Characteristics



Notes: This figure plots the distribution of selected mortgage characteristics for LEP and non-LEP borrowers in the NSMO sample. The mortgage characteristics are loan amount in $100K (panel A), loan-to-value ratio (panel B), debt-to-income ratio (panel C), and interest rate (panel D).

2 Additional Tables

Table B1. LEP Status and Search Behavior

Dependent variable	Number of lenders		Why apply to multiple lenders?		
	seriously considered	applied to	find better loan terms	concern over qualification	learn information
	(1)	(2)	(3)	(4)	(5)
LEP	-0.065***	-0.024**	0.016	0.105***	0.075***
	(0.015)	(0.012)	(0.017)	(0.020)	(0.021)
Hispanic	0.049***	0.065***	0.012	0.043**	0.098***
	(0.018)	(0.015)	(0.016)	(0.019)	(0.021)
Asian	0.110***	0.058***	0.005	0.117***	0.133***
	(0.021)	(0.017)	(0.017)	(0.022)	(0.024)
Black	0.110***	0.116***	0.007	0.006	0.041*
	(0.021)	(0.017)	(0.018)	(0.021)	(0.022)
D.V. mean (LEP)	1.643	1.296	0.821	0.407	0.425
D.V. mean (non-LEP)	1.719	1.303	0.822	0.270	0.319
Observations	37,720	37,720	8,569	8,569	8,569
Quarter FEs	Yes	Yes	Yes	Yes	Yes
Tract type FEs	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes
Risk FEs	Yes	Yes	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the relationship between LEP status and borrowers' search behavior. The dependent variables in the first two columns are the number of lenders people seriously considered and the number of lenders people finally applied to. The dependent variables in columns 3 to 5 are three dummy variables indicating the reason of applying to multiple lenders. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. Robust standard errors are reported in parentheses. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table B2. LEP Status, Interest Rate, and 90-Day Delinquency

Sample	All	Purchase	Refinance	First-time borrowers	Repeat borrowers
	(1)	(2)	(3)	(4)	(5)
	Panel A. Interest rate				
LEP	0.032***	0.027**	0.034**	0.038*	0.028***
	(0.010)	(0.014)	(0.013)	(0.021)	(0.010)
	Panel B. 90-Day delinquency				
LEP	0.003	0.005	0.0002	0.005	0.001
	(0.003)	(0.004)	(0.004)	(0.007)	(0.003)
Observations	37,720	19,268	16,937	7,338	30,382
Quarter FEs	Yes	Yes	Yes	Yes	Yes
Tract type FEs	Yes	Yes	Yes	Yes	Yes
Risk FEs	Yes	Yes	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the relationship between LEP status and interest rate (panel A) and 90-day mortgage delinquency (panel B). Interest rate is winsorized at 1% and 99% level. Column 1 uses the full sample. Columns 2 and 3 use the sample of purchase and refinance loans, respectively. Columns 4 and 5 use the sample of loans borrowed by first-time borrowers and repeat borrowers, respectively. All regressions include origination quarter fixed effects and census tract type fixed effects. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan type, loan purpose, loan term, interest type, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. Robust standard errors are reported in parentheses. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

C Robustness Checks

1 UAD

The triple-difference estimates using NSMO have implied that reducing language frictions could streamline LEP Hispanic borrowers' application process. In line with this, I estimate a difference-in-differences model below to present suggestive evidence of the positive policy impact on LEP borrowers' experience with property appraisals.

The outcome of interest is the fraction of home appraisals receiving a lower price than the contract price. When this unexpected type of appraisal occurs, the mortgage amount will be capped because lenders base the loan on the appraisal. This could delay or even derail the mortgage closing. A common solution to this problem is to schedule another home appraisal in the hopes of obtaining a higher appraised value. Therefore, I use this ratio as a measure of how troublesome the mortgage application is at the county level. The data come from the Uniform Appraisal Dataset (UAD) Aggregate Statistics Data File, which aggregates Enterprise single family appraisal records at the county-quarter level.[2]

My difference-in-differences results from estimating Equation (3) at the county-quarter level demonstrate a positive effect on LEP borrowers' appraisal experience. Column 1 of Table C1 shows that the probability of the appraised price below the contract price decreased by about 16.1 percentage points for treated borrowers following the policy shock. This result echoes the triple-difference estimate reported in Table 5, where I find that the probability of having more than one home appraisal decreased by 12.5 percentage points for LEP Hispanic borrowers. Interestingly, the magnitudes of two estimates are quite close, suggesting that the share of low-priced appraisals can be used as a proxy for the inconvenience of getting a mortgage. Columns 2 to 4 of this table show that the 25th percentile, median, and 75th percentile of the appraised value did not change much after the policy shock. These results suggest that the FHFA Language Access Plan had little impact on the local housing market, which is consistent with the intention and content of this policy.

[2]The UAD appraisal records only include loans requiring traditional appraisals. For each property, it keeps the final appraisal record. Therefore, I use the proportion instead of the count of appraisals to study if borrowers encounter more than one home appraisal.

Table C1. Effect on Home Appraisals

Dependent variable	Share of appraisal < offer (1)	Appraised value (p25) (2)	Appraised value (p50) (3)	Appraised value (p75) (4)
LEP share $\times$ Post	-0.161***	0.062	-0.051	0.015
	(0.026)	(0.055)	(0.074)	(0.126)
Observations	36,216	36,216	36,216	36,216
County FEs	Yes	Yes	Yes	Yes
Quarter FEs	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes

Notes: This table reports the difference-in-differences estimates of the policy effect on home appraisals. The dependent variables in columns 1 to 4 are the share of appraisals with a value lower than the contract price, the 25th percentile appraised value, the median appraised value, and the 75th percentile appraised value, respectively. *LEP share* is the share of LEP Hispanic people before July 2019, and the share of LEP Hispanics and Chinese starting from July 2019. *Post* equals one after June 2018. All specifications include county fixed effects, quarter fixed effects, and additional controls, which include median household income, total population, and the proportion of Hispanics, Asians, and black people. Standard errors are reported in parentheses and are clustered at the state level. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

2 NSMO

Table C2. Effect on Mortgage Performance: Robustness

Sample	All	Purchase	Refinance	First-time borrowers	Repeat borrowers
	(1)	(2)	(3)	(4)	(5)
	Panel A. 60-Day delinquency				
LEP × Hispanic × Post	-0.009	0.002	-0.041	0.013	-0.032
	(0.026)	(0.043)	(0.028)	(0.053)	(0.021)
	Panel B. Default				
LEP × Hispanic × Post	-0.003	-0.003	-0.004	0.002	-0.000
	(0.008)	(0.012)	(0.008)	(0.016)	(0.004)
Observations	35,553	18,118	15,977	6,739	28,807
Quarter FEs	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes
Post × Tract type FEs	Yes	Yes	Yes	Yes	Yes
Post × Risk FEs	Yes	Yes	Yes	Yes	Yes
Post × Loan controls	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the triple-difference estimates of the policy effect on 60-day delinquency and default. Column 1 uses the full sample. Columns 2 and 3 use the sample of purchase and refinance loans, respectively. Columns 4 and 5 use the sample of loans borrowed by first-time borrowers and repeat borrowers, respectively. *Post* equals one if the mortgage was originated after June 2018. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan type, loan purpose, loan term, interest rate type, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. Robust standard errors are reported in parentheses. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table C3. Robustness Test: Drop Mortgages Originated after June 2019

Dependent variable	Redo paperwork (1)	Balloon payment (2)	Interest rate (3)	Consider multi. lenders (4)
LEP × Hispanic × Post	-0.148**	-0.208***	-0.091	0.143
	(0.064)	(0.067)	(0.088)	(0.088)
Observations	34,871	34,871	34,871	34,871
Quarter FEs	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Post × Tract type FEs	Yes	Yes	Yes	Yes
Post × Risk FEs	Yes	Yes	Yes	Yes
Post × Loan controls	Yes	Yes	Yes	Yes

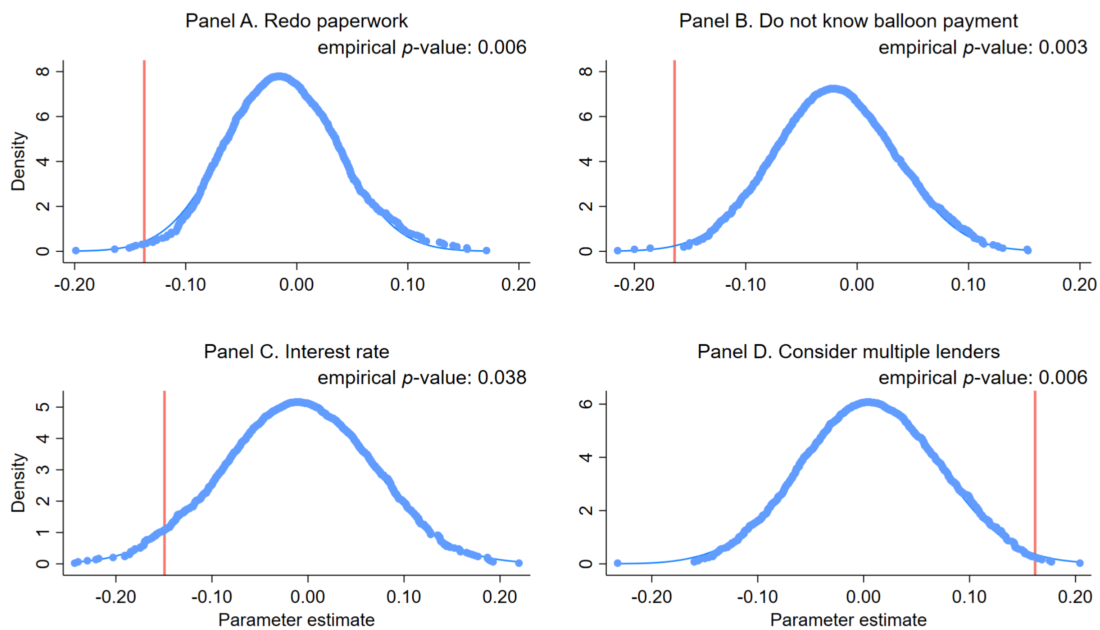
Notes: This table reports the triple-difference estimates in the NSMO sample that excludes mortgages originated after June 2019. *Post* equals one if the mortgage was originated after June 2018. Column 1 uses the same specification as column 4 in Panel B of Table 5. Column 2 uses the same specification as column 4 in Panel C of Table 5. Column 3 uses the same specification as column 1 in Panel A of Table 6. Column 4 uses the same specification as column 1 in Table 7.

