Does Fintech Lending Amplify the Transmission of Monetary Policy?

Isaac N. Green[*†]

January 4, 2023

Abstract

I find that fintech (online-based non-bank) lenders in residential mortgage markets amplify the transmission of monetary policy. In a difference-in-differences setting, I show that when mortgage rates fall, mortgage refinance activity is stronger in counties with higher fintech lender presence (as measured by either the number of active fintech lenders or their local market shares). Local retail expenditures and small business investment also increase in high-fintech jurisdictions. To address endogeneity concerns, I exploit the strictness of state-level regulations for certifying new non-bank mortgage lenders, which I show affects the rate of fintech entry into various states. Using these state-level differences, I compare adjacent counties on opposite sides of state borders with differential numbers of licensed fintech lenders, and show that there is a discontinuous positive jump in refinancing activity in states with greater numbers of fintech firms. Finally, I find evidence that fintech monetary transmission is related to its ability to overcome credit frictions in underserved areas: when rates fall, the refinance and consumption growth effects of fintech lending are strongest in counties with large racial/ethnic minority communities, low population density, and few physical bank branches.

[*]Researcher(s)' own analyses calculated (or derived) based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business.

[†]The conclusions drawn from the NielsenIQ data are those of the researcher(s) and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

1 Introduction

The residential mortgage market in the United States has changed substantially following the 2008 financial crisis. Traditional depository institutions, particularly the largest banks, have seen their share of new mortgage originations steadily shrink, as newer online-based lenders have entered the market and disrupted the traditional brick-and-mortar business model. These online-based non-bank lenders, which I call "fintech lenders" in this paper, use technology to substitute for the role traditionally played by human loan officers. These lenders have developed algorithms to score potential borrowers, generate customized interest rate quotes, and automatically search public records for relevant property information, among other advances.[1] This technology has been shown to improve the efficiency of mortgage markets in various ways. Fintech lenders are able to process mortgage applications and originate loans faster than other types of firms (see Fuster, Plosser, Schnabl, and Vickery, 2019) and may use "big-data" to better screen mortgage applicants (Buchak, Matvos, Piskorski, and Seru, 2018), potentially reducing cognitive biases that afflict human loan officers (Bartlett, Morse, Stanton, and Wallace, 2019).

While the existing literature mostly focuses on the microeconomic effects of fintech lending, the residential mortgage market has been shown to have important connections to the macroeconomy, particularly in its role in transmitting the effects of interest rate shocks to households (Bernanke and Gertler, 1995; Beraja, Fuster, Hurst, and Vavra, 2019; Di Maggio, Kermani, and Palmer, 2020; Greenwald, 2018). Given this link, it is likely that the rise of fintech mortgage lending has an effect on the transmission of monetary policy.

In this paper, I investigate this possibility. My central hypothesis is that fintech lenders amplify the effects of expansionary monetary policy by alleviating market frictions that impede mortgage refinancing when interest rates on new mortgages fall. By allowing a larger number of borrowers to take advantage of beneficial refinancing opportunities, fintech lenders may induce stronger consumer spending in the wake of interest rate declines, amplifying the effects of Fed policy changes.

I test this hypothesis using annual mortgage lending data from the Home Mortgage Disclosure Act (HMDA) database, in addition to monthly data from Fannie Mae, covering the post-crisis period (2010-2019). This time period coincides with the inception of a large number of new fintech lenders, as well as the rapid expansion of the few fintech lenders that existed in prior years. The HMDA dataset captures nearly the entire universe of residential mortgage loans extended during this period across all lenders (both fintech and non-fintech). I collapse these data into a county-year panel in order to exploit geographic variation across various types of mortgage lending.

I first establish that when mortgage rates fall, fintech lenders are associated with stronger refinancing activity. Specifically, I estimate an interaction regression, in which I regress annual county-level growth in mortgage refinance loans on a lagged measure of local fintech concentration, and the interaction of this variable with the spread between average coupon rates on outstanding fixed-rate mortgages and current 10-year Treasury yields. This

[1] See NerdWallet, "What is an online mortgage?" https://www.nerdwallet.com/best/mortgages/online-mortgage-lenders, accessed 10/12/2021.

mortgage spread captures the difference between prevailing market interest rates and rates paid by borrowers with outstanding mortgages, and thus gives a measure of the incentive to refinance. I consider two measures for assessing the extent of local fintech market penetration: a lagged count of the total number of fintech lenders originating loans in a given county and year, and the lagged share of fintech-originated refinance loans as a fraction of the county's total refinancing volume.

Consistent with my hypothesis, I find that when mortgage rates fall, refinance activity is stronger in counties with greater exposure to fintech lenders. Baseline regression results suggest that for every percentage point fall in mortgage rates, each additional fintech lender active in a county in year t-1 is associated with 1.3% stronger refinance growth in year t. Similarly, a 1% increase in a county's fintech market share in year t-1 is associated with an additional .2% of year-t refinance growth, for each percentage point fall in mortgage rates.

Counties with high and low levels of fintech market penetration may also differ from one another along other, unobserved dimensions, that relate to mortgage lending. One particular concern is that fintech lenders are attracted to counties that are poised for strong future refinance loan demand. To address endogeneity concerns, I adopt an identification approach that makes use of the geographic expansion of fintech lenders over time. Fintech lending is a relatively recent phenomenon, and at the beginning of my sample period, in 2010, most fintech lenders were quite small. Despite possessing online lending technology that would potentially allow them to originate mortgages across the entire country at a low cost, in the nascent stages of their development, most fintech lenders nonetheless originated mortgages in only a small handful of states, before gradually expanding. I conjecture that the staggered timing with which lenders tend to enter state mortgage markets is affected by the regulatory protocol governing non-bank mortgage lenders. Given that there is no equivalent of a national bank charter for non-depository institutions, fintech firms must become licensed by state-level regulators in each state in which they want to originate loans. In a logistic regression setting, using hand-collected data on state-level licensing requirements, I show that states with the most restrictive licensing requirements see slower fintech entry. Specifically, high licensing application costs, net-worth requirements, laws requiring the establishment of physical (i.e. brick-and-mortar) branch locations, and the number of other qualitative application requirements, all decrease the probability that a fintech lender will enter a particular state before a given year in the sample.

I then make use of this exogenous source of variation in fintech entry across states, and compare adjacent counties located across state borders from one another in neighboring states with different numbers of licensed fintech lenders. For each year of the sample, I identify all pairs of bordering states which differ in the number of fintech lenders they have licensed, labeling states with a greater number of fintech lenders than their neighbor as "treated" states. I then form my sample by retaining only counties located close (within 50 or 100 miles) to the shared border with their paired state. By doing so, I generate a sample of counties with similar demographics and housing markets, but with differential access to fintech lending. Using a regression discontinuity framework, I find that refinance loan growth is between 1%-3.3% stronger in "treated" counties, located in states with more fintech lenders than their neighbor. These differences are far larger during years in which interest rates fall and the refinance incentive is high. A 1% widening of mortgage spreads amplifies the treatment effect by 1.5 to 6.4 percentage points, depending on the specification.

The goal of expansionary Fed policy is to stimulate the economy by inducing stronger consumer spending, business investment, or other economic activity. Thus, for fintech lending to amplify the effects of monetary policy, stronger refinancing activity must transmit to other outcomes. To study local consumer spending in the wake of expansionary monetary policy, I make use of store-level retail sales data from Nielsen. I aggregate this data to the county level, employing several filters to ensure the comparability of observations across counties, and the consistency of this measure of spending across time. Returning to my baseline interaction regression specification, I use my new measure of county-level retail spending to assess whether consumer spending growth is stronger in high-fintech jurisdictions amid falling interest rates. I find evidence consistent with this hypothesis: the addition of a single fintech lender to a county's mortgage market is predicted to raise retail consumption growth by .2%, an effect which doubles in magnitude after a 1% widening of mortgage interest rate spreads. Similar results are observable in other outcome variables related to local consumer demand shocks.

Prior research on fintech mortgage lending has proposed different mechanisms through which fintech lenders may reduce market frictions. If fintech lenders expand refinancing only because they are able to process more applications in a short time than other lenders, as described by Fuster et al. (2019), then there is no reason why the impact of fintech presence should vary substantially across regions. On the other hand, if fintech lenders are better at screening particular types of borrowers, or if online lending technology is more useful in places with less access to the brick-and-mortar banking system (such as remote or sparsely populated areas), then fintech lending may be more potent as a facilitator of monetary policy in particular areas of the country.

Continuing with my baseline interaction regression approach, I examine whether fintech-driven monetary transmission (in the form of fintech effects on refinancing and consumption growth) is stronger in regions where borrowers have limited access to traditional finance, either due to a history of lending discrimination or lack of physical access to brick-and-mortar bank branches. To do so, I interact my measures of local fintech presence with quartile indicator variables that capture where a county falls within the distribution of counties, as sorted by racial or ethnic composition, population density, or bank branch prevalence.

I find that conditional on a 1% widening of mortgage interest rate spreads, a unit increase in the number of fintech lenders active in a county at time t-1 predicts 1.9% stronger refinance loan growth in counties with the smallest share of White residents (i.e. in the bottom 25% of counties, sorted by White population shares), but only .6% stronger refinance growth in counties with the largest White population shares, a difference which is statistically significant at the 5% level. I find similar results when sorting counties by their share of Hispanic or Latino residents. Using similar tests, I show that high fintech presence is more strongly associated with refinance growth in regions with few bank branches, and low population densities. Moreover, the same correlations broadly hold when using consumer retail spending growth as the left-hand side variable, suggesting that there is pass-through from household balance sheets to other local outcomes.

This paper contributes to a growing body of literature on the interplay of housing and monetary policy (e.g. Chen, Michaux, and Roussanov, 2020; Drechsler, Savov, and Schnabl, 2019; Taylor, 2007), particularly those which relate to the so-called refinance channel of monetary policy, through which a decrease in interest rates generates increased refinancing and stronger consumer spending among those who refinance (see, e.g. Beraja et al., 2019; Di Maggio, Kermani, and Palmer, 2020; Eichenbaum, Rebelo, and Wong, 2018; Scharfstein and Sunderam, 2018). Relative to these papers, my study is unique in showing how the rise of a new class of intermediaries with different lending technology can influence the strength of this channel.

This paper is also related to a number of recent studies that have focused on the rapid emergence of fintech lenders over the past several years (see, e.g. Balyuk, Berger, and Hackney 2020; Chernenko, Erel, and Prilmeier 2019; Gopal and Schnabl 2020; Philippon 2016; Stulz 2019). The study which relates most closely to this paper is Fuster et al. (2019). Given their findings on the impact of fintech lenders in alleviating microeconomic frictions in lending markets, the authors speculate that these technological advances may enhance the effectiveness of Fed policy, and estimate a regression similar to my baseline analysis, showing a correlation between fintech lending and refinance credit growth. Relative to that study, my analysis focuses more closely on how the effect of fintech lending on credit growth varies alongside changes in interest rates, and across different time horizons. It is also novel in using state-level regulatory frictions to identify a causal channel in addition to presenting correlations. I also add new evidence suggesting that amid falling rates, consumption growth is stronger in the aftermath of fintech-induced refinancing, a result which is important in establishing the amplifying effects of fintech lenders on monetary transmission. Finally, my paper is novel in suggesting that in addition to broadly amplifying the transmission of monetary policy, fintech lending also expands the geographic reach of Fed policy through its unique ability to penetrate areas that elude the traditional banking system.

The rest of this paper is organized as follows. Section 2 introduces the data sources and key variables and discusses how fintech lending varies over regions and how it co-varies with county-level demographics. Section 3 presents the results of my baseline analysis on fintech presence and refinance credit growth. Section 4 delves further into identification of this effect and presents the cross-border analysis on fintech credit expansion. Section 5 examines the link between fintech lending and consumer spending growth. Section 6 shows how the strength of fintech-driven monetary policy transmission varies across regions according to county-level demographics and regional traits. Section 7 concludes.

2 Data

2.1 HMDA and other Data Sources

My primary source of information on mortgage lending activity is the Home Mortgage Disclosure Act (HMDA) database. Under HMDA, lenders meeting certain requirements must enter information annually on all residential mortgage loans that they originate. Residential loans include home loans for purchase, refinancing (including cash out refinancing), and home improvement. HMDA reporting requirements cover virtually all lenders, including non-bank mortgage originators, so the lending information in HMDA covers nearly the full universe of mortgage lending activity in the United States.[2]

HMDA data are reported at the loan-level. For each loan, identifying information is included for the lender, and demographic information on the borrower (e.g. race and annual income) is also generally available. Importantly, the location of the underlying property is also provided at the county level. Information on the purpose of each transaction, that is, whether a loan is for a home purchase, refinance, or for home-improvement, is also available, as is information on the presence or absence of underlying government guarantees (e.g. whether a loan is FHA or VA guaranteed). The majority of my analyses will make use of HMDA data, aggregated at the county level. Since HMDA is reported at the loan level, however, I can identify loan volumes by lender type, and thus calculate the volume of home refinance transactions originated by fintech firms (as well as other lenders) by local market. The county-level HMDA panel that I construct covers the years 2010-2019.[3]

I merge the county-level aggregated HMDA data with county-level data from a number of other sources. I obtain several useful variables from the United States Census Bureau. I obtain demographic information from the United States Census and American Community Survey (ACS), and obtain information on small business activity from the Census' County Business Patterns (CBP) database. I add data on county-level unemployment rates from the Bureau of Labor Statistics' (BLS) Local Area Unemployment data sets. I make use of Call Report data from the FDIC when matching banks and other lenders to the HMDA data. I also acquire information from the FDIC's Summary of Deposits (SOD) database on the locations of bank branches. I use the SOD data to determine the number of bank branches located in each US county.

The primary advantage of the HMDA data is its completeness. Since HMDA-reported transactions represent a substantial majority of mortgage transactions, statistics drawn from the HMDA data are very likely to be reflective of the overall residential mortgage market. However, for some parts of the analysis in this paper (i.e. analyzing the speed of refinancing by fintech firms) it will be desirable to get a picture of mortgage market activity at a higher frequency. As such, in addition to the merged county-level HMDA panel, I also conduct a set of tests on

[2]Exemptions to reporting requirements apply to small banks and other lenders with total assets below an annually announced threshold. Nonetheless, HMDA requirements apply to the majority of institutions and the vast majority of total loan volume, and can thus be see as the near-universe of mortgage loans in the United States.

[3]The HMDA data covers all counties in the US and Puerto Rico. I drop counties in Puerto Rico, and any counties with multiple years without refinance lending transactions.

data from the Fannie Mae Single Family Loan Performance dataset. The Fannie Mae data consists of information on roughly 35 million loans sold to and securitized by Fannie Mae between 2000 and 2020. All of the loans within this dataset are fully amortizing, full-documentation loans. I focus on loans with initial maturities between 15 and 30 years with fixed rates.[4] The Fannie Mae data provide information on loan performance at a monthly frequency, including information on the size of each monthly pay-down or pre-payment, the loan-age and time-to-maturity, and whether a loan is delinquent or in forbearance. Information is displayed for each loan in the portfolio until it fully amortizes, prepays, defaults, or, in rare cases, is removed from the Fannie Mae portfolio for other reasons.[5] The Fannie Mae data also contains static information, such as the interest rate at origination, borrower credit scores and LTV ratios, and whether the purchaser of the home underlying the mortgage is a first-time home buyer. Information on the location of each underlying property is given at the 3-digit ZIP code level. Since credit scores are an important determinant of mortgage market activity, I extract average annual credit scores from each ZIP code, which I merge with the county-level HMDA data. Using data from the department of Housing and Urban Development (HUD) "crosswalk" files, I translate the ZIP code data to the county level.[6]

In addition to the sources of data mentioned above, obtain information on weekly long-term bond yields from the United States Treasury, which I utilize when constructing monthly and annual mortgage spreads. Finally, I construct a measure of county-level consumer retail-spending using Retail Scanner Data provided by Nielsen. Additional details on these sources of data will be discussed in the ensuing sections, and in the appendix.

2.2 Fintech Lending

In Table 1, Panel A, I present information on the home refinance lending activity of the fintech firms in my sample. To identify fintech lenders in my sample, I begin by combining the sets of fintech lenders identified in Buchak et al. 2018 and Fuster et al. 2019. I make a few additions to this set of lenders, including SoFi Lending and Zillow Home Loans, both of which are associated with well known technology firms, and thus have the potential to incorporate big-data methods into their screening processes. I also add LenderFi, a more recent market entrant which uses technology extensively in the origination process. Fintech lending increases over time, both in dollar volume and as a percentage of total refinancing loan volume. Fintech loans make up roughly 4% of refinance lending activity in 2010, and gradually rise to roughly 15% of the market. In 2010, there were 12 fintech firms making loans, some of which were very small. This number rises to 22 by 2018. The largest fintech lender in the sample, by some

[4] Since the dataset consists only of loans securitized by Fannie Mae, it does not contain information on so-called non-qualifying (formerly "sub-prime") mortgages originated to borrowers with low credit scores or high loan-to-value (LTV) ratios, nor does it contain information on so-called "jumbo" mortgages which have loan amounts above a pre-set conforming limit. It also excludes most FHA- and VA-guaranteed mortgages, which are primarily the province of Ginnie Mae.

[5] So-called "put-backs" occur when Fannie Mae removes a loan from an MBS it has issued and requires its originator to repurchase it. Such an event most frequently occurs when Fannie Mae finds that some of the information on the underlying mortgage documentation is fraudulent or misrepresented.

[6] Neither ZIP codes nor counties are subsets of one another (3-digit ZIP codes are somewhat larger than counties, on average). The mapping process uses information from HUD that details the percentage of a ZIP Code's addresses that lie within a particular county. Mapping ZIP codes to counties is thus not an exact procedure.

margin, is Quicken Loans, which comprises roughly 60% of all fintech lending, by volume, in 2010, though this share drops gradually as the sample progresses. Panel B in displays information on fintech lending at the county level. At the beginning of the sample, in 2010, the median county has only two active fintech lenders with a market share of 4.3%, and the 90th percentile county has six fintech lenders and a fintech market share of 9.5%. By 2019, the median and 90th percentile counties have seven and sixteen fintech lenders, respectively, while the equivalent figures for fintech market share are 16.6% and 29.4%.

In order to assess whether regional markets with more fintech lending see stronger refinance credit growth, I also need to define a measure of regional fintech presence. Establishing a meaningful geographic footprint of lenders with little physical presence is a difficult undertaking fraught with potential endogeneity issues. When observing where fintech firms make loans, it is unclear whether these firms lend in a particular region because of conscious decisions made by the firms themselves (e.g. to become licensed in a particular state, to target online advertisements toward, or give favorable interest rate terms to, potential borrowers in a particular region, etc.) or if they do so because of high borrower demand for fintech services in an area. I address these difficulties in two ways. First, I will consider multiple alternate measures for assessing the regional presence of fintech firms. I will discuss the benefits and drawbacks of each measure, and I will display baseline results with each of these across a number of specifications. Secondly, in Section 4, I will utilize an identification procedure that looks at differences in the number of licensed fintech lenders across states, utilizing potentially exogenous differences in regional fintech presence.

The first measure of regional fintech presence that I will utilize is a simple count of the number of fintech firms that originate a refinance loan within a given county and year. In regression specifications, I will refer to this variable as *FintechCount*. The most obvious drawback of the *FintechCount* variable in assessing an areas fintech exposure is that could grow larger by virtue of one or two fintech lenders originating a single loan in a county. It would be hard to argue that such activity would represent a meaningful increase in the extent to which fintech lenders pervade that county. On the other hand, if *FintechCount* is, on average, a meaningful proxy for the number of fintech lenders that are *willing* to originate loans in an area, then the count of fintech firms would be a useful measure of the effect that *access* to fintech lenders has on a local market.[7] While the count of active fintech originators may be a noisy measure of fintech activity, given the low variability of the measure, and the large number of US counties, there is hope that the large number of observations would uncover the average effects of fintech lender access on a regional market.

The second measure that I use to define fintech concentration is the regional market share of fintech lenders within the refinance segment of mortgage originations. That is, I divide the volume of refinance loans originated by fintech lenders in a given county and year, by the total volume of refinance loans originated in that county

[7] This variable may do a good job of capturing the extent to which there is a competitive environment local market among fintech lenders. Even if fintech firms lower the costs associated with refinancing, they may not pass along the cost savings to consumers in the form of lower rates unless there is some competitive reason for them to do so. See Taylor (2007) for a discussion of these issues in an environment that does not explicitly analyze fintech behavior.

and year. I refer to this variable as *FintechShare.* While *FintechShare* benefits from the fact that it is a more quantitatively rich measure of fintech activity than the count variable, using this variable (lagged by a year) to predict refinance credit growth faces its own issues. First, the denominator of *FintechShare* depends on the activity of all of the other intermediaries in a county. As such, fintech market shares can become large both if fintech firms expand their origination activity, or else if other intermediaries cut back. Buchak et al. 2018 argue that non-bank lenders (of which fintech firms are a subset) increased their market shares in areas with weak intermediaries that had to raise capital in the post-2010 period and subsequently cut back lending.[8] Thus, high fintech market shares could signal that the aggregate credit supply is contracting due to the retrenchment of other intermediaries.

