Left-digit bias in the FinTech era*

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Abstract

I show that a significant share of mortgage borrowers select rates within a 1 bp window just below whole-number rates (i.e. .99X-ending rates). Using a quasi-regression discontinuity design, I estimate that borrowers selecting these rates have all-in credit costs that are 7.4 bp higher than observably identical borrowers with whole-number rates just above them. This translates to $627 more in upfront fees paid by these borrowers. Cross-sectional tests reveal that borrowers selecting .99X-ending rates pay more because they overvalue the economic benefit of changing the left-most digit of their mortgage rate, which I interpret as left-digit bias. I further find that FinTech borrowers are more prone to left-digit bias, paying higher credit costs as a result. Using within-FinTech variation in application speed, I show that FinTech lending increases bias by facilitating more intuitive (bias-prone) decision-making in borrowers. This result is not driven by the selection of biased borrowers to FinTechs. In total, my results show that cognitive bias is an important driver of mortgage contract selection and that FinTech lending enhances its effect.

Keywords: behavioral bias, mortgage market, FinTech

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1 Introduction

Prior work shows that households exhibit left-digit bias when comparing prices in many settings, including the supermarket and auto loans (Argyle, Nadauld, and J, 2020; Jiang, 2021; Strulov-Shlain, 2023); however, there is limited evidence of this bias in the residential mortgage market. For most households, their mortgage is the largest, most important liability, and they will shop for one multiple times in their life. If present, then left-digit bias potentially has a significant effect on the household balance sheet, with many implications for public policy.[1] In this paper, I document left-digit bias in the US mortgage market, its marginal effect on credit cost, and how it is affected by FinTech lending.

Left-digit bias was originally introduced in the Marketing literature to explain the proliferation of 9-ending prices Thomas and Morwitz (2005). It is defined as consumers' overestimation of the economic impact of left-digit changing prices just below whole-number thresholds. Given that households exhibit this bias while shopping for other items, it is possible that they do so while shopping for a mortgage. That said, the mortgage origination process is complicated, and households may adapt their shopping practices in this setting. Further, pricing conventions in the secondary mortgage market potentially constrain lenders' ability to adjust rates to consumers biases in the primary market.

Empirically, left-digit bias produces bunching in the distribution of prices just below left-digit changing thresholds. I begin this paper by documenting bunching in the distribution of mortgage rates just below whole-number thresholds. Past studies use smooth density functions (*a la* McCrary (2008)) to identify bunching just below left-digit changing prices (Argyle et al., 2020; Jiang, 2021; Strulov-Shlain, 2023). However, pricing conventions in the secondary mortgage market produce natural bunching in the mortgage rate distribution at 12.5 bp increments. This invalidates the use of smooth density functions to identify bunching below left-digit chaning thresholds. To calibrate density expectations around whole-number

[1] Among these include consumer finance protection and monetary policy.

rates (i.e. .0-ending rates), I use loan densities around other 12.5 bp thresholds (e.g. .125, .25-ending rates, etc.). When looking at the other 12.5 bp thresholds, I find that nearly all the loans occur precisely at the 12.5 bp increment. However, when looking specifically at loans around whole-number thresholds, I detect a significant share of loans within a 1-bp window just below the 12.5 bp increment (i.e. .99X-ending rates). I interpret this as bunching in the distribution of rates at left-digit changing thresholds.

Bunching in the distribution of rates just below left-digit changing thresholds is also consistent with supply-side explanations (e.g. lender influences), in addition to borrowers' left-digit bias. To tease apart left-digit bias from supply-side influences, I collect millions of HMDA application records on rates offered by lenders but not accepted by borrowers. Following the approach of Argyle et al. (2020), I then compare the distribution of selected mortgage rates to the distribution of rates offered by lenders but rejected by borrowers. I find that bunching of rates just below left-digit changing thresholds is greater for the distribution of selected rates than offered rates, suggesting that it is driven by borrowers' preferences.

While consistent with overvaluing the economic difference of a left-digit changing rate (i.e. left-digit bias), demand-driven bunching just below left-digit changing thresholds is potentially explained by other factors. To tease apart left-digit bias from alternative mechanisms, I estimate whether borrowers selecting these rates pay more than borrowers selecting the nearest whole-number rate. Because the mortgage market is a partitioned price market, where the costs of mortgage credit are separated into an interest rate and upfront fees, selecting a rate just below a left-digit changing threshold (relative to the nearest whole-number rate) independently affects borrower fees. As a result, I can directly estimate whether borrowers selecting these rates pay more by estimating the effect of selecting these rates on borrowers' all-in credit cost, which includes their rate and upfront fees.

However, estimating this effect is challenging for two reasons. The first is a simple measurement problem. Because upfront fees are recorded as a dollar amount, they must be converted to a rate-equivalent variable in order to measure borrowers' all-in credit cost. To do so, I follow the approach of Kalda, Pearson, and Sovich (2025), which uses a benchmark in the secondary mortgage market to equate upfront fee payments with monthly interest payments made over the life of a loan. To measure borrowers' all-in credit cost, I sum their rate-equivalent fees with their interest rate.

The second factor challenging the estimation of the effects of selecting a .99X-ending rate is the non-random assignment of borrowers at and just below whole-number thresholds. As a result, raw comparisons of credit costs are likely biased by differences in timing, geography, loan product selection, and borrower credit risk. To estimate the effects of selecting a .99X-ending rate, I use a quasi-regression discontinuity model *a la* Lee and Lemieux (2010). Using the granular borrower and loan information available in HMDA, I compare the credit cost of borrowers whose rates are within a 1-bp window to observably similar borrowers at the nearest whole-number rate. The identifying variation in this model comes from borrowers with identical credit profiles, seeking the same type of loan, in the same census tract, and at the same time. Using this model, I estimate that selecting a .99X-ending rate raises borrowers' all-in credit cost by 7.4 bps. This translates to roughly $627 more in upfront fees paid by these borrowers.

Two broad mechanisms potentially explain this result. Either borrowers choosing .99X-ending rates do so because these rates signal value-relevant information (e.g. lender quality) or because they are biased. To tease apart these two mechanisms, I use cross-sectional heterogeneity in borrower origination channel and income. Recent evidence suggests that borrowers using a broker (as opposed to shopping on their own) attain better outcomes because brokers are expert shoppers. Additionally, higher income borrowers generally achieve better outcomes when shopping for a loan due to their higher financial sophistication. If information explains the higher credit costs of borrowers selecting .99X-ending rates, then we would expect brokered borrowers and higher income borrowers selecting these rates to also pay higher credit cost. However, consistent with borrowers selecting .99X-ending rates being biased, I find that these borrowers do not pay higher credit costs if they use a broker or have higher income.

Having shown that borrowers selecting .99X-ending rates are biased, I then isolate left-digit bias from alternative biases or financial mistakes (e.g. inattention, unobserved sophistication). To do so, I compare the effect of selecting a .99X-ending on credit costs to the effect of selecting a rate just below a midpoint of a whole-number threshold (i.e. .49X-ending rate). Borrowers bunching just below midpoints of whole-number thresholds have an economically similar move in their interest rate ($\approx$ 1 bp) and also have a digit change (going from .50 to .49X). However, borrowers bunching at this point do not have a change in the left-most digit of their rate. Additionally, these borrowers are likely subject to the other cognitive errors or financial mistakes potentially confounding previous estimates. This enables me to tease apart left-digit bias from alternative stories. I find that the effects of selecting a .99X-ending rate on borrower credit cost is much greater than the effect of selecting a .49X-ending rate. Based on this evidence, I conclude that borrowers are subject to left-digit bias when selecting residential mortgages.

After documenting left-digit bias in mortgages, I then study how FinTech lending affects this cognitive bias. Since the early 2010s, the market share of online-oriented, Fintech lenders has grown significantly (Buchak, Matvos, Piskorski, and Seru, 2018). Though this trend has received growing attention from academic researchers, evidence on how this trend affects borrowers' cognitive biases is limited. Broadly, FinTech lenders are characterized by a more automated application process, leading to faster credit decisions (Fuster, Plosser, Schnabl, and Vickery, 2019). While this automation facilitates a more convenient application process, it may also facilitate faster, more bias-prone decision making by borrowers (Kahneman, 2011).

To test whether FinTech lending increases left-digit bias among borrowers, I first compare the distribution of FinTech and non-FinTech loans. I find more bunching of .99X-ending rates in the distribution of FinTech loans than non-FinTech loans. This result is unique to FinTechs and is not a characteristic of all nonbank lenders. Motivated by this finding, I then test whether FinTech borrowers are more biased using a variation of the quasi-regression discontinuity model used to identify left-digit bias in earlier results. This model compares the difference in credit costs for borrowers selecting .99X-ending rates for FinTech loans relative to the difference for non-FinTech loans. I find that this difference is 12 bps greater for FinTech borrowers, indicating that they are more biased.

This result, while consistent with FinTech lending increasing bias, is also consistent with the selection of borrowers who are *ex ante* more biased to FinTechs. I present two broad methods for isolating the causal effects of FinTechs from alternative stories. In the first method, I estimate a fully saturated variation of the heterogeneity test used to identify greater bias in FinTech borrowers. This model includes interactions of FinTech lending with observable borrower and loan characteristics, absorbing variation in FinTech borrower bias attributable to these characteristics. When estimating this model, I still find that FinTech borrowers are more biased, suggesting that this result is not driven by selection on observables.