Table C4. Placebo Tests

Dependent variable	Redo paperwork (1)	Balloon payment (2)	Interest rate (3)	Consider multi. lenders (4)
	Panel A. Change $Post_t$			
LEP × Hispanic × Post	-0.064	0.038	0.069	-0.027
	(0.060)	(0.060)	(0.064)	(0.067)
Observations	30,645	30,645	30,645	30,645
	Panel B. Change $Hispanic_i$			
LEP × Asian × Post	-0.044	0.032	0.005	0.022
	(0.061)	(0.086)	(0.089)	(0.097)
Observations	34,748	34,748	34,748	34,748
Quarter FEs	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Post × Tract type FEs	Yes	Yes	Yes	Yes
Post × Risk FEs	Yes	Yes	Yes	Yes
Post × Loan controls	Yes	Yes	Yes	Yes

Notes: This table reports two triple-difference placebo tests using NSMO. In panel A, I assume that the policy change happened in July 2016. The sample includes mortgages originated before July 2018. *Post* equals one if the mortgage was originated after June 2016. In panel B, I assume that the initial translated documents were in Asian languages instead of Spanish. *Post* equals one if the mortgage was originated after June 2018.

Figure C1. Placebo Test: Randomly Assigned LEP Status



Notes: This figure plots the distribution of the triple-difference coefficients from 1000 placebo tests. In each iteration, I randomly select a group of observations and assume that they are LEP borrowers. To preserve the true market share of LEP borrowers, this simulated NSMO has the same number of LEP borrowers as the real NSMO. I then estimate Equation (2) using the simulated NSMO and get one placebo coefficient $\hat{\beta}_5$. The vertical red line represents the estimated triple-difference coefficient using the true LEP status in NSMO. Empirical p-value is calculated as the number of iterations when the placebo estimated effect is larger than the true estimated effect divided by 1000. Panel A uses the same specification as column 4 in panel B of Table 5. Panel B uses the same specification as column 4 in panel C of Table 5. Panel C uses the same specification as column 1 in panel A of Table 6. Panel D uses the same specification as column 1 in Table 7.

3 HMDA

Table C5. TWFE Estimation with Heterogeneous Treatment Effects

Dependent variable	# Applications (10K) (1)	Incomplete share (2)	Denial rate (3)	# Originations (10K) (4)
D_{ct}	0.201***	-0.686**	-1.118***	0.065***
	(0.037)	(0.277)	(0.320)	(0.022)
No. of switchers	1,902	1,902	1,902	1,902
County FEs	Yes	Yes	Yes	Yes
Year × State FEs	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes

Notes: This table reports the TWFE estimation results following De Chaisemartin and d'Haultfoeuille (2020). The dependent variables in columns 1 to 4 are the number of mortgage applications (in ten thousands), the share of incomplete applications, the application denial rate, and the number of originations (in ten thousands), respectively. D_{ct} is the share of potential treated LEP people in county c in year t. The estimation uses counties whose treatment level changed in absolute value by less than 0.5% as control groups. All specifications include county fixed effects, state-year fixed effects, and additional controls, which include median household income, total population, and the proportion of Hispanics, Asians, and black people. Standard errors are reported in parentheses and are clustered at the state level. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table C6. Falsification Tests

Dependent variable	# Applications (10K) (1)	Share of incomplete app. (2)	Denial rate (3)	# Originations (10K) (4)
	Panel A. Change $Post_t$			
LEP share × Post	0.011	0.015	-0.013	0.015
	(0.061)	(0.037)	(0.034)	(0.048)
Observations	19,623	19,623	19,623	19,623
	Panel B. Asian borrowers			
LEP share × Post	0.018	-0.039	-0.067*	0.016
	(0.014)	(0.038)	(0.037)	(0.012)
Observations	12,936	12,936	12,936	12,936
County FEs	Yes	Yes	Yes	Yes
Year × State FEs	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes

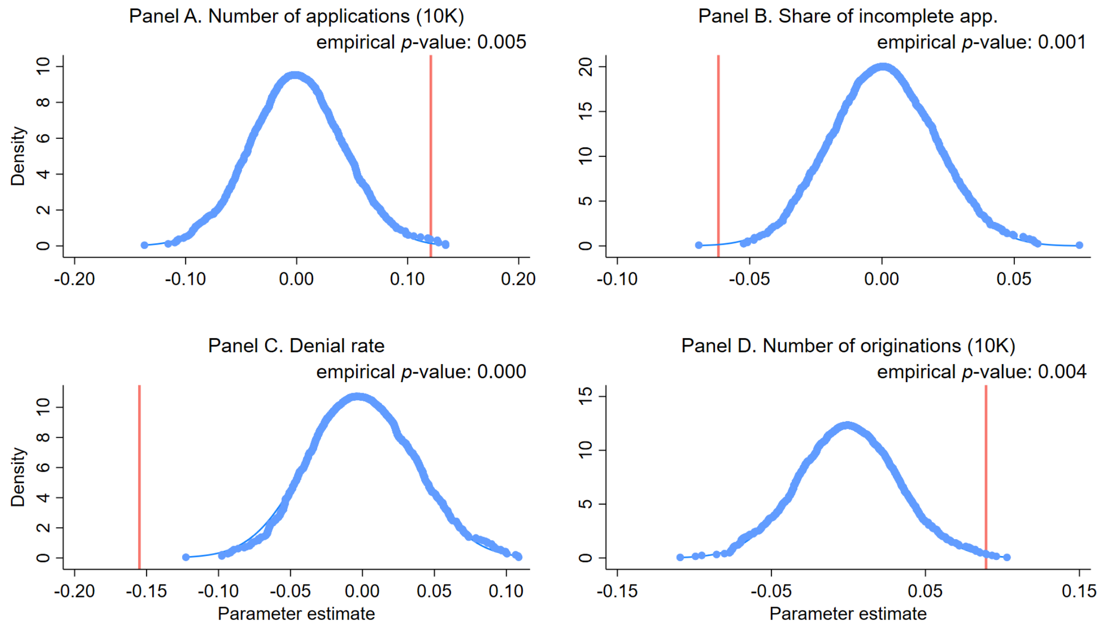
Notes: This table reports two falsification tests of the difference-in-differences estimates of the policy effects on mortgage credit access of conventional purchase loans. In Panel A, I assume that the policy change happened in 2016 and use HMDA data from 2011 to 2017. In Panel B, I use the sample of Asian borrowers to calculate the dependent variables. The dependent variables in columns 1 to 4 are the number of mortgage applications (in ten thousands), the share of incomplete applications, the application denial rate, and the number of originations (in ten thousands), respectively. *LEP share* is the share of LEP Hispanic people. *Post* equals one after 2015 and 2017 in Panels A and B, respectively. All specifications include county fixed effects, state-year fixed effects, and additional controls, which include median household income, total population, and the proportion of Hispanics, Asians, and black people. Standard errors are reported in parentheses and are clustered at the state level. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table C7. Effect on Accessing Mortgage Credit: Hispanic Borrowers

Dependent variable	# Applications (10K) (1)	Share of incomplete app. (2)	Denial rate (3)	# Originations (10K) (4)
LEP × Post	0.169***	-0.074***	-0.183***	0.114***
	(0.050)	(0.021)	(0.064)	(0.037)
Observations	17,147	17,147	17,147	17,147
County FEs	Yes	Yes	Yes	Yes
Year × State FEs	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes

Notes: This table reports the difference-in-differences estimates of the policy effect on mortgage credit access of conventional purchase loans for Hispanic borrowers. The dependent variables in columns 1 to 4 are the number of mortgage applications (in ten thousands), the share of incomplete applications, the application denial rate, and the number of originations (in ten thousands), respectively. *LEP share* is the share of LEP Hispanic people. *Post* equals one after 2017. All specifications include county fixed effects, state-year fixed effects, and additional controls, which include median household income, total population, and the proportion of Hispanics, Asians, and black people. Standard errors are reported in parentheses and are clustered at the state level. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Figure C2. Placebo Test: Randomly Assigned LEP Share



Notes: This figure plots the distribution of the difference-in-differences estimates from 1000 placebo tests. In each iteration, I randomly assign the share of LEP Hispanics and LEP Chinese at the county level within each state and then run the difference-in-differences regressions. The underlying regressions use the same specification as Panel A in Table 9. The vertical red line represents the estimated difference-in-differences coefficient using the real data. Empirical p-value is calculated as the number of iterations when the placebo estimated effect is larger than the true estimated effect divided by 1000.

4 GSE and FHA Snapshot^{+}

In this subsection, I exploit across-ZIP code variation in exposure to the FHFA Language Access Plan to identify the effect of reducing language frictions on interest rates and mortgage risk. I first use GSE data to construct a ZIP code-level measure of interest rates. In particular, I regress the raw interest rate on a set of loan characteristics, retrieve the residual as the conditional interest rate for each mortgage, and average this variable at the 3-digit ZIP code level at a monthly frequency. Motivated by the heterogeneous effects found in Table 6, I use different types of mortgages when aggregating the conditional interest rate. To accommodate this data structure, I estimate a modification of Equation (3) as follows:

$$Y_{zm} = \alpha + \beta D_{zm} + \gamma X_{zy} + \delta_z + \delta_m + \epsilon_{zm} \tag{C.1}$$

where

$$\mathrm{D}_{zm} = \begin{cases} 0, \quad \text{if } \ m \leq \text{June 2018}, \\ \text{Hispanic LEP share}_z, \quad \text{if } \ \text{July 2018} \leq m \leq \text{June 2019}, \\ \text{Hispanic LEP share}_z + \text{Chinese LEP share}_z, \quad \text{if } \ m \geq \text{July 2019}. \end{cases}$$

In this expression, z, m, and y index 3-digit ZIP code, month, and year respectively. The treatment intensity D_{zm} in this specification can vary within a year because of the monthly data. X_{zy} is the same collection of demographic control variables as above at the 3-digit ZIP code-year level. δ_z and δ_m are ZIP code fixed effects and month fixed effects.

Panel A of Table C8 shows that reducing language frictions helped LEP borrowers find cheaper GSE loans. Column 1 indicates that providing translated mortgage documents led to a decrease in interest rate for LEP borrowers by 12.7 basis points. This magnitude is comparable to that from a triple-difference estimation in the NSMO sample. Consistent with the triple-difference results, columns 2 to 5 demonstrates that the effect on purchase loans was about 34% larger than that on refinance loans, and the price effect on first-time borrowers was roughly 27% larger than that on repeat borrowers. As shown in the last two columns of Panel A, the effect on mortgages originated through retailers was both economically and statistically more significant than that on mortgages originated through brokers. These results imply two key conditions under which the policy had a greater impact: (1) borrowers or lenders lacked experience; and (2) borrowers and lenders had direct interactions.

I next find that the policy did not increase the overall risk of the local mortgage market. To measure mortgage performance, I follow the same steps as described above to calculate the monthly ZIP code-level conditional 90-day delinquency rate for GSE loans. As can be seen in Panel B of Table C8, there is little evidence that the policy change had an impact on local mortgage performance. The conditional delinquency rate has partialled out borrowers' ex-ante risk factors, so the above finding does not take into account the potential effect on borrowers' risk composition. If the policy introduced more subprime LEP borrowers to the mortgage market, the overall stability of the mortgage market would be at greater risk. I address this concern in two steps. First, I estimate the policy effect on the average credit scores of originated mortgages, which is a measure of ex-ante mortgage risk. Panel C of Table C8 shows that instead of deteriorating origination standards, the FHFA Language Access Plan had a positive impact on borrowers' creditworthiness. This suggests that some well-qualified LEP borrowers were excluded from the mortgage market due to language frictions before the policy change. Combined with the findings about the extensive margin effect, this result implies that the policy not only expand financial inclusion, but also expand to the appropriate population. Second, I use an unconditional measure, the CFPB monthly delinquency rate, as the dependent variable to re-estimate the difference-in-differences model. This unconditional delinquency rate is defined as the number of records where the borrower has missed mortgage payments divided by the total number of outstanding mortgages in the National Mortgage Database (NMDB). Table C9 presents an insignificant estimated effect on 90-day delinquency rate and a measure of early stage delinquencies, 30–89 day mortgage delinquency rate, at both the county and CBSA levels. In sum, these results imply that reducing language frictions did not introduce extra risks to the mortgage market.