Another issue with the *FintechShare* variable is that it may, in part, proxy for the effect it is trying to measure. In other words, since one hypothesis regarding fintech lenders is that they may be able to more easily reach borrowers that are less easily screened by banks, a high fintech market share in year t-1 may signal that fintech firms have already targeted and refinanced those borrowers, and that there are thus fewer borrowers available to be refinanced in subsequent years.

In Figure 1, I display the regional patterns associated with both of these measures of fintech lending. The two maps shown in the diagram display the average number of active fintech lenders, and the average market share of fintech refinance lending, at the county level, across the full sample from 2010-2019. As measured by the number of fintech firms active within a county, fintech lenders are most prominent in large urban and suburban areas with high populations. This tendency comes as little surprise, since areas with the largest housing markets might reasonably also be expected to have the largest volume of fintech loans. Since I will always control for county-level populations in my regression analyses, it will be instructive to look at where *FintechCount* is high relative to a county's population, as I will illustrate shortly. Regional fintech presence is somewhat different when looking at market shares. The counties where fintech lenders represent the largest fraction of the total refinancing market tend to be counties with low population densities, often in the western United States. States with large number of high fintech-share counties include Nevada, New Mexico, and Alaska, each of which cover large land areas with dispersed populations.[9]

In addition to these two these main measures of fintech concentration, for robustness, when conducting analyses using monthly Fannie Mae data, I will also look to estimate the effects of recent growth in the number of fintech refinance loans. This measure of fintech activity will not be subject to the effects of the idiosyncratic behavior of other intermediaries (as market shares would be) and since it is expressed as a growth rate, it will not be strongly correlated with county sizes. I will discuss this measure in further detail in the sections that follow.

[8] Capital constraints for these firms were likely binding due to financial crisis-era losses and new regulatory capital requirements, which began to be implemented after the 2010 Dodd-Frank Act was passed.

[9] Each of these states also feature physical barriers (i.e. deserts in the case of Nevada and New Mexico, and tundra in Alaska) which might make them difficult to access and contribute to their being poorly connected to the traditional financial system.

2.3 What Explains Regional Fintech Concentration?

Fintech lending is likely to be explained by a number of supply- and demand-related factors, which also correlate with regional housing market activity. The purpose of this section is to better understand the factors that correlate with regional fintech activity and to introduce the reader to some of the observable factors I will need to control for when examining the relationship between fintech activity and credit growth.

To gauge the extent to which various county-level observables are able to explain regional variation in fintech lending, I begin by regressing my measures of regional fintech concentration on a number of county observables. Summary statistics on this county-level merged HMDA panel are shown in Table 2. In addition, simple pairwise correlations between groups of variables are displayed in Appendix Table 1. I estimate regressions of the following form:

$$\begin{aligned} Fintech_i &= \alpha + \beta X_i + \varepsilon_i, \\ & where\ Fintech\ \epsilon\ \{FintechCount, FintechShare\} \end{aligned} \tag{1}$$

where the subscript i indexes counties, *Fintech* measures the average level of fintech presence in a county across all sample years, and X is a vector of county-level observables, which are also averaged across sample years.

Table 3 displays the results of estimating (1) on various sets of county observables. Panel A displays results where the dependent variable is *FintechShare*, while Panel B displays results for the *FintechCount* specifications. I have five sets of county-level observables, categorized with different labels in the leftmost column of the table. The variables labeled as "HMDA Mortgage Variables" consist of a county's share of FHA guaranteed mortgages and so-called "jumbo" mortgages. FHA mortgages are a riskier segment of the market given to lower income borrowers. Existing evidence suggests that non-bank lenders target this loan segment, as they may have a regulatory advantage in originating these mortgages.[10] Jumbo mortgages consist of loans originated for amounts above the maximum amount for loans eligible for sale to the government sponsored mortgage agencies (GSEs).[11] Since non-bank lenders sell the vast majority of the mortgages they originate to one of the GSEs, areas where there is high demand for jumbo mortgages may deter fintech entry.

The HMDA mortgage variables, together with the list of "Demographic Variables" displayed in Table 3 comprise the set of baseline county-level control variables, which I will use in credit growth regressions in the following section.[12] The demographic variables include county populations, average incomes, unemployment rates, population density, and the employment to population ratio. These two sets of variables are available for a broad set of US counties, at an annual frequency. In addition to these variables, I have "ACS Mortgage Variables" and "ACS

[10] See Agarwal et al. (2020).

[11] The so-called GSEs: Fannie Mae, Freddie Mac, and Ginnie Mae.

[12] I will add average credit scores, calculated via the ZIP code level Fannie Mae data to complete the set of baseline right-hand side variables.

Demographic Variables," which extend my set of observable characteristics. These variables are only available for larger counties with populations over 60,000. I refer to these variables, combined with the baseline controls, as the "full" set of right-hand side control variables, and I estimate separate regressions that include this full set of controls and the smaller set of counties.[13] The final set of county-level observables is labeled "Bank Branch Presence," and includes the number of bank branches per-capita and per square mile.

The results shown in Table 3 suggest that while the county-level maps in Figure 1 highlight differences between the *FintechCount* and *FintechShare* variables in measuring local fintech concentrations, the two measures correlate similarly with county-level observables, particularly once accounting for the strong co-variation between the count of fintech firms and a county's population. In the first column of each panel, I show how fintech lending activity varies with characteristics of a county's local mortgage market. Both measures have a strong positive association with the average share of FHA-guaranteed loans among refinance loans in a county, and both measures suggest that fintech lenders have a stronger presence in counties with greater homeownership.

In the second column of each panel, I look at fintech presence relative to the set of baseline controls. The results suggest that fintech firms originate loans in counties with wealthier borrowers and lower unemployment rates, but also with a smaller share of employed people relative to the total population. Fintech firms also originate loans in counties with lower population densities. In column (3) of each panel, I add demographic and mortgage market-related variables from the American Community Survery (ACS). The ACS data suggest that fintech presence is stronger in areas where a larger share of the population is 65 years old or above. On the county level then, there is little evidence that fintech firms target younger, technologically savvy borrowers. Instead, this finding suggests that fintech lending may be more appealing to those who have some experience taking out a mortgage, and thus may not need the extra hand-holding provided by interactions with human mortgage lenders.[14]

The ACS data also allow an assessment of the racial and ethnic composition of the counties in which fintech lenders originate mortgages. I include a county's White and Black population shares in the column (3) regressions.[15] Both measures of fintech presence suggest that a decrease in a county's white population, in favor an equal percentage increase in that county's Black population predict an increase in the level of fintech activity.[16] Additionally, both the number of active fintech firms and fintech market shares are strongly positively associated with a county's Hispanic or Latino share of the population. Taken as a whole, then, fintech firms tend to be more active in counties with larger concentrations of racial or ethnic minorities.[17]

[13] Limiting the sample to counties with populations above 60,000 excludes roughly the bottom two-thirds of counties, by population.

[14] More experienced borrowers may also be more likely to refinance when it is optimal for them to do so, as suggested by Browne et al. (1996), suggesting borrower experience could be a confounding factor in regressions of refinance growth on fintech activity.

[15] This leaves members of the American Indian and Asian/Pacific Island communities, as well as those who identify as "Some Other Race" as the residual population.

[16] The signs of coefficients on the Black and White population share variables differ according to which measure of fintech presence is used, however the magnitudes yield similar intuition. The signs of the White and Black coefficients in column (3) in panel A suggests that fintech market shares are higher in counties with higher concentrations of those identifying as some race other than Black or White, while panel B suggests that the number of active fintech lenders is smaller in such places. However, both sets of regressions show smaller (more negative) coefficients for White population shares than for Black population shares.

[17] The census' treatment of race and ethnicity makes the analysis rather more confusing. On the US Census, respondents do not have an option of identifying as Hispanic or Latino when selecting a race, despite the fact that many Americans who identify as Hispanic

Finally, in column (4), I estimate how fintech presence co-varies with the presence of bank branch locations. Both measures of fintech presence suggest that fintech lenders are more active in areas with fewer branches per-capita. The evidence with respect to the number of bank branches per square-mile is mixed, after controlling for population density.[18]

3 Interest Rate Declines and Fintech Credit Expansion: A Difference-in-Differences Approach

In this section I begin to test the aggregate effects of fintech lending. I ask whether fintech firms amplify monetary policy by increasing the availability of mortgage refinance credit when interest rates decline. If fintech lenders are able to more efficiently process mortgage applications and assess credit-worthiness, then the presence of fintech lenders in a local housing market could have an expansionary effect on the aggregate supply of credit. If, as suggested by Fuster et al. 2019, automated lending technology is more valuable when demand for credit is strong, then the activity of fintech lenders should have the strongest effects on aggregate credit provision in declining interest rate environments, when a larger proportion of borrowers have an incentive to refinance.

3.1 Baseline Specifications and Results

I first look to assess the correlation between fintech lending activity and the supply of home refinancing credit by exploiting regional variation in fintech lending activity. I ask whether the growth of mortgage refinance credit is stronger in local markets that have a more concentrated fintech presence, particularly when rates fall and the aggregate refinance incentive is strongest.

To assess the relationship between regional credit growth and fintech lending, I make use of the HMDA data, aggregated at the county level, to estimate the following regressions, where I use the subscripts i and t to index counties and years, respectively:

$$\Delta_1 Refivol_{i,t} = \alpha_t + \beta \cdot Fintech_{i,t-1} + \gamma \cdot Fintech_{i,t-1} \cdot \Delta_{avg} Rates_t + \delta \cdot Controls_{i,t-1} + \varepsilon_{i,t} \tag{2}$$

where I adopt the convention that for a variable, y,

$$\Delta_k y_{i,t} = y_{i,t} - y_{i,t-k} \ and, \ \Delta_{avg} y_{i,t} = y_{i,t} - \bar{y}_i$$

where the last y term in the final expression denotes the county average, across all sample years.

or Latino would identify this as a racial categorization as well as their ethnicity. Hispanic/Latino origin is instead treated separately. Thus, under racial identity, many Hispanic/Latino Americans identify either as White, or as "Some other race."

[18] Bank branches per square-mile and population density are strongly collinear with a correlation coefficient of .913 (see Appendix Table 1).

The variable denoted as *Refivol* is defined as the natural logarithm of the aggregate county-level volume of refinance loans originated in year t. Thus, the outcome variable is the log-change in total refinance credit in county i from year t-1 to year t. I will use *Refivol* as of the year 2010 to weight observations in these regressions. The intuition is that refinance growth should be weighted more strongly in counties with larger housing markets. I choose the year 2010 because fintech still comprised a minimal portion of the overall market at this point, so choosing this year minimizes the potential effects of fintech lending on the weighting variable.[19]

I use the growth rate, rather than the level of refinancing primarily to account for the fact that different counties have very differently-sized housing markets. Without such a normalization, specifications without county fixed-effects would be highly suspect, since it would be a difficult task to control for time-invariant cross-county differences in housing market activity with only county observables. Moreover, using growth rates allows county fixed-effects, when added to a specification, to control for general time trends in the growth-rates of refinancing within a county, rather than simply the level of refinancing. Use of fixed effects in such a manner will help to alleviate concerns that fintech lenders simply enter counties where refinancing activity exhibits persistently strong growth during this period.

The intercept term, α in (2) has a time subscript, t, to denote the presence of year fixed-effects in all specifications. Since county-level refinancing growth rates are strongly correlated over time due to the aggregate interest rate environment and other macroeconomic fundamentals, it will be important to include year fixed-effects to isolate the cross-sectional differences in which I am primarily interested.[20]

The variable labeled *Fintech* denotes a county-level measure of fintech market presence, defined either as the *FintechCount* or *FintechShare* variables described earlier. These enter in lagged form in (2). The next key variable, labeled *Rates* is a measure of the aggregate refinance incentive at time t. This variable, though labeled *Rates* for intuition, is really an interest rate spread. It is defined as the difference between the average coupon interest rate (i.e. the rate at origination) of outstanding fixed-rate mortgage loans minus the yield to maturity on 10-year US Treasury debt. This variable captures the difference between the average rate paid by those with a currently outstanding mortgage, and the level of market interest rates, and thus represents the potential cost savings associated with refinancing.

The coefficients on the two fintech variables have the following interpretation. The β coefficient reveals whether, in an average interest rate environment, the growth of refinancing credit is stronger in counties with a strong fintech presence. A positive coefficient on this variable would indicate that refinance growth is greater in high-fintech counties. The γ coefficient then describes how much this estimated fintech effect increases or decreases conditional on a 1% increase in the aggregate refinance incentive.[21] Positive loadings on both of these coefficients

[19] Choosing population rather than housing market size produces similar results.

[20] Time fixed-effects will also be important in dealing with the general upward trend in fintech activity over time. Removing the covariation between fintech activity and year fixed effects removes the potentially mechanical relationship that could arise between refinancing and fintech activity if, for example, refinancing growth happens to be strongest in the later years of the sample when fintech shares are also high.

[21] The year-over-year standard deviation of this spread is roughly .8%.

would suggest that aggregate county-level results mirror the microeconomic stylized facts which suggest that technological advances in mortgage lending are more valuable in strong housing demand environments. The *Rates* variable primarily moves around when market interest rates (i.e. Treasury yields) change. Thus, a widening spread should be thought of, intuitively, as indicating a falling interest rate environment.

In addition to the variables related to fintech presence, the above specification also controls for a number of county-level observables. I will display results from regressions that include both the so-called "baseline" controls and the "full" set of controls, as defined in Section 2. In addition to the true county-level controls mentioned, I also add to a county's mean borrower credit score, which I derive from Fannie Mae data. I will present results of specifications both with and without county-level fixed-effects.

The regression framework outlined in equation (2) can be thought of as analogous to a difference-in-differences approach. In this interpretation, the β coefficient can be thought of as giving the differences in refinancing activity between high- and low-fintech regions in normal times, while the γ coefficient details how this refinancing behavior changes in response to interest rate shocks. While it is likely that fintech firms enter county-level housing markets endogenously, it may be plausible that the deliberations that give rise to fintech market-entry in a county at time t-1 do not anticipate subsequent changes to the aggregate interest rate environment at time t. The identification assumption underlying the difference-in-differences interpretation would be that, in the absence of fintech activity, and controlling for county-level observables, high- and low-fintech counties would have similar growth of refinance credit in response to a change in the aggregate interest rate environment.

Table 4 displays the results of estimating equation (2) on the merged panel of county-level HMDA data, using the two measures of fintech market presence. Panel A displays coefficients on the two *FintechCount* variables (i.e. the pure count variable and its interaction with *Rates*), while Panel B shows coefficients for the *FintechShare* variables. The specifications across the four columns vary in the sets of observables that are included as controls, as well as in their inclusion or exclusion of county fixed-effects. Specifications with the "Full" set of right-hand side observables include variables from the American Community Survey data, which restricts the sample only to large counties, subsequently reducing the number of observations in these specifications.

The coefficients on the *FintechCount* variable, displayed in Panel A, suggest that the marginal addition of a single fintech lender to a county's residential mortgage market at time t-1 corresponds to between .4%-.8% stronger refinancing growth in the subsequent year. This relationship is strengthened in environments where interest rates are declining and the incentive for borrowers to refinance is high. The interaction coefficients in Panel A suggest that conditional on a 1% increase in the aggregate refinance incentive at time t (i.e. a 1% rise in the standardized *Rates* variable from equation (2)), the estimated effect of an additional fintech lender on refinancing rises by 1.4%-3.0%, suggesting a total effect on refinance credit growth of between 1.9%-3.8%. These results suggest that the estimated impact of an additional fintech lender at time t-1 grows roughly 5-7 times larger conditional on a 1% widening of interest rate spreads at time t.

The *FintechShare* coefficients in Panel B mirror the results in panel A. Panel B suggests that a 1% increase in fintech market shares in the refinance segment of the market, in year t-1, predicts between .36%-1.09% stronger refinance credit growth at time t. The largest *FintechShare* coefficient is obtained in the specification that includes the full set of controls as well as county fixed-effects. The results across all specifications in Panel B suggest that the estimated effect of fintech firms gaining market share grows larger when the aggregate refinance incentive increases. The increase in the estimated fintech effect on refinance growth, conditional on a 1% widening of the aggregate refinance incentive, ranges from .23%-.60%. Given the *FintechShare* coefficients, the loadings on the *FintechShare*Rates* variable suggest that the effect of fintech lending on subsequent credit growth becomes at least 60% larger upon a 1% widening of interest rate spreads.

One potential issue with the *FintechCount* variable concerns its covariation with county population. It is apparent from Figure 1 that areas with larger populations, and housing markets, have a larger number of active fintech lenders. While I control for population in all specifications, if population has highly nonlinear effects on refinancing at the high -end of the distribution (for example, if the largest cities have sophisticated borrowers that are more attuned to interest rate movements), then this could have an impact on the results in specifications without county fixed-effects. While the inclusion of county fixed-effects solves this particular issue, the largest urban areas have very different housing markets than other counties, and it may thus be concerning if the association between fintech concentration and refinancing growth didn't hold in smaller counties. In Table 5 I display the results associated with the estimation of equation (2) across population-sorted sub-samples. Panel A suggests that the *FintechCount* coefficients are fairly consistent across the population distribution. The coefficient on the interaction term (i.e. *FintechCount*Rates*) remains stable from the second through fourth population quartiles, at a value of .016. Coefficients on the non-interacted fintech term are slightly weaker at the lower end of the population distribution, but not dramatically so.

In Appendix Table 2 I display results of two additional tests of the robustness of my results. First, I assess the possibility that fintech lenders enter counties while refinancing growth is already strong, because their business models prioritize the refinance sector of the market. In this case, the ability of the lagged count of fintech lenders to forecast refinancing growth might be due to the persistence of a temporary refinancing surge that began prior to or contemporaneously with fintech firm arrival. I attempt to rule out this possibility by re-estimating a version of equation (2) that controls for the lagged growth of refinancing. I display the results of these specifications in panel A of Appendix Table 2. The results across all specifications mirror the baseline results, with coefficient estimates that are quite similar in magnitude to those presented in the baseline analysis, in Table 4.

Next, I look at whether fintech presence differs from other that of intermediaries. It may be the case that entry into a local mortgage market by intermediaries of any type is broadly predictive of subsequent refinance activity. For example, an increase in the total number of intermediaries originating loans in a region may signify that the local market is becoming more competitive. These competitive effects may result in more borrowers being able to refinance. To consider this possibility, I estimate analogous regressions to those displayed in panel A of Table 4 (i.e. the *FintechCount* regressions) but instead of using *FintechCount* on the right-hand side, I include counts of other intermediaries. In panel B of Appendix Table 2 I show results of within-county estimates of equation (2) which include these additional intermediaries.[22] The three new intermediary-count variables, which I name *OtherNonbank*, *LargeBank*, and *SmallBank*, refer, respectively, to counts of non-bank non-fintech lenders, large bank lenders (with assets over $50 billion) and small bank lenders (with assets below $50 billion).

The results of this analysis suggest that fintech firms are indeed different than other types of intermediaries. In an average interest rate environment, the coefficients in Appendix Table 2, panel B, suggest that the heightened presence of these other types of intermediaries actually forecasts lower subsequent refinancing volume. For each of these types of intermediaries, a widening of interest rate spreads diminishes this negative association, however, in most cases, a 1% increase in the refinance incentive would not be sufficient (or would be barely sufficient) to make the aggregate association positive. For example, the coefficients on *OtherNonbank* of -.003 and -.002, combined with the interaction effect of .001, suggest that even in the event that interest rate spreads widened by 1%, the presence of other non-bank intermediaries would still forecast between .1% and .2% lower refinance growth in the year the interest rate shock took place.[23]

3.2 The Timing of Refinancing After Interest Rate Movements

The results shown up to this point illustrate that regions where fintech lenders are more active tend to have stronger refinancing growth, particularly in declining interest rate environment where the aggregate incentive to refinance is high. However, the HMDA data displays mortgage lending transactions only at an annual frequency, while intra-year variation in interest rates can be substantial. On average, the time between an initial submission of a mortgage application and the closing of a loan is 2-3 months. Thus, if strong refinancing growth in high-fintech regions has to do, in part, with the ability of fintech lenders to remove capacity constraints from local markets by more efficiently processing mortgage applications, then when refinancing incentives increase, the relative differences in refinancing growth between high- and low-fintech areas should manifest relatively quickly.