To the extent that observable characteristics are correlated with unobserved factors potentially confounding my results, the fully saturated model may isolate the causal effects of FinTech lending. As a more direct way to control for the unobserved selection of biased borrowers to FinTechs, I also present within-FinTech cross-sectional tests. These tests use cross-sectional differences in the incremental effect of FinTechs on application processing speed. Further, these tests absorb variation attributable to unobserved differences in FinTech borrowers selecting .99X-ending rates, as well as unobserved differences in the bias of borrowers selecting loans that allow for (tech-enabled) faster application speeds. In each cross-sectional test, I find that the incremental effect of FinTech lending on left-digit bias is greater in the distribution of loans where its effect on application processing speed is greater. This finding is significant, robust, and suggests that FinTech lending increases cognitive bias.

1.1 Contribution to Finance Literature

This paper contributes to three strands of the Finance literature. The first is the vast literature exploring how behavioral biases influence financial markets.[2] While early work focused largely on asset markets (De Bondt and Thaler, 1985; HIrshleifer, 2001), more recent work documents the role of these biases in debt markets (Stango and Zinman, 2009). In particular, a growing strand of research explores the role of behavioral biases in household credit markets (Kuchler and Pagel, 2021; Medina, 2021). Of particular relevance, Argyle et al. (2020) and Jiang (2021) document left-digit bias in the selection of auto loan monthly payments. To my knowledge, this paper is the first to document left-digit bias in the residential mortgage market.

Secondly, this paper contributes to the literature exploring the determinants of the cost of mortgage credit. A significant body of literature explores the supply-side determinants of credit costs, such as lender funding costs (Liu, 2019), lender competition (Jiang, 2023), securitization markets (Huh and Kim, 2022), and government-sponsored entities (Hurst, Keys, Seru, and Vavra, 2016; Kalda et al., 2025). However, recent work has documented the important role of demand-side determinants, such as borrower financial sophistication (Woodward and Hall, 2012; Agarwal, Ben-David, and Vincent, 2017), search costs and intensity (Allen, Clark, and Houde, 2019; Bhutta, Fuster, and Hizmo, 2024), use of brokers (Stone, 2025), in addition to borrower race (Ambrose, Conklin, and Lopez, 2021; Bhutta and Hizmo, 2021). My paper contributes to this literature by highlighting that borrowers' left-digit bias is a significant determinant of mortgage credit cost.

Finally, this paper also contributes to the growing literature on FinTech adoption. Chen, Wu, and Yang (2019) broadly documents the growth of financial technology in recent years. Several studies analyze the impacts of FinTech adoption in asset markets (Chiu and Koeppl, 2019; D'Acunto, Prabhala, and Rossi, 2019). A growing body of literature focuses on the

[2] For a review of the behavioral finance literature, see Hirshleifer (2015).

impact of FinTechs in credit markets (Vallee and Zeng, 2019; Berg, Fuster, and Puri, 2022). Of particular relevance to this paper, several recent papers explore the role of FinTech lending in the mortgage market (Buchak et al., 2018; Fuster et al., 2019; Bartlett, Morse, Stanton, and Wallace, 2022). My paper contributes to this literature by studying how FinTech lending affects the incidence of cognitive bias. To my knowledge, this paper is one of the firsts to do so.

1.2 Contribution to Marketing Literature

In addition to the Finance literature, this paper makes a couple contributions to the Marketing literature. The first is the literature on consumer purchasing behavior in partitioned pricing markets. Morwitz, Greenleaf, and Johnson (1998) first introduced the term partitioned pricing and documented its effects on consumer demand. Since then, many researchers have explored this pricing structure, documenting when and how it affects consumer purchasing behavior (Gabaix and Laibson, 2006; Burman and Biswas, 2007). I contribute to this literature by showing how partitioned pricing interacts with left-digit bias to affect consumer behavior. Left-digit effects were first documented empirically in Thomas and Morwitz (2005), and have since been studied extensively in the Marketing literature.[3] To my knowledge, this paper is one of the firsts to explore the relationship between left-digit bias and partitioned pricing structures.

Secondly, this paper contributes to the vast literature on consumer purchasing behavior in the online marketplace. Of particular relevance, Ratchford, Soysal, Zentner, and Gauri (2022) surveys the entire literature on online retailing, stating a need for research exploring how online retailing relates to price dispersion. I contribute directly to this research question, documenting the important role of left-digit bias in creating price dispersion between online and offline retail channels.

[3] See, for instance, Manning and Sprott (2009); Argyle et al. (2020); Jiang (2021); List, Muir, Pope, and Sun (2023); Strulov-Shlain (2023).

1.3 Roadmap

The rest of the paper proceeds as follows. Section 2 provides relevant background information. Section 3 describes the data in this study. Section 4 documents left-digit bias in the residential mortgage market. Section 5 explores how FinTech lending affects left-digit bias. Section 6 concludes.

2 Background Information

This papers studies mortgage borrowers' sensitivity to the left-most digit of of their interest rate. In particular, this paper focuses on the subgroup of borrowers selecting .99X-ending rates over the nearest whole-number rate. This sensitivity may affect borrowers' loan selection at different stages of the mortgage origination process. In this section, I describe the mortgage origination process and discuss the potential role of left-digit bias in that process.

Borrowers begin the mortgage origination process with a preliminary, "soft" search of available mortgages and their rates. This search mostly involves perusing lender websites and making informal contact with loan officers. Figure 1 provides an example of the rates marketed on a lender's website on May 16, 2025. As shown in this figure, all marketed rates also include the points associated with the offer. Points are a sub-component of fees that are directly tied lender rates. From the preliminary search, borrowers decide which loan product to pursue and which lenders to contact. A biased borrower shopping for a 20-year FRM may be more likely to contact this lender than a lender marketing a rate of 7.0% on their website for the same product.

When a borrower chooses to contact a lender for a formal quote, the lender will ask the borrower for basic information (e.g. income, assets) and run a credit check. Based on the information obtained during this process, lenders will provide a menu of legally binding offers to the borrower. As shown in Figure 2, lenders provide a range of quotes with different points/rate combinations; the borrower has the option to pay more (or less) points in exchange for receiving a lower (higher) interest rate. In addition to detailing the loan type, rate, points, and other fees, the lender will also detail how long they are willing to provide that offer (generally somewhere between 15 and 60 days). From the menu of quotes provided by different lenders, the borrower chooses which quote to "lock" in with the lender. Biased borrowers may be sensitive to .99X-ending rates when comparing lender point/rate sheets.

After locking the quote with a lender, the borrower begins the application and underwriting phase of the loan. This phase generally takes somewhere between 30 and 45 days. During this time, borrowers submit all relevant information and lenders underwrite the borrower and the property securing the loan. If this process does not reveal any material changes to the borrower's credit quality, then the lender is legally bound to provide the offer locked by the borrower. However, borrowers have the option to negotiate better loan terms with lenders during this process. If there is a material change in the interest rate environment, or the borrower finds a better offer during this time, they can negotiate a better offer from the lender. During this time, borrowers' sensitivity to the left-most digit of the interest rate marketed by other lenders can affect the terms they negotiate with their lender. Once these negotiations are complete and the lender has finished underwriting the loan, the lender funds the loan.

3 Data

3.1 Home Mortgage Disclosure Act (HMDA) Data

In this paper, I use public HMDA data from 2018 to 2023. This data covers roughly 90% of all mortgage applications during that period. Important for this study, public HMDA data includes precise information on the rate associated with each mortgage application, identifying the rate to a tenth of a basis point (i.e. rates are in X.XXX% format). Additionally, since 2018, the public HMDA file includes detailed information on all upfront fees paid by mortgage borrowers. This allows me to calculate the all-in (or full) credit costs inclusive of upfront fees. Lastly, this data also includes rich information on borrower, loan, and lender characteristics for each mortgage application in the data. I use this data to tighten identification arguments and run cross-sectional tests.

3.2 Sample Filters and Data Cleaning

My analysis focuses on a representative sample of residential, fixed-rate mortgage applications that either resulted in an originated loan or were offered by a lender but not accepted by a borrower. I only keep applications for loans with maturities of either 15 or 30 years (which are the most common type). Additionally, I only keep applications for loans secured by single-family, site-built properties (dropping apartments and manufactured homes). Further, I only keep applications for loans that will be used either to purchase a home or refinance an existing mortgage (both standard rate refis and cashout). Lastly, I drop all loans with exotic features.[4] To mitigate the influence of outliers in my analysis, I trim all continuous, numeric variables (including interest rates) at the 1st and 99th percentiles.

3.3 Summary Stats

Table 1 presents summary stats for the full sample of 34+ Mn mortgage applications meeting the criteria mentioned above. This sample includes applications during different monetary policy environments; interest rates range from 2.125% to 7.5%, with the median rate being 3.5%. 3% (or 1.07 mn of the rates in this sample) are rates offered by lenders but rejected by borrowers. The sample split between purchase and refinance loan applications is

[4]This includes reverse mortgage applications, as well as applications for loans with balloon payments, negative amortization, interest-only payments, prepayment penalties, or other non-amortizing features.

balanced, with 55% of the sample being purchase loan applications. Additionally, statistics on the distribution of lenders in my sample roughly matches statistics reported elsewhere (Urban Institute, 2023). Lastly, this sample includes data on all different types of mortgages, including conventional conforming, conventional jumbo, and FHA/VA loans. Combined, these summary stats suggest I have constructed a comprehensive and representative sample of residential mortgage applications. In the next section, I use this sample to study left-digit bias in the US mortgage market.