In addition, I use the FHA Snapshot^{+} data set to repeat the above analysis for FHA loans.[3] The only difference is that I aggregate the outcomes of interest at the 5-digit ZIP code level at a monthly frequency. Table C10 reports a very similar pattern of results for FHA loans. After the policy shock, the interest rates dropped, the ex-ante mortgage risk decreased, and the mortgage performance remained unchanged. Similar to my above findings, the policy impact on interest rates was larger for less experienced borrowers.

[3]The original FHA Snapshot data have detailed location information but lack key variables about origination and performance. The Ginnie Mae data have these variables but the mortgage location is at the state level. Therefore, I match FHA Snapshot with the Ginnie Mae data to construct the FHA Snapshot^{+} data set.

Table C8. Effect on Mortgage Rate and Risk of GSE Loans

Sample	All	Purchase	Refinance	First-time borrowers	Repeat borrowers	Channel: retail	Channel: broker
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A. Average conditional interest rate						
LEP share × Post	-0.127**	-0.154***	-0.115	-0.152*	-0.120*	-0.108**	-0.044
	(0.060)	(0.053)	(0.100)	(0.078)	(0.069)	(0.053)	(0.079)
	Panel B. Average conditional 90-day delinquency rate						
LEP share × Post	0.021	0.029	0.018	0.039	0.016	0.015	0.011
	(0.016)	(0.020)	(0.016)	(0.024)	(0.014)	(0.018)	(0.029)
	Panel C. Average FICO scores						
LEP share × Post	7.744***	8.846***	7.065***	8.986***	6.883***	5.458***	8.234***
	(1.702)	(1.060)	(1.694)	(2.394)	(1.777)	(1.910)	(2.809)
Observations	52,435	52,088	52,160	51,234	52,382	52,341	44,854
ZIP3 code FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the difference-in-differences estimates of the policy effect on average conditional interest rate (Panel A), average conditional 90-day delinquency rate (Panel B), and average FICO scores (Panel C) of GSE loans. The dependent variables are at the 3-digit ZIP code-month level. The conditional outcome is obtained by averaging the residuals after regressing the raw interest rate (90-day delinquency) on origination month fixed effects, lender fixed effects, LTV × FICO score grids, loan purpose, loan term, property type, and occupancy status. Column 1 uses the full sample. Columns 2 and 3 use the sample of purchase and refinance loans, respectively. Columns 4 and 5 use the sample of loans borrowed by first-time borrowers and repeat borrowers, respectively. Columns 6 and 7 use the sample of loans originated through retail lenders and brokers, respectively. *LEP share* is the share of LEP Hispanics before July 2019, and the share of LEP Hispanics and Chinese starting from July 2019. *Post* equals one after June 2018. All specifications include 3-digit ZIP code fixed effects, origination month fixed effects, and additional controls, which include median household income, total population, and the proportion of Hispanics, Asians, and black people at the 3-digit ZIP code level. Standard errors are reported in parentheses and are clustered at the state level. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table C9. Effect on Overall Mortgage Performance

Panel A. County level

Dependent variable	90–day delinquency rate	30-89 delinquency rate
	(1)	(2)
LEP share × Post	-0.193	-0.502
	(0.587)	(0.303)
Observations	33,624	33,624
County fixed effects	Yes	Yes
Month fixed effects	Yes	Yes
Additional controls	Yes	Yes

Panel B. CBSA level

Dependent variable	90–day delinquency rate	30-89 delinquency rate
	(1)	(2)
LEP share × Post	0.122	0.251
	(0.568)	(0.335)
Observations	12,924	12,924
CBSA fixed effects	Yes	Yes
Month fixed effects	Yes	Yes
Additional controls	Yes	Yes

Notes: This table reports the difference-in-differences estimates of the effect of language help on the overall mortgage performance. *LEP share* is the share of LEP Hispanics before July 2019, and the share of LEP Hispanics and Chinese starting from July 2019. *Post* equals one after June 2018. The dependent variables are the 90–day delinquency rate and the 30-89 delinquency rate in columns 1 and 2, respectively. The data come from the National Mortgage Database (NMDB) and provided by Consumer Financial Protection Bureau (CFPB). All specifications include location fixed effects (county in panel A and CBSA in panel B), month fixed effects, and additional controls, which include median household income, total population, and the proportion of Hispanics, Asians, and black people. All regressions are weighted by total population. Standard errors are reported in parentheses and are clustered at the state level in panel A and at the CBSA level in panel B. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table C10. Effect on Mortgage Rate and Risk of FHA Loans

Sample	All	Purchase	Refinance	First-time borrowers	Repeat borrowers	Channel: retail	Channel: broker
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A. Average conditional interest rate						
LEP share × Post	-0.133***	-0.163***	-0.051***	-0.159***	-0.099***	-0.144***	-0.122***
	(0.019)	(0.022)	(0.019)	(0.023)	(0.023)	(0.022)	(0.033)
Observations	592,464	479,204	338,413	400,288	438,232	509,430	251,743
	Panel B. Average conditional 90-day delinquency rate						
LEP share × Post	0.015	-0.003	0.044*	-0.008	0.029	-0.015	0.023
	(0.015)	(0.018)	(0.024)	(0.017)	(0.037)	(0.013)	(0.025)
Observations	592,464	479,201	338,291	400,286	217,651	509,399	251,735
	Panel C. Average FICO scores						
LEP share × Post	17.876***	22.277***	6.096	21.257***	11.155***	11.180***	21.195***
	(2.087)	(2.970)	(4.330)	(3.003)	(3.902)	(2.770)	(4.044)
Observations	592,464	479,204	338,415	400,289	438,233	509,431	251,747
ZIP5 code FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the difference-in-differences estimates of the policy effect on average conditional interest rate (Panel A), average conditional 90-day delinquency rate (Panel B), and average FICO scores (Panel C) of FHA loans. The dependent variables are at the 5-digit ZIP code-month level. The conditional outcome is obtained by averaging the residuals after regressing the raw interest rate (90-day delinquency) on origination month fixed effects, lender fixed effects, LTV × FICO score grids, loan purpose, loan term, property type, and occupancy status. Column 1 uses the full sample. Columns 2 and 3 use the sample of purchase and refinance loans, respectively. Columns 4 and 5 use the sample of loans borrowed by first-time borrowers and repeat borrowers, respectively. Columns 6 and 7 use the sample of loans originated through retail lenders and brokers, respectively. *LEP share* is the share of LEP Hispanics before July 2019, and the share of LEP Hispanics and Chinese starting from July 2019. *Post* equals one after June 2018. All specifications include 5-digit ZIP code fixed effects, origination month fixed effects, and additional controls, which include median household income, total population, and the proportion of Hispanics, Asians, and black people at the 3-digit ZIP code level. Standard errors are reported in parentheses and are clustered at the 3-digit ZIP code level. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

D Machine Learning

1 Training Sample

I use the micro-level 2015-19 American Community Survey (ACS) to construct the training sample as follows:

1. Keep household heads who are older than 18.
2. Keep household heads in contiguous states.
3. Keep household heads who have mortgages or purchase contracts.
4. Keep household heads who moved into the dwelling unit within 12 months or less.
5. Use person weights (PERWT) to expand the sample.[4]

This training sample tries to represent home buyers between 2015 and 2019. The summary statistics of this sample are reported in Table D1.

2 Training Procedure

First, I randomly select 80% of the ACS sample as the training set, and the remaining 20% of the sample will be used as a test set to prevent overfitting. Useful features should exist in both the training sample and the prediction sample, so I select household income, ethnicity, race (a Black indicator, an Asian indicator, and a white indicator), gender, and a series of state-year fixed effects. In total, there are 251 features used for prediction.

I specify **objective** and **eval_metric** to set the learning task as a logistic regression for binary classification. TO fine-tune the model, I use GridSearchCV to pick the optimal set of the following hyperparameters.[5] **reg_lambda** is the L2 regularization term on weights. Increasing this value will make model more conservative. **learning_rate** (η) is the step size shrinkage used in update to prevent overfitting. **gamma** is the minimum loss reduction required to make a further partition on a leaf node of the tree. The larger gamma is, the more conservative the algorithm will be.

[4]PERWT indicates how many persons in the U.S. population are represented by a given person in an IPUMS sample.

[5]GridSearchCV is the process of performing hyperparameter tuning in order to determine the optimal values for a given model.

max_depth is the maximum depth of a tree. Increasing this value will make the model more complex and more likely to overfit. **scale_pos_weight** controls the balance of positive and negative weights. **colsample_bytree** is the subsample ratio of columns when constructing each tree. **subsample** is set to be 0.8, which means that XGBoost would randomly sample 80% of the training data prior to growing trees. **n_estimators** is the number of trees.

I train two classifiers for the full sample and the sample of Hispanics, separately. For Hispanic borrowers, they are classified as LEP if any classifier predicts a positive class. The machine learning algorithm, XGBOOST, is capable of predicting a probability of being LEP. The mapping from this continuous variable to a crisp class label is achieved by using a threshold, where all values equal or greater than the default threshold (0.5) are mapped to LEP, and all other values are mapped to non-LEP. After applying threshold-moving to tune these parameters that generate the best overall performance in the test set, I set the thresholds at 0.5 for the full model and 0.9 for the Hispanic model, respectively.

For performance comparison, I use a traditional Logit model as the benchmark. The Logit model for the full sample and the Hispanic sample both include an L1 penalty term, C.[6] Similarly, I use GridSearchCV to pick the optimal regularization parameter C. The final results are reported in Table D2. The Logit model tends to predict every observation as non-LEP. As a result, the model has a very low recall rate for LEP people. By contrast, my XGBoost model has higher precision, recall, and overall accuracy than the Logit model.

3 Discussion on Prediction Performance

The machine learning model predicts that 3.3% of all and 11.6% of Hispanic people are LEP borrowers in the HMDA$^+$ sample. Table D3 compares several key characteristics between LEP and non-LEP borrowers, based on the machine learning prediction. Consistent with the NSMO sample, a typical LEP borrower has a lower income, credit score, and mortgage amount but a higher DTI, mortgage rate, and delinquency rate. This suggests that the prediction is definitely better than a random guess.