To get a sense of the timing of refinancing relative to interest rate movements, I make use of the Single-family Loan Performance dataset from Fannie Mae. The higher frequency of the Fannie Mae data allow for a more refined observation of the dynamics of fintech lending alongside interest rate changes. In Panel A of Appendix Table 3 I show that the results of my baseline analysis, from section Section 3.1, can be replicated over a shorter (three month) time horizon using this dataset. Next, I estimate impulse responses using local projections, as described

[22]Versions of these regressions without county fixed-effects are not displayed. The results of the analogous specifications, without fixed-effects, suggest that counties with more large non-bank lenders and large banks have subsequently stronger refinancing activity, though the results are much weaker than the *FintechCount* results. The presence of small banks negatively forecasts subsequent refinance activity in these specifications.

[23]This estimate is derived from the sum of the *OtherNonbank* coefficient and the *OtherNonbank*Rates* coefficients.

by Jorda (2005). In particular, I estimate OLS regressions of the form

$$\Delta_h Refivol_{i,t+h} = \alpha_t^h + \sum_{k=0}^{2} \beta_k^h \cdot \Delta_1 Refivol_{i,t-k} + \gamma^h \cdot \Delta_3 Fintech_{i,t-1}$$
$$+ \delta^h \cdot \Delta_3 Fintech_{i,t-1} \cdot \Delta_1 Rates_t + \lambda^h \cdot \Delta_1 Controls + \varepsilon_{i,t+h} \quad (3)$$

where h=1, 2, ...5 denote the time horizon, in months, over which the regressions are estimated. The impulse responses are the dynamic evolution of the *Fintech* coefficients, γ^h and δ^h. In practice, I will plot the sum of these coefficients (and report both coefficients in appendix tables).

In contrast to equation (2), all variables are now expressed as growth rates, rather than levels. With monthly data, it is easier to control for several lags of past refinancing growth (i.e. without sacrificing several years of sample data). To the extent that there is a persistent fintech effect on refinance growth related to the overall level of fintech presence, this effect should generally be impounded in the lagged growth of refinance activity. Thus, this specification should better control for the correlation between very recent changes in fintech presence and refinance growth. In theory, if a stronger fintech presence leads to stronger refinancing, then once we control for recent refinance activity, the areas with the largest increases in refinance growth should be those with an increasing fintech presence. For the same reason, I opt to use changes in interest rate spreads in these specifications, rather than levels. Doing so allows me to better capture the timing of true interest rate shocks.[24]

Another important point is that when estimating each specification in differences, I am able to look at changes in fintech lending activity without concern about the differential size of housing markets across geographic areas. That is, I can re-define the fintech variable to be the log growth rate of the number of total fintech loans, without worrying that the sizes of these values simply pick up the sizes of regional housing markets. This is useful because the FNMA panel only explicitly identifies eight of the largest fintech firms in its panel. Smaller lenders, whose combined loan volume does not constitute at least 1% of national origination volume in a given year, are listed as "other" in the FNMA data.

Panels B and C of Appendix Table 3 display the impulse responses that result from these specifications, with panel B displaying results where *Fintechshare* is used as the right-hand side fintech variable, and panel C showing results where the growth rate of the number of fintech loans is used. In Figure 2 panels A and B, I plot the impulse responses associated with the latter of these sets of specifications. The impulse responses suggest that the effect of fintech lending on refinance growth is both quick and persistent. The results in panel C of Appendix Table 3 suggest that for every one percent of growth in the number fintech loans from time t-3 to time t-1, and conditional on an interest rate shock in the form of a 1% widening of mortgage spreads from time t-1 to time t, we can expect

[24] If refinancing incentives have been large for some time, then lagged refinancing activity should also be high, and the growth of refinancing relative to its own growth in the recent past should not be particularly strong. It is instead when we see a dramatic change in recent interest rate spreads when refinancing should pick up relative to its recent behavior.

.056% stronger growth in refinancing within the first month after the interest rate shock. This aggregate fintech effect rises to .128% after two months and .180% after three months, after which point the effect levels off. The effect is quite persistent, leveling off in the fourth month after the interest rate shock, and only declining slightly by the fifth month, from .187% to .179%. One reason for the persistence of the fintech effect may be that interest rates are also persistent, and many homeowners do not attempt to refinance right away, even when it is in there interest to do so (see, for example Stanton, 1995 and Agarwal, Rosen, and Yao, 2013). Thus, if interest rates decline at time t, it is likely that some borrowers who have a refinance incentive will nonetheless not immediately submit applications to refinance. If rates remain low for multiple months, some of these inattentive borrowers may ultimately refinance with a delay.[25]

In the final set of tests in this section, I look to assess whether the correlation between fintech lending and refinance credit growth is stronger amidst a spike in regional credit demand. When regional demand is strong, and local loan officers are overwhelmed by the surge in activity, lenders with more extensively automated lending technology should have an advantage in processing the onslaught of loan requests. To test this proposition, I develop a proxy for local refinance demand, which considers the total stock of Fannie Mae-guaranteed mortgages in a region with a refinance incentive. Early work on the mortgage-backed securities market by Richard and Roll (1989) suggests that mortgage pools exhibit very low pre-payment rates at issuance and then begin to pre-pay more extensively as underlying loans age, with mortgage pools becoming fully seasoned 30-60 months after initial issuance. As such, I proxy for changes in local refinance demand by looking at changes in the total stock of mortgages, aged 30-months or more, with a refinance incentive. Specifically, I define the local refinance incentive in ZIP code i as

$$OutstandingStock_i = log \left(\sum_k Ratespread_{i,k} \cdot Mortgagebalance_{i,k} \right)$$

where the sum is taken over all mortgages, k, with loan ages greater or equal to 30 months, and less than or equal to 240 months.[26] The variable *Ratespread* denotes the interest rate spread on each individual mortgage[27] while the variable *Mortgagebalance* is the remaining principal balance on each mortgage. The *OutstandingStock* variable gives the total value of outstanding fixed rate Fannie Mae-securitized mortgages in a 3-digit ZIP code weighted by their individual interest rates at origination (less current market interest rates). Intuitively, mortgages that carry higher interest rates have more to gain from refinancing, and the sum of outstanding balances on mortgages within a ZIP-code give a measure of the total supply of mortgage loans available to be refinanced. I estimate the following set of local projections,

[25] Thus, even if the average mortgage application is processed in 2-3 months, refinancing volume may remain high several months after an initial interest rate shock due to the delayed reaction of some borrowers in submitting refinance applications.

[26] As mortgages become highly seasoned, they become increasingly unlikely to pre-pay, as the remaining mortgage balance becomes low relative to the fixed costs associated with refinancing.

[27] That is, the interest rate at origination for each mortgage minus the prevailing 10-year Treasury yield

$$\Delta_h Refivol_{i,t+h} = \alpha_t^h + \sum_{k=0}^{6} \beta_k^h \cdot \Delta_1 Refivol_{i,t-k} + \sum_{\tau \epsilon T} \gamma_\tau^h \cdot 1_{t=\tau}^h \cdot \Delta_3 Fintech_{i,t-3} + \delta^h \cdot \Delta_3 OutstandingStock_{i,t}$$
$$+ \eta^h \cdot \Delta_3 Fintech_{i,t-3} \cdot \Delta_3 OutstandingStock_{i,t} + \lambda^h \cdot Controls_{i,t} + \varepsilon_{i,t} \quad (4)$$

for h=1,2,...5. In the above equation, I denote T as the set of all sample dates (i.e. Jan. 2010-Dec. 2019), and the term $1_{t=\tau}$ denotes an indicator function that takes a value of one at time t and zero otherwise. Thus, the above specification suggests that I allow lagged fintech activity to have separate coefficients at each date in the sample, in order to control for time-series variation in the effect of fintech lending as it relates to the interest rate environment. Thus, the interaction term $\Delta_3 Fintech_{i,t-3} \cdot \Delta_3 OutstandingStock_{i,t}$ looks at the strength of fintech lending as a predictor for refinancing growth only as it varies in the cross-section. Intuitively, this specification asks, if we observe an increase in the local supply of mortgages with a refinance incentive in a subset of ZIP codes from time t-2 to time t, whether the estimated effects of recent fintech activity on refinance growth are stronger in these ZIP codes.[28]

Figure 2 panels C and D display impulse responses associated with these specifications. They depict the evolution of the interaction effect between fintech lending and the expansion of the local supply of mortgages with a refinance incentive. As the impulse responses illustrate, the association between fintech lending and local demand shocks is positive at all time horizons from 1-5 months. This suggests that when a local mortgage market sees an increase in the number of local homeowners with a refinance incentive, the benefits associated with having a stronger regional fintech presence become more substantial than they would otherwise be. The estimated fintech effect, interacted with the regional refinance incentive, is strongest in the third month after the local demand shock, and decays in months 4-5, remaining positive for the entire time horizon.

4 Identification of Fintech Effects Using a Cross-border Approach

Despite my attempt to control for factors that might simultaneously influence fintech regional fintech lending patterns and housing market activity, endogeneity remains a concern. For example, the use of online lending platforms is stronger amongst sophisticated borrowers who may be more likely to refinance when they have an incentive to do so. Calculating the optimal time to refinance is a complex problem, and an extensive literature suggests that many borrowers make mistakes when doing so, either refinancing too early, or waiting too long to do so (see, e.g. Agarwal, Driscoll, and Laibson, 2013; Deng and Quigley, 2012; Stanton, 1995). Existing research also suggest that borrower characteristics and experience play a role in determining the extent to which borrowers

[28]The cross-sectional demand proxy boils down to an assessment of whether a) new mortgage origination was high in a ZIP code 30 months ago, and b) whether these borrowers had relatively high rates at origination.

make refinancing mistakes (see LaCour-Little, 1999; Agarwal, Rosen, and Yao, 2013). There is no perfect measure for borrower experience or sophistication, making it difficult to control for the possibility that fintech uptake is higher among this class of borrowers.

To better identify a causal connection between fintech lending and credit supply expansion, in this section, I will attempt to isolate a source of exogenous variation in regional fintech presence. To do so, I make use of the staggered timing of market entry by fintech lenders in various state-level mortgage markets. In the first part of this section I will argue that state-level regulatory factors likely played a role in the timing with which firms entered various states. In the second part of this section I will discuss and present the results of my identification scheme, which makes use of comparisons of county-level credit growth amongst counties on opposite sides of state borders, in pairs of states that have differing numbers of active fintech lenders.

4.1 Regulatory Barriers and Fintech Market Entry

My approach for identification makes use of the staggered timing with which fintech lenders entered various state-level mortgage markets. While some fintech lenders were already well established prior to 2010, there were a number of new firms that only began to originate mortgages in 2011 or later. Among the fintech lenders that existed at the start of my sample, a number of these lenders were still fairly new and originated mortgages in only a few states. Over time, new fintech firms emerged, and existing lenders began to expand their regional presence. Figure 3 displays an example of how this process unfolded for an individual fintech lender, CashCall mortgage. While CashCall existed prior to the start of my sample in 2010, it had a fairly minimal presence at that time, originating loans only in California and Florida. In the following year, CashCall expanded across most of the western states, and a handful of others. By 2015, it had grown to serve almost all states.

In the aggregate, this process generated variation in the number of active fintech lenders across states, with some states accumulating a large number of fintech lenders very early on, while others saw a relatively late influx of fintech lenders. In Figure 4 I show the total number of fintech lenders active in each state across the same years displayed in Figure 3. It is apparent that population appears to play a role in state-level differences, with California, Florida, and Texas maintaining relatively high numbers of active fintech lenders in all years.[29] Nonetheless, there are also more curious patterns. New York, and other states in the Northeast tend to have sparse fintech coverage, despite large populations, while states like Idaho, Oklahoma, and Colorado appear to have heavy fintech concentrations relative to their populations.

At first glance, it may be puzzling that newer fintech lenders did not immediately enter all or almost all state mortgage markets. After all, one advantage of online mortgage lending should be that it reduces the required

[29] It is relatively intuitive that state population should have some bearing on the location of fintech lending. Even if we are convinced of the theory that states' regulatory environments play a role in lenders' decisions regarding where to originate loans, lenders likely consider both the benefits of entering a state in along with the costs. Once state-level licensing costs are paid by a lender, larger states offer bigger markets in which to originate loans, generating potentially higher revenue per dollar paid in administrative costs.

physical presence of the lenders that possess this technology. In practice, however, there are a few reasons why we might observe this staggered entry across states. First, while online mortgage applications are the bedrock of their business models, a number of fintech lenders do hire loan officers that are available to meet with potential applicants. At a minimum, most mortgage loans still involve some in-person interaction upon closing of the loan. More importantly, unlike banks, which can apply for national charters, non-bank mortgage lenders must apply separately for licenses in each state. Each state has its own requirements for approving new mortgage lenders, with some states more stringent than others.

To examine the timing of fintech market entry in conjunction state-level regulatory factors, I collect information on state licensing requirements for non-bank mortgage lenders from the Nationwide Multistate Licensing System (NMLS). The NMLS is the system of record for non-depository financial services licensing. State regulators allow applicants to submit their application materials through the NMLS portal, enabling firms seeking to apply for licenses in multiple states to utilize a centralized system.[30] Via its website, the NMLS provides checklists complete with each state's individual licensing requirements. Using these checklists, I compile information on the licensing requirements in each state. I then use this information to create four quantitative variables meant to capture the costliness of each state's requirements.

The first of these variables is the dollar value of the application fees associated with submitting a licensing request. These application fees are not, in themselves, likely to constitute a significant deterrent for a firm seeking to enter a state mortgage market. Application fees average only $1,001 and hit a maximum of $3,000. However, since application fees are used to recoup the costs incurred by state regulators when they review an application, application fees are likely a proxy for the length and extensiveness of the review process. Since a non-trivial portion of the review process involves responding to follow-up questions from regulators, application fees are likely an indicator for the time and labor costs associated with the application process that are not captured by the written application requirements.

The second variable consists of the minimum net worth, in dollars, required for mortgage lenders. Like the application fees, net worth requirements seem unlikely to bind for moderately sized firms. Minimum required net worth averages under $100,000 and tops out at $1 million.[31] However, when applying for a license, firms must submit financial statements proving that they meet these minimum requirements, and are subject to periodic review once entering a state. This suggests that the documentation and verification costs associated with proving satisfactory net worth are likely to be higher in states where requirements are stricter.

The third variable is a dummy variable that takes a value of one if a state is a so-called "brick and mortar" state. Brick and mortar states require that mortgage originators maintain a physical branch presence within the state. This requirement runs counter to the business model of many fintech lenders, which focuses on maintaining

[30]After a firm submits an application, state regulators can provide commentary and give updates on their application statuses. At the time of writing, all states utilized the NMLS for licensing mortgage loan originators.

[31]The relatively small average level reflects the fact that a number of states do not have any minimum net worth requirement.

a minimal branch presence and originating most loans via online platforms. As such, I separate this variable from other qualitative application requirements.

Finally, I take a simple count of other mortgage licensing requirements in each state. These include such requirements as submitting audited financials, completing criminal background checks and credit history checks for upper management and minority shareholders, maintaining a minimum number of employees certified as licensed (individual) lenders in the state[32], submitting plans for anti-money laundering compliance, and submitting documentation certifying access to a bank-provided line of credit.[33]

To assess the extent to which state-level barriers of entry are able to predict the timing with which fintech lenders enter state mortgage markets, I use a logistic regression approach to estimate the probability that a lender will be active in a state mortgage market as of a given year, t. I use firm-level HMDA data to generate the set of years in which each lender enters a given state's market and estimate specifications which assume that the probability a lender is active in a given state assumes the following form:

$$Log\left(\frac{Prob_{i,j,t}^{Active}}{1-Prob_{i,j,t}^{Active}}\right) = \alpha_t + \beta \cdot log(Population_{j,t-1}) + \gamma \cdot Regulations_j \tag{5}$$

where $Prob^{Active}$ denotes the probability that a firm will operate in state i in year t. The dependent variable takes a value of one if firm i has began originating loans in state j by year t, and a value of zero otherwise. In setting up the data, a firm does not enter into the regression until the first year of its existence so equation (5) is estimated on an unbalanced panel. This specification assesses the strength of the association between a state's regulatory environment and the number of firms that enter that state early on in the sample. If a firm enters a state in 2011, for example, the dependent variable for that firm-state pair would take a value of zero in 2010, and would be equal to one from 2011-2019. If that same firm enters a different state for the first time in 2016, the firm-state dependent variable sequence would be six zeroes (from 2010-2015) followed by four ones (from 2016-2019). Thus, by construction, this specification places heavy weights on the states that received an influx of fintech lenders early in their histories. The variable *Regulations*, listed above, denotes a vector containing combinations of the four regulatory variables defined previously.

In Table 6, I show the results of estimating equation (5) on various combinations of regulatory variables. Each coefficient is an estimate of how a unit change to a covariate affects the log-odds ratio associated with the probability of state-level market entry. Across all specifications, each of the regulatory variables has a negative coefficient. The strongest regulatory predictor of a firm's probability of early entry in a given state is the state's application

[32]So-called "qualified individual" requirements vary by state, but generally require that some number of high-level employees either become licensed at the individual level, to become mortgage brokers within the state, or submit documents suggested that they have been certified in another state. These requirements also mandate certification that high-level employees have achieved a certain minimum number of years working in the mortgage industry.

[33]Credit line requirements vary across states, with some states requiring that mortgage originators maintain access to a minimum line of credit of a particular size, while others merely require proof of access to credit. As such, I do not code this as a separate quantitative variable.

cost. This suggests that these application costs are indeed a proxy for the level of time and scrutiny applied to an application by state regulators. The coefficients on the application cost variable suggest that a marginal dollar increase in application costs decreases the market-entry odds-ratio by between 24-34%, a magnitude which would seem inconceivable if application costs did not serve as a proxy for some latent factor. The next strongest effect, in terms of magnitude, is the brick and mortar dummy variable. While including the other regulatory covariates diminishes the level of statistical significance of the brick and mortar variable somewhat, it maintains significance at the 10% level, at a minimum, across all specifications. States with brick and mortar requirements are estimated to have log-odds of firm entry between .17 and .32 lower in a given year.[34] The number of qualitative application requirements also appears to have a substantial predictive effect on the pace of firm market entry into states, with coefficients between -.04 and -.065 across all specifications. This suggests that the addition of more licensing paperwork in the form of financial statement submissions, and background check documents, as well as other requirements, have a substantial effect on the timing with which firms enter state mortgage markets.

4.2 Cross Border Approach and Results

I utilize an identification approach that only makes comparisons across pairs of bordering states. In particular, I focus on comparisons of counties located close to the borders of adjacent states with differential levels of fintech activity. The underlying idea is that by focusing on pairs of bordering states, and on counties located close to their adjacent borders, the impact of regionally correlated but unobserved factors relevant to housing market outcomes should be much less severe than it would be when comparing geographically distant regions. Given state-level licensing requirements, a lender that is licensed to operate in one state, but not another, must "artificially" refrain from lending beyond the border of the state in which it is licensed, inducing a potential discontinuity.

My empirical approach takes the form of a cross-border regression discontinuity framework. The use of state borders as a tool for studying similar regions facing differential policy environments dates back, at least, to Card and Krueger (1994), and state-border regression discontinuity frameworks have been used by Holmes (1998) and by Pence (2006). Unlike these studies, the effects I seek to identify are not static. Rather than studying the effects of a set of policies, per se, I look to study the effects of fintech market entry which arises both from the creation and expansion of new and young fintech lenders, and from static regulatory factors. As such, my study seeks to exploit time-varying differences in fintech presence while controlling for time-invariant differences across states. Recent research that has made use of a state-border scheme alongside time and regional fixed-effects includes Campello, Gao, and Xu (2019), Ljunquist and Smolyansky (2018), and Moretti and Wilson (2017).