4 Results

4.1 Bunching at .99X-Ending Rates

Prior work shows that demand for auto loans bunches just below left-digit changing thresholds of the monthly payment distribution (Argyle et al., 2020; Jiang, 2021). In this section, I show that demand for US mortgages bunches just below left-digit changing thresholds of the rate distribution. Figure 3 plots the distribution of common rate endings for the full sample of 30+ million residential mortgage rates either selected by borrowers or offered by lenders. Nearly 90% of rates occur in 12.5 bp increments (i.e. .0, .125, .25, .375, .5, .625, .75, .875). This phenomenon is widely attributed to pricing conventions in the secondary mortgage market, where lenders hedge their exposure to mortgage rates.

Panel A of Figure 4 plots the distribution of rate endings for the 10% of loans with rates not occurring at a 12.5 bp increment. Most of those rates occur within a 1 bp window just below whole-number thresholds. To visualize this in the full distribution of rates, Panel B of Figure 4 plots the full distribution of rate endings for all rates in my sample. Between every other 12.5 bp increment, there is minimal loan density; however, between rate endings of .875 and whole-number rates, there is significant loan density in the 1 bp window just below whole-number thresholds. Figure 5 plots the density of loans around each whole-number threshold in my sample, showing that this phenomenon is persistent at each whole-number threshold in my sample. I label this bunching at .99X-ending rates.

Bunching of .99X-ending rates is consistent with left-digit bias, but it is also consistent with supply-side influences, such as lender marketing or secondary market demand. To test whether bunching at .99X-ending rates is borrower (i.e. demand) driven or driven by supply-side influences, I compare the distribution of rates selected by borrowers to the distribution of rates offered by lenders but rejected by borrowers.[5] If the bunching is borrower-driven, then we would expect greater bunching in the distribution of rates selected by borrowers. Formally, I estimate the following regression model for the subsample of loans with .99X-ending or whole-number rates:

$$.99XEnd_i = \alpha + \beta_1 * Selected_i + \epsilon_i \quad (1)$$

$.99XEnd_i$ is an indicator equal to 1 if the loan has a .99X-ending rate and 0 if it has a whole-number rate. $Selected_i$ is an indicator for whether the loan was chosen (i.e. not rejected) by a borrower. β_1 is the decimal increase in the likelihood that the loan has a .99X-ending rate conditional on the loan being selected by a borrower. If bunching of .99X-ending rates is demand-driven, then we would expect β_1 to be positive.

Column 1 of Table 2 presents estimates of Model 1. Consistent with borrower demand driving the bunching of .99X-ending rates, I find that loans selected by borrowers are much more likely to end in .99X than loans offered by lenders but rejected by borrowers. To verify that this result is not driven by overall differences in the distribution of rates, I report estimates with a Rate Band fixed effect (FE). The Rate Band FE uniquely identifies each whole-number threshold. Thus, β_1 is estimated by comparing the distribution of selected and offered rates at the same whole-number threshold. Column 2 shows that this result is robust to Rate Band fixed effects. In sum, this subsection documents demand-driven

[5]Note, this approach is also used in Argyle et al. (2020) to tease apart monthly payment targeting in auto loans from supply-side influences.

bunching at.99X-ending rates.

4.2 Credit Cost for .99X-Rate Borrowers

Demand-driven bunching at .99X-ending rates is consistent with borrowers overvaluing the economic benefit of reducing the left-most digit of mortgage rates (i.e. left-digit bias), but it is also consistent with several explanations that do not involve these borrowers paying more. In this section, I study whether borrowers selecting a .99X-ending rate, as opposed to the nearest whole-number rate, pay more. Because the US mortgage market is a partitioned price market, where the all-in cost of mortgage credit includes (separately quoted) interest rates and upfront fees, selecting a .99X-ending rate can independently affect borrowers' upfront fees. This potentially leads to economically meaningful differences in their all-in (or full) credit cost, despite receiving rates that are economically similar to borrowers selecting whole-number rates.

To study effects on credit costs, I must first convert borrower-paid upfront fees to a rate-equivalent measure such that the dual rate and fee effects of borrowers' rate selections can be netted. In measuring rate-equivalent fees, I borrow the following formula from Kalda et al. (2025):

$$NetWtdFees_i = \frac{NetFees_i}{(LAmt * 3.25)} * 100$$

$NetFees_i$ is the dollar amount of upfront fees paid by the borrower net of lender rebates, and $LAmt_i$ is the dollar amount of the originated loan. This approach scales upfront fees by the product of the borrowers' loan amount and the Fannie Mae buy-up multiple. This multiple, which is estimated as 3.25 in Kalda et al. (2025), is a measure of the relative cost to lenders of deferring loan insurance payments to the future. Here, it is used to set a common denominator for fees paid upfront and interest payments made over the life of the loan. Lastly, to get these scaled fees as a percentage point, just like $Rate_i$, I multiply them by 100.

Columns 1 through 3 of Table 3 displays summary stats for the subsample of borrowers selecting .99X-ending and whole-number rates. Note, summary stats for $NetWtdFees_i$ roughly match those produced in Kalda et al. (2025), suggesting that this measure is correctly calculated. $AllInCost_i$ is the sum of $Rate_i$ and $NetWtdFees_i$, which represents the percentage point all-in credit cost. Row 3 of Columns 4 through 6 compare $AllInCost_i$ of borrowers selecting .99X-ending rates to those selecting whole-number rates. On average, I find that borrowers selecting .99X-ending rates have a higher all-in credit cost than those selecting whole-number rates. However, this raw comparison is likely biased by differences in timing, geography, loan product selection, as well as differences in borrower credit risk. This results in comparisons of borrowers seeking a loan at different times and different whole-number rate thresholds.

The ideal comparison takes a pool of borrowers with identical credit profiles who are shopping for the same type of loan, in the same geographic area, and at the same time. I approximate this ideal comparison in the following quasi-regression discontinuity model *a la* Lee and Lemieux (2010):

$$\begin{aligned} AllInCost_i &= \alpha + \beta_1 * .99XEnd_i + \\ & f(income_i) + f(LAmt_i) + CLTV_i + \delta_{RB,MSA,Yr} + \gamma_{RB,LP,Yr} + Tract + DTI + Age + \epsilon_i \end{aligned} \tag{2}$$

δ is a Rate Band X MSA X Origination Year FE, and γ is a Rate Band X Loan Product X Origination Year FE. The Rate Band FE identifies the whole-number threshold of the loan (e.g. 3.99%-4.00%, *etc.*), and the Loan Product FE identifies the loan program (e.g. GSE, FHA), loan term (i.e. 30-yr, 15-yr), loan purpose (i.e. purchase or refinance), jumbo status, and CLTV bucket.[6] $Tract$, DTI, and Age are (respectively) census tract, debt-to-income ratio bucket, and applicant age FEs.[7] $f(income_i)$ and $f(LAmt_i)$ are cubic functions

[6]CLTV buckets are based in GSE underwriting guidelines.

[7]The DTI ratio and applicant age buckets are based on the categorizations in the public HMDA data.

in borrower income and loan amount, and $CLTV_i$ is a linear function in CLTV ratio.

The purpose of Model 2 is to flexibly control for differences in geography, timing, loan product selection, and borrower credit risk that may independently drive variation in credit cost. β_1 is the percentage point difference in credit costs for borrowers selecting .99X-ending rates relative to observably similar borrowers with whole-number rates in the same 1-bp window. Column 1 of Table 4 presents estimates of Model 2. I estimate that borrowers selecting .99X-ending rates pay 7.4 bps more than whole-number borrowers in the same 1bp window, with observably similar credit profiles, in the same census tract, shopping for the same loan type, and in the same year. Columns 2 and 3 present estimates of Model 2 substituting sub-components of $AllInCost_i$ as the outcome variable. I find that borrowers selecting .99X-ending rates pay 8.3 bps more in $NetWtdFees_i$, which, when combined with the mechanical effect on $Rate_i$, produces the observed effects on credit cost.

Column 4 presents estimates of Model 2 substituting the $NetFees_i$ as the outcome variable. On average, borrowers selecting .99X-ending rates over the nearest whole-number rate pay roughly \$627 more in upfront fees. To tease apart which components of fees that borrowers selecting .99X-ending rates pay more, Columns 5 through 7 present estimates substituting the sub-components of $NetFees_i$ as the outcome variable. These components include discount points ($Points_i$), lender rebates ($LenderCredits$), and other origination charges ($OtherCharges_i$). I find that most of the increased fees paid by borrowers selecting .99X-ending rates are points.

In sum, Table 4 shows that borrowers selecting .99X-ending rates pay more than if they had selected the nearest whole-number rate. This finding is inconsistent with explanations of bunching at .99X-ending rates that do not involve these borrowers paying more. In the next subsection, I tease apart bunching due to left-digit bias from information-based explanations of my results thus far.

4.3 Bias Vs. Quality

The results so far are consistent with loan quality-based explanations, such as borrowers selecting .99X-ending rates receiving better loan officer service or faster loan processing speed, in addition to borrower bias. To tease apart these two stories, I use cross-sectional heterogeneity in borrower origination channel. Borrowers whose loan is originated through the broker channel rely on an expert advisor (i.e. a mortgage broker) to shop for a loan on their behalf. Recent studies indicate that brokers are informed intermediaries, obtaining better outcomes than retail borrowers who shop on their own (Robles-Garcia, 2020; Allen, Clark, Houde, and Trubnikova, 2024; Stone, 2025). Due to brokers' expertise and experience, they are more likely to avoid financial errors and are more sensitive to differences in the quality of loan service.