However, about 4.9% of all and 22% of Hispanic household heads are LEP borrowers in the training sample. Therefore, the machine learning model under-predicts LEP borrowers in both the full and the Hispanic sample. This is probably because

[6] C is the inverse of regularization strength. Smaller values specify stronger regularization.

the training sample and the prediction sample have different distributions in some aspects. Comparing the first column in Table D1 and Table D3, the training sample has a larger fraction of minority borrowers. Figure D1 plots the income distribution of Hispanic households (truncated at \$150K) in both samples. The HMDA$^+$ sample has a much smaller share of households with incomes lower than \$50K. Because minorities and low-income people are more likely to be LEP, the share of LEP people in the prediction sample may be smaller than that in the training sample.

Figure D1. Income Distribution of Hispanic Households

Notes: This figure plots the income distribution of Hispanic households (truncated at \$150K) in the ACS sample and the HMDA$^+$ sample.

Table D1. Summary Statistics of the ACS Sample

Sample	All (1)	LEP (2)	Non-LEP (3)
Female	0.458	0.384	0.462
	(0.498)	(0.486)	(0.499)
Minority	0.251	0.864	0.220
	(0.434)	(0.343)	(0.414)
Age	41.824	43.547	41.736
	(13.789)	(12.241)	(13.857)
Married	0.612	0.714	0.607
	(0.487)	(0.452)	(0.488)
College	0.504	0.290	0.515
	(0.500)	(0.454)	(0.500)
Income<$50K	0.194	0.350	0.186
	(0.396)	(0.477)	(0.389)
Observations	3,687,402	178,928	3,508,474

Notes: This table reports descriptive statistics of the training set for machine learning. The data come from the micro-level ACS 2015-2019. I select adult household heads who own their homes with mortgages and moved into the current residence within 12 months or less. The sample is expanded based on the individual weights in ACS. All table entries represent sample means and standard deviations in parentheses. Summary statistics are presented for all observations in column 1 as well as separately for LEP (column 2) and non-LEP borrowers (column 3).

Table D2. Machine Learning Model Performance

Model (1)	Class (2)	Precision (3)	Recall (4)	Accuracy (5)
Panel A. Full sample				
Logit	0	0.952	0.999	0.952
	1	0.542	0.005	
XGBoost	0	0.989	0.995	0.985
	1	0.886	0.787	
Panel B. Hispanics sample				
Logit	0	0.786	0.997	0.785
	1	0.657	0.023	
XGBoost	0	0.954	0.969	0.939
	1	0.882	0.831	

Notes: This table reports the performance of a benchmark Logit model and the XGBoost model in the full test sample (panel A) and the test sample of Hispanics (panel B). See Appendix D for details of the models. Class 1 stands for LEP and class 0 stands for non-LEP. For each class, precision is the fraction of relevant instances among the retrieved instances, and recall is the fraction of relevant instances that are retrieved. Accuracy is the ratio of correct predictions of two classes to all instances.

Table D3. Summary Statistics of the HMDA+ Dataset (Purchase Loans)

Sample	All (1)	LEP (2)	Non-LEP (3)
Female	0.344	0.292	0.345
	(0.475)	(0.455)	(0.476)
Minority	0.159	0.488	0.147
	(0.366)	(0.500)	(0.354)
Income<\$50K	0.201	0.316	0.196
	(0.401)	(0.465)	(0.397)
FICO score	722.537	715.904	722.783
	(59.982)	(58.146)	(60.036)
Conventional loan	0.641	0.577	0.643
	(0.480)	(0.494)	(0.479)
LTV	85.353	85.296	85.355
	(14.479)	(14.943)	(14.462)
DTI	37.667	40.612	37.557
	(10.100)	(10.157)	(10.082)
Interest rate	4.220	4.229	4.220
	(0.559)	(0.563)	(0.558)
90-Day delinquency	0.099	0.131	0.098
	(0.299)	(0.337)	(0.297)
Observations	4,349,661	144,371	4,205,290

Notes: This table reports descriptive statistics of the prediction sample (purchase loans in the HMDA+ dataset) for machine learning. LEP status is based on the prediction made by the XGBoost machine learning model (see Appendix D). All table entries represent sample means and standard deviations in parentheses. Summary statistics are presented for all observations in column 1 as well as separately for LEP (column 2) and non-LEP borrowers (column 3).

E Triple-difference Model with Treatment Status Misclassification

1 Derivation of Bias

I abstract away from control variables and fixed effects and rewrite the triple-difference model in simpler notation as:

$$Y_t = \alpha + \beta_0 L + \beta_1 H + \beta_2 P + \beta_3 L \times H + \beta_4 L \times P + \beta_5 H \times P + \beta_6 L \times H \times P + \varepsilon_t. \quad \text{(E.1)}$$

This is a canonical triple-difference model with a $2 \times 2 \times 2$ setup. There are two time periods, namely pre- and post-implementation of the FHFA Language Access Plan, denoted as $t = 0$ and $t = 1$. P is a dummy variable indicating the post-policy period ($t = 1$). L equals one if the borrower has limited English proficiency in the data, and H equals one if the borrower is Hispanic. However, the LEP status in the data, L, is a misclassified version of the true LEP status, L^*, which is unobservable. I use a latent variable, ρ, as an indicator for misclassification. When $\rho = 0$, $L = L^*$. When $\rho = 1$, $L = 1 - L^*$. The treatment status D is also a dummy variable, which takes the value of one only when $P = 1$, $L^* = 1$, and $H = 1$. Let Y_t and $Y_t(D)$ represent the observed outcome and the potential outcome in period t if the treatment status is D. I am interested in identifying the average treatment effect on the treated (ATT) that can be written as:

$$\text{ATT} = \mathbb{E}[Y_1(1) - Y_1(0) \mid L^* = 1, H = 1].$$

Under the assumption that $\mathbb{E}(\varepsilon_t | L, H, P) = 0$, the standard triple-difference estimator can be defined as:

$$\begin{aligned} \theta_{DDD} =& (\mathbb{E}[Y_1 \mid L = 1, H = 1] - \mathbb{E}[Y_0 \mid L = 1, H = 1]) \\ &- (\mathbb{E}[Y_1 \mid L = 1, H = 0) - \mathbb{E}[Y_0 \mid L = 1, H = 0]) \\ &- (\mathbb{E}[Y_1 \mid L = 0, H = 1) - \mathbb{E}[Y_0 \mid L = 0, H = 1]) \\ &+ (\mathbb{E}[Y_1 \mid L = 0, H = 0) - \mathbb{E}[Y_0 \mid L = 0, H = 0]). \end{aligned} \quad \text{(E.2)}$$

Before presenting the main result, I state the parallel trends assumption in this triple-difference model with treatment misclassification.

Assumption E.1. *(Parallel Trends)*

$$
\begin{gathered}
\left(\mathbb{E}\left[Y_1(0) \mid L=1, H=1\right]-\mathbb{E}\left[Y_0(0) \mid L=1, H=1\right]\right) \\
-\left(\mathbb{E}\left[Y_1(0) \mid L=1, H=0\right]-\mathbb{E}\left[Y_0(0) \mid L=1, H=0\right]\right) \\
= \\
\left(\mathbb{E}\left[Y_1(0) \mid L=0, H=1\right]-\mathbb{E}\left[Y_0(0) \mid L=0, H=1\right]\right) \\
-\left(\mathbb{E}\left[Y_1(0) \mid L=0, H=0\right]-\mathbb{E}\left[Y_0(0) \mid L=0, H=0\right]\right).
\end{gathered}
$$

Note that this parallel trends assumption is based on the observed LEP status instead of the true one. This is because when I run triple-difference regressions using the HMDA$^+$ data set, I implicitly proceed with this assumption instead of the assumption stated in terms of L^*. If there is no misclassification of L^*, then Assumption E.1 helps to identify the ATT (Olden and Møen, 2022). However, as L is not always equal to L^*, the following lemma reveals that the triple-difference estimator cannot recover the ATT and is a weighted average of the ATT for the correctly classified and misclassified treatment groups.

Lemma E.1. *In a $2 \times 2 \times 2$ canonical triple differences design with treatment status misclassification, if Assumption E.1 holds, then the triple-difference estimator (defined as Equation (E.2)) can be written as:*

$$
\begin{aligned}
\theta_{DDD} = & \ \mathbb{E}\left[Y_1(1)-Y_1(0) \mid \rho=0, L^*=1, H=1\right] \mathbb{P}(\rho=0 \mid L=1, H=1) \\
& -\mathbb{E}\left[Y_1(1)-Y_1(0) \mid \rho=1, L^*=1, H=1\right] \mathbb{P}(\rho=1 \mid L=0, H=1)
\end{aligned}
$$

Proof. Using the notation of potential outcome framework and treatment misclassification, I have:

$$
\begin{aligned}
\mathbb{E}[Y_1 \mid L=1, H=1] &= \mathbb{E}[Y_1(1) L^*+Y_1(0)(1-L^*) \mid L=1, H=1] \\
&= \mathbb{E}[(Y_1(1)-Y_1(0)) L^* \mid L=1, H=1]+\mathbb{E}[Y_1(0) \mid L=1, H=1].
\end{aligned}
$$

The first equality reflects that some people with $L=1$ are actually not treated because they are misclassified. Similarly, other terms in the triple-difference estimator can be written as:

$$
\mathbb{E}[Y_0 \mid L=1, H=1]=\mathbb{E}[Y_0(0) \mid L=1, H=1],
$$

$$
\mathbb{E}[Y_1 \mid L=1, H=0]=\mathbb{E}[Y_1(0) \mid L=1, H=0],
$$

$$
\begin{aligned}
\mathbb{E}[Y_0 \mid L=1, H=0] &= \mathbb{E}[Y_0(0) \mid L=1, H=0], \\
\mathbb{E}[Y_1 \mid L=0, H=1] &= \mathbb{E}[Y_1(1)L^* + Y_1(0)(1-L^*) \mid L=0, H=1] \\
&= \mathbb{E}[(Y_1(1) - Y_1(0))L^* \mid L=0, H=1] + \mathbb{E}[Y_1(0) \mid L=0, H=1], \\
\mathbb{E}[Y_0 \mid L=0, H=1] &= \mathbb{E}[Y_0(0) \mid L=0, H=1], \\
\mathbb{E}[Y_1 \mid L=0, H=0] &= \mathbb{E}[Y_1(0) \mid L=0, H=0], \\
\mathbb{E}[Y_0 \mid L=0, H=0] &= \mathbb{E}[Y_0(0) \mid L=0, H=0].
\end{aligned}
$$

The fourth term shows that although some people have $L = 0$ in the data set, they are actually treated because their true LEP status is $L^* = 1$. In other cases, the misclassification of L^* does not affect potential outcomes, because either $H = 0$ or $T = 0$. Substituting these terms into Equation (E.2), I get

$$
\theta_{DDD} = \mathbb{E}[(Y_1(1) - Y_1(0))L^* \mid L=1, H=1] - \mathbb{E}[(Y_1(1) - Y_1(0))L^* \mid L=0, H=1].
$$