To construct my sample I begin by identifying the number of fintech firms operating in each state by year, using HMDA data, as in Figure 4. I then identify all pairs of states which share a border and which contain a

[34] The states with brick and mortar requirements are Arizona, Missouri, Nevada, and Texas. In Texas, the brick and mortar requirement is apparently easier to circumvent than in these other states, though I do not explicitly model this distinction.

differential number of fintech lenders in a given year. For each state pair, I label the state with a larger number of fintech lenders as the "treated" state, and the adjacent state with which it is paired as the "control" state.[35] I then identify the set of counties in both the treated and untreated state that lie within a given distance of their shared state border, with the distance between a county and border calculated using the population centroids of each county. I will estimate specifications where I use distances of 50 and 100 miles from a state border as cutoff points for a county's inclusion in the sample.[36]

Figure 4 gives further insight into the construction of the sample. In that figure, I select several state pairs that appear in the sample in various years. Since the sample of treated and control states changes from year to year as fintech firms expand into new states, this diagram should be thought of as illustrating pairs of states at a single point in time. In the figure, the counties that are shaded red denote the set of counties, in treated states, which are located within 50 miles of the state's border with its paired control state. Counties highlighted in gray represent control state counties within 50 miles of the shared border. Each county that appears in the sample references one or more specific border. For example, counties in the northeastern corner of Texas are treated counties that reference the Texas-Oklahoma border. Counties in the Southwestern part of Texas reference the Texas-New Mexico border. Some shaded counties in the Northwest panhandle of Texas are located within 50 miles of both Oklahoma and New Mexico. Such counties would appear in the sample twice.[37]

The key assumption underlying my identification scheme is that unobserved factors that influence mortgage lending activity in border counties of adjacent states are continuous (as a function of border distance) across state borders, and are time invariant. This implies that fintech firms do not choose to enter one state, and not its neighboring state, for reasons related to differential lending opportunities in the border counties of those states. My empirical approach is insensitive to large unobserved differences between non-neighboring states (since it only compares adjacent states), to differential lending opportunities in adjacent states outside of border counties,[38] and even to average differences in adjacent states' border counties that do not change over time. The empirical specifications I estimate will assume the following form, where subscripts i, j, b, and t index counties, states, borders, and time respectively:

$$\Delta_1 Refivol_{i,j,b,t} = \alpha_t + \sum_{\nu \epsilon B} \beta_\nu \cdot 1_{b=\nu} \cdot distance_{i,b} + \gamma \cdot Treat_{j,t} + \delta \cdot Treat_{j,t} \cdot Rates_t + \lambda \cdot Controls_{i,t-1} + \mu_{(j,b)} + \varepsilon_{i,j,b,t} \quad (6)$$

[35]Alaska and Hawaii will not appear in the analysis, as they do not share borders with any other states.

[36]Given this methodology, a single state can be in both the treated sample and in the set of "control" (or paired) states if it shares a border with one state that has fewer licensed fintech firms than it does, and shares a border with another state with more fintech lenders. However, the set of counties within that state that reference those two state borders will, in general be different, though some overlap is possible.

[37]It is also worth noting that some counties in western states are located on state borders but remain unshaded. By and large, this is because county distances are counted using population centroids of counties, and these unshaded counties have large towns located more than 50 miles from the state border. Loving County, in southwestern part of Texas, next to New Mexico, is an example of a county excluded from the sample for a different reasons. Namely, it is small enough that it does not any have mortgage lending transactions in a number of sample years, and is dropped from the sample.

[38]For example, this methodology would not lead to bias if a fintech firm chose to enter one state and not another neighboring state for reasons having to with strong lending opportunities in an urban area outside of border counties.

In the specifications above, the variable *Treat* assumes a value of one if county i is located within a state, j, which is a treated state with respect to the border, b. The *Rates* variable is as defined in equation (2). I let B denote the set of all state borders in the sample. The term *distance* denotes a county's distance from border b and the distance variable soaks up unobservable differences between counties that vary as a function of their distance from the border, with a separate distance coefficient estimated for each state pair. In versions of the equation (6) regressions that use 100 mile bandwidths for sample selection, I will also include a squared border distance term. The term $\mu_{(j,b)}$ denotes a set of fixed effects for borders or for states, which will vary across the set of regressions that I estimate.

I will also make one additional sample refinement in some of the specifications I estimate. Specifically, I will display results of regressions in which I exclude border counties that contain large metropolitan areas (defined as any of the top 100 metropolitan areas, by population, residing within 50 miles of a state border). Doing so potentially improves sample selection by excluding counties that are likely to have very different characteristics than other counties near the border, and thus may improve the level of comparability between counties located in the treatment and control groups in a given year.

In Table 7 I show results from estimates of various versions of equation (6). The coefficients on the *Treat* variable suggests that in an average interest rate environment, refinancing growth is between 1% and 3.3% stronger in areas with a larger number of active fintech lenders. The largest coefficients on the *Treat* variable are obtained in specifications that exclude the largest metro areas from the set of border counties. Much of this increase in the treatment effect in these specifications comes from the exclusion of the New York City metropolitan area which had particularly strong refinancing activity in some years. The *Treat***Rates* coefficients suggest that the treatment effect of having a larger number of fintech lenders than adjacent states increases in declining interest rate environments. A 1% widening of mortgage spreads is estimated to increase the treatment effect by between 1.4% and 6.5%, effects which are statistically significant at the 1% level across all specifications. The exclusion of large metropolitan areas tends to diminish the estimated size of the effect of the interaction between interest rates and fintech presence, as the interaction effect is roughly four percentage points larger in specifications which include the full set of counties. This suggests that some of the large fintech refinance effects in years when interest rates decline are driven by high refinancing activity in dense urban areas.

In the appendix, I conduct further analyses to assess the similarity of the characteristics of treated and untreated counties in my sample, and to assess the robustness of my results.

5 Does Fintech Credit Expansion Boost Local Economies?

When interest rates decline, the ability to refinance has the potential to increase the wealth of existing mortgage borrowers by allowing them to reduce their interest costs, extract equity from their homes when it is relatively attractive to do so, or extend the maturity of their loans and reduce their monthly payments. In this section, I look to assess whether, by inducing more borrowers to refinance, fintech lenders promote subsequent gains in household spending in regions where they are active. Existing literature has uncovered a robust link between mortgage credit, refinancing, and household consumption plans (e.g. Campbell and Cocco, 2003; Hurst and Stafford, 2004; Koijen, Van Hemert, and Van Nieuwerburgh, 2009). However, it is not self evident that fintech lending will spur stronger consumption growth. The speed and intensity of consumption gains in the wake of an expansion in home refinance credit depends on the regional distribution of home equity (Beraja et al., 2019), home prices (Mian, Sufi, and Rao, 2014), income, and marginal propensities to consume (Agarwal et al., 2020; Mian and Sufi, 2011). It is thus important to investigate the empirical association between fintech lending and household spending to shed light on the macroeconomic effects of fintech presence.

If fintech lending generates consumption effects as a result of refinancing activity, such spending increases may bolster other economic outcomes. If wealth gains stemming from refinancing activities lead consumers to spend more on goods and services produced within their local area, then we may see local employment growth and business expansion as a side-effect of stronger consumption activity. For example, Mian, Sufi, and Verner (2020) find that credit supply shocks that fuel consumer credit growth foster employment gains and business expansion in local non-tradable goods. In the final part of this section I will evaluate the correlation between fintech credit expansion and local business growth.

5.1 Fintech Lending and Local Retail Spending

In order to investigate whether fintech lending has an effect on consumption, I will need a measure of consumer spending that is likely to capture the behavior of local consumers (i.e. those who reside in a given county) so that it can plausibly be linked to local mortgage refinancing activity. I will also need to consider spending categories which are likely to see an uptick in demand amidst a wave of mortgage refinancing. Some prior studies on local consumption effects of housing outcomes have focused on automobile expenditures (e.g. Mian, Sufi, and Rao 2014; Agarwal et al. 2020) and on broad measures of credit card spending. While large durable expenditures on automobiles are a plausible response to home refinancing for those who take out large amounts of home equity, those who refinance merely to lower their monthly mortgage payments are unlikely to save enough to afford a car that they otherwise would not have purchased.

In order to capture new spending that might plausibly arise from refinancing activity, I instead focus on a measure of local retail expenditures. The measure of retail spending that I create comes from records of purchases at a large sample of mass market retail establishments that upload their purchases to a centralized database. Mass market retailers, (commonly referred to as "big-box" retailers) sell a wide variety of general purpose consumer goods such as home furnishings and kitchen supplies, clothing, electronics, automotive accessories (e.g. motor oil), school supplies, beauty products, and many other items. In comparison to measures of spending that focus on large durable consumption goods, local retail expenditures are more likely to respond elastically to relatively modest increases in income.[39]

I base my measure of local consumption on information from Nielsen's Retail Scanner dataset. Nielsen collects weekly sales information from a panel of over 30,000 national retail store locations. The dataset is referred to as Retail Scanner data because the dataset is populated when items are brought to a check-out counter and their product codes are scanned via a laser scanner. The dataset is populated with highly granular information the products purchased, allowing for the calculation of sales volume by product categories. Importantly, the database reports the geographic location of each store in the panel, and each purchase is indexed by its individual store location, allowing for precise identification of where each purchase took place.[40]

The key outcome variable in my analysis of local consumption is the log change in local retail spending from time t to time t+1. In order to build a meaningful measure of consumption growth for a given year, t+1, I begin by identifying the sample of stores that operate within a county. Since the Nielsen panel changes from year to year, I consider only stores that were active in both year t, and in year t+1, and I further refine this sample by keeping only stores that reported at least 26 weeks of data in both years. Keeping only sets of stores that are active in both years allows for an assessment of expenditure growth which is unaffected by changing panel composition. Further, I annualize spending at each store, thereby eliminating potential sample biases resulting from stores that report data less frequently than others. I also consider only counties that contain five or more retail store locations in an attempt to minimize the extent to which results in a given county might be unrepresentative of true county-level retail purchase activity. For verification, I ensure that all of my consumption results are robust to considering a smaller sample constructed only from retail store locations that are active for all years of the study. Doing so ensures that results are not driven by the changing sample composition across years.[41]

I build two measures of county-level retail expenditure growth using the Nielsen data. The first measure, which I refer to as "total retail spending" (or *TotalRetail*) consists of spending within all product codes outside of food/groceries and pharmacy/medicine. I exclude these two categories of spending, as food and medicine are appear to be the categories least likely to respond elastically to changes in income.[42] I refer to my second measure of retail expenditures as "discretionary retail spending" (or *DiscretionaryRetail*); to construct it, I begin with my measure of total retail spending and remove personal hygiene/toiletries, school supplies, car maintenance and

[39]Moreover, in contrast to broader measures (e.g. credit card expenditures) which assess purchases at a broader array of merchants (e.g. in the hotel and tourism sector, and specialty/niche retailers) local mass market retailers appear, intuitively, to generate large portions of their expenditures from consumers living outside of the local areas that they serve. Most counties of sufficient size have their own "big box" retailers. It is unlikely that consumers would have to drive far in order to find a purveyor of generic household goods.

[40]To my knowledge, this is the first study that has used Nielsen scanner data as a broad proxy for local consumption. Other studies have used Nielsen data as a proxy for local spending on certain product categories. See, e.g. Cotti et al. (2021)

[41]I will include year fixed-effects in all regressions. However, part of the analysis asks about whether the effects of fintech activity are stronger in years in which interest rates decline. In principle these coefficients could be biased if strong spending growth in a particular year is driven by a large influx of new panel participants (i.e. retail stores) that were active in two consecutive years (so as to be included in the calculation for consumption growth in that year) but not in other years.

[42]Of course, it is plausible that consumers could substitute to more expensive categories of food and to be less sensitive to sales/coupons, etc. However, I remain comfortable with the assumption that grocery purchases are relatively inelastic compared to other retail categories.

repair products, and home maintenance and repair products. The idea behind this measure is to further refine the definition of retail spending to include categories of purchases most likely to respond elastically to modest changes in consumer income.

To examine the correlations between fintech activity and consumption growth I return to the functional form of my baseline analysis and ask whether consumption growth is stronger in the year after a fintech-led wave of refinancing activity. Specifically, I estimate

$$\Delta_1 Spending_{t+1} = \alpha_t + \beta \cdot Fintech_{i,t-1} + \gamma \cdot Fintech_{i,t-1} \cdot \Delta_{avg} Rates_t + \delta \cdot Controls_{i,t-1} + \varepsilon_{i,t+1}$$
$$Spending \in \{TotalRetail,\ DiscretionaryRetail\} \tag{7}$$

relative to the set-up of equation (2), the only distinctions are the change to the dependent variable, and the time horizon, where I now look at the year after an interest rate shock. The idea underlying this is that for borrowers that refinance, it will likely take some time for the effect of lower monthly payments to outweigh the initial outlay of refi fees paid to the lender up front.

Table 8 displays the results of estimating equation (7) using the *FintechCount* measure for fintech market activity. In Appendix Table 5, I display analogous results with the *FintechShare* measure of fintech activity. Results for both measures of consumption are shown. Results are fairly consistent across both measures of consumption growth. Depending on the inclusion or exclusion of county fixed-effects, the results suggest that the presence of an additional fintech lender at time t-1 forecasts between .09% and .2% stronger consumption growth. Coefficients on the *FintechCount* variable are statistically significant at the 1% level across all specifications. Given previous results on refinancing, suggesting that the marginal addition of a fintech lender corresponds to between .4%-.8% stronger refinancing growth, the results in Table 8 suggest that consumption gains are between 10% and 50% as large as the initial refinancing surge. While the high end of this estimate seem excessively large, prior research has suggested that consumption growth effects can be large when homeowners extract equity from their homes (e.g. Mian and Sufi, 2011; Agarwal et al., 2020).

The interest rate interaction terms are also large, suggesting that when refinancing is particularly high, in the wake of widening spreads between outstanding mortgages and prevailing market interest rates, consumption growth is larger in the year after this wave of refinancing occurs. Coefficient estimates suggest that a 1% widening of rate spreads increases the fintech effect by between .18% and .23%. This suggests that the effect of fintech presence becomes two to three times stronger in a falling interest rate environment. The *FintechShare* results tell a largely similar story, though the same patterns only hold in regressions that contain county fixed-effects.[43]

[43] Appendix Table 5 suggests that a 1% increase in the refinance market share of fintech lenders at time t-1 predicts between .04% and .31% stronger retail consumption growth at time t+1. Interest rate interaction terms are inconsistently signed across specifications, with values ranging between -.05 and .07, with positive interaction effects obtaining in specifications with county fixed effects.

5.2 Fintech Lending and Local Business Growth

If the presence of fintech lending brings about an increase in consumption among those who refinance, and if these consumers tend to spend locally, then the positive consumption effects of fintech-induced refinancing might also bring about positive outcomes for regional businesses. The effects of these local spending surges would likely be most impactful for small businesses that cater to local consumers. Larger businesses with a broader reach, and those in industries like tourism and manufacturing that have customer bases outside of the areas in which their production is located, would likely gain little from a boom in local consumption. Thus, to investigate whether there is an association between fintech activity and local business activity, I focus on small businesses operating within the non-tradable sector. According to Bahadir and Gumus (2016) and Mian, Sufi, and Verner (2020) businesses in the non-tradable sector should be most affected by credit expansions that target households, as is the case in this study.

My primary source of information on small business activity comes from the US Census Bureau's County Business Patterns (CBP) database. The database contains employment and payroll information at the county level for a number of industry groups, as classified by the North American Industry Classification System (NAICS) codes of each industry group. I follow the industry classification procedure of Mian and Sufi (2014b) in order to identify industries that belong to the non-tradable sector. I further refine this group of businesses to include only establishments with 100 or fewer employees. The key outcome variables I will focus on within the CBP data are total employment and the number of small business establishments in a county. If local consumption grows stronger as a result of a positive wealth shock to home owners, local businesses that serve these consumers may expand, bolstering employment. Stronger consumption may also encourage new businesses to enter a local market, or forestall the demise of businesses on the margin of failing, leading to a greater number of small business establishments.

Stronger local consumption may also fuel the level of investment by small businesses, as some of these businesses expand to keep up with growing demand. To generate a viable proxy of small business investment at the county level, I utilize data on small business lending from Community Reinvestment Act (CRA) disclosures. I focus on the total volume, and the total number of loans granted to businesses with assets under \$1 million.

To determine whether there is evidence consistent with the notion that fintech credit expansion fosters stronger growth for small businesses, I run analogous specifications to the consumption growth models of the previous subsection, with the only modification being the left-hand side outcome variable. I therefore estimate

$$\Delta_1 Smallbus_{t+1} = \alpha_t + \beta \cdot Fintech_{i,t-1} + \gamma \cdot Fintech_{i,t-1} \cdot \Delta_{avg} Rates_t + \delta \cdot Controls_{i,t-1} + \varepsilon_{i,t+1}$$

$$Smallbus \,\epsilon\, \{EmploymentNonTradable,\ EstabNonTradable,\ LoanCount,\ LoanVol\} \tag{8}$$

where *EmploymentNonTradable* gives non-tradable sector employment, *EstabNonTradable* denotes the number of small business establishments in the non-tradable sector, *LoanCount* gives the total number of small business loans to firms with under $1 million in total assets, and *LoanVol* gives the total dollar volume of such loans. All variables are expressed in log form, so that growth-rates are measured as log differences.

Table 9 displays the results of these regressions, with coefficients on the *FintechCount* variables. The results suggest that small business activity tends to pick up in the year after a fintech-induced refinancing surge. Coefficients on the *FintechCount* variable in columns (1)-(4) suggests that the presence of an additional fintech lender at time t-1 predicts between .3% and .6% stronger growth in the number of small business establishments in the non-tradable sector at time t+1 and between .3% and .4% stronger growth in small business employment. The interaction of *FintechCount* with widening mortgage interest rate spreads is also positive and statistically significant in these specifications. The interest rate interaction term coefficients in these regressions tend to be stronger in specifications that include county fixed-effects. In these specifications, in columns (2) and (4), a 1% widening in interest rate spreads amplifies the fintech effect by .9% and 1%, respectively, for small business establishment growth and employment growth.

Columns (5)-(8) suggest that small business lending is stronger in regions with a more concentrated fintech presence. An additional fintech lender at time t-1 forecasts between .3% and .5% stronger growth in the total number of small business loans extended between time t and t+1. Interaction terms have positive and significant coefficients in all four of these specifications, suggesting that small business lending growth is stronger in years that follow large expansions of fintech-induced refinance credit. To the extent that small business lending is a proxy for the level of investment undergone by these businesses, the results in columns (5)-(8) suggest that small local businesses invest more in places in which fintech lenders expand the supply of credit.

6 Does Fintech Lending Help Fed Policy Reach Underserved Communities?

In the last portion of this paper I ask whether the effect of fintech lending on the transmission of monetary policy varies across geographical regions and communities. If fintech lenders affect markets by alleviating microeconomic frictions faced by potential borrowers, then fintech lending may have the strongest impact on monetary transmission in places in which these frictions are most binding.

One market imperfection that may be improved by the presence of fintech lenders is the poor access to credit extended to minority borrowers (see, e.g. Bayer, Ferreira, and Ross, 2018; Bostic, 1996; Browne et al., 1996; Cheng, Lin, and Liu, 2015; Ghent, Hernandez-Murillo, and Owyang, 2014). Discriminatory effects can arise from cognitive biases, from a so-called "taste for discrimination" among White loan originators (Becker, 1957), or from the use of race as a proxy for economically important traits for which obtaining reliable information is costly

(Ladd, 1998). Recent research by Bartlett et al. (2019) suggests that fintech lenders are less discriminatory than other categories of lenders. They find, broadly, that loans to racial and ethnic minorities carry higher interest rates than loans extended to similarly situated White borrowers, but that these interest rate mark-ups are lower on fintech loans. Moreover, they find no evidence that fintech lenders reject minority applicants at a higher rate than White borrowers. They interpret this latter finding as evidence that fintech lenders alleviate cognitive biases of human loan officers.[44]

Fintech lenders may also help overcome borrowing constraints in geographic regions where borrowers have limited access to the traditional financial system due to the scarcity of brick-and-mortar bank branches. Existing research suggests that potential borrowers' local access to financial services is an important determinant of real outcomes (e.g. Burgess and Pande, 2005; Cetorelli and Strahan, 2006; Gilje, 2019; Jayaratne and Strahan, 1996). Rural areas with sparse populations often have few bank branches, and the transportation and time costs associated with going through the face-to-face mortgage lending process at physical branch locations are likely to be higher for residents of these areas. Recent research by Erel and Liebersohn (2020) on small business loans have suggested that fintech lenders are particularly active in regions with little access to the traditional banking system and are more likely to expand the overall supply of small business credit in these regions.[45]

Given the research on fintech lenders and access to finance, it is plausible that fintech lenders broaden the Fed's reach by allowing the refinance channel of monetary policy to operate efficiently in areas where financial frictions dampen its transmission. In the remainder of this section, I will test these hypotheses by looking within and across counties. I will ask whether, within a county, minority populations gain more access to credit when fintech firms enter, and whether this association is stronger during monetary expansions. I also ask, across counties, whether the effect of fintech lending on credit growth and retail spending is stronger in sparsely populated areas, regions with few bank branches, and regions with significant minority populations.