If borrowers selecting .99X-ending rates receive better loan service, then we may expect the effect of selecting .99X-ending rates on credit costs to be the same for brokered and non-brokered borrowers. If, instead, borrowers selecting .99X-ending rates overpay, then one would expect brokered borrowers selecting .99X-ending rates to pay less fees. Columns 1 and 2 of Table 5 present split-sample estimates of Model 2 across non-brokered (i.e. Retail) and brokered borrowers. Consistent with borrowers selecting .99X-ending rates being biased, I find that moving to a .99X-ending rate significantly increases the credit cost for retail borrowers, but has no effect on brokered borrowers.

As additional evidence, I also present split-sample estimates across borrower income. Borrowers with higher income likely have higher financial sophistication and, like brokers, borrowers with higher financial sophistication make less cognitive errors and are likely more aware of differences in quality between lenders (Agarwal et al., 2017). Columns 3 and 4 of Table 5 present split-sample estimates of Model 2 across borrowers in the bottom 80% of the income distribution and the top 20% of the income distribution. Again, consistent with borrowers selecting .99X-ending rates being biased, I find that the more sophisticated borrowers selecting .99X-ending rates do not pay significantly more than their whole-number rate counterparts.

4.4 Left-digit Bias Vs. Alt Biases

So far, my results show that borrowers selecting .99X-ending rates are biased; however, alternative biases, other than left-digit bias, potentially explain these results. To isolate overpayment due to left-digit bias from alternative stories, I use systematic bunching in the rate distribution within a 1 bp window just below midpoints of whole-number rates (i.e. .49X-ending rates). Borrowers selecting .49X-ending rates over the nearest midpoint receive a change in the second digit (from the left) in their rate, but no change in the leftmost digit. Additionally, these borrowers are likely as susceptible to other cognitive errors as borrowers selecting .99X-ending rates. By comparing changes in credit costs for borrowers selecting .99X-ending rates to changes in credit costs for borrowers selecting .49X-ending rates, I isolate overpayment due to left-digit bias.

4.4.1 Bunching at Midpoints

I begin this subsection by documenting bunching in the distribution of .49X-ending rates (as I did for .99X-ending rates). As shown in Figure 4, a significant share of loans not occurring at a 12.5 bp increment occur within a 1 bp window mid-points of whole-number thresholds (i.e. bunching of .49X-ending rates). Figure 6 plots the density of loans around each midpoint in my sample, showing that this result is persistent across the rate distribution.

To verify that bunching at .49X-ending rates is driven by borrower demand rather than supply-side conditions, I estimate the following model (similar to Model 1) for the subsample of rates occurring around midpoints of whole-number thresholds:

$$.49XEnd_i = \alpha + \beta_1 * Selected_i + \epsilon_i \tag{3}$$

$.49XEnd_i$ is an indicator equal to 1 if the rate occurs in the 1 bp window below the whole number threshold and 0 if the rate occurs at a midpoint. β_1 is the decimal increase increase in the likelihood that the loan's rate is .49X-ending, conditional on being selected by a borrower. If the bunching just below midpoints is borrower driven, then we would expect β_1 to be positive. Columns 1 and 2 of Table 6 present estimates without and with Rate Band FEs. Consistent with borrower demand driving the bunching of .49X-ending rates, I find that bunching is greater for the distribution of selected rates than the distribution of offered rates.

4.4.2 Isolating Overpayment due to Left-Digit Bias

Having documented bunching of .49X-ending rates, I now use this bunching to isolate overpayment due to left-digit bias. Formally, I estimate the following model for the subgroup of borrowers with rates at or just below whole-number rates or midpoints:

$$AllInCost_i = \alpha + \beta_1 * .X9XEndRate_i + \beta_2 * .X9XEnd_i * .99XEnd_i + f(income_i) + \\ f(LAmt_i) + CLTV_i + \delta_{RB,MSA,Yr} + \gamma_{RB,LP,Yr} + Tract + DTI + Age + \epsilon_i \quad (4)$$

All the FEs and controls are defined the same as the are in Model 2. As in that model, the Rate Band FE is defined as the 1-bp window of the borrower's rate. Thus, rates at or just below midpoints of whole numbers are defined as their own rate band. $X9EndRate_i$ is an indicator equal to 1 if the borrower's rate is .49X-ending or .99X-ending. β_1 is the difference in costs for borrowers with a .49X-ending rate relative to observably similar borrowers with a .5-ending rate. β_2 is the difference in the change in credit costs for borrowers moving just below the whole-number thresholds relative to the change in credit costs for borrowers moving just below midpoints.

Table 7 presents estimates of Model 4. My estimate for β_1 is economically and statistically insignificant from zero and my estimate for β_2 is similar in magnitude to the estimates in

Table 4. This result suggests that borrowers selecting .99X-ending rates overpay because they overvalue the economic benefit of changing the left-most digit of their interest rate (i.e. left-digit bias).

5 Left-digit Bias and FinTech Lending

As documented in several recent papers, the market share of (online-oriented) FinTech mortgage lenders has grown in recent years (Buchak et al., 2018; Fuster et al., 2019). These lenders are characterized by more automated application processes, leading to faster credit decisions. This trend of technological adoption is mirrored in other household credit markets, including the market for personal loans and credit cards (Chen et al., 2019; Berg et al., 2022). Up until now, there is limited evidence regarding how this trend affects borrowers' propensity for financial mistakes, yet understanding this is key to understanding the regulatory implications of FinTech adoption.

In this section, I study the effect of FinTech lending on left-digit bias. On one hand, FinTechs may lower the cost of applying for a loan, allowing borrowers more time and energy to engage in a deliberate (or less biased) mortgage search. Alternatively, the speed of FinTech lending may facilitate more automatic, intuitive decision making by borrowers. This, in turn, may lead to a greater incidence of behavioral bias in their price evaluations (Kahneman, 2011).

5.1 FinTech Bunching at .99X-Ending Rates

I begin by comparing the distribution of FinTech and non-FinTech loans. If FinTech lending increases left-digit bias, then we should observe greater bunching at .99X-ending rates in the distribution of FinTech loans. To identify FinTech lenders in my sample, I use the list of mortgage lenders in Fuster et al. (2019).[8] I then estimate the following model for the subsample of loans with either .99X-ending or whole-number rates:

$$.99XEnd_i = \alpha + \beta_1 * FinTech_l + \epsilon_i \tag{5}$$

$FinTech_l$ is a lender-level indicator for whether the loan was originated by a FinTech lender. β_1 is the decimal difference in the selection of .99X-ending rates in the distribution of FinTech loans.

Column 1 of Table 8 reports estimates of Model 5. I find that FinTech borrowers are 47 percentage points more likely to select a .99X-ending rate conditional on having a loan near a whole-number rate. As in prior tables, I also presents estimates with Rate Band FEs, where β_1 is estimated by comparing the distribution of FinTech and non-FinTech rates at the same whole-number threshold. Column 2 of Table 8 shows that this result is robust to Rate Band FEs.

However, since all FinTech lenders in my sample are also Nonbanks, these results are potentially driven by overall differences in the distribution of Nonbank loans. To verify that this result is unique to FinTechs, I estimate the following model:

$$.99XEnd_i = \alpha + \beta_1 * Nonbank + \beta_2 * Nonbank * FinTech + \epsilon_i \tag{6}$$

$Nonbank_i$ is an indicator for whether the lender is a Nonbank. β_1 is the decimal increase in bunching in the distribution of Nonbank loans relative to Bank loans. β_2 is the decimal increase in bunching for FinTech borrowers relative to non-FinTech, Nonbank borrowers. Columns 3 and 4 of Table 8 present results without and with Rate Band FEs. These estimates imply that the greater bunching in the distribution of FinTech loans observed in Columns 1 and 2 are unique to FinTechs and not a characteristic of Nonbanks.

[8] These lenders include: Quicken Loans, Loandepot.com, Guaranteed Rate, Movement Mortgage, Everett Financial, and Better.com.

5.2 FinTech Credit Cost at .99X-Ending Rates

Greater bunching in the distribution of FinTech loans is consistent with FinTech lending increasing bias, but it is also consistent with explanations that do not involve FinTech borrowers being more biased (e.g. FinTech marketing practices). To isolate FinTech borrowers being more biased from alternative stories, I compare the credit costs of FinTech borrowers selecting .99X-ending rates to non-FinTech borrowers selecting the same rates. Formally, I estimate the following model:

$$\begin{aligned} AllInCost_i &= \alpha + \beta * .99XEnd_i + \beta_2 * .99XEnd_i * Fintech_l + \beta_3 * FinTech_l \\ & f(income_i) + f(LAmt_i) + CLTV_i + \delta_{RB,MSA,Yr} + \gamma_{RB,LP,Yr} + Tract + DTI + Age + \epsilon_i \end{aligned} \tag{7}$$

This model is identical to Model 2, except that it now includes $FinTech_l$ and a $FinTech_l * .99XEnd_i$ interaction term. β_1 is the effect of selecting a .99X-ending rate for non-FinTech borrowers and β_2 is the relative difference in the effect for FinTech borrowers. Note, β_3 absorbs overall differences in the credit cost of FinTech and non-FinTech lenders. If FinTech borrowers are more biased, then β_2 should be positive. Column 1 of Table 9 reports estimates of Model 7. Consistent with the intensity of left-digit bias being greater for FinTech borrowers, I estimate that the effect of selecting .99X-ending rates is greater for FinTech borrowers than non-FinTech borrowers.