Therefore,

$$
\begin{aligned}
\theta_{DDD} = \mathbb{E}\left[Y_1(1) - Y_1(0) \mid \rho=0, L^*=1, H=1\right] \mathbb{P}(\rho=0 \mid L=1, H=1) \\
- \mathbb{E}\left[Y_1(1) - Y_1(0) \mid \rho=1, L^*=1, H=1\right] \mathbb{P}(\rho=1 \mid L=0, H=1)
\end{aligned}
$$

as $L^* = 1, L = 1$ is equivalent to $\rho = 0, L^* = 1$ or $\rho = 0, L = 1$, and $L^* = 1, L = 0$ is equivalent to $\rho = 1, L^* = 1$ or $\rho = 1, L = 0$. $\square$

This lemma shows that the bias direction of the triple-difference estimator is ambiguous. To see this, I rewrite the ATT as:

$$
\begin{aligned}
\text{ATT} = \mathbb{E}[Y_1(1) - Y_1(0) \mid L^*=1, H=1, \rho=1]\mathbb{P}(\rho=1 \mid L^*=1, H=1) \\
+ \mathbb{E}[Y_1(1) - Y_1(0) \mid L^*=1, H=1, \rho=0]\mathbb{P}(\rho=0 \mid L^*=1, H=1).
\end{aligned}
$$

When $\mathbb{E}[Y_1(1) - Y_1(0) \mid L^* = 1, H = 1, \rho = 1]$ and $\mathbb{E}[Y_1(1) - Y_1(0) \mid L^* = 1, H = 1, \rho = 0]$ are both positive, the ATT is positive. However, it is possible that θ_{DDD} is negative in this case, because the relationship between $\mathbb{P}(\rho = 0 \mid L = 1, H = 1)$ and $\mathbb{P}(\rho = 1 \mid L = 0, H = 1)$ is undetermined. The misclassification in this lemma is arbitrary, which leads to this pessimistic result. I then show that a reasonable assumption on the of structure of misclassification can help to identify both the direction and magnitude of the bias.

Assumption E.2. *(Non-differential Misclassification)*

$$\rho \perp\!\!\!\perp (Y_1(1), Y_1(0)) \mid L^*, H$$

This assumption states that misclassification is not correlated to potential outcomes conditional on the true treatment status. This assumption is likely to hold in my context, because the misclassification mechanically comes from a pure statistics exercise. Then the following proposition holds.

Proposition E.1. *Under Assumptions E.1 and E.2, the triple-difference estimator can be written as:*

$$\theta_{DDD} = ATT(\mathbb{P}(\rho = 0 \mid L = 1, H = 1) + \mathbb{P}(\rho = 0 \mid L = 0, H = 1) - 1).$$

If $1 - \mathbb{P}(\rho = 1 \mid L = 1, H = 1) - \mathbb{P}(\rho = 1 \mid L = 0, H = 1) \in (0, 1]$*, then the triple-difference estimator has attenuation bias.*

Proof. Assumption E.2 implies that

$$\mathbb{E}[Y_1(1) - Y_1(0) \mid \rho, L^*, H] = \mathbb{E}[Y_1(1) - Y_1(0) \mid L^*, H]$$

Thus,

$$\begin{aligned}
\theta_{DDD} &= \mathbb{E}[Y_1(1) - Y_1(0) \mid L^* = 1, H = 1](\mathbb{P}(\rho = 0 \mid L = 1, H = 1) - \mathbb{P}(\rho = 1 \mid L = 0, H = 1)) \\
&= \text{ATT}(\mathbb{P}(\rho = 0 \mid L = 1, H = 1) + \mathbb{P}(\rho = 0 \mid L = 0, H = 1) - 1) \\
&= \text{ATT}(1 - \mathbb{P}(\rho = 1 \mid L = 1, H = 1) - \mathbb{P}(\rho = 1 \mid L = 0, H = 1)).
\end{aligned}$$

If $1 - \mathbb{P}(\rho = 1 \mid L = 1, H = 1) - \mathbb{P}(\rho = 1 \mid L = 0, H = 1) \in (0, 1]$, then θ_{DDD} is downward biased compared to the true ATT. □

Proposition E.1 shows that the triple-difference estimate is smaller than the true ATT when the misclassification problem is not too severe. Because non-Hispanic people are always untreated, the bias is only determined by the precision in the Hispanic sample.

2 Lower Bound of the ATT

Proposition E.1 links the performance of machine learning models to the bias magnitude. However, the true LEP status is not observable in the prediction sample, so I cannot evaluate the prediction performance in this sample. To convert the triple-difference estimate to the ATT, I impose two additional assumptions that allow me to pin down the range of the precision in the prediction sample.

Assumption E.3. *The fraction of LEP people among Hispanic borrowers in the prediction sample is lower than that in the training sample.*

Figure D1 provides supporting evidence for Assumption 3, as it shows that the training sample has a larger fraction of low-income Hispanic households than the prediction sample. Given this assumption, because about 22% of Hispanic household heads in the ACS sample are LEP, there will be at most 94,194 LEP Hispanic borrowers in the HMDA$^+$ sample. If I know the number of true positive instances that my machine learning model predicts, I can evaluate the prediction performance. I state another assumption needed to achieve this.

Assumption E.4. *The machine learning model does not perform better in the prediction sample than in the test sample.*

Then Assumption 4 also stands because the prediction sample does not represent the test sample perfectly. Under these two assumptions, I can calculate a lower bound of the ATT. Table E1 presents the confusion matrix of Hispanic borrowers in the HMDA$^+$ data set. Each element of this matrix, as well as the precision and recall metrics, can be expressed by two unknowns: the number of true positive instances (denoted as x) and the number of LEP Hispanic borrowers in reality (denoted as y). Assumption E.3 implies that $y \leq 94194$. Assumption E.4 implies that the precision and recall metrics in the prediction sample are smaller than those reported in Panel B of Table D2. Therefore, finding the lower bound of the ATT is equivalent to solving the following constrained maximization problem:

$$
\begin{aligned}
\max_{x,y} \quad & \frac{x}{49857} + \frac{381634 - y + x}{381634} \\
\text{s.t.} \quad & y \leq 94194, \\
& precision_1 = \frac{x}{49857} \leq 0.882, \\
& recall_1 = \frac{x}{y} \leq 0.831, \\
& precision_0 = \frac{381634 - y + x}{381634} \leq 0.954, \\
& recall_0 = \frac{381634 - y + x}{431491 - y} \leq 0.969
\end{aligned}
$$

Solving this system, I obtain the maximum value of 1.72 for the sum of prediction precision for two classes (i.e., $\mathbb{P}(\rho = 0 \mid L = 1, H = 1) + \mathbb{P}(\rho = 0 \mid L = 0, H = 1)$). This is achieved when the number of LEP Hispanic borrowers is 55,765 and the number of true positive instances is 38,210. Therefore, according to Proposition E.1, the ATT in this case is the triple-difference coefficient multiplied by 1.39.

Notice that this is the lower bound of the ATT. Given the same triple-difference coefficient, any deviation of the actual number of LEP Hispanic borrowers or the machine learning performance from the value that achieves the maximum of the precision sum will generate a larger ATT. For example, if the fraction of LEP people among Hispanic borrowers is the same in the training sample and the prediction sample (i.e., $y = 94194$), x will be smaller than 39401. Furthermore, the sum of the precision of two classes is smaller than 1.65, and then $ATT = \theta_{DDD} \times 1.54$. If $y = 45179$ and $x = 29914$, then $ATT = \theta_{DDD} \times 5$. In this case, the magnitude of the triple-difference estimates in the HMDA$^+$ sample is comparable to that of the NSMO sample.

Table E1. Confusion Matrix of the Prediction Sample

		Data: 0	Data: 1	
Prediction	0	TN=381,634-y+x	FN=y-x	381,634
	1	FP=49,857-x	TP=x	49,857
		431,491-y	y	

Notes: This table is the confusion matrix of the Hispanic prediction sample. Class 0 stands for non-LEP, and class 1 stands for LEP. Each row represents the classification of the model, and each column represents the classification of the data. All data points can be separated into four groups based on the data and the prediction: true negative, false negative, false positive, and true positive. Since the the number of true positive instances ($TP = x$) and the actual number of LEP Hispanic borrowers (y) are unknown, each cell in this confusion matrix is expressed in terms of x and y.

F Additional Figures and Tables

1 Additional Figures

Figure F1. Language Translation Disclosure

English

Notice to Borrowers about Language

Your mortgage loan transaction is likely to be conducted in English. The information you receive and the official documents you will sign will likely be in English.

We want you to understand the transaction. Translations may be available to complement the English language documents. These documents are to help you understand the transaction. Your lender or servicer may not be able to provide you with translation services or translated documents.

Language assistance and resources may also be available through housing counseling agencies approved by the U.S. Department of Housing and Urban Development (HUD). You can find a list of HUD-approved housing counseling agencies at www.hud.gov/counseling.

1. Select "housing counseling agency" near you, then select your state.
2. To locate housing counseling agencies in your area that speak your language, select "Click here to narrow your search" and select a language.
3. Or, call HUD at 800-569-4287 for help in finding a counselor.

Information about housing counselors is also available at www.consumerfinance.gov/find-a-housing-counselor.

Espanol

Aviso para los prestatarios sobre el idioma

Es probable que la transacción de su préstamo hipotecario se lleve a cabo en inglés. La información que reciba y los documentos oficiales que firme probablemente estarán en inglés.

Queremos que entienda la transacción. Es posible que haya servicios de traducción disponibles para complementar los documentos que están en inglés. El objetivo de estos documentos es ayudarlo a entender la transacción. Es posible que su prestamista o su proveedor de servicio no puedan proporcionarle servicios de traducción ni los documentos traducidos.

Es posible que también haya recursos y asistencia de idioma disponibles a través de agencias de asesoramiento sobre vivienda aprobadas por el Departamento de Vivienda y Desarrollo Urbano de los Estados Unidos (U.S. Department of Housing and Urban Development, HUD). Puede encontrar una lista de agencias de asesoramiento sobre vivienda aprobadas por el HUD en www.hud.gov/counseling.