6.1 Within-county Analysis of Fintech Lending and Credit Composition

I first turn to my within-county analysis of the effects of fintech lending. I look at the association between fintech market presence and the composition of refinance credit. If fintech lenders have a particular advantage in screening racial and ethnic minority applicants, then the presence of fintech lenders should allow more of these borrowers to obtain credit. This effect should be strongest in declining interest rate environments when financial technology is most beneficial. To investigate this possibility, I make use of the county-level HMDA dataset, breaking down

[44]Since GSE guarantees eliminate credit risk in the conforming segment of the mortgage market, the authors argue that higher rejection rates for minority applicants among non-fintech lenders are likely inconsistent with profit maximizing behavior. They cite the fact that minority borrowers continue to pay relatively high interest rates (as compared to White borrowers) as evidence that fintech algorithms may use variables correlated with race strategically, to proxy for lenders' likely market power. For example, variables correlated with race may inform borrowers' likelihood to shop around among many different lenders, or proxy for the likelihood that the borrower lives in a so-called "financial desert" with limited access to financial services.

[45]See, also, **?** which presents evidence that fintech consumer loans target areas with low penetration by traditional banks.

total refinance loans by loan-type. In particular, I sort total refinance loans within a county by the borrower's race and ethnicity. I also examine whether riskier loan segments (FHA loans and loans secured by junior liens) expand alongside fintech entry. I estimate the following set of within-county regressions:

$$\Delta_1 Reficomposition_{i,t} = \alpha_{1,t} + \alpha_{2,i} + \beta \cdot Fintech_{i,t-1} + \gamma \cdot Fintech_{i,t-1} \cdot \Delta_{avg} Rates_t + \delta \cdot Controls_{i,t-1} + \varepsilon_{i,t} \quad (9)$$

where *Reficomposition* is the percentage of total home refinance loans (by volume) that to go borrowers within a particular category. The coefficients $\alpha_{1,t}$ and $\alpha_{2,i}$ denote the presence of year and county fixed effects, respectively.

I first look at how fintech lending co-varies with the racial composition of borrowers. I examine the association between fintech lenders, and the share of non-white borrowers among all refinance credit within a county. I label a loan as going to a non-White borrower if the primary applicant on the loan lists a race other than White/Caucasian on his or her loan application.[46] For the purposes of this analysis, I exclude borrowers who do not list a race, or who are listed as belonging to "some other race."[47] I exclude those in the "some other race" category due to evidence suggesting that a substantial percentage of those identifying as belonging to this category also identify as Hispanic/Latino, a group which I will study separately.[48] Borrowers who identify as Black or African American make up the plurality of the non-White racial group and appear to drive the results discussed below.

I display the associations between fintech lending, interest rates, and credit composition in Table 10. I display a similar table, presenting results of an analysis that uses *FintechShare* rather than *FintechCount* in (7). In column (1) I present the results of the analysis where *RefiComposition* is the non-White share of refinance loans. The coefficient on the *FintechCount* variable is .002, suggesting that within a county, a unit increase in the number of active fintech lenders at time t-1 forecasts a .2% increase in the share of refinance loans originated to non-White borrowers in the subsequent year. This effect becomes stronger if interest rate spreads widen at time t. Conditional on a 1% widening of mortgage interest rate spreads at time t, the estimated effect of a marginal increase in fintech lenders rises by .0049. That is, a unit increase in the number of active fintech lenders at time t-1 would instead be associated with a .69 percentage-point increase (=.2%+.49%) in the share of mortgage loans going to non-White borrowers at time t.

I next look at analogous results for Hispanic or Latino borrowers. I display these results in column (2) of Table 10. In a similar fashion to the results for non-White borrowers, the results in column (2) suggest that a strong fintech presence precedes a sharp increase in the share of Hispanic or Latino-identifying borrowers who receive refinance loans. The *FintechCount* variable in this regression is .0017 suggesting that an additional fintech

[46] Mortgage applications can have multiple co-applicants. I consider only the race of the primary applicant.

[47] Thus, I ignore the phenomenon documented in Agarwal et al. (2020) that a larger percentage of fintech borrowers do not list their race on their loan applications, presumably because racial anonymity is easier to achieve via an exclusively online approval process. While I imagine that this phenomenon is likely to lead to an understatement of the shift toward non-White borrowers when fintech firms enter a market (due to the presumed greater incentive of non-White borrowers to remain anonymous), this is speculative.

[48] See, for example, https://www.npr.org/2021/09/30/1037352177/2020-census-results-by-race-some-other-latino-ethnicity-hispanic

lender is associated with an increase, by .17%, in the share of loans within a county that go to Hispanic or Latino borrowers. The interaction of *FintechCount* with *Rates* has a coefficient of .0028 suggesting that a widening of interest rate spreads by 1% predicts a .28 percentage point amplification of the fintech effect.

I next consider the possibility that fintech lenders change the composition of lending in favor of riskier categories of loans. Buchak et al. (2018) suggest that non-bank lenders[49] may have a comparative advantage in riskier loan segments due to the regulatory advantage they enjoy over depository institutions. I thus consider the possibility that FHA-loans, which are issued to lower income borrowers, are facilitated by increased fintech presence. I also consider the possibility that loans secured by junior liens on a property also see stronger origination upon fintech entry. Columns (3) and (4) display results of the FHA-share and junior lien regressions, respectively. FHA loans exhibit inconclusive results in the sense that the *FintechCount* and interaction coefficients display opposite signs. The fintech coefficients in the junior lien regressions are both positively-signed, suggesting a positive association between fintech lending and riskier loans. Since junior loans against junior liens are associated with borrowers extracting equity from their houses, these results suggest that fintech lenders may help borrowers take advantage of their home equity for the purposes of consumption, particularly in falling interest rate environments when the terms associated with doing so are likely to be favorable.

6.2 Do Fintech Lenders Amplify Monetary Policy in Underserved Areas? Assessing the Cross-county Evidence

I next address the question of how the effects of fintech lending vary across regions according to the demographic make-up and geographical features of these areas. I first assess whether estimated effects of fintech firms on credit growth are stronger in regions with larger shares of racial and ethnic minorities and in areas with sparser populations or fewer bank branches. I then ask whether these same regions see stronger consumption growth in the wake of these refinancing booms. To uncover the cross-regional variation of fintech effects, I will estimate regressions of the form

$$\begin{aligned}\Delta_1 Refivol_{i,t} = \alpha_t + \sum_{j=1}^{4} \beta_j \cdot 1_{i\epsilon Q_j} \cdot Fintech_{i,t-1} + \\ \sum_{j=1}^{4} \gamma_j \cdot (1_{i\epsilon Q_j} \cdot Fintech_{i,t-1} \cdot \Delta_{avg} Rates_t) + \delta \cdot Controls_{i,t-1} + \varepsilon_{i,t}\end{aligned} \tag{10}$$

where the term $1_{i\epsilon Q_j}$ is an indicator function that takes a value of one if county i is in the jth quartile of the distribution of counties, as sorted by a particular demographic or geographic trait, and zero otherwise. Thus, I display fintech effects across each quartile of the population to show how the strength of fintech effects varies

[49] This applies to non-bank lenders generally, rather than to fintech lenders specifically.

across regions.

The retail consumption data are not available for the full set of counties included in the HMDA panel. The retail consumption data covers roughly 30% of US counties, most of which are on the larger end of the population distribution. Thus, rather than divide this sample into quartiles according to county-level traits, I instead extend the diff-in-diff specification from equation (2) into a triple-diff framework that looks at the interaction between fintech effects and regional traits. These regressions will take the form

$$\begin{aligned}\Delta_1 Spending_{i,t+1} = \alpha_t + \beta \cdot Trait_{i,t-1} + \gamma \cdot Fintech_{i,t-1} + \delta \cdot Fintech_{i,t-1} \cdot \Delta_{avg} Rates_t + \\ \eta \cdot Fintech_{i,t-1} \cdot Trait_{i,t-1} + \theta \cdot Trait_{i,t-1} \cdot \Delta_{avg} Rates_t + \\ \lambda \cdot Fintech_{i,t-1} \cdot Trait_{i,t-1} \cdot \Delta_{avg} Rates_t + \mu \cdot Controls_{i,t-1} + \varepsilon_{i,t}\end{aligned} \tag{11}$$

where the *Spending* variable is as defined in equation (7) and *Trait* refers to one of the demographic or geographic characteristics on which I sort counties. In these specifications, the η and λ coefficients will be of primary interest. They will reveal how the effect of fintech presence, and the interaction between fintech lenders and interest rate shocks varies according to the characteristics of individual counties.

I first turn my attention to the effects of fintech market presence on credit growth, as it varies across counties, and ask whether estimated fintech effects are stronger in counties with larger racial minority populations. Analogously to the previous section, I sort counties based on their shares of White residents. In Table 11, in the first column, labeled "% White," I show the results of estimating equation (10) on the HMDA sample sorted on the basis of counties' White population shares. The *FintechCount* coefficients in that table show that the estimated effect of fintech lending on refinance credit growth is larger in counties where White residents make up a smaller portion of the population. The *FintechCount* coefficient for the quartile of counties with the smallest White population share attains a value of .006, suggesting that a unit increase in active fintech lenders predicts .6% stronger refinance credit growth in counties with the fewest White residents. This coefficient remains fairly stable across the first three quartiles of the distribution of counties and declines substantially, to .001, for the top quartile. This phenomenon is mirrored across the set of *Fintech*Rates* interaction terms. The interaction coefficients decline monotonically across the county distribution, however the degree of this decline is fairly mild until reaching the top quartile of the population. The interaction effects decline from .013 to .005 from the first to fourth quartiles. The differences between first and fourth quartile coefficients are -.005 and -.008 for the *FintechCount* and interaction coefficients, respectively; the first of these differences is significant at the 5% level, while the latter attains only marginal statistical significance at the 10%. The results are broadly supportive of the notion that the presence of fintech lenders in a local market is more meaningful in markets with a larger proportion of minority borrowers.

However, differences are not meaningful between counties outside of the top quartile.[50]

I next examine how fintech effects vary according to the Hispanic or Latino population of a local market. Column (2) of Table 11 displays these results. In this regression, the first quartile denotes the set of counties with the smallest Hispanic or Latino share of the population. From the first quartile to the top quartile the *FintechCount* coefficients grow from .003 to .006, a difference which is statistically significant at the 1% level. A similar pattern is observable in the interaction coefficients, which rise from .002 to .009 from the first to fourth quartiles, suggesting that the fintech-induced transmission of interest rate shocks to credit growth is stronger in counties with a higher Hispanic/Latino population share. I interpret these results as broadly consistent with the notion that part of the advantage of fintech lenders in propagating the effects of monetary policy lies in their ability to overcome discrimination or cognitive biases that hamper lenders.

I next ask whether fintech lending can help Fed policy reach rural areas, and counties that are poorly connected to the banking system. In column (3) of Table 11 I examine how fintech credit growth effects vary by population density. For both the *FintechCount* and interaction coefficients, a monotonic pattern is observable across the distribution of counties, with fintech effects achieving their maximum potency in the most sparsely populated counties. Coefficients on the *FintechCount* variable steadily drop from .010, in the most sparsely populated areas to .006 in the densest counties, suggesting that the amount of additional credit growth predicted by a marginal increase in active fintech firms drops by .4% as we move from sparsely populated to densely populated counties. Meanwhile, the interest rate interaction terms drop from .018 to .011 from Q1 to Q4. The differences between the Q1 and Q4 coefficients are significant at the 1% level for both the count and interaction coefficients.

I display the results of regressions that sort counties by the accessibility of bank branches in columns (4)-(5) of Table 11. Column (4) displays results where counties are sorted by branches per-capita. The availability of fintech mortgage lenders may be more valuable in counties with fewer branches per capita if the small number of branches signals a greater likelihood that bank staff will become unable to process all outstanding mortgage applications in a timely manner when demand is high. The coefficients in column (4) appear consistent with this hypothesis. The *FintechCount* coefficient progresses from a high of .009 in counties with the smallest number of branches per capita to .001 (and a statistically insignificant coefficient) in counties with the smallest number of fintech lenders. The difference between coefficients in the first and fourth quartiles are significant at the 1% level. This suggests that in counties in which borrowers have many alternatives to fintech lenders and traditional finance is readily available, the expansionary effects of fintech lenders on the credit supply are muted. Results are similar when counties are sorted by the number of branches per square mile.[51]

In the final part of this analysis, I look at whether retail spending growth patterns are consistent the credit

[50] Many of these counties are likely to be located in rural areas with smaller populations. While I control for population and population density on the right-hand side of all regressions, it is unclear whether these counties differ from more diverse counties along other dimensions than race.

[51] However, in this specification, the interaction between fintech presence and interest rate spreads is strongest in the the set of counties with the largest number of branches, on a county-size adjusted basis, a result which is not consistent with the notion that fintech firms transmit monetary policy in areas where traveling to a branch is most costly.

growth results. If the aggregate results with respect to retail spending growth, presented in section 5, are indeed driven by the effects of refinancing on borrower wealth, then we might expect consumption growth to be strongest in the counties where refinance growth is strongest. An important caveat to this, however, would be that consumption patterns should only mirror refinance growth if the demographic and geographic traits upon which I sort counties also have no bearing on borrowers' marginal propensities to consume. For example, if sparsely populated rural areas contain residents that are more frugal and less likely to consume out of positive income shocks, then the consumption response could theoretically be muted in these counties, even if fintech firms do cause credit expansions.

With this caveat in mind, I turn to the results of the consumption analysis, with results displayed in Table 12. I first assess whether consumption growth is stronger in areas with smaller White population shares. The results of this analysis are shown in column (1). In this specification, only the coefficient on the *FintechCount*Rates*White* variable is suggestive of a negative relationship between a county's White population share and its consumption growth. That is, in an average interest rate environment, it appears that the racial make-up of a county has no impact of the strength of fintech presence on future consumption growth (given a positive but insignificant coefficient of .0003 on the *FintechCount* variable). However, a 1% increase in interest rates is estimated to generate a negative fintech effect (with a coefficient of -.0088 on the triple-diff coefficient).

Mirroring the refinance results, the estimated fintech effect on consumption growth also appears to be stronger in areas with large Hispanic populations. The second column of Table 12 displays an interaction coefficient between *FintechCount* and a county's Hispanic population of .0011, suggesting that consumption growth related to a marginal increase in the number of active fintech lenders is .11 percentage points higher in a county with a 100% Hispanic population than in a county with a 0% Hispanic share. This disparity becomes more dramatic in falling interest rate environments. The coefficient on the triple-diff interaction term, *FintechCount*Hisp.*Rates* is .0132. This suggests that the strong effects that fintech lenders have on credit growth in counties with large shares of Hispanic borrowers also find their way into spending growth.

In columns (3) and (4) I show how the consumption growth effects of fintech lenders co-vary with bank branch presence. Column (3) displays the branch per capita results. The two interaction coefficients (*FintechCount*Brnch./Pop.* and *FintechCount*Rates*Brnch./Pop.*) detail how the estimated effect of fintech presence changes as we move from counties with zero bank branches to counties with a bank branch for every resident (which of course do not exist). The first of these interaction terms has a coefficient of -.0067, which suggests that moving from a county without branches to one that is fully saturated with branches induces a weakening by .67 percentage points of the effect of a marginal increase in fintech lenders on retail consumption growth. The triple-diff interaction coefficient attains a value of -.0256, suggesting that the differential effect of fintech lending on consumption growth in counties with few bank branches is more pronounced in the year following an interest rate shock. This suggests that fintech lenders are particularly adept at amplifying the stimulative effects of Fed policy in counties where individuals have less access to brick-and-mortar bank branches. These results are mirrored, in column (4), by the interaction regressions sorted by branches per square mile. Again, the interaction terms are negative and statistically significant at the 1% level, suggesting that fintech lending effects on consumption growth are stronger in areas where residents have to travel long distances in order to reach a bank branch.

7 Conclusion

Fintech lenders facilitate the transmission of monetary policy. In falling interest rate environments, when mortgage borrowers have strong incentives to refinance, the presence of fintech lenders increases the aggregate availability of credit. A high regional concentration of fintech lenders also predicts stronger retail spending growth in the year after a widening of interest rate spreads. I find evidence linking the expansionary effects of fintech lending to the ability of these lenders to alleviate various microeconomic frictions in credit markets. The fact that fintech lending predicts rapid credit growth, within 1-3 months after an interest rate shock, gives credence to the idea expressed by Fuster et al. (2019) that fintech lenders alleviate capacity constraints when loan demand is particularly strong, perhaps because automated technology is less easily overwhelmed than human loan officers.

However, the regional patterns associated with fintech monetary amplification suggest that this is not the entire story. The ability of fintech lenders to expand the supply of credit is more pronounced in areas where residents are poorly served by the traditional banking system, either because there simply aren't many physical bank branches, or because traditional banks are worse at accurately evaluating residents' creditworthiness. Thus, the macroeconomic evidence on fintech-induced credit growth is consistent with evidence uncovered by Buchak et al. (2018) and Bartlett et al. (2019). Importantly, these findings suggest fintech lending improves the Fed's ability to stimulate the economy in a downturn, particularly in areas where households' responses to monetary easing are typically muted, due to poor access to financial services.

As fintech lending and the mortgage market evolve, time will tell exactly how the changing technological and institutional environments in this market affect economic policymakers. There has yet to be a true financial crisis in the fintech era, making it unclear how this new class of intermediaries will react in a crisis environment. Since these lenders rely on short-term funding from traditional banks, rather than from more stable deposits, it is conceivable that an evaporation of liquidity in the midst of a credit crunch could have a severe impact on fintech firms. Nonetheless, since fintech lenders sell the mortgages they originate quickly, and are less leveraged than the traditional banking sector, perhaps they are less prone to adverse short-term funding conditions than their bank counterparts. The evidence in this paper suggests, however, that provided these lenders maintain adequate funding sources during a crisis, that their technological advantages provide the Fed with additional ammunition in responding to downturns. Fast and convenient online lending options appear to bolster the supply of credit and allow a broader range of potential borrowers to benefit from the refinancing channel of monetary policy.

References

Agarwal, S., G. Amromin, S. Chomsisengphet, T. Landvoigt, T. Piskorski, A. Seru, and V. Yao, (2020). "Mortgage refinancing, consumer spending and competition: evidence from the Home Affordable Refinancing Program," *Review of Economic Studies*, forthcoming.

Agarwal, S., J. C. Driscoll, and D. Laibson, (2013). "Optimal mortgage refinancing: a closed form solution," *Journal of Money, Credit, and Banking*, vol. 45(4), pages 591-622.

Agarwal, S., R. Rosen, and V. Yao (2013). "Why do borrowers make mortgage refinancing mistakes?" Working paper, No. 2013-02, Federal Reserve Bank of Chicago.

Bahadir, B., and I. Gumus (2016). "Credit decomposition and business cycles in emerging market economies," *Journal of International Economics*, vol. 103(C), pages 250-262.

Balyuk, T., A. N. Berger, and J. Hackney (2020). "What is fueling fintech lending? The role of banking market structure," Emory University Working Paper.

Bartlett, R., A. Morse, R. Stanton, and N. Wallace (2019). "Consumer-lending discrimination in the fintech era", NBER Working Papers 25943.

Bayer, P., F. Ferreira, and S. L. Ross (2018). "What drives racial and ethnic differences in high-cost mortgages? The role of high-risk lenders," *Review of Financial Studies*, vol. 31(1), pages 175-205.

Becker, G. (1957). "The economics of discrimination," Chicago: University of Chicago Press.

Beraja, M., A. Fuster, E. Hurst, and J. Vavra (2019). "Regional heterogeneity and the refinancing channel of monetary policy," *Quarterly Journal of Economics*, vol. 134(1), pages 109-183.

Bernanke, B., and M. Gertler (1995). "Inside the black box: the credit channel of monetary policy transmission," *Journal of Economic Perspectives*, vol. 9(4), pages 27-48.

Bostic, R. (1996). "The role of race in mortgage lending: revisiting the Boston Fed study," Federal Reserve Board of Governors Working Paper.

Browne, L. E., J. McEneany, A.H. Munnell, and G. M. B. Tootell (1996). "Mortgage lending in Boston: interpreting HMDA data," *American Economic Review*, vol. 86(1), pages 25-53.

Buchak, G., G. Matvos, T. Piskorski, and A. Seru (2018). "Fintech, regulatory arbitrage and the rise of shadow banks," *Journal of Financial Economics*, vol. 130(3), pages 453-483.

Burgess, R., and R. Pande (2005). "Do rural banks matter? Evidence from the Indian social banking experiment," *American Economic Review*, vol. 95(3), pages 780-795.

Campbell, J. Y., and J. Cocco (2003). "Household risk management and optimal mortgage choice," *Quarterly Journal of Economics*, vol. 188(4), pages 1449-1494.

Campello, M., J. Gao, and Q. Xu, (2019). "Personal taxes and firm skill hiring: evidence from 27 million job postings," Kelley School of Business Research Paper No. 19-35.

Card, D., and A. Krueger, (1994). "Minimum wages and employment: a case study of the fast-food industry in New Jersey and Pennsylvania," *American Economic Review*, vol. 84(4), pages 772-293.

Cetorelli, N., and P. E. Strahan (2006). "Finance as a barrier to entry: bank competition and industry structure in local U.S. markets," *Journal of Finance*, vol. 61(1), pages 437-461.