One potential concern is that the estimates in Column 1 are driven by the selection of more biased borrowers to FinTechs, rather than the causal effect of FinTech lending. To mitigate this concern, I re-estimate Model 7 including $FinTech_l$ terms interacted with all FEs and controls. This model controls for differences in observable borrower or loan characteristics that may independently drive left-digit bias among borrowers. Column 2 of Table 9 presents estimates of this model. Consistent with selection on observables not driving the estimates in Column 1, I find that my initial estimates are robust to this specification.

5.3 Cross-Sectional Tests on App Speed

So far, the results in this section show that FinTech borrowers are more biased and that selection on observables does not explain this result. These results are consistent with the faster application speeds of FinTech lending increasing the incidence of cognitive bias, but it is also consistent with the unobserved selection of biased borrowers to FinTechs. In this subsection, I present tests that use within-FinTech variation in application speed to isolate my results from the unobserved selection of biased borrowers. These tests absorb unobserved differences in borrowers selecting .99X-ending rates from FinTechs, thereby permitting identification under weaker assumptions.

5.3.1 Refi vs. Purchase Loans

In my first test, I use cross-sectional variation in loan purpose. As documented in Fuster et al. (2019), the incremental difference in application processing speed for FinTechs is greater for refinances than loans originated to purchase a home. The authors attribute this finding to regulatory hurdles specific to purchase loans (e.g. in-person property appraisals) that restrict automation. By comparing differences in the degree of bias among FinTech borrowers in the distribution of purchase and refinance loans, I can isolate the effect of FinTech lending from unobserved selection.

Table 10 presents split-sample estimates of Model 7 across purchase and refinance loans. In this test, I am using the $.99XEnd_i * FinTech_l$ interaction term in the distribution of purchase loans to absorb unobserved differences in the bias of FinTech borrowers selecting .99X-ending rates. If FinTech lending causes an increase in left-digit bias through its application processing speed, then one would expect the coefficient on the $.99XEnd_i * FinTech_l$ interaction term to be greater for refinance than purchase loans. Consistent with FinTech lending causing an increase in left-digit bias, I find that the incremental bias of FinTech borrowers is greater for refinance loan borrowers.[9]

5.3.2 Conforming Vs. Jumbo Loans

In this next test, I use cross-sectional variation in conforming loan status. The underwriting standards for conforming loans, set by government or quasi-government agencies, apply broadly to all conforming loans, are well-documented, and based on hard information. Because these standards are universal and well-documented, the benefit of automation towards application processing speed are great. However, the underwriting standards for non-conforming (or jumbo) loans, which have loan amounts greater than the conforming loan limit, are dispersed and based on the preferences of individual jumbo loan investors, restricting automation of the application process.

By comparing differences in the effects of FinTech lending on left-digit bias for conforming and purchase loan borrowers, I can also isolate the causal effects of FinTech lending from unobserved selection. Table 11 presents split-sample estimates of Model 7 across jumbo and conforming loans. This test uses the $.99XEnd_i * FinTech_l$ interaction term in the jumbo loan distribution to absorb unobserved differences in the *ex ante* bias of FinTech borrowers selection .99X-ending rates. Consistent with the greater application speed of FinTechs increasing left-digit bias, I estimate that the effect of FinTech lending on bias is greater (and statistically significant) for conforming than jumbo loans.

In total, the results of this section document greater left-digit bias among FinTech borrowers. Cross-sectional tests suggest that this result is driven by the faster application speeds of FinTechs facilitating intuitive, bias-prone selections by borrowers.

[9]Note, this result is statistically significant.

6 Conclusion

Despite ample evidence of behavioral bias in borrower selections in other household credit markets, there is limited evidence of its role in the residential mortgage market. Further, there is limited evidence on how FinTech lending, which encompasses a growing share of many household credit markets, impacts borrowers' bias. This paper fills that gap in the literature.

I first document borrower-driven, bunching of .99X-ending mortgage rates. Then, using a quasi-regression discontinuity research design to compare the credit cost of borrowers selecting .99X-ending rates to observably similar borrowers selecting the nearest whole-number rate, I estimate that borrowers selecting these rates pay 7.4 bps more. Using cross-sectional tests on borrower financial sophistication, I show that this finding is driven by borrowers' bias. Then, using bunching at other points of the rate distribution, I show that this result is driven by borrowers overvaluing the economic benefit of a left-digit changing rate (i.e. left-digit bias). I then explore how FinTech lending impacts left-digit bias. I find that FinTech borrowers are much more biased. Using granular, within-FinTech variation in mortgage contract selections, I isolate the causal effect of FinTech lending from selection on unobservables. I find that the faster application speeds of FinTech lending increases borrowers' bias.

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7 Tables and Figures

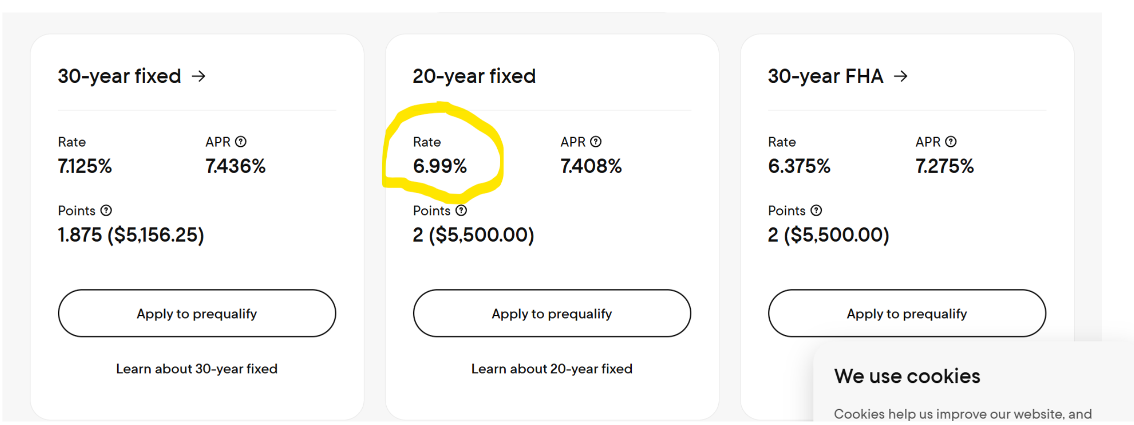

Figure 1: Example of Rates Marketed on a Lender's Website

Table 1: Summary Stats (Full Sample of Selected and Offered Rates)

	Mean	Min	P10	P50	P90	Max
Rate (pps)	3.884	2.125	2.750	3.500	5.625	7.500
Income (1000s)	118.816	24.000	47.000	98.000	215.000	595.000
CLTV (pps)	77.296	24.000	51.000	80.000	97.000	101.522
LoanAmt (1000s)	310.001	55.000	135.000	275.000	535.000	1225.000
Selected (1/0)	0.969					
Purchase (1/0)	0.549					
FRM30 (1/0)	0.884					
ConvConforming (1/0)	0.763					
ConvJumbo (1/0)	0.033					
FHA (1/0)	0.127					
VA (1/0)	0.068					
BrokerLoan (1/0)	0.128					
TraditionalNonbank (1/0)	0.551					
FinTechNonbank (1/0)	0.128					
Observations	34401432					

This table presents summary stats for the full distribution of rates offered by lenders meeting the criteria discussed in Section 3 of the text.

Table 2: Bunching at .99X-Ending Rates is Borrower-Driven

	(1) .99XEnd (1/0)	(2) .99XEnd (1/0)
Selected (1/0)	0.0593*	0.0581*
	(2.22)	(2.21)
# of Obs.	4942597	4942597
Mean of Y	0.480	0.480
RateBand FE?	NO	YES

t statistics in parentheses

Errors clustered at Lender level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table presents estimates of β_1 in the following model:

$$.99XEnd_i = \alpha + \beta_1 * Selected_i + \epsilon_i$$

Column 1 presents estimates without Rate Band fixed effects and Column 2 presents estimates with them. Rate Band fixed effects uniquely identify each whole-number threshold. Thus, the estimate of β_1 reported in Column 2 is identified comparing selected and offered rates at the same whole-number threshold. See Section 4 of the text for variable definitions.

Table 3: Summary Stats of Loans Near Whole Number Rates

	(1) Mean	(2) Min	(3) Max	(4) .99XEnd	(5) .0End	(6) Diff
.99XEnd (1/0)	0.49	0.00	1.00			
Rate (pps)	3.87	2.99	7.00	3.88	3.85	0.02
AllInCosts (pps)	4.12	0.87	9.93	4.19	4.06	0.14^{*}
NetWtdFees (pps)	0.26	-2.13	3.46	0.32	0.20	0.11^{*}
NetFees($)	2083.84	-3838.10	13182.95	2565.51	1626.53	938.98^{**}
DiscPnts($)	1216.78	0.00	11825.55	1617.90	835.95	781.95^{**}
OthrFees($)	867.06	-15560.59	13180.00	947.62	790.58	157.04^{*}
LndrCrdts($)	342.35	0.00	6554.00	309.61	373.44	-63.83
Purchase (1/0)	0.53	0.00	1.00	0.46	0.60	-0.13^{*}
RateRefi (1/0)	0.26	0.00	1.00	0.28	0.24	0.04^{**}
Cashout (1/0)	0.21	0.00	1.00	0.26	0.16	0.09
FRM30 (1/0)	0.93	0.00	1.00	0.94	0.91	0.03^{*}
FRM15 (1/0)	0.07	0.00	1.00	0.06	0.09	-0.03^{*}
ConvConforming (1/0)	0.80	0.00	1.00	0.87	0.75	0.12^{***}
ConvJumbo (1/0)	0.03	0.00	1.00	0.01	0.05	-0.04^{***}
FHA (1/0)	0.11	0.00	1.00	0.08	0.13	-0.05^{**}
VA (1/0)	0.05	0.00	1.00	0.04	0.06	-0.02^{*}
USDA (1/0)	0.01	0.00	1.00	0.00	0.01	-0.01^{***}
Income (1000s)	115.29	24.00	595.00	111.91	118.51	-6.60^{*}
CLTV (pps)	77.06	24.00	101.52	75.36	78.67	-3.32^{**}
LoanAmt (1000s)	304.55	55.00	1225.00	301.36	307.59	-6.23
Observations	4408518			2147076	2261442	

Errors (in diff-in-means test) clustered at Lender level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table presents summary stats for the distribution of loans selected by borrowers that have either .99X-ending or whole-number rates. Columns 1 through 3 present summary stats. Columns 4 through 6 present raw differences in mean observables across borrowers selecting .99X-ending and whole-number rates.