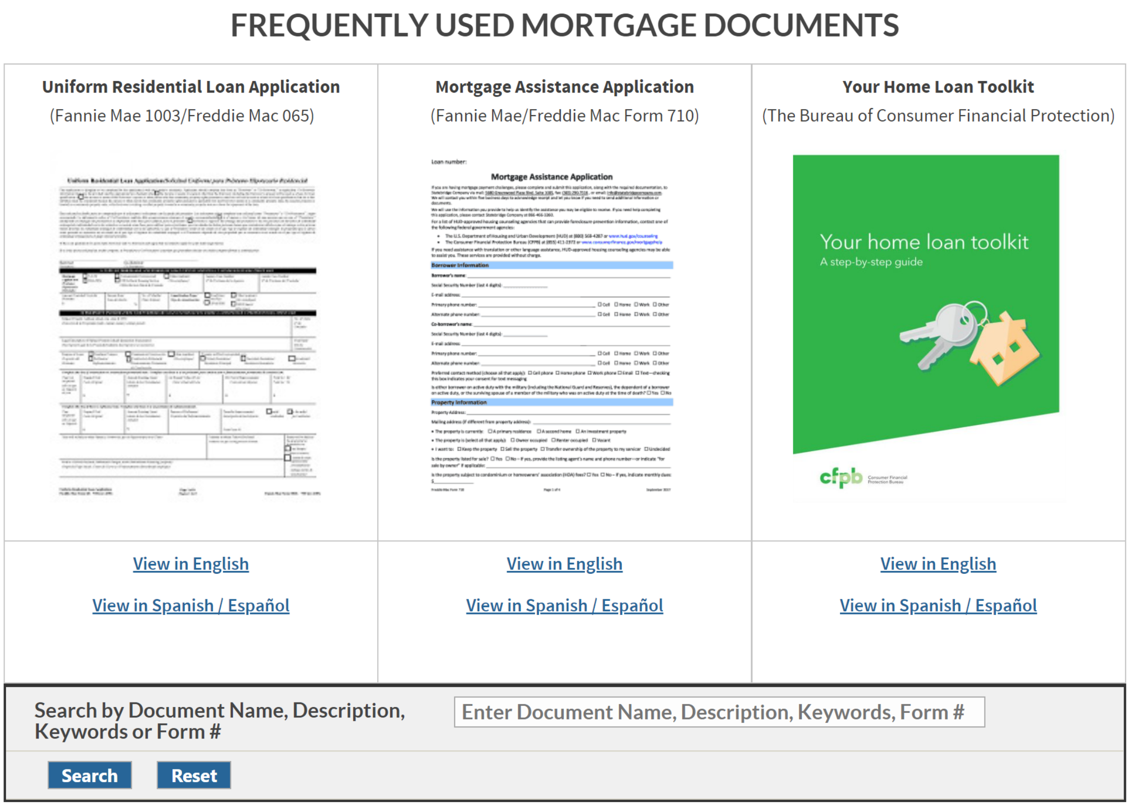
1. Seleccione una agencia de asesoramiento sobre vivienda cercana a su domicilio haciendo clic en "housing counseling agency" (agencia de asesoramiento sobre vivienda); luego, elija su estado.
2. Para encontrar agencias de asesoramiento sobre vivienda en su área en las que se hable su idioma, seleccione "Click here to narrow your search" (Haga clic aquí para limitar la búsqueda) y elija un idioma.
3. O bien llame al HUD al 800-569-4287 para que lo ayuden a encontrar a un asesor.

También puede obtener información sobre asesores de vivienda en www.consumerfinance.gov/find-a-housing-counselor.

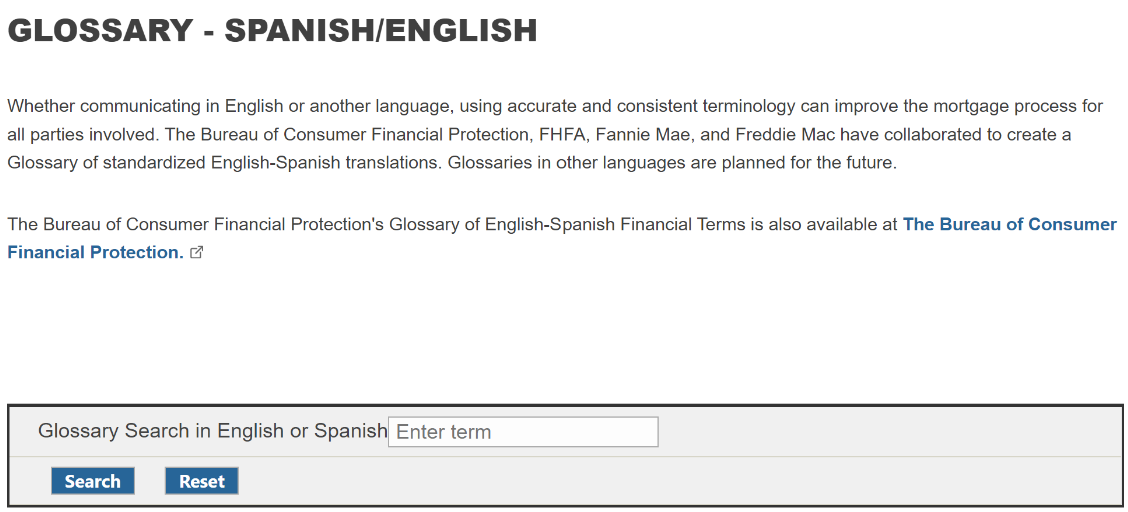
Notes: This figure shows the English version and Spanish version of Language Translation Disclosure.

Figure F2. Snapshot of Mortgage Translations Clearinghouse

(a) Translated Documents



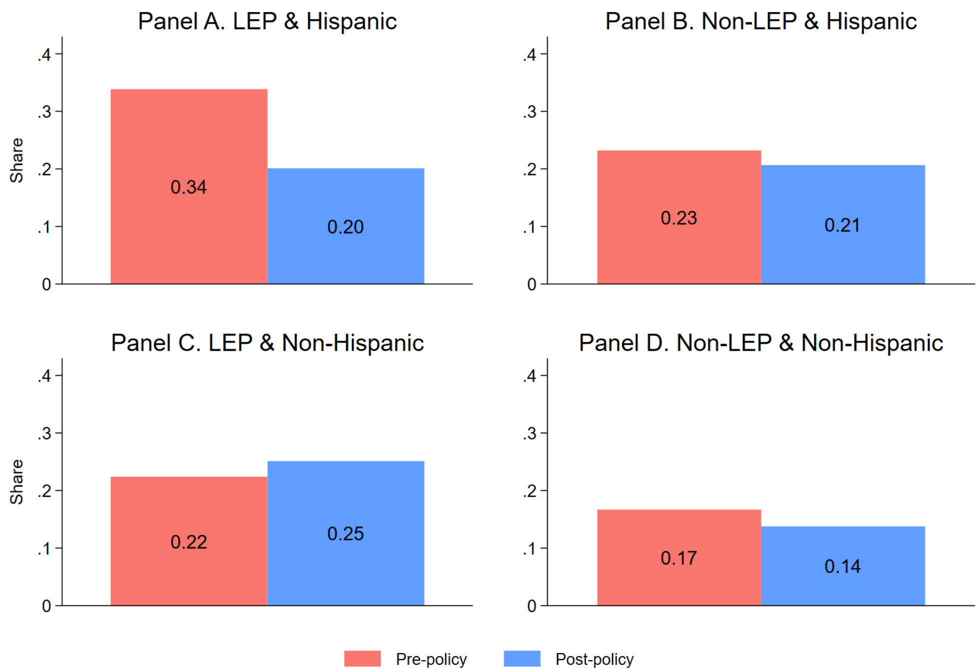
(b) Glossary



Notes: This figure is the snapshot of Mortgage Translations Clearinghouse website in January 2019, which is retrieved from Wayback Machine (`https://archive.org/web/`).

Figure F3. Triple-Differences Raw Comparison

(a) resolve credit report errors



(b) answer further request for income or asset information

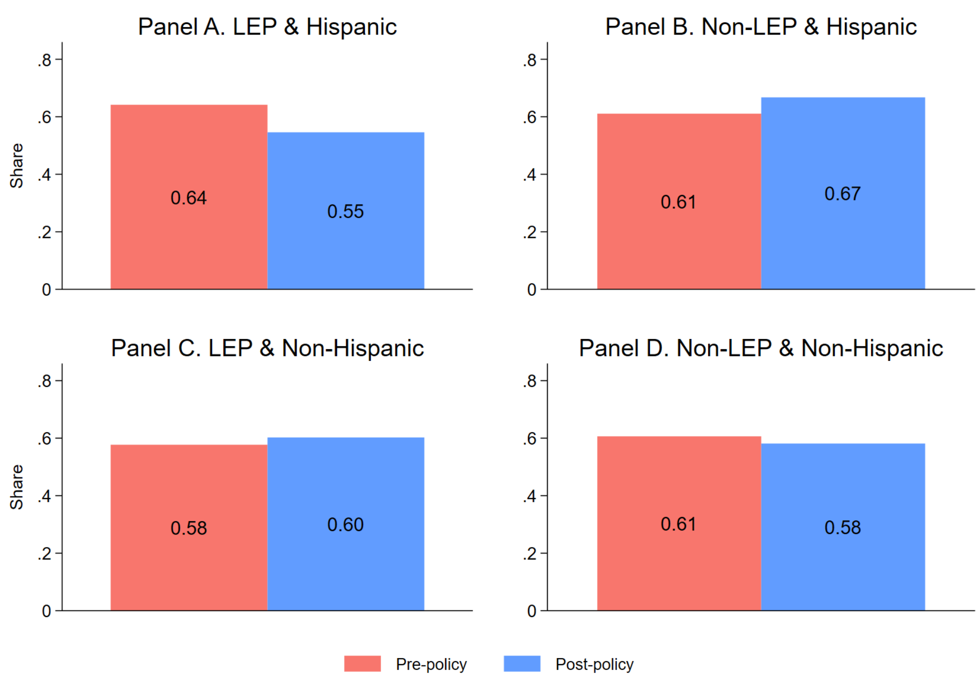
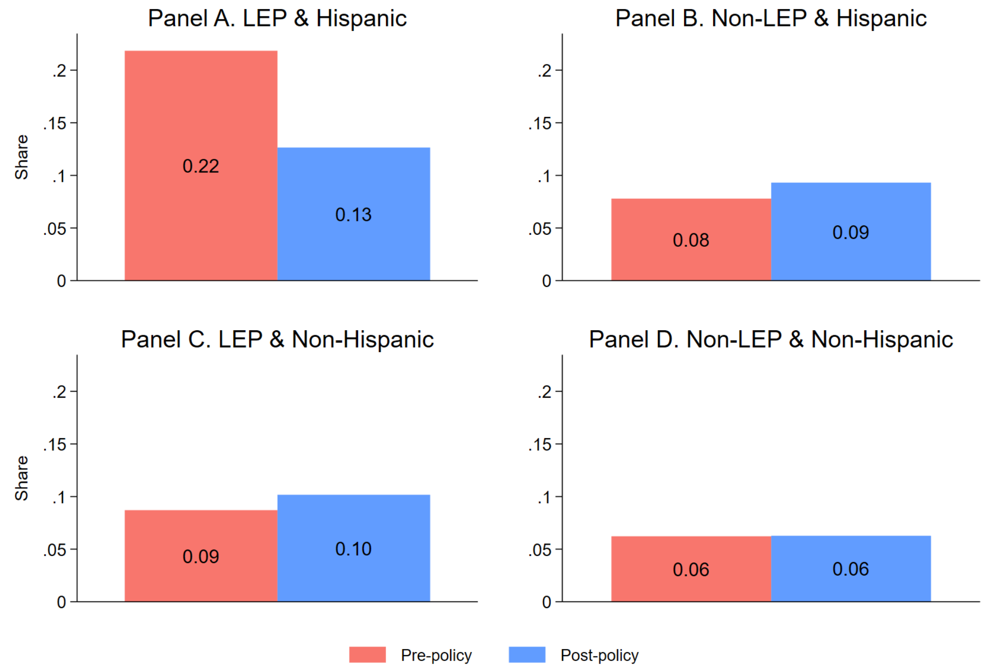


Figure F3. Triple-Differences Raw Comparison (cont.)