Chen, H., M. Michaux, and N. Roussanov (2020). "Houses as ATMs: mortgage refinancing and macroeconomic uncertainty," *Journal of Finance*, vol. 75(1), pages 323-375.

Cheng, P., Z. Lin, and Y. Liu (2015). "Racial discrepancies in mortgage interest rates," *Journal of Real Estate Finance and Economics*, vol. 51(1), pages 101-120.

Chernenko, S., I. Erel, R. Prilmeier (2019). "Why do firms borrow directly from non-banks?" NBER Working Papers 26458.

Cotti, C. D., C. J. Courtemanche, J. C. Maclean, E. T. Nesson, M. F. Pesko, N. Tefft (2021). "The effects of e-cigarette taxes on e-cigarette prices and tobacco product sales: evidence from retail panel data," NBER Working Papers 26724.

Deng, Y., and J. M. Quigley (2012). "Woodhead behavior and the pricing of residential mortgages," NUS Institute of Real Estate Studies Working Paper.

Di Maggio, M., A. Kermani, and C. J. Palmer, (2020). "How quantitative easing works: evidence on the refinancing channel," *Review of Economic Studies*, vol. 87(3), pages 1498-1528.

Drechsler, I., A. Savov, and P. Schnabl (2019). "How monetary policy shaped the housing boom," NBER Working Papers 25649.

Eichenbaum, M., S. T. Rebelo, A. Wong (2018). "State dependent effects of monetary policy: the refinancing channel," NBER Working Papers w25152.

Erel, I., and J. Liebersohn, (2020). "Does fintech substitute for banks? Evidence from the Paycheck Protection Program," NBER Working Papers 27659.

Fuster, A., M. Plosser, P. Schnabl, and J. Vickery (2019). "The role of technology in mortgage lending," *Review of Financial Studies*, vol. 32(5), pages 1854-1899.

Ghent, A. C., R. Hernandez-Murillo, and M. T. Owyang (2014). "Differences in subprime loan pricing across races and neighborhoods," *Regional Science and Urban Economics*, vol. 48(C), pages 199-215.

Gilje, E. P. (2019). "Does local access to finance matter? Evidence from U.S. oil and natural gas shale booms," *Management Science*, vol. 65(1), pages 1-18.

Gopal, M. and P. Schnabl (2020). "The rise of finance companies and fintech lenders in small business lending," New York University Working Paper.

Greenwald, D. (2018). "The mortgage credit channel of macroeconomic transmission," MIT Sloan Research Paper No. 5184-16.

Holmes, T. J., (1998). "The effect of state policies on the location of manufacturing: evidence from state borders," *Journal of Political Economy*, vol. 106(4), pages 667-705.

Hurst, E., and F. Stafford, (2004). "Home is where the equity is: mortgage refinancing and household consumption," *Journal of Money, Credit, and Banking*, vol. 36(6), pages 985-1014.

Jagtiani, J., and C. Lemieux (2018). "Do fintech lenders penetrate areas that are underserved by traditional banks?" Journal of Economics and Business, vol. 100(C), pages 43-54.

Jayaratne, J., and P. E. Strahan (1996). "The finance-growth nexus: evidence from bank branch deregulation," *Quarterly Journal of Economics*, vol. 111(3), 639-670.

Jordà, O. (2005). "Estimation and inference of impulse responses by local projections," *American Economic Review*, vol. 95(1), pages161-182.

Koijen, R., O. Van Hemert, and S. Van Nieuwerburgh (2009). "Mortgage timing," *Journal of Financial Economics*, vol. 93(2), pages 292-324.

LaCour-Little, M. (1999). "Another look at the role of borrower characteristics in predicting mortgage prepayments," *Journal of Housing Research*, vol. 10(1), pages 45-60.

Ladd, H. F. (1998). "Evidence on discimination in mortgage lending," *Journal of Economic Perspectives*, vol. 12(2), pages 41-62.

Ljunquist, A., and M. Smolyansky, (2018). "To cut or not to cut: on the impact of corporate taxes on employment and income," Working Paper.

Mian, A., and A. Sufi (2011). "House prices, home equity-based borrowing, and the household leverage crisis," *American Economic Review*, vol. 101(5), pages 2132-2156.

Mian, A., and A. Sufi (2014a). "House price gains, and U.S. household spending, from 2002 to 2006," NBER Working Papers 20152.

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

Mian, A., A. Sufi, and K. Rao (2014). "Household balance sheets, consumption, and the economic slump," *Quarterly Journal of Economics*, vol. 128(4), pages 1687-1726.

Mian, A., A. Sufi, and E. Verner (2020). "How does credit supply expansion affect the real economy? The productive capacity and household demand channels," *The Journal of Finance*, vol. 75(2), pages 949-994.

Moretti, E., and D. Wilson, (2017), "The effect of state taxes on the geographical location of top earners: evidence from star scientists," *American Economic Review*, vol. 107(7), pages 1858-1903.

Pence, K. M., (2006). "Foreclosing on opportunity: state laws and mortgage credit," *Review of Economics and Statistics*, vol. 88(1), pages 177-182.

Philippon, T. (2016). "The fintech opportunity," NBER Working Papers 22476.

Richard, S. F., and R. Roll (1989). "Pre-payments on fixed-rate mortgage-backed securities," *Journal of Portfolio Management*, vol. 15(3), pages 73-82.

Scharfstein, D., and A. Sunderam (2018). "Market power in mortgage lending and the transmission of monetary policy," Working Paper, Harvard University.

Stanton, R. (1995). "Rational prepayment and the valuation of mortgage-backed securities," *Review of Financial Studies*, vol. 8(3), pages 677-708.

Stulz, R. (2019). "FinTech, BigTech, and the future of banks," NBER Working Papers 26312.

Taylor, J. B. (2007). "Housing and monetary policy," NBER Working Papers 13682.

Tables and Figures

Table 1

This exhibit presents summary statistics on the refinance lending behavior of fintech firms in the county-level HMDA sample. Panel A describes fintech lending by year. The second column, labelled "Total Fintech Refis," displays the aggregate volume of refinance credit supplied by fintech firms, in millions of dollars, in each year of the sample. The third column displays the share of total refinancing credit originated by fintech lenders. The fourth column displays the mean quantity of refinance credit originated by an individual fintech firm, while the fifth column shows the amount originated by the largest lender in the sample. The sixth column gives a count of the number of fintech firms in the sample each year. Panel B displays summary statistics at the county level. I display mean, median, and 90th percentile levels of fintech activity. The second through fourth columns describe the number of fintech lenders that operate in individual counties (e.g. the median column displays the median number of fintech lenders that originate a home refinance loan in a given county during each year of the sample). Columns 5-7 display the total value of fintech refinance loans, in millions, at the county level, while columns 8-10 show market shares of fintech lenders in county-level markets for home refinance credit.

Panel A
Fintech Summary Statistics by Year

Year	Total Fintech Refis ($ Millions)	Fintech Share of Total Refis	Mean Fintech Firm Refis	Max Fintech Firm Refis	Count of Fintech Firms	Largest Fintech Lender
2010	41,393	.043	3,449	24,987	12	Quicken Loans
2011	44,969	.051	2,811	27,147	15	Quicken Loans
2012	105,596	.074	5,866	65,045	17	Quicken Loans
2013	112,763	.107	6,265	68,966	17	Quicken Loans
2014	75,461	.150	3,972	45,230	17	Quicken Loans
2015	115,988	.151	6,105	59,833	17	Quicken Loans
2016	152,853	.162	7,643	71,050	18	Quicken Loans
2017	111,323	.186	5,060	57,049	20	Quicken Loans
2018	87,871	.142	3,661	48,716	22	Quicken Loans
2019	182,840	.153	7,618	101,503	22	Quicken Loans

Panel B
Fintech Summary Statistics by County and Year

	Count of Fintech Firms			Fintech Lending ($ Millions)			Fintech Market Share		
Year	Mean	Median	90th Pctl.	Mean	Median	90th Pctl.	Mean	Median	90th Pctl.
2010	2.68	2	6	13.01	1.21	21.59	.050	.043	.095
2011	3.89	3	8	14.10	1.49	23.97	.060	.053	.109
2012	4.75	4	10	33.15	2.23	54.29	.069	.061	.121
2013	5.60	5	11	35.34	3.00	61.72	.105	.098	.177
2014	5.91	5	12	23.74	2.66	42.99	.168	.161	.270
2015	6.39	6	13	36.44	3.23	57.77	.166	.157	.264
2016	6.92	6	13	47.94	3.77	75.45	.170	.164	.267
2017	7.27	6	15	34.90	3.37	59.52	.197	.192	.307
2018	7.53	6	15	27.51	3.05	49.36	.187	.170	.306
2019	7.88	7	16	57.26	4.43	89.32	.180	.166	.294

Table 2

This table shows summary statistics for the county-level HMDA dataset merged with demographic and economic information from the US Census, and other sources. The sumary statistics show the average, median, standard deviation, 1st and 3rd quartiles and the 90th percentile value of each variable. The "Observations" column displays the number of non-missing observations in the merged data set. "Total Loans" shows the total volume of all loans (for purchase or refinance) in millions of dollars, while "Total Refis" describes the volume of refinance loans. "FHA Share" describes the share of refinance loans that are FHA-guaranteed, while "Jumbo Share" describes the share of loans that exceed the conforming limit. "No. Bank Branches" gives the number of physical brick-and-mortar bank branch establishments in the county, as given by the FDIC's Summary of Deposits data. "Branches Per Cap." gives the total number of bank branches on a per capita basis, while "Branches/Mi Sq." gives the total number of bank branches per square mile. "Pct. w/ Mortgage" describes the share of a county's home owners with an outstanding mortgage balance, while "Pct. Renting" gives the proportion of a county's households that rent their homes. "Pct. Black," "Pct. White," and "Pct. Hispanic" give the share of a county's population that identify as Black or African American, White, or Hispanic/Latino of any race. "Pct. College Degree" describes the proportion of the population with at least a bachelor's degree, while "Pct. Over 65" describes the proportion of a county's population over the age of 65.

Summary Statistics at the County-level							
	Observations	Mean	Quartile1	Median	Quartile3	90th Pctl.	St. Dev.
Total Loans	31862	560.3	15.45	54.48	227.0	1014	2626
Total Refis	31862	275.7	6.837	24.11	101.5	442.6	1505
FHA Share	31862	0.271	0.182	0.255	0.339	0.434	0.130
Jumbo Share	31862	0.048	0	0.023	0.058	0.125	0.080
Population	31210	102,218	11,167	25,995	68,053	206,295	326,268
Avg. Wage	31096	36,490	30,590	34,800	40,180	47,300	9,654
Employment/Pop.	31210	0.444	0.397	0.445	0.492	0.531	0.072
Unemployment Rate	31210	0.063	0.041	0.057	0.08	0.104	0.029
Pop. Density	31209	269.2	17.17	45.26	117.35	396.3	1781
No. Bank Branches	30987	29.99	5	11	23	61	75.50
Branches Per Cap.	30987	0.466	0.279	0.381	0.543	0.829	0.304
Branches/Mi Sq.	30987	0.086	0.008	0.018	0.041	0.119	0.623
Poverty Rate	8179	0.144	0.104	0.140	0.178	0.214	0.056
Pct. w/ Mortgage	7937	0.634	0.594	0.625	0.673	0.725	0.069
Pct. Renting	7747	0.142	0.019	0.037	0.265	0.336	0.141
Pct. Black	7746	0.110	0.025	0.065	0.147	0.271	0.124
Pct. White	7872	0.807	0.766	0.836	0.891	0.931	0.130
Pct. Hispanic	8179	0.115	0.037	0.066	0.135	0.263	0.132
Pct. College Degree	8179	0.293	0.210	0.278	0.358	0.451	0.114
Pct. Over 65	8179	0.152	0.126	0.148	0.171	0.197	0.042

Table 3

This exhibit shows coefficients and standard errors from estimating equation (1). Panel A expresses fintech presence as the average fintech refinance market share, from 2010-2019. Panel B expresses fintech presence as the average total number of fintech lenders that originate refinance loans in a county. The variables labeled as “HMDA Mortgage Variables” consist of mortgage market characteristics taken from the HMDA data. The variables are defined as described in Table 2. “Demographic Variables” consist of county-level information for which annual data are available for the majority of counties. “Log Population” is the natural log of a county’s total population; “Avg. Wage” is the average income for employed persons in a county; “Employment/Pop.” is a county’s employment/population ratio. “Log Pop. Density” is the natural logarithm of a county’s population density (population divided by square mile land area). “ACS Mortgage Variables” and “ACS Demographic Variables” are taken from the American Community Survey (ACS). The ACS Demographic variables describe a county’s poverty rate, and the racial and ethnic composition of its residents. “Pct. College Degree” and “Pct. Over 65” are the share of a county’s population with at least a bacehlor’s degree, and the percentage of a county’s population over the age of 65, respectively. The variables labeled as “Bank Branch Presence” come from the FDIC’s Summary of Deposits (SOD) database. In both panels, standard errors are listed in parentheses. Coefficient significance levels of 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

Panel A
Regressing Average Fintech Refinance Share on County-level Characteristics

		(1)	(2)	(3)	(4)
HMDA Mortgage Variables	FHA Share	.148***	.169***	.134***	.155***
		(.006)	(.004)	(.008)	(.005)
	Jumbo Loan Share	.116***	-.053***	-.041***	-.060***
		(.010)	(.007)	(.011)	(.007)
Demographic Variables	Log Population		.003***	.005***	-.0001
			(.001)	(.001)	(.001)
	Avg. Wage		.078***	.032***	.069***
			(.003)	(.005)	(.003)
	Employment/Pop.		-.464***	-.227***	-.426***
			(.009)	(.023)	(.010)
	Unemployment Rate		-1.348***	-.914***	-1.382***
			(.022)	(.034)	(.022)
	Log Pop. Density		-.009***	-.004***	-.011***
			(.001)	(.001)	(.001)
ACS Mortgage Variables	Pct. Mortgage	-.038***		-.025**	
		(.010)		(.010)	
	Pct. Renting	-.241***		-.126***	
		(.005)		(.006)	
ACS Demographic Variables	Poverty Rate			.040**	
				(.016)	
	Pct. White			-.077***	
				(.008)	
	Pct. Black			-.035***	
				(.009)	
	Pct. Hispanic			.087***	
				(.006)	
	Pct. College Degree			.017**	
				(.009)	
	Pct. Over 65			.442***	
				(.020)	
Bank Branch Presence	Branches Per Cap.				-.021***
					(.001)
	Branches Per Mi Sq.				.036***
					(.004)

Panel B					
Regressing Fintech Firm Count on County-level Characteristics					
		(1)	(2)	(3)	(4)
HMDA Mortgage Variables	FHA Share	3.614***	2.184***	3.165***	1.077***
		(.387)	(.126)	(.421)	(.125)
	Jumbo Loan Share	18.325***	4.653***	3.471***	5.541***
		(.630)	(.191)	(.610)	(.189)
Demographic Variables	Log Population		2.347***	1.923***	1.963***
			(.019)	(.061)	(.020)
	Avg. Wage		1.144***	1.244***	1.156***
			(.071)	(.253)	(.071)
	Employment/Pop.		-7.047***	-2.010	-3.395***
			(.246)	(1.261)	(.252)
	Unemployment Rate		-45.66***	-53.34***	-45.86***
			(.600)	(1.871)	(.586)
	Log Pop. Density		-.112***	-.451***	.102***
			(.015)	(.044)	(.017)
ACS Mortgage Variables	Pct. Mortgage	4.943***		-.111	
		(.646)		(.556)	
	Pct. Renting	-13.03***		-7.201***	
		(.306)		(.330)	
ACS Demographic Variables	Poverty Rate			-3.889***	
				(.904)	
	Pct. White			1.053**	
				(.427)	
	Pct. Black			5.333***	
				(.508)	
	Pct. Hispanic			6.676***	
				(.324)	
	Pct. College Degree			-.157	
				.473	
	Pct. Over 65			12.35***	
				(1.100)	
Bank Branch Presence	Branches Per Cap.				-1.364***
					(.033)
	Branches Per Mi Sq.				-2.330***
					(.110)

Table 4

This exhibit displays the results of estimating equation (2) using the merged county-level HMDA sample. Panel A displays results where the *Fintech* variable from equation (2) is defined as the lagged count of fintech firms active within a county. The table displays the coefficients on the *FintechCount* variable and the interaction between *FintechCount* and the *Rates* variables, as described in equation (2). Panel B displays results where the right-hand side *Fintech* variable is the lagged refinance market share of fintech firms in the county (*FintechShare*). In each panel, the columns display results across specifications which differ on the basis of the set of included controls, and the inclusion or exclusion of county fixed-effects. The set of controls labeled as "Baseline" includes the set of mortgage market and demographic/economic controls labelled as "HMDA Mortgage Variables" and as "Demographic Variables" in Table 3, plus average credit scores from Fannie Mae data. The specifications where the set of controls is labeled as "Full," adds to the baseline set of controls by including information from the American Community Survey (i.e. the variables labeled as "ACS Mortgage Variables" and "ACS Demographic Variables" in Table 3). I list the number of observations and adjusted R-squared values for each regression at the bottom of each of the panels. Adjusted R-squared values for the county fixed-effects specifications are calculated net of fixed-effects (i.e. they are estimated after the data has been de-meaned by county). I list Huber-White standard errors in parentheses beneath each coefficient. Significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

Panel A

Dependent Variable: First Difference of Log Refi Volume

	(1)	(2)	(3)	(4)
FintechCount	.006***	.004***	.004***	.008***
	(.001)	(.001)	(.001)	(.002)
FintechCount*Rates	.013***	.016***	.023***	.030***
	(.001)	(.004)	(.001)	(.002)
Controls	Baseline	Full	Baseline	Full
N	27760	6484	27760	6484
Adj. R-squared	.897	.912	.555	.849
Year Fixed Effects	✓	✓	✓	✓
County Fixed Effects			✓	✓

Panel B

Dependent Variable: First Difference of Log Refi Volume

	(1)	(2)	(3)	(4)
FintechShare	.361***	.475***	.571***	1.09***
	(.030)	(.064)	(.053)	(.117)
FintechShare*Rates	.234***	.598***	.455***	.603***
	(.055)	(.113)	(.058)	(.121)
Controls	Baseline	Full	Baseline	Full
N	27760	6484	27760	6484
Adj. R-squared	.896	.912	.501	.830
Year Fixed Effects	✓	✓	✓	✓
County Fixed Effects			✓	✓

Table 5

This exhibit depicts results from the estimation of equation (2) on subsamples of the county-level HMDA dataset, sorted into quartiles based on county population. Panel A depicts results that use the *FintechCount* variable as the key regressor, while Panel B shows analogous results for the *FintechShare* variable. Tables present coefficients on the *Fintech* variable and the *Fintech*Rates* interaction term as described in equation (2). All regressions are estimated with the set of "Baseline" controls (and do not include the extended ACS variables). Each column contains results for a different population subsample, with the labels Q1-Q4 indicating the population quartile referenced in each specification. Huber-White standard errors are listed beneath each coefficient and significance at the 10%, 5%, and 1% levels are denoted by *, **, and ***, respectively.

Panel A				
Dependent Variable:	Log Refi Volume (First-Difference)			
	Q1	Q2	Q3	Q4
FintechCount	.002	.001	.005***	.005***
	(.002)	(.002)	(.001)	(.001)
FintechCount*Rates	.010	.016***	.016***	.016***
	(.010)	(.002)	(.002)	(.002)
N	6822	6973	6981	6984
Adj. R-squared	.276	.573	.733	.857
Year Fixed Effects	✓	✓	✓	✓

Panel B				
Dependent Variable:	Log Refi Volume (First-Difference)			
	Q1	Q2	Q3	Q4
FintechCount	.200***	.382***	.458***	.478***
	(.045)	(.055)	(.054)	(.059)
FintechCount*Rates	-.263***	-.039	.028	.439***
	(.083)	(.094)	(.096)	(.109)
N	6822	6973	6981	6984
Adj. R-squared	.475	.701	.803	.910
Year Fixed Effects	✓	✓	✓	✓

Table 6

This table displays results of estimating equation (5) using data from HMDA. Data are expressed at the state-year-firm level. Each column of this table denotes a different specification, containing various combinations of regulatory policy variables. The dependent variable takes a value of one if a firm has entered state j by year t, and a value of zero otherwise. The first regulatory variable, "Application Cost" is the cost, in dollars, of submitting an application to become a licensed mortgage originator in state j. "Qualitative Reqs Count," is a simple count of the number of significant qualitative requirements on a state's application checklist. "Net Worth Requirement" is the minimum level of capital required of non-bank lenders. "Brick & Mortar" is a dummy variable that takes a value of one if state j requires licensed lenders to maintain a physical branch presence in the state. The row labeled "AIC" reports the Akaike Information Criterion associated with the regression. *, **, and *** represent statistical significance (via a Z-statistic) at the 10%, 5%, and 1% levels, respectively. All models are estimated via a maximum likelihood approach.