Loan Amount	Rate with 0 Mortgage Points	Mortgage Points Paid at Closing	Cost of Mortgage Points (1% of loan amount)	Discounted Rate (Determined by Lender)	Monthly Payment (Principal + Interest)	Total Cost of Mortgage (P+I) Over 30-Year Term	Total Interest Savings Over 30-Year Term
$150,000	6.5%	0	$0	6.5%	$948.10	$341,317	--
$150,000	6.5%	1	$1,500	6.25%	$923.58	$332,487	$8,829
$150,000	6.5%	1.5	$2,250	6.125%	$911.42	$328,110	$13,207
$150,000	6.5%	2	$3,000	6.0%	$899.33	$323,757	$17,559

Figure 2: Example of Point/Rate Combinations Offered by a Lender on Single Loan Product

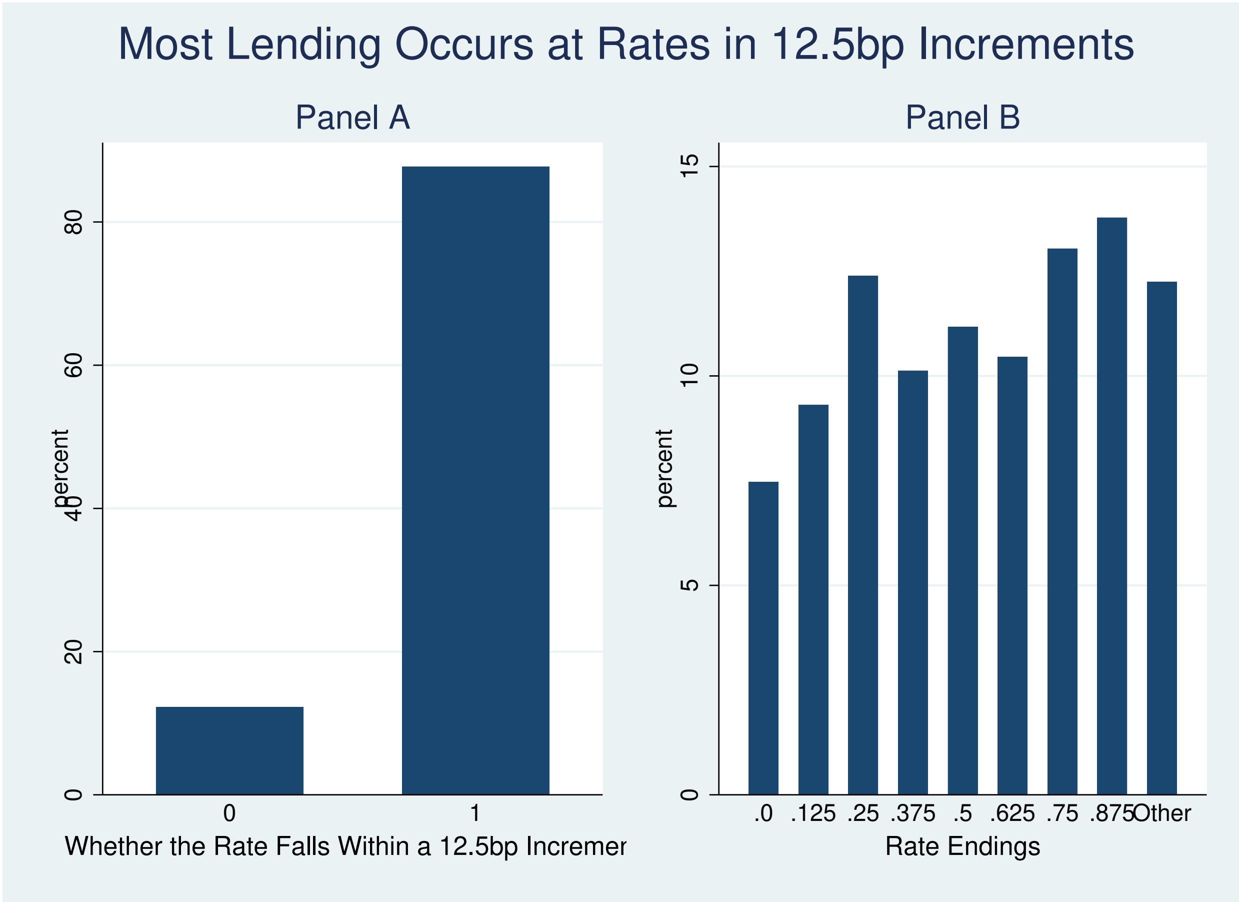

Figure 3: Lending At 12.5 bps Increments

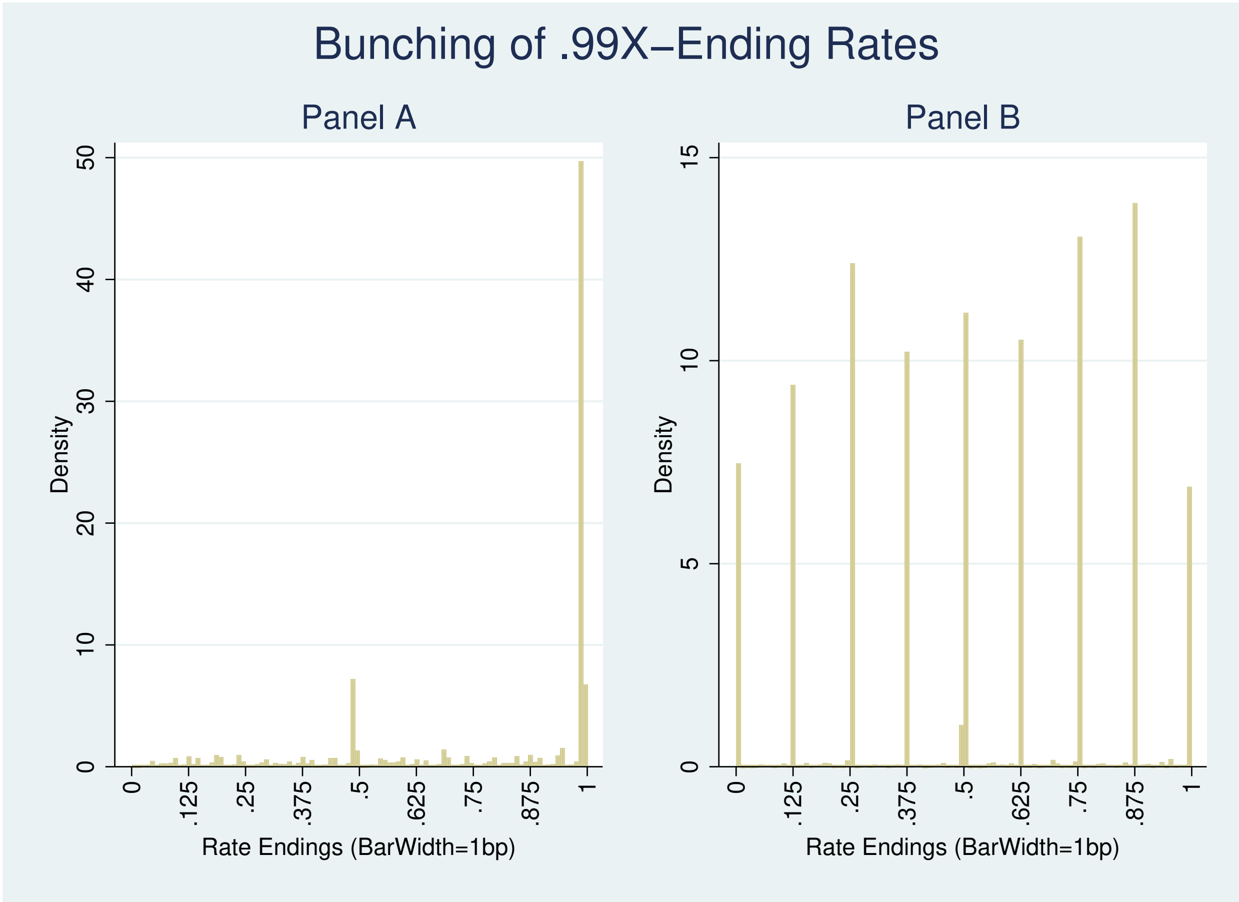

Figure 4: Bunching of .99X-Ending Rates

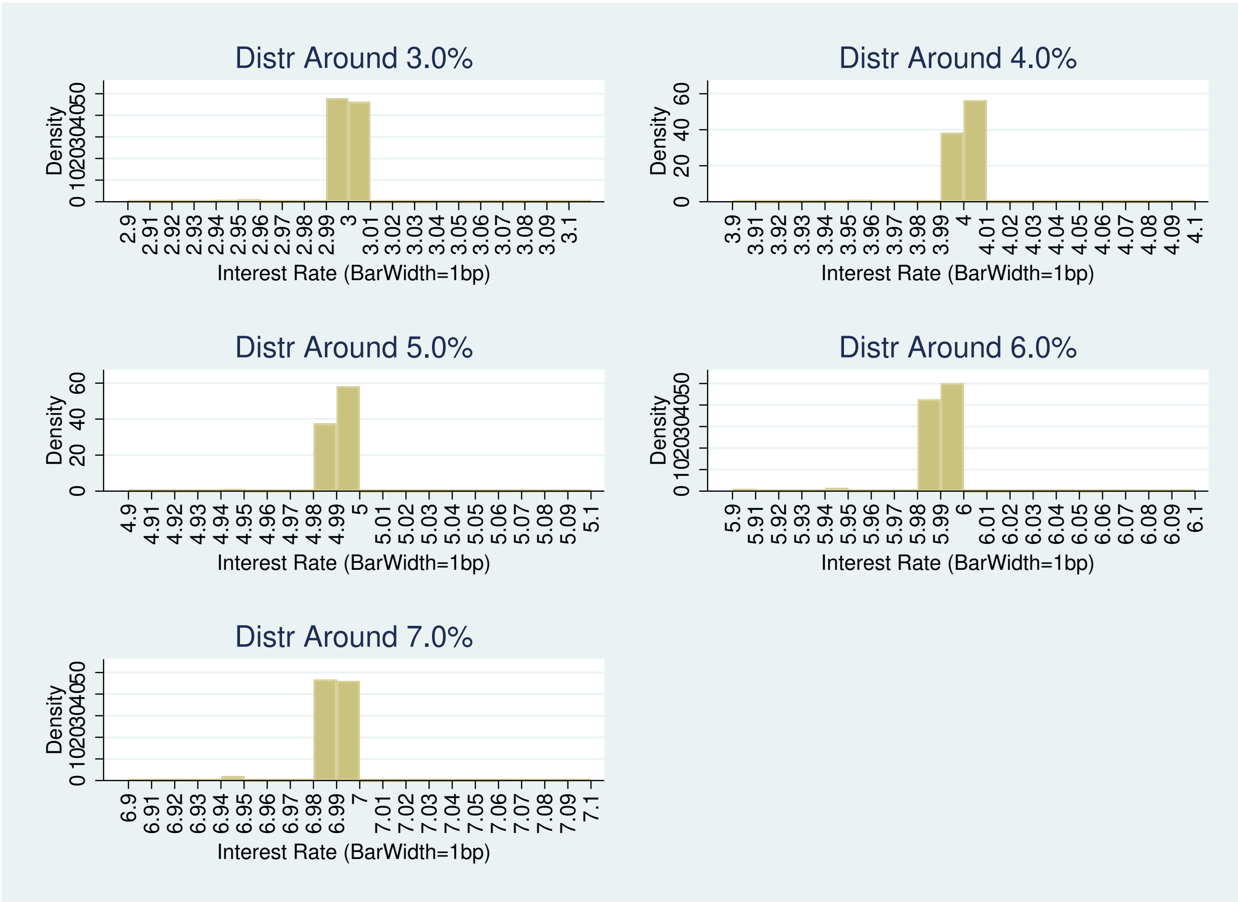

Figure 5: Distribution of Rates Around Each Whole-Number Rate

Table 4: Selecting a .99X-Ending Rate Increases Credit Cost

	(1) AllInCosts (pps)	(2) Rate (pps)	(3) NetWtdFees (pps)	(4) NetFees($)	(5) DiscPnts($)	(6) OthrFees($)	(7) LndrCrdts($)
.99XEnd (1/0)	0.0737*	-0.00905***	0.0827*	627.4*	491.2*	136.1**	-76.36
	(2.06)	(-14.82)	(2.30)	(2.36)	(2.23)	(2.64)	(-1.07)
# of Obs.	4404951	4404951	4404951	4404951	4404951	4404951	4404951
Mean of Y	4.122	3.865	0.258	2083.5	1216.6	866.9	342.4
RateBand X MSA X Yr FE	YES	YES	YES	YES	YES	YES	YES
RateBand X Prgrm X Yr FE	YES	YES	YES	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES	YES	YES	YES
DTI FE	YES	YES	YES	YES	YES	YES	YES
Age FE	YES	YES	YES	YES	YES	YES	YES
f(Inc)	YES	YES	YES	YES	YES	YES	YES
f(LAmt)	YES	YES	YES	YES	YES	YES	YES
CLTV	YES	YES	YES	YES	YES	YES	YES