(c) have more than one appraisal



(d) do not know if it is an ARM

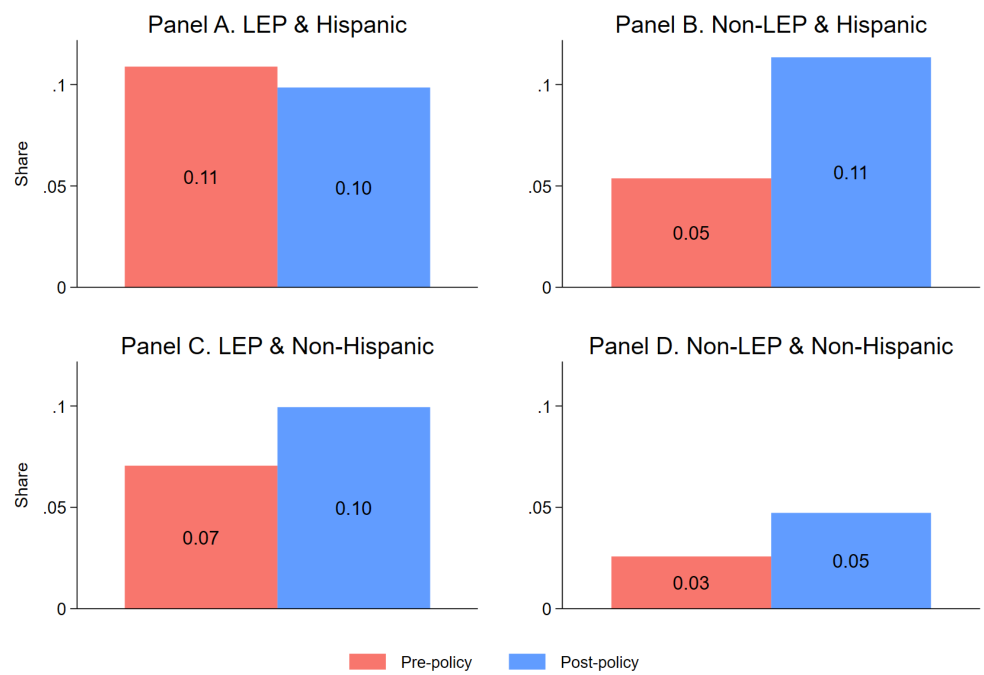
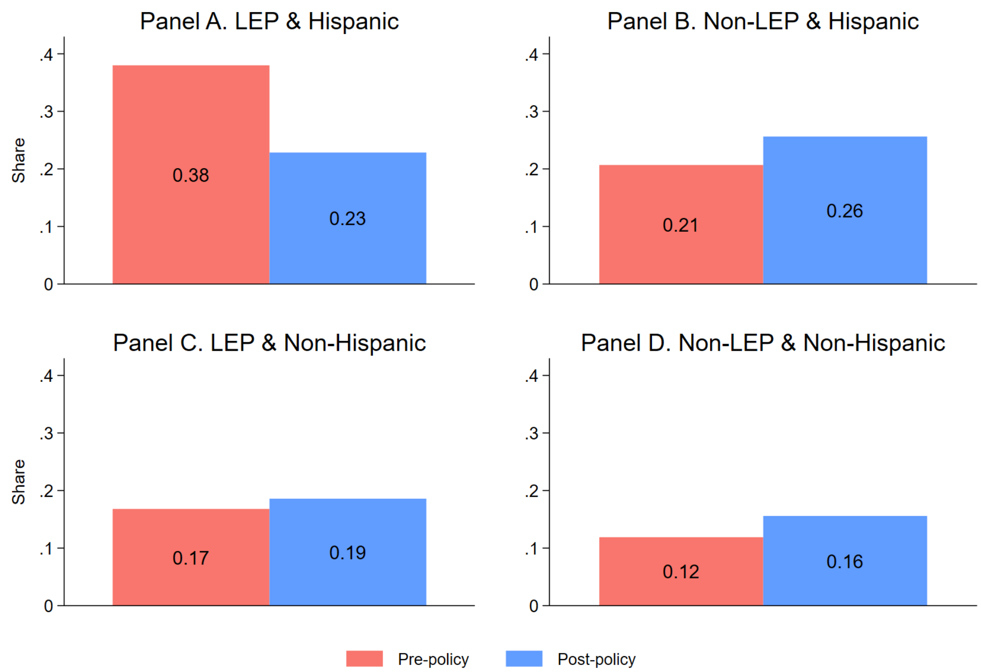


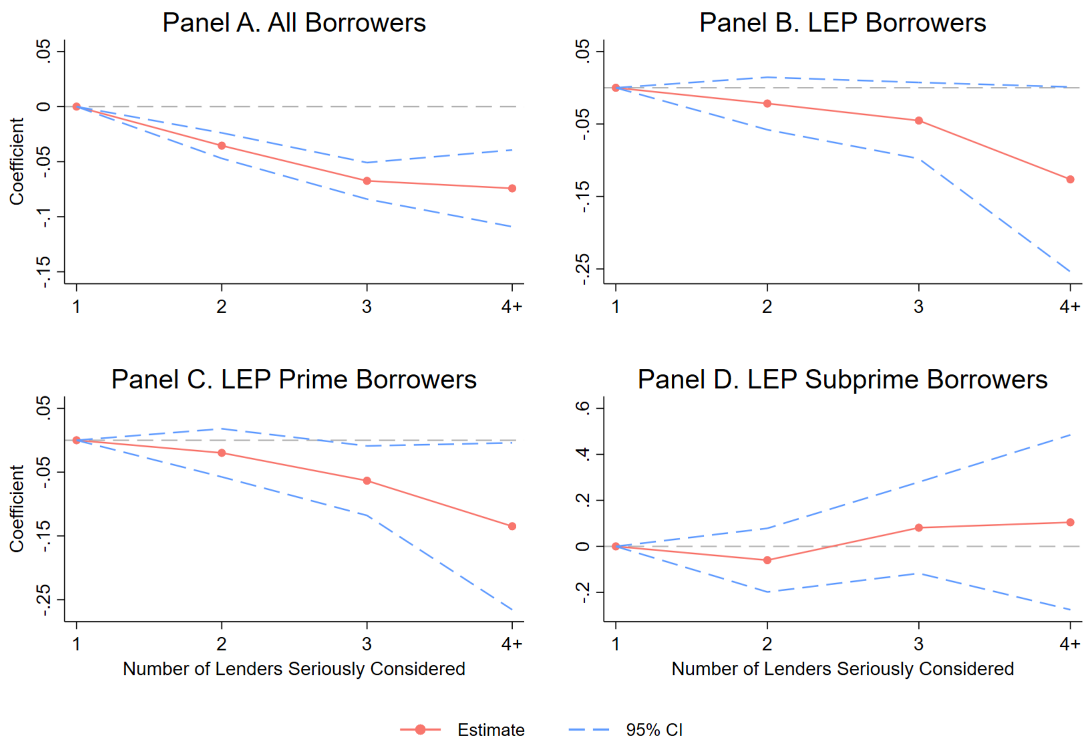
Figure F3. Triple-Differences Raw Comparison (cont.)

(e) do not know if it has a balloon payment



Notes: This figure plots the raw triple-difference comparison for five outcomes: the share of borrowers who resolved credit report errors, the share of borrowers who answered further request for income or asset information, the share of borrowers who had more than one home appraisal, the share of borrowers who did not know if the mortgage has an adjustable interest rate, and the share of borrowers who did not know if the mortgage has a balloon payment. Each panel plots for a certain type of borrowers: LEP and Hispanic in Panel A, non-LEP and Hispanic in Panel B, LEP and non-Hispanic in Panel C, and non-LEP and non-Hispanic in Panel D. The pre-policy shares are denoted by red bars, and the post-policy shares are denoted by blue bars. The number in the bar represents the corresponding share.

Figure F4. Consumer Search and Realized Mortgage Rates

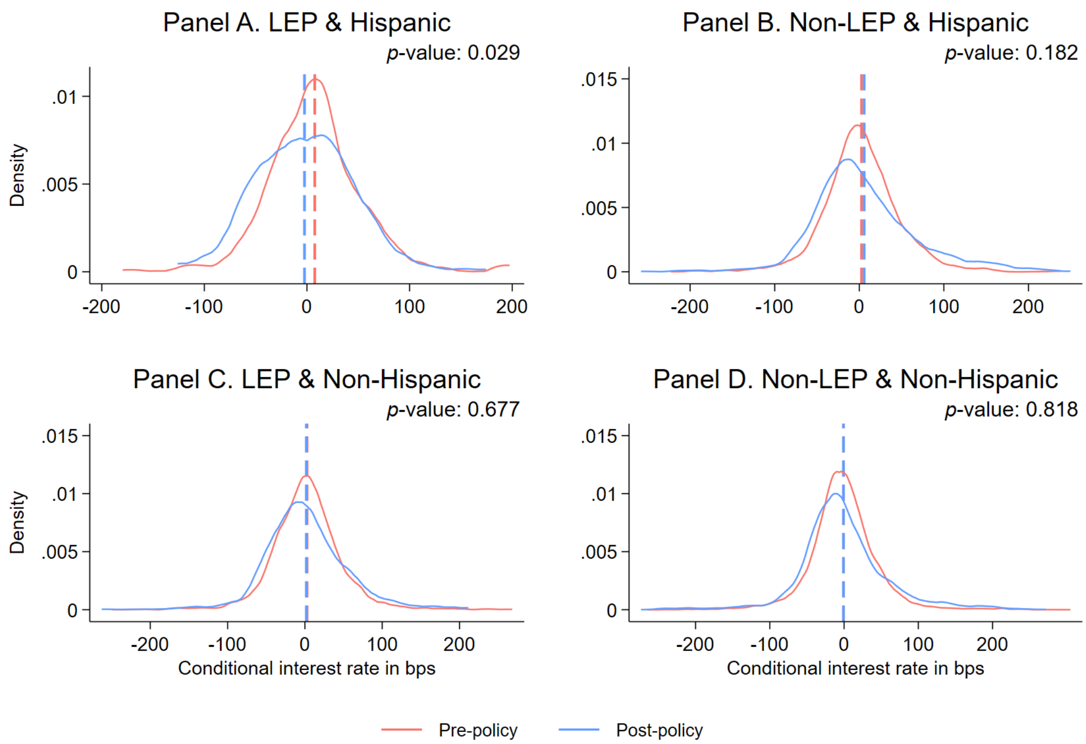


Notes: This figure plots the relationship between the number of lenders seriously considered and realized mortgage rates. I estimate the following specification:

$$r_{it} = \alpha + \sum_{s \geq 2} \beta_s \mathbb{1}\{s_i = s\} + \gamma X_i + \delta_t + \epsilon_{it}$$

where the dependent variable r_{it} is the mortgage rate of borrower i who took out the mortgage at time t. The independent variable of interest, s_i, is the number of lenders that a borrower seriously considered (capped at 4) before taking up a mortgage. This figure plots the coefficients β_s for 4 types of borrowers: all borrowers in panel A, LEP borrowers in panel B, prime LEP borrowers in panel C, and subprime borrowers in panel D. Subprime borrowers have a FICO score lower than 640.