	Predicting Firm Market Entry by State Logistic Regression Results							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Population	.415***	.337***	.350***	.356***	.373***	.382***	.388***	.403***
	(.027)	(.027)	(.028)	(.028)	(.028)	(.029)	(.029)	(.029)
Application Cost	-.346***					-.278***	-.265***	-.241***
	(.042)					(.046)	(.047)	(.047)
Qualitative Reqs Count		-.065***	-.064***	-.068***	-.068***	-.040***	-.040***	-.045***
		(.010)	(.010)	(.010)	(.010)	(.011)	(.011)	(.011)
Net Worth Requirement				-.0006***	-.0007***			-.0006***
				(.0002)	(.0002)			(.0002)
Brick & Mortar			-.263***		-.319***		-.172*	-.252**
			(.097)		(.097)		(.099)	(.100)
N	8544	8544	8544	8544	8544	8544	8544	8544
AIC	8808	8830	8825	8818	8810	8796	8795	8786
Fixed Effects:								
Year	✓	✓	✓	✓	✓	✓	✓	✓

Table 7

This table displays the results of the cross-border analysis of section 4. Each column displays coefficients attained from estimating a version of equation (6). Here, I display the coefficients on *Treat* and the *Treat*Rates* interaction terms from these models. The specifications displayed here vary along three dimensions, including the distance cutoff used for sample inclusion (with "Bandwidth" denoting this maximum distance, in miles), the set of fixed effects, and the inclusion or exclusion of counties containing large urban areas (defined as MSAs in the top 100 by population) as denoted by the "Sample Subset" row.

Cross-Border Analysis of US States Dependent Variable: First Difference of Log Refi Volume								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat	.010	.024**	.020***	.033***	.014**	.019*	.018***	.033***
	(.007)	(.010)	(.006)	(.009)	(.007)	(.011)	(.006)	(.010)
Treat*Rates	.055***	.055***	.015***	.014***	.063***	.064***	.020***	.020***
	(.005)	(.005)	(.004)	(.004)	(.003)	(.003)	(.003)	(.003)
Sample Subset	All	All	Excl. Large Metros	Excl. Large Metros	All	All	Excl. Large Metros	Excl. Large Metros
Bandwidth (Mi.)	50	50	50	50	100	100	100	100
Fixed Effects:								
Year	✓	✓	✓	✓	✓	✓	✓	✓
State	✓	✓	✓	✓	✓	✓	✓	✓
Border		✓		✓		✓		✓

Table 8

This table displays results from estimating versions of equation (7), with each column of the table presenting results from a separate specification. The dependent variables are total retail spending (columns (1) and (2)) and discretionary retail spending (columns (3) and (4)), as defined in Section 5.1. Within a dependent variable, the specifications differ in their inclusion of county fixed effects (with columns (2) and (4) displaying results where these fixed-effects are included). The rows of the table display coefficients of the *FintechCount* variable and the *FintechCount*Rates* interaction term. Huber White standard errors are displayed in parentheses beneath each coefficient. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Response of Retail Spending to Fintech Presence Dependent Variables: First Difference of Log Retail Spending				
	Total Retail		Discretionary Retail	
	(1)	(2)	(3)	(4)
FintechCount	.0020***	.0009**	.0020***	.0009
	(.0001)	(.0004)	(.0002)	(.0004)
FintechCount*Rates	.0018***	.0019***	.0021***	.0023***
	(.0003)	(.0003)	(.0003)	(.0003)
N	10766	10766	10766	10766
Adj. R-squared	.622	.429	.602	.427
Year Fixed Effects	✓	✓	✓	✓
County Fixed Effects		✓		✓

Table 9

This table displays the association between fintech lending and local small business outcomes at the county level, with results generated via estimation of equation (8). Each column represents a different regression specification, which differ on the basis of their outcome variable and the inclusion or exclusion of county fixed-effects. The table reports coefficients on the *FintechCount* and *FintechCount*Rates* variables. "Estab. NonTr." is defined as the number of small business establishments in the non-tradable sector. "Emp. NonTr." describes the total employment in non-tradable sector small businesses. "Loan Count" expresses the total number of small business loans (to businesses with assets under $1 million), while "Loan Vol" is the the dollar volume of such loans. All variables are expressed as annual growth rates (i.e. one-year log differences). Standard errors are displayed in parentheses beneath each coefficient. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Response of Local Small Business Variables to Fintech Presence Dependent Variables Expressed as Log Differences (t to t+1)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Estab NonTr.	Estab NonTr.	Emp. NonTr.	Emp. NonTr.	Loan Count	Loan Count	Loan Vol	Loan Vol
FintechCount	.0031***	.0062***	.0036***	.0043***	.0025***	.0051***	.0007	.0057***
	(.0003)	(.0006)	(.0005)	(.0010)	(.0003)	(.0005)	(.0006)	(.0013)
FintechCount*Rates	.0021***	.0085***	.0044***	.0119***	.0018***	.0023***	.0051***	.0042**
	(.0004)	(.0004)	(.0006)	(.0007)	(.0004)	(.0005)	(.0010)	(.0011)
N	24611	24611	24611	24611	21563	21563	21563	21563
Adj. R-squared	.771	.059	.720	.063	.267	.061	.036	-.001
Fixed Effects:								
Year	✓	✓	✓	✓	✓	✓	✓	✓
County		✓		✓		✓		✓

Table 10

This table displays results from estimating equation (9) using a number of dependent variables. Each column displays results from a different specification, where each specification differs on the basis of its outcome variable. Dependent variables are expressed as shares of total county-level refinance credit, and first differences are taken. Thus, "Non-White Share" refers to the first difference in the share of refinance loans that went to non-White borrowers, with the share calculated as the total volume of loans to non-white borrowers divided by the total volume of refinance loans for the county. "Hispanic Share" is analogously defined for Hispanic/Latino borrowers. "FHA Loans" refers to the share of FHA guaranteed loans. "Junior liens" refers to loans backed by a subordinate lien on the property (i.e. not a first lien mortgage). Coefficients are displayed for the *FintechCount* and *FintechCount*Rates* interaction terms. Standard errors are displayed beneath each coefficient. *, **, and *** denote statistical significance at the 10%, 5%, and 1%, levels, respectively.

Refinance Credit Composition Regressions				
Dependent Variables: Percentage Point Change of Refinance Composition				
	Non-White Share	Hispanic Share	FHA Loans	Junior Liens
	(1)	(2)	(3)	(4)
FintechCount	.0020***	.0017***	-.0004***	.0002***
	(.0002)	(.0001)	(.00001)	(.0001)
FintechCount*Rates	.0049***	.0028***	.0010***	.0002***
	(.0002)	(.0001)	(.0001)	(.0001)
N	27760	27760	27760	27760
Adj. R-squared	.026	.014	.068	.010
Year Fixed Effects	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓

Table 11

This table displays results generated by estimating equation (10). Each column of the table displays results of a different variant of equation (10), where the sample is sorted into quartiles according to a different county-level characteristic. The dependent variable in each equation is the log growth of refinancing from time t-1 to time t. The rows of the table display coefficients associated with the *FintechCount* variable interacted with quartile indicator functions (variables which take a value of one if a county is in a given quartile of the distribution, as sorted by a particular trait, and zero otherwise) and the *FintechCount*Rates* interaction terms, also interacted with quartile indicators (the *FintechCount*Rates* interaction terms are denoted as "Count*Rates" below, to save space). The term "Q1" in the leftmost column denotes the quartile indicator associated with the bottom quartile of the distribution; "Q2" represents the second quartile, and so on. The first column, labeled "% White" indicates that counties are sorted based on their White population. The next column, labeled "% Hispanic," displays results where counties are sorted according to their percentage of Hispanic/Latino residents. The third column, labeled "Pop. Density," displays results from the population density-sorted sample. The last two columns display results where counties are sorted by the number of bank branch locations that they contain. "Branches/Pop." denotes the number of branches per capita, while "Branches/Mi Sq." denotes the number of bank branches per square mile. Standard errors are shown in parentheses beneath each coefficient. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Refinance Results Sorted by County Traits Dependent Variable: Log Refi Volume (First-Difference)	% White (1)	% Hispanic (2)	Pop. Density (3)	Branches/Pop. (4)	Branches/Mi Sq. (5)
FintechCount*Q1	.006***	.003***	.010***	.009***	.007***
	(.001)	(.001)	(.001)	(.001)	(.001)
FintechCount*Q2	.007***	.005***	.008***	.001	.004***
	(.001)	(.001)	(.001)	(.001)	(.001)
FintechCount*Q3	.006***	.006***	.007***	.005***	.002***
	(.001)	(.0004)	(.001)	(.001)	(.001)
FintechCount*Q4	.001	.006***	.006***	.001	.003*
	(.002)	(.0004)	(.0004)	(.003)	(.002)
Count*Rates*Q1	.013***	.002***	.018***	.009***	.012***
	(.001)	(.001)	(.003)	(.001)	(.001)
Count*Rates*Q2	.012**	.009***	.016***	.013***	.008***
	(.001)	(.001)	(.002)	(.001)	(.001)
Count*Rates*Q3	.010***	.009***	.016***	.007***	.004***
	(.001)	(.001)	(.001)	(.002)	(.001)
Count*Rates*Q4	.005	.009***	.011***	.006	.015***
	(.004)	(.001)	(.001)	(.006)	(.003)
Count Q4-Q1	-.005**	.003***	-.004***	-.008**	-.004***
Count*Rates Q4-Q1	-.008*	.007***	-.007***	-.003	.003
N	27760	27760	27760	27760	27760
Adj. R-squared	.903	.914	.899	.902	.900

Table 12

This table displays results from estimating equation (11). The dependent variable is the log change in discretionary spending, with discretionary spending described in Section 5.1. Each column shows results from a different specification, where specifications vary based on the county-level characteristic interacted with fintech variables. Column (1) displays results where fintech variables are interacted with the percentage of White residents in a county. The "*FintechCount*White*Rates*" variable denotes the "triple-diff" interaction between interest rate spreads, the count of fintech lenders, and the White population share of a county. Similarly, "Hisp." denotes a county's Hispanic/Latino population share, with "*FintechCount*Hisp.*Rates*" denoting the triple-diff interaction between the local count of fintech lenders, interest rate spreads, and a county's Hispanic/Latino population. The terms "Brnch./Pop" and "Brnch./Mi. Sq." denote the the number of bank branches per-capita, and the number of bank branches per square mile. Coefficients relating to these quantities are displayed in columns (3) and (4), respectively. Standard errors are displayed in parentheses beneath each coefficient. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Heterogeneous Consumption Responses to Fintech Dependent Variable: First Difference of Log Discretionary Spending				
	(1)	(2)	(3)	(4)
FintechCount	.0016***	.0016***	.0034***	.0019***
	(.0003)	(.0002)	(.0002)	(.0002)
FintechCount*Rates	.0086***	-.0006	.0090***	.0024***
	(.0014)	(.0004)	(.0007)	(.0003)
FintechCount*White	.0003			
	(.0004)			
FintechCount*White *Rates	-.0088***			
	(.0017)			
FintechCount*Hisp.		.0011***		
		(.0003)		
FintechCount*Hisp. *Rates		.0132***		
		(.0017)		
FintechCount *Brnch./Pop.			-.0067***	
			(.0005)	
FintechCount *Brnch./Pop.*Rates			-.0256***	
			(.0022)	
FintechCount *Brnch./Mi. Sq.				-.0001***
				(.00003)
FintechCount* Brnch./Mi. Sq.*Rates				-.0003***
				(.0001)
N	10766	10766	10766	10766
Adj. R-squared	.605	.606	.610	.604

Figure 1

This figure displays the geographic profile of fintech activity from 2010-2019. Counties are shaded to reflect sample averages of the number of active fintech lenders (on top) and the fintech market share of refinancing loans (on the bottom). Darkly shaded areas represent counties with high fintech activity, while lighter-shaded areas have less fintech activity. Small counties with inconsistent market activity (i.e. counties without mortgage refinancing in every year of the study) are dropped from the sample. Legend labels on top display the range of average fintech counts for counties shaded in a given color, while legend labels on bottom give the equivalent range for fintech market shares.

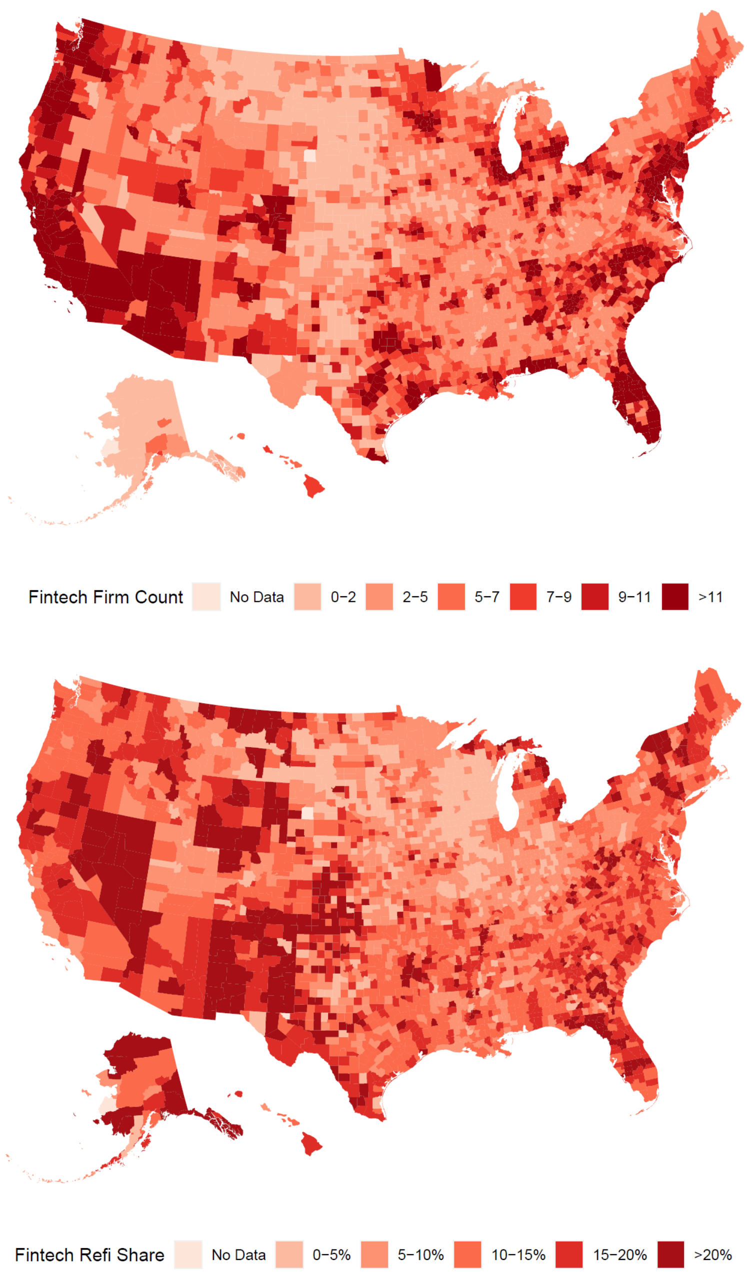


Figure 2

This figure displays impulse responses generated from estimating equations (3) and (4) using data from the Fannie Mae Single Family Loan Performance dataset. Panels A and B of this figure show impulse responses from equation (3). The impulse responses plot the behavior of refinancing activity in response to an increase in fintech lending. The blue line in panels A and B plots the sum of the coefficients γ^h and δ^h on the *Fintech* variable and the *Fintech*Rates* interaction. Impulse responses are plotted over time horizons ranging from 1-5 months. Panel A shows impulse responses estimated from equations which omit ZIP code fixed-effects, while Panel B includes these fixed-effects. Dotted blue lines display the 95 percent confidence intervals for the impulse responses. Panels C and D display impulse responses generated from equation (4). They display the coefficients on the interaction between the *Fintech* and *OutstandingStock* variables, over 1-5 month time horizons.

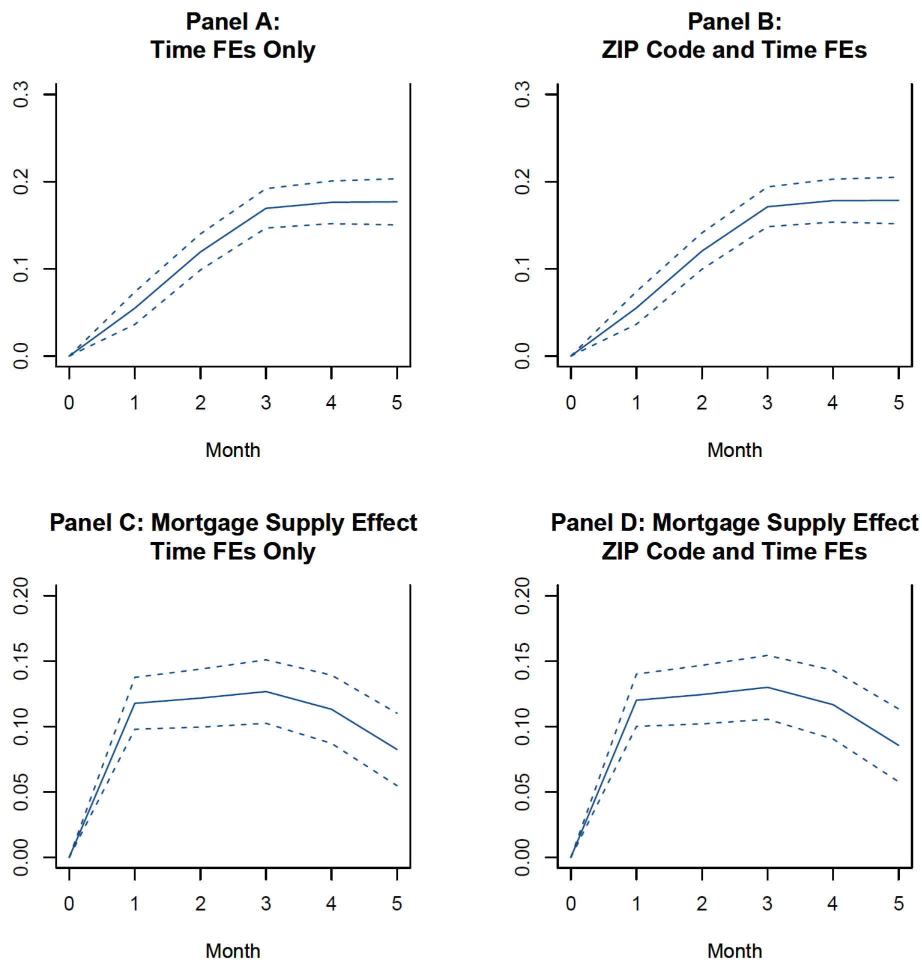


Figure 3

This figure gives an example of the progression with which some fintech lenders (i.e. those that were smaller and less established early on in the sample) entered state mortgage markets. The pictures below show the timing of market entry by a single firm, CashCall Mortgage, during the first half of the sample, from 2010-2015 (at which point it had begun originating mortgage loans in almost every state). The figure was generated using loan-level information from the Home Mortgage Disclosure Act database. States shaded in red represent the set of states in which CashCall originated a positive dollar value of mortgages in a given year, while those in white represent states in which CashCall did not make any loans.