t statistics in parentheses

Errors clustered at Lender level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table presents estimates of β_1 in the following model:

$$y_i = \alpha + \beta_1 * .99XEnd_i + f(income_i) + f(LAmt_i) + CLTV_i + \delta_{RB,MSA,Yr} + \gamma_{RB,LP,Yr} + Tract + DTI + Age + \epsilon_i$$

For variable definitions and an economic interpretation of these estimates, see Section 4 of the text.

Table 5: Bias Drives Credit Cost Effect, Not Information

	(1-Retail)	(2-Broker)	(3-LowInc)	(4-HighInc)
	AllInCosts (pps)	AllInCosts (pps)	AllInCosts (pps)	AllInCosts (pps)
.99XEnd (1/0)	0.0829*	0.0200	0.0788*	0.0477
	(2.14)	(1.43)	(2.11)	(1.83)
# of Obs.	3849798	541849	3581747	809927
Mean of Y	4.123	4.095	4.148	3.993
RateBand X MSA X Yr FE	YES	YES	YES	YES
RateBand X Loan Program X Yr FE	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES
DTI FE	YES	YES	YES	YES
Age FE	YES	YES	YES	YES
f(Inc)	YES	YES	YES	YES
f(LAmt)	YES	YES	YES	YES
CLTV	YES	YES	YES	YES

t statistics in parentheses

Errors clustered at Lender level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table presents split-sample estimates of β_1 in the following model:

$$AllInCosts_i = \alpha + \beta_1 * .99XEnd_i + f(income_i) + f(LAmt_i) + CLTV_i + \delta_{RB,MSA,Yr} + \gamma_{RB,LP,Yr} + Tract + DTI + Age + \epsilon_i$$

Columns 1 and 2 present separate estimates for Retail and Brokered borrowers. Columns 3 and 4 present separate estimates for borrowers in the bottom 80% and the top 20% of the income distribution. For variable definitions and an economic interpretation of the results, See Section 4 of the text.