Figure F5. Triple-Differences Raw Comparison: Distribution of Interest Rate



Notes: This figure plots the distributions of the pre-policy (in red) and post-policy (in blue) average conditional interest rate in basis points for 4 types of borrowers: LEP and Hispanic in panel A, non-LEP and Hispanic in panel B, LEP and non-Hispanic in panel C, and non-LEP and non-Hispanic in panel D. Conditional interest rate is the mean of raw interest rate plus the residual after regressing raw interest rate on origination quarter fixed effects, census tract type fixed effects, loan type, loan term, loan purpose, property tpye, occupancy type, and interest type. The p-values are associated with the null hypothesis that the pre-policy mean is equal to the post-policy mean.

2 Additional Tables

Table F1. Effect on Debt-to-Income Ratio

Sample	All	Purchase	Refinance	First-time borrowers	Repeat borrowers
	(1)	(2)	(3)	(4)	(5)
LEP × Hispanic × Post	-2.816**	-2.854*	-4.842**	-3.220	-2.933*
	(1.345)	(1.686)	(2.348)	(2.133)	(1.777)
Observations	35,553	18,118	15,977	6,739	28,807
Quarter FEs	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes
Post × Tract type FEs	Yes	Yes	Yes	Yes	Yes
Post × Risk FEs	Yes	Yes	Yes	Yes	Yes
Post × Loan controls	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the triple-difference estimates of the policy effect on debt-to-income ratio. Column 1 uses the full sample. Columns 2 and 3 use the sample of purchase and refinance loans, respectively. Columns 4 and 5 use the sample of loans borrowed by first-time borrowers and repeat borrowers, respectively. *Post* equals one if the mortgage was originated after June 2018. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan type, loan purpose, loan term, interest rate type, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. Robust standard errors are reported in parentheses. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table F2. Effect on Financial Literacy

Dependent variable: $\mathbb{1}$(familiar with ...)	Mortgage types (1)	Down payment (2)	Credit history (3)	Market rate (4)
LEP × Hispanic × Post	-0.043	-0.054	-0.038	0.007
	(0.068)	(0.070)	(0.067)	(0.067)
Pre-policy treated mean	0.319	0.425	0.706	0.421
Observations	35,553	35,553	35,553	35,553
Quarter FEs	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Post × Tract type FEs	Yes	Yes	Yes	Yes
Post × Risk FEs	Yes	Yes	Yes	Yes
Post × Loan controls	Yes	Yes	Yes	Yes

Notes: This table reports the triple-difference estimates of the policy effect on borrowers' financial literacy about the mortgage market. The dependent variables are indicators for whether the borrower was familiar with the market when they started their application. *Post* equals one if the mortgage was originated after June 2018. All regressions include origination quarter fixed effects and census tract type fixed effects. Demographic controls include race, ethnicity, gender, age and its squared, marital status, education, and household income. Risk fixed effects are the full pairwise interactions between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan purpose, property type, and occupancy status. All regressions are weighted by the analysis weight in NSMO. Robust standard errors are reported in parentheses. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table F3. Effect on Accessing Mortgage Credit: Conventional Refinance Loans

Dependent variable	# Applications (10K) (1)	Share of incomplete app. (2)	Denial rate (3)	# Originations (10K) (4)
LEP share × Post	-0.445*	0.002	0.020	-0.211
	(0.240)	(0.020)	(0.026)	(0.145)
Observations	25,253	25,253	25,253	25,253
County FEs	Yes	Yes	Yes	Yes
Year × State FEs	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes

Notes: This table reports the difference-in-differences estimates of the policy effect on the credit access of conventional refinance loans. The dependent variables in columns 1 to 4 are the number of mortgage applications (in ten thousands), the share of incomplete applications, the application denial rate, and the number of originations (in ten thousands), respectively. *LEP share* is the share of LEP Hispanic people before 2018, and the share of LEP Hispanics and Chinese in 2019. *Post* equals one after 2017. All specifications include county fixed effects, state-year fixed effects, and additional controls, which include median household income, total population, and the proportion of Hispanics, Asians, and black people. Standard errors are reported in parentheses and are clustered at the state level. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table F4. Effect on Accessing Mortgage Credit: All Purchase Loans

Dependent variable	# Applications (10K) (1)	Share of incomplete app. (2)	Denial rate (3)	# Originations (10K) (4)
LEP share × Post	0.014	-0.082***	-0.106***	0.026
	(0.061)	(0.012)	(0.032)	(0.046)
Observations	25,255	25,255	25,255	25,255
County FEs	Yes	Yes	Yes	Yes
Year × State FEs	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes

Notes: This table reports the difference-in-differences estimates of the policy effect on the credit access of all purchase loans. The dependent variables in columns 1 to 4 are the number of mortgage applications (in ten thousands), the share of incomplete applications, the application denial rate, and the number of originations (in ten thousands), respectively. *LEP share* is the share of LEP Hispanic people before 2019, and the share of LEP Hispanics and Chinese in 2019. *Post* equals one after 2017. All specifications include county fixed effects, state-year fixed effects, and additional controls, which include median household income, total population, and the proportion of Hispanics, Asians, and black people. Standard errors are reported in parentheses and are clustered at the state level. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table F5. Heterogeneous Effect on Accessing Mortgage Credit by Share of Hispanics and Chinese

Dependent variable	# Applications (10K) (1)	Share of incomplete app. (2)	Denial rate (3)	# Originations (10K) (4)
	Panel A. Small share of Hispanics and Chinese			
LEP × Post	0.791***	-1.001***	-2.172**	0.550***
	(0.243)	(0.335)	(1.046)	(0.167)
Observations	12,558	12,558	12,558	12,558
	Panel B. Large share of Hispanics and Chinese			
LEP × Post	0.086	-0.037*	-0.090*	0.065
	(0.053)	(0.022)	(0.046)	(0.039)
Observations	12,649	12,649	12,649	12,649
County FEs	Yes	Yes	Yes	Yes
Year × State FEs	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes

Notes: This table reports the difference-in-differences estimates of the heterogeneous effect of language help on mortgage credit access across racial composition. The sample is split based on the share of Hispanic and Chinese people. *LEP share* is the share of LEP Hispanic people before 2019, and the share of LEP Hispanics and Chinese in 2019. *Post* equals one after 2017. The dependent variables in columns 1 to 4 are the number of mortgage applications (in ten thousands), the share of incomplete applications, the application denial rate, and the number of originations (in ten thousands), respectively. All specifications include county fixed effects, state-year fixed effects, and additional controls, which include median household income, total population, and the proportion of Hispanics, Asians, and black people. Standard errors are reported in parentheses and are clustered at the state level. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table F6. Heterogeneous Effect on Accessing Mortgage Credit by Lender Competition

Dependent variable	# Applications (10K) (1)	Share of incomplete app. (2)	Denial rate (3)	# Originations (10K) (4)
	Panel A. Low HHI			
LEP × Post	0.248**	-0.104***	-0.181***	0.173**
	(0.098)	(0.024)	(0.054)	(0.073)
Observations	12,246	12,246	12,246	12,246
	Panel B. High HHI			
LEP × Post	-0.007	-0.016	-0.120**	0.0003
	(0.009)	(0.034)	(0.054)	(0.005)
Observations	12,979	12,979	12,979	12,979
County FEs	Yes	Yes	Yes	Yes
Year × State FEs	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes

Notes: This table reports the difference-in-differences estimates of the heterogeneous effect of language help on mortgage credit access across lender competition. The sample is split based on the HHI of conventional purchase loan originations for Hispanic and Asian borrowers in 2017. *LEP share* is the share of LEP Hispanic people before 2019, and the share of LEP Hispanics and Chinese in 2019. *Post* equals one after 2017. The dependent variables in columns 1 to 4 are the number of mortgage applications (in ten thousands), the share of incomplete applications, the application denial rate, and the number of originations (in ten thousands), respectively. All specifications include county fixed effects, state-year fixed effects, and additional controls, which include median household income, total population, and the proportion of Hispanics, Asians, and black people. Standard errors are reported in parentheses and are clustered at the state level. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table F7. Heterogeneous Effect on Accessing Mortgage Credit by LEP Share

Dependent variable	# Applications (10K) (1)	Share of incomplete app. (2)	Denial rate (3)	# Originations (10K) (4)
	Panel A. Low LEP share			
LEP share × Post	1.507***	-1.349***	-3.781***	0.998***
	(0.321)	(0.380)	(1.260)	(0.227)
Observations	12,607	12,607	12,607	12,607
	Panel B. High LEP share			
LEP share × Post	0.081	-0.038*	-0.094*	0.063
	(0.054)	(0.020)	(0.048)	(0.040)
Observations	12,478	12,478	12,478	12,478
County FEs	Yes	Yes	Yes	Yes
Year × State FEs	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes

Notes: This table reports the difference-in-differences estimates of the heterogeneous effect of language help on mortgage credit access across treatment level. The sample is split based on the LEP share at the level of 1%. The dependent variables in columns 1 to 4 are the number of mortgage applications (in ten thousands), the share of incomplete applications, the application denial rate, and the number of originations (in ten thousands), respectively. *LEP share* is the share of LEP Hispanic people before 2019, and the share of LEP Hispanics and Chinese in 2019. *Post* equals one after 2017. All specifications include county fixed effects, state-year fixed effects, and additional controls, which include median household income, total population, and the proportion of Hispanics, Asians, and black people. Standard errors are reported in parentheses and are clustered at the state level. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.

Table F8. Effect on Interest Rate: Adding Post $\times$ Lender $\times$ County FEs

Sample	Purchase	First-time borrowers	Repeat borrowers	Channel: retail	Channel: broker
	(1)	(2)	(3)	(4)	(5)
LEP $\times$ Hispanic $\times$ Post	-0.034***	-0.046***	-0.004	-0.043***	-0.017
	(0.008)	(0.011)	(0.012)	(0.011)	(0.012)
Observations	3,779,493	1,616,120	2,111,259	2,428,526	1,325,020
Month FEs	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes
Post $\times$ Lender $\times$ County FEs	Yes	Yes	Yes	Yes	Yes
Post $\times$ Risk FEs	Yes	Yes	Yes	Yes	Yes
Post $\times$ Loan controls	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the triple-differences estimates of the policy effect on interest rate. Column 1 uses all purchase loans in the HMDA$^+$ sample. Columns 2 and 3 use the sample of loans borrowed by first-time borrowers and repeat borrowers, respectively. Columns 4 and 5 use the sample of loans originated through retail lenders and brokers, respectively. *Post* equals one if the mortgage was originated after June 2018. All regressions include origination month fixed effects and *Post*-lender-county fixed effects. Risk fixed effects are the full pairwise interaction between LTV bins and FICO score bins, following the Loan-Level Price Adjustment Grid of Fannie Mae. Loan controls include loan type, loan term, property type, and occupancy status. All regressions are weighted by the analysis weight in the HMDA$^+$ sample. Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, 1% are denoted by *, **, and ***, respectively.