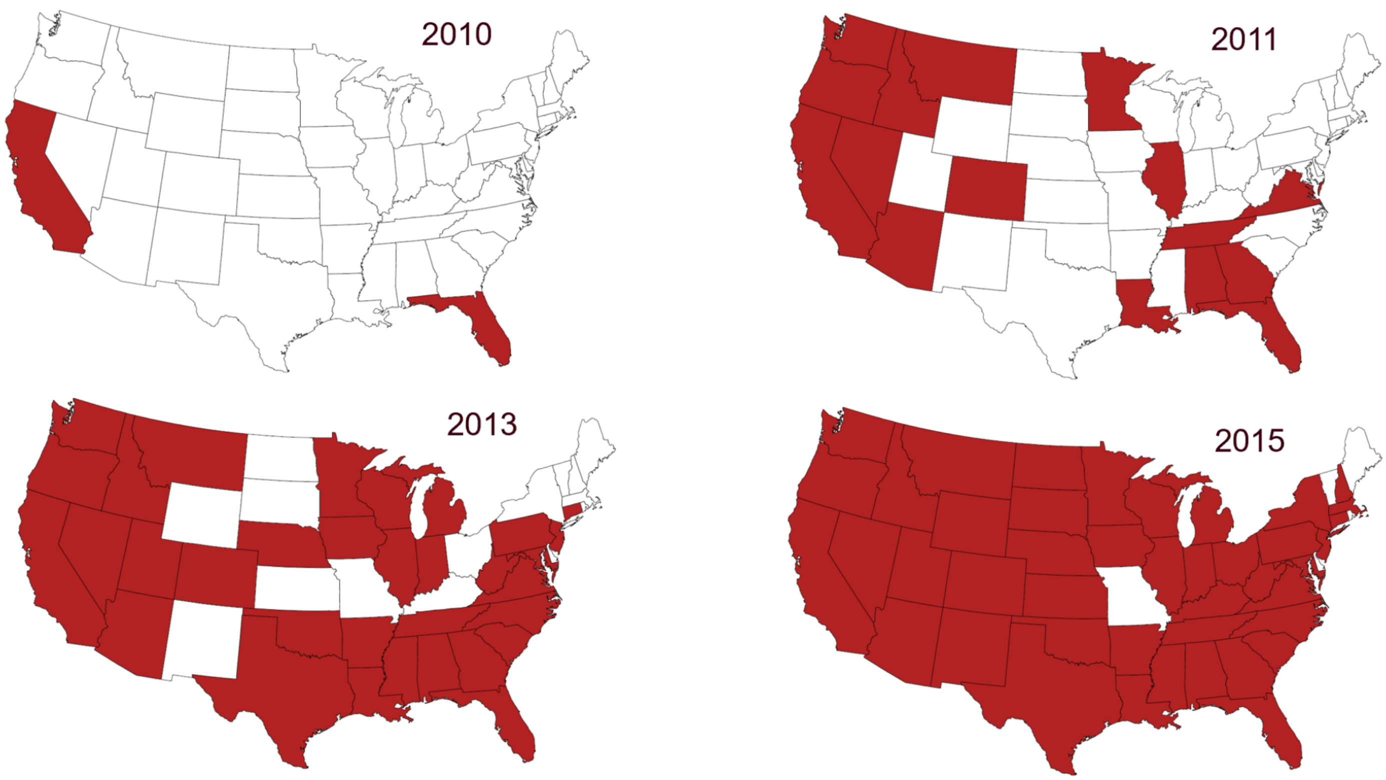


Figure 4

This figure displays the aggregate analogue of Figure 3. Itshows the total number of fintech firms that have entered each state, by year, matching the years displayed in Figure 3. States with darker shading have a larger number of active fintech firms. The figure was generated using loan level HMDA data. The numbers next to each legend label give the number of firms in states colored with a given hue. In 2013, the dispersion in state-level fintech counts is rather wide, an accordingly, states with 15 or more fintech lenders are labeled as ">=15."

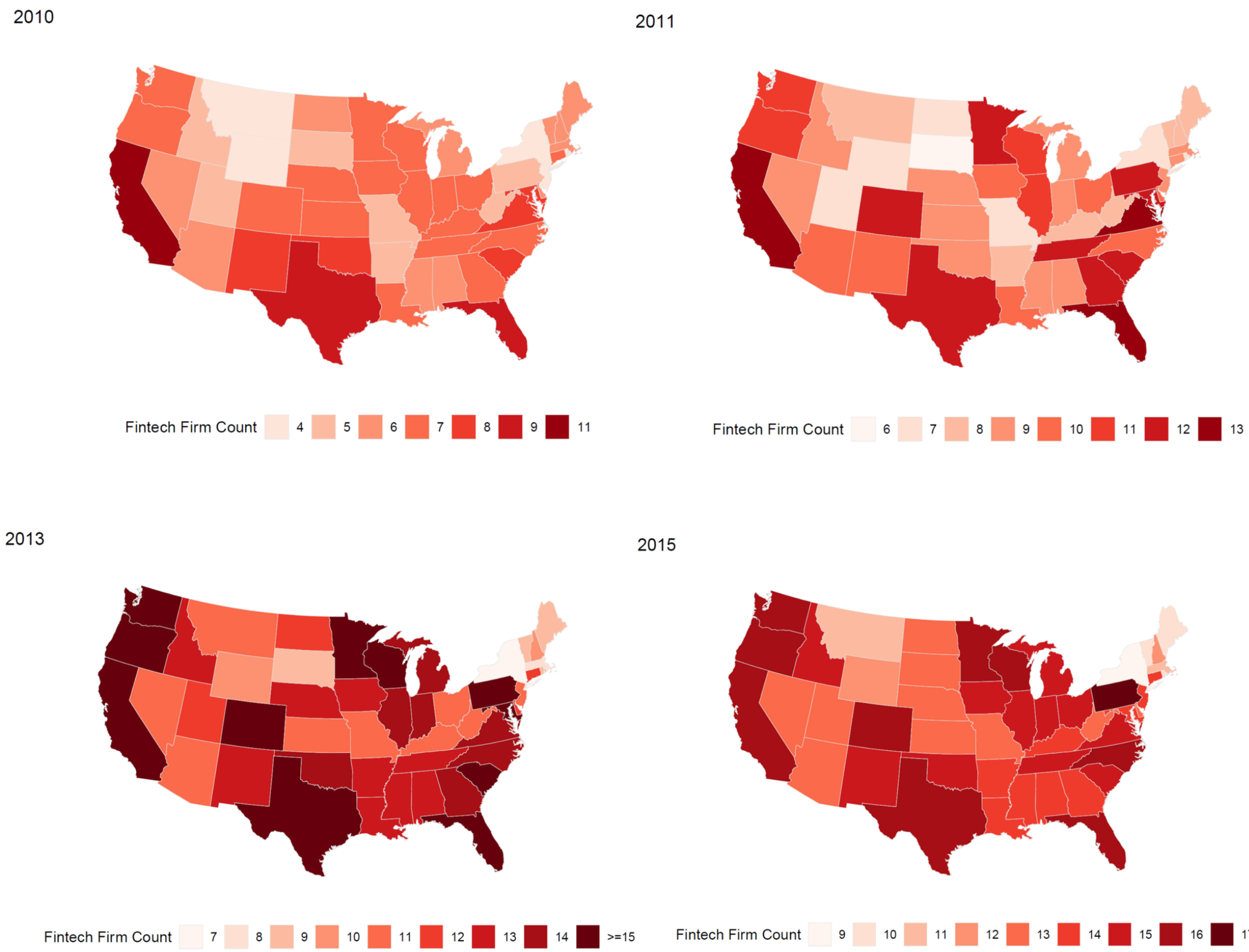
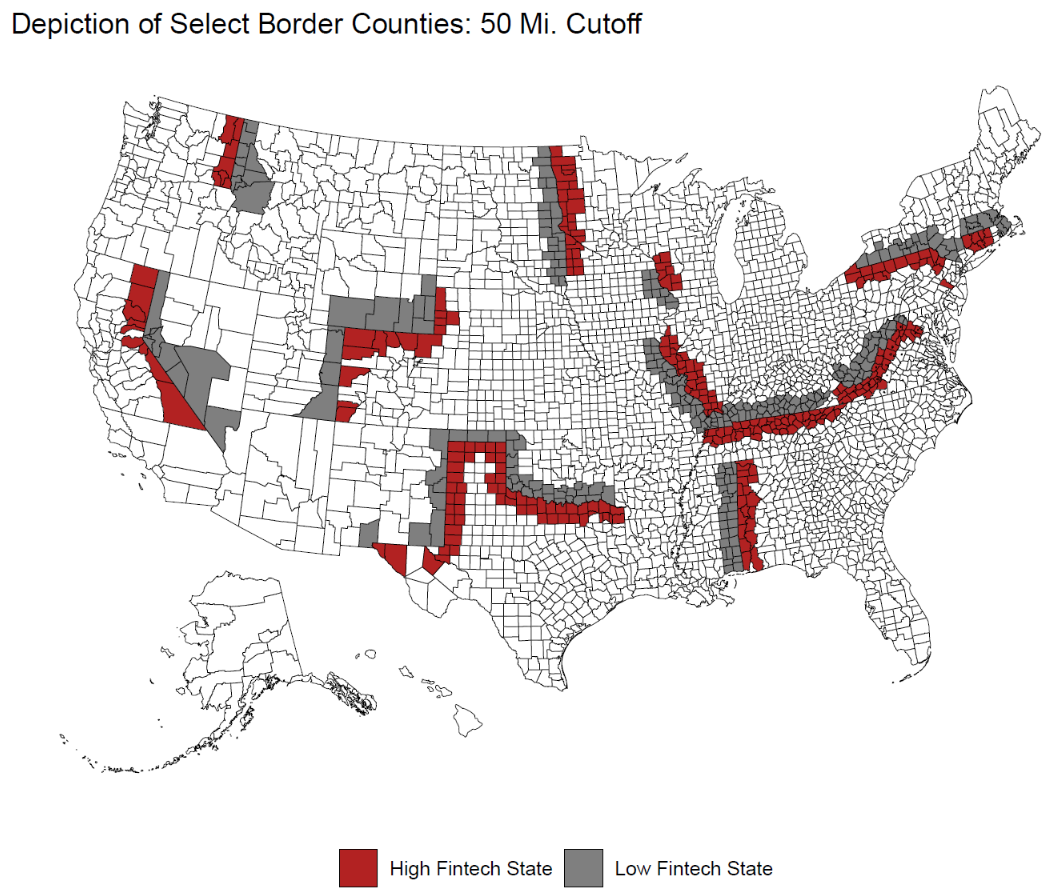


Figure 5

This figure gives intuition for the empirical approach discussed in Section 4.2 and estimated via equation (6). It illustrates how sample counties are selected given a group of treated and control states. State pairs are selected by identifying the set of bordering states that have different numbers of active fintech firms in a given year. To generate this figure, I selected a group of treatment and control states that appear in the sample on a number of occasions, though not necessarily in all years. Counties residing in a treated state in this example are shaded in red, while counties in a paired control state are shaded in gray. Only counties located within 50 miles of their paired state border are shaded. Border distances are determined using population centroids in each county. Some counties located on state borders (in western states with large counties) are nonetheless excluded from the sample if most of their population lives more than 50 miles away from the shared border.



Appendix

Table 1

This table displays pairwise correlations between variables that comprise the merged county-level HMDA panel. Panel A shows pairwise correlations between the main variables that comprise the "Baseline" sample, which includes county-level demographic information from sources other than the American Community Survey. Variable names follow the naming conventions of Table 2 and Table 3. Panel B displays pairwise correlations between the main variables comprising the extended set of controls, including obsevables derived from American Community Survey data. See Table 2 and Table 3 in the set of main exhibits for variable definitions.

Panel A
Correlations: Baseline Controls Sample (W/o Census ACS Data)

	Ft. Refi	Ft. Count	Pop.	Wage	Unem.	Emp./Pop.	Dens.	FHA	Jumbo	Br./Pop.	Br./Mi
Fntch. Share	1										
Fntch. Count	0.220	1									
Log-Pop.	-0.052	0.750	1								
Avg. Wage	0.131	0.391	0.361	1							
Unemp.	-0.256	-0.248	0.067	-0.240	1						
Emp./Pop.	-0.099	0.125	0.018	0.263	-0.575	1					
Pop. Density	-0.028	0.142	0.255	0.267	-0.008	0.064	1				
FHA Share	0.303	0.093	0.030	-0.014	-0.048	-0.192	-0.070	1			
Jumbo Shr.	-0.099	0.179	0.094	0.181	-0.161	0.188	0.176	-0.368	1		
Brnch/Pop.	-0.085	-0.457	-0.564	-0.175	-0.244	0.358	-0.074	-0.189	0.077	1	
Brnch/Mi sq.	-0.037	0.106	0.198	0.263	-0.012	0.072	0.913	-0.079	0.180	-0.030	1

Panel B
Correlations: Full Controls Sample (Counties with Available ACS Data)

	Ft. Refi	Ft. Count	Poverty	% Mtge.	% Rent	% Black	% White	% Hisp.	% >65	% Coll.
Fntch. Share	1									
Fntch. Count	0.544	1								
Poverty Rate	-0.082	-0.266	1							
Pct. Mtge.	-0.229	-0.111	-0.099	1						
Pct. Rental	-0.550	-0.516	0.314	0.336	1					
Pct. Black	0.028	0.088	0.322	0.015	0.087	1				
Pct. White	-0.087	-0.175	-0.319	-0.020	-0.108	-0.779	1			
% Hispanic	0.153	0.242	0.206	0.021	0.059	-0.104	-0.188	1		
Pct. Over 65	0.310	0.137	-0.140	-0.207	-0.336	-0.214	0.317	-0.201	1	
Pct. College	-0.133	0.198	-0.380	0.154	0.034	0.027	-0.084	-0.143	-0.219	1

Table 2

This exhibit displays the results of several robustness tests of the baseline difference-in-difference analysis. Panel A displays results analogous to Panel A of Table 4, which are estimated from a version of equation (2) which controls for the lagged growth of refinancing. Panel B displays within-county results similar to Panel A of Table 4, which use counts of other intermediaries rather than of fintech firms as the key right-hand side variables. These specfcations take the form $\Delta_1 Refivol_{i,t} = \alpha_t + \beta \cdot Intermediary_{i,t-1} + \gamma \cdot Intermediary_{i,t-1} \cdot \Delta_{avg} Rates_t + \delta \cdot Controls_{i,t-1} + \varepsilon_{i,t}$ where Intermediary is one of *OtherNonbank*, *LargeBank*, or *SmallBank*. Huber White standard errors are listed in parentheses beneath each coefficient. Significance at the 10%, 5%, and 1% levels are given by *, **, and ***, respectively.

Panel A: Controlling for Lagged Refi Growth Dependent Variable: Log Refi Volume (First-Difference)	(1)	(2)	(3)	(4)
FintechCount	.005***	.003***	.012***	.008***
	(.0004)	(.0008)	(.0009)	(.002)
FintechCount*Rates	.012***	.011***	.016***	.014***
	(.0005)	(.001)	(.0006)	(.001)
Controls	Baseline	Full	Baseline	Full
N	24656	5772	24656	5772
Adj. R-squared	.909	.921	.566	.855
Year Fixed Effects	✓	✓	✓	✓
County Fixed Effects			✓	✓

Panel B: Falsification Tests with Non-Fintech Intermediaries Dependent Variable: Log Refi Volume (First-Difference)	(1)	(2)	(3)	(4)	(5)	(6)
OtherNonbank	-.003***	-.002***				
	(.0002)	(.0002)				
OtherNonbank*Rates	.001***	.001***				
	(.0001)	(.0001)				
LargeBank			-.011***	-.001		
			(.001)	(.001)		
LargeBank*Rates			.012***	.009***		
			(.001)	(.001)		
SmallBank					-.004***	-.002***
					(.0002)	(.0002)
SmallBank*Rates					.001***	.0003***
					(.0001)	(.0001)
Controls	Baseline	Full	Baseline	Full	Baseline	Full
N	27760	6484	27760	6484	27760	6484
Adj. R-squared	.557	.850	.556	.845	.558	.867
Year Fixed Effects	✓	✓	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓	✓	✓

Table 3

This exhibit displays correlations between fintech activity and refinance credit growth using monthly data from Fannie Mae. Panel A displays *FintechShare* coefficients from estimating an equation of the form $\Delta_3 Refivol3mo_{i,t} = \alpha_t + \beta \cdot Fintech_{i,t-1} + \gamma \cdot Fintech_{i,t-1} \cdot \Delta_{avg} Rates_{i,t} + Controls_{i,t-1} + \varepsilon_{i,t}$. This is a 3-month regression analogue of equation (2), so that the dependent variable, *Refivol3mo*, is the total refinancing activity over a three month period (months t through t+2), and *FintechShare* is defined as the market share of fintech lenders over a 3-month period (from time t-3 to t-1). Panels B and C depict impulse responses generated from local projections. Specifically, they plot values of fintech coefficients from estimating equation (3) for time horizons of 1-5 months (i.e. values of h=1...5). Panel B displays these coefficients for the *FintechLoanGrowth* variable, which is a percent change in the total number of loans originated by fintech firms.

Panel A: Baseline Regressions with FNMA Data
Dependent Variable: First Difference of Log Refi Volume

	(1)	(2)
FintechShare	.033***	.041***
	(.012)	(.013)
FintechShare*Rates	.045**	.039**
	(.019)	(.020)
Year-Month FEs	✓	✓
ZIP-Code FEs		✓

Panel B: Local Projection Impulse Responses with Market Shares
Dependent Variable: First Difference of Log Refi Volume

	(t+1)	(t+2)	(t+3)	(t+4)	(t+5)
FintechShare	.028**	.040***	.055***	.079***	.087***
	(.014)	(.015)	(.017)	(.018)	(.019)
FintechShare*Rates	.048**	.085**	.114***	.147***	.153***
	(.020)	(.023)	(.025)	(.026)	(.028)
Year-Month FEs	✓	✓	✓	✓	✓
ZIP-Code FEs	✓	✓	✓	✓	✓

Panel C: Impulse Response with Growth in Fintech Loans
Dependent Variable: First Difference of Log Refi Volume

	(t+1)	(t+2)	(t+3)	(t+4)	(t+5)
FintechLoanGrowth	.001	.008***	.009***	.009***	.001
	(.002)	(.002)	(.002)	(.002)	(.002)
FintechLoanGrowth*Rates	.055***	.120***	.171***	.178***	.178***
	(.010)	(.011)	(.012)	(.013)	(.014)
Year-Month FEs	✓	✓	✓	✓	✓
ZIP-Code FEs	✓	✓	✓	✓	✓

Table 4

This table shows results from estimating

	Cross-Border Analysis of US States Dependent Variable: First Difference of Log Refi Volume					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	.0034	.0056	.0053	.0057*	.0010	.0034
	(.0043)	(.0038)	(.0036)	(.0032)	(.0030)	(.0027)
Treat*Rates	.0556***	.0155***	.0562***	.0153***	.0618***	.0197***
	(.0046)	(.0042)	(.0038)	(.0035)	(.0032)	(.0029)
Sample Subset	All	Excl. Large Metros	All	Excl. Large Metros	All	Excl. Large Metros
Bandwidth (Mi.)	50	50	70	70	100	100
Fixed Effects:						
Year	✓	✓	✓	✓	✓	✓
State	✓	✓	✓	✓	✓	✓
Border	✓	✓	✓	✓	✓	✓

Table 5

This table shows results from estimating versions of equation (7). The dependent variables are total retail spending and discretionary retail spending, with each column of the table representing a separate specification. Columns (1) and (2) display regression results where total retail spending is the outcome variable, while columns (3) and (4) depict results where discretionary retail is the dependnt variable. The table displays results of specifications that both include and exclude county fixed effects. The rows of the table display coefficients of the *FintechShare* variable and the *FintechShare* interaction with interest rate spreads as detailed in equation (7) and as described in section 3. Standard errors are displayed in parentheses beneath each coefficient. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Response of Retail Spending to Fintech Presence Dependent Variables: First Difference of Log Retail Spending				
	Total Retail		Discretionary Retail	
	(1)	(2)	(3)	(4)
Lagged Fintech Market Share	.044***	.288***	.049***	.312***
	(.011)	(.020)	(.011)	(.021)
Lagged Share*Rates	-.047**	.062***	-.048***	.066***
	(.019)	(.020)	(.021)	(.021)
N	10766	10766	10766	10766
Adj. R-squared	.611	.431	.591	.426
Year Fixed Effects	✓	✓	✓	✓
County Fixed Effects		✓		✓

Table 5

This table displays results from estimating equation (9). Each column of the table shows results from a different specification; each specification differs on the basis of the dependent variable used in the estimation. Dependent variables are expressed as shares of total county-level refinance credit, and first differences are taken. Thus, "Non-White Share" refers to the first difference in the share of refinance loans that went to non-White borrowers, with this share calculated as the total volume of loans to non-white borrowers divided by the total volume of refinance loans for the county. "Hispanic Share" is analogously defined for Hispanic/Latino borrowers. "FHA Loans" refers FHA guaranteed loans. "Junior liens" refers to refinances of loans backed by subordinate liens (i.e. not a first lien mortgage). Standard errors are displayed beneath each coefficient. *, **, and *** denote statistical significance at the 10%, 5%, and 1%, levels, respectively.

Refinance Credit Composition Regressions				
Dependent Variables: Percentage Point Change of Refinance Composition				
	FHA Loans	Junior Liens	Hispanic Share	Non-White Share
	(1)	(2)	(3)	(4)
FintechShare	.113***	.010***	.013**	.0001
	(.008)	(.003)	(.006)	(.001)
FintechShare*Rates	.249***	.062***	.099***	.040***
	(.009)	(.009)	(.006)	(.009)
N	27760	27760	27760	27760
Adj. R-squared	.050	.011	.020	.005
Year Fixed Effects	✓	✓	✓	✓
County Fixed Effects	✓	✓	✓	✓