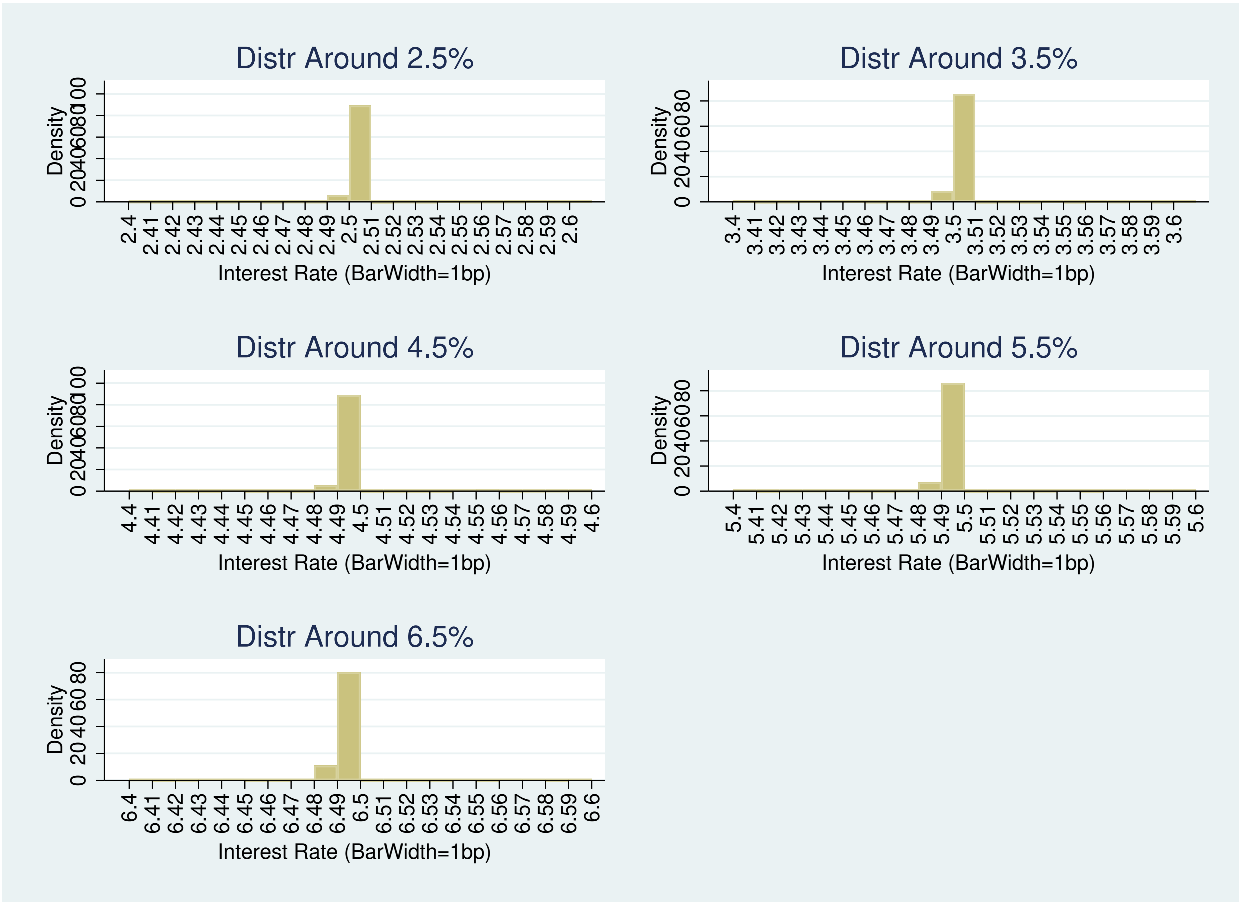

Figure 6: Distribution of Rates Around Each Midpoint

Table 6: Bunching at .49X-Ending Rates is Borrower-Driven

	(1)	(2)
	Pnt49XEnd (1/0)	Pnt49XEnd (1/0)
Selected (1/0)	0.0194	0.0220*
	(1.94)	(2.22)
# of Obs.	4198206	4198206
Mean of Y	0.0843	0.0843
RateBand FE?	NO	YES

t statistics in parentheses

Errors clustered at Lender level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table presents estimates of β_1 in the following model:

$$.49XEnd_i = \alpha + \beta_1 * Selected_i + \epsilon_i$$

Column 1 presents estimates without Rate Band fixed effects and Column 2 presents estimates with them. Rate Band fixed effects uniquely identify each midpoint threshold. Thus, the estimate of β_1 reported in Column 2 is identified comparing selected and offered rates at the same midpoint. See Section 4 of the text for variable definitions.

Table 7: Left-digit Bias Explains Results, Not Other Errors/Biases

	(1) AllInCosts (pps)	(2) Rate (pps)	(3) NetWtdFees (pps)	(4) NetFees($)	(5) DiscPnts($)	(6) OthrFees($)	(7) LndrCrdts($)
.X9XEnd (1/0)	-0.0132	-0.00861***	-0.00462	-23.86	-49.29	25.43	-13.35
	(-1.59)	(-18.97)	(-0.57)	(-0.40)	(-0.73)	(0.44)	(-0.33)
.99XEnd * .X9XEnd (1/0)	0.0866*	-0.000446	0.0870*	648.5*	539.0*	109.5*	-62.80
	(2.28)	(-1.20)	(2.28)	(2.31)	(2.04)	(2.36)	(-0.75)
# of Obs.	8116815	8116815	8116815	8116815	8116815	8116815	8116815
Mean of Y	4.173	3.919	0.254	2033.7	1185.3	848.5	344.4
RateBand X MSA X Yr FE	YES	YES	YES	YES	YES	YES	YES
RateBand X Prgrm X Yr FE	YES	YES	YES	YES	YES	YES	YES
Tract FE	YES	YES	YES	YES	YES	YES	YES
DTI FE	YES	YES	YES	YES	YES	YES	YES
Age FE	YES	YES	YES	YES	YES	YES	YES
f(Inc)	YES	YES	YES	YES	YES	YES	YES
f(LAmt)	YES	YES	YES	YES	YES	YES	YES
CLTV	YES	YES	YES	YES	YES	YES	YES

t statistics in parentheses

Errors clustered at Lender level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table presents estimates of β_1 and β_2 in the following model:

$$y_i = \alpha + \beta_1 * .X9XEndRate_i + \beta_2 * .X9XEnd_i * .99XEnd_i + f(income_i) + f(LAmt_i) + CLTV_i + \delta_{RB,MSA,Yr} + \gamma_{RB,LP,Yr} + Tract + DTI + Age + \epsilon_i$$

For variable definitions and an economic interpretation of these results, see Section 4 of the text.

Table 8: FinTech Bunching of .99X-ending Rates

	(1) .99XEnd (1/0)	(2) .99XEnd (1/0)	(3) .99XEnd (1/0)	(4) .99XEnd (1/0)
FinTech (1/0)	0.472***	0.476***		
	(7.40)	(6.81)		
Nonbank (1/0)			0.326***	0.321***
			(5.88)	(6.05)
Nonbank * FinTech (1/0)			0.360***	0.363***
			(5.87)	(5.46)
# of Obs.	4408518	4408518	4408518	4408518
Mean of Y	0.487	0.487	0.487	0.487
RateBand FE?	NO	YES	NO	YES

t statistics in parentheses

Errors clustered at Lender level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Columns 1 and 2 present estimates of β_1 in the following model:

$$.99XEnd_i = \alpha + \beta_1 * FinTech_l + \epsilon_i$$

Columns 3 and 4 present estimates of β_1 and β_2 in the following model:

$$.99XEnd_i = \alpha + \beta_1 * Nonbank_l + \beta_2 * Nonbank_l * FinTech_l + \epsilon_i$$

For variable definitions and an economic interpreation of these estimates, see Section 5 of the text.

Table 9: FinTech Borrowers Are More Biased

	(1) AllInCosts (pps)	(2) AllInCosts (pps)
.99XEnd (1/0)	0.0312**	0.0368**
	(2.96)	(2.96)
Pnt99XEnd * FinTech (1/0)	0.120**	0.0969**
	(2.91)	(3.06)
FinTech (1/0)	.0225	
	(0.92)	
# of Obs.	4404951	4396683
Mean of Y	4.122	4.121
RateBand X MSA X Yr FE	YES	YES
RateBand X Prgrm X Yr FE	YES	YES
Tract FE	YES	YES
DTI FE	YES	YES
Age FE	YES	YES
f(Inc)	YES	YES
f(LAmt)	YES	YES
CLTV	YES	YES
FinTech Interactions	NO	YES

t statistics in parentheses

Errors clustered at Lender level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table presents estimates of β_1, β_2, and β_3 in the following model:

$$AllInCost_i = \alpha + \beta * .99XEnd_i + \beta_2 * .99XEnd_i * Fintech_l + \beta_3 * FinTech_l$$
$$f(income_i) + f(LAmt_i) + CLTV_i + \delta_{RB,MSA,Yr} + \gamma_{RB,LP,Yr} + Tract + DTI + Age + \epsilon_i$$

Column 1 presents estimates without interacting the $FinTech_l$ indicator with the full suite of FEs and controls and Column 2 presents estimates with these interactions included in the model. For variable definitions and an economic interpretation of these results, see Section 5 of the text.

Table 10: Bias of FinTech Borr Driven by App Speed (Purchase vs. Refi)

	(1-Purchase) AllInCosts	(2-Refi) AllInCosts
.99XEnd (1/0)	0.0226*	0.0410**
	(2.11)	(3.28)
Pnt99XEnd * FinTech (1/0)	0.0758	0.126***
	(1.55)	(3.61)
FinTech (1/0)	0.0128	0.0343
	(0.58)	(1.10)
# of Obs.	2342584	2055551
Mean of Y	4.379	3.827
RateBand X MSA X Yr FE	YES	YES
RateBand X Prgrm X Yr FE	YES	YES
Tract FE	YES	YES
DTI FE	YES	YES
Age FE	YES	YES
f(Inc)	YES	YES
f(LAmt)	YES	YES
CLTV	YES	YES

t statistics in parentheses
Errors clustered at Lender level
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table presents split-sample estimates of β_1, β_2, and β_3 in the following model:

$$AllInCost_i = \alpha + \beta * .99EndRate_i + \beta_2 * .99EndRate_i * Fintech_l + \beta_3 * Fintech_l + f(income_i) + f(LAmt_i) + CLTV_i + \delta + \gamma + Tract + DTI + Age + \epsilon_i$$

Column 1 presents estimates for purchase loans and Column 2 presents estimates for refinance loans. For variable definitions and an economic interpretation of these results, see Section 5 of the text.

Table 11: Bias of FinTech Borrower Driven by App Speed (Jumbo vs. Conforming)

	(1-Jumbo) AllInCosts (pps)	(2-Conforming) AllInCosts (pps)
.99XEnd (1/0)	0.0113	0.0316**
	(1.33)	(2.94)
Pnt99XEnd * FinTech (1/0)	0.0926***	0.119**
	(5.65)	(2.83)
FinTech (1/0)	-0.00431	0.0242
	(-0.36)	(0.93)
# of Obs.	118864	4277342
Mean of Y	3.648	4.135
RateBand X MSA X Yr FE	YES	YES
RateBand X Prgrm X Yr FE	YES	YES
Tract FE	YES	YES
DTI FE	YES	YES
Age FE	YES	YES
f(Inc)	YES	YES
f(LAmt)	YES	YES
CLTV	YES	YES

t statistics in parentheses

Errors clustered at Lender level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table presents split-sample estimates of β_1, β_2, and β_3 in the following model:

$$AllInCost_i = \alpha + \beta * .99EndRate_i + \beta_2 * .99EndRate_i * Fintech_l + \beta_3 * Fintech_l + f(income_i) + f(LAmt_i) + CLTV_i + \delta + \gamma + Tract + DTI + Age + \epsilon_i$$

Column 1 presents estimates for jumbo loans and Column 2 presents estimates for conforming loans. For variable definitions and an economic interpretation of these results, see Section 5 of the text.