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Essays on Financial Intermediation

By

Sooji Kim

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Business Administration

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor David Sraer, Chair
Professor Amir Kermani
Professor Jón Steinsson

Spring 2023

Essays on Financial Intermediation

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Abstract

Essays on Financial Intermediation

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This dissertation consists of two chapters that study how financial intermediaries pass through monetary policy and other macroeconomic shocks to households and small businesses in the real economy. In addition to documenting and quantifying the extent to which these economic shocks are transmitted by financial intermediaries, both essays examine how the transmission is done so heterogeneously, affecting different segments of the United States population differently.

The first chapter of my dissertation is coauthored with Ulysses Velasquez. In this paper, we show evidence of monetary policy passthrough to rental tenants through the refinancing channel of monetary policy transmission. Although the refinancing channel has historically focused on homeowners and their residential mortgages, we examine whether commercial mortgage refinances by property owners have any effect on tenants in the form of rent prices. By employing a difference-in-differences framework and exploiting contractual restrictions on debt prepayment as an instrument, we find that rent prices fall by around 3% after a building refinances over the post-crisis period, a period over which interest rates fell to the zero lower bound. Moreover, we find greater effects on rent prices in buildings whose property owners experience a greater interest rate change after refinancing; a refinancing-induced 1 pp reduction in the mortgage rate is associated with around a 2% decline in rent prices, consistent with property owners passing through changes in interest expenses to rental tenants in the form of rent prices. As commercial property owners often have less control over when to refinance relative to homeowners, due to differences in contract terms, higher rates are more likely to be passed through to renters as higher housing costs than to homeowners during tightening periods. As households with certain demographics—racial minorities, young people, lower income—are more likely to be renters, such dynamics raise questions about how monetary policy may affect inequality.

The second chapter of my dissertation is also coauthored with Ulysses Velasquez. Using

the COVID-19 pandemic as the crisis setting, we investigate whether downturns in small business credit supply differ by the type of financial intermediary and the resulting implications on aggregate credit supply for areas with varying levels of nonbank reliance. We find that, after controlling for local demand shocks, fintech lenders reduce their supply of small business credit significantly, relative to both banks and nonfintech nonbanks, which is consistent with fintechs being more funding constrained during economic crises. Moreover, this reduction by fintechs is more severe in areas with high fintech entry. Finally, this decrease is not completely offset by corresponding increases in credit supply by either banks or nonfintech nonbanks, and thus, total credit supply falls in areas with high fintech entry, relative to other areas. As areas with lower incomes and higher minority shares have seen more pronounced increases in fintech presence over time, our results suggest that it is exactly these areas that experience more severe credit crunches during downturns.

To Ulysses

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Acknowledgments

Coauthor information: Chapters 1 and 2 of this dissertation are coauthored with Ulysses Velasquez, a Ph.D. student at the Stanford Graduate School of Business.

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Finally, I am deeply grateful for my parents and sister, who have always been my biggest supporters. I would not be who I am today without them. I would like to specially thank my sister Sooyeon, her husband Nick, and their dog Penny for all the good times we spent together when they lived in the Bay Area. And last but not least, I would like to thank Ulysses for his unwavering support and steadfast partnership. I would not have been able to finish this dissertation without him.

Chapter 1

Revisiting the Refinancing Channel of Monetary Policy: Spillovers to Tenants

1.1 Introduction

Monetary policy remains one of the most powerful tools economists and policymakers have to influence the direction of the economy. However, there is still much to be learned about how changes in the policy rate trickle down to affect the consumption and investment decisions of individual firms and households. One proposed channel, commonly referred to as the "refinancing channel," focuses on how changes in interest rates affect the incentives of borrowers, primarily homeowners with residential mortgages, to refinance, which may then affect their available income and, thereby, their consumption. And in fact, a number of papers have shown that lower mortgage rates, driven downwards by lower policy rates, do indeed increase homeowners' propensity to refinance, leading to reduced interest payments and increased consumption for homeowners (Di Maggio et al., 2020, Cloyne et al., 2020).

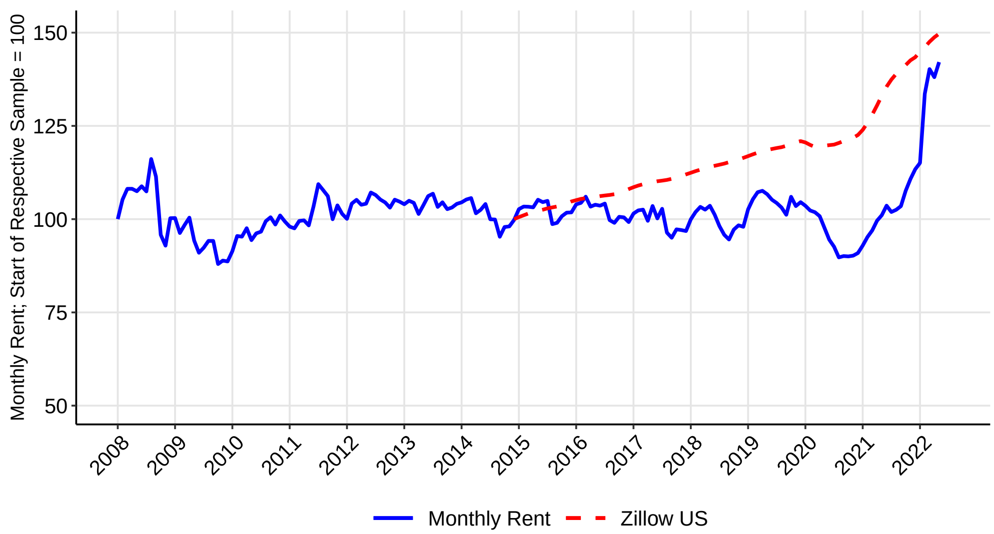
However, in 2019, homeowners made up just 64% of U.S. households[1] and only 48% in urban neighborhoods.[2] Thus, renters make up a significant proportion of the U.S. population, and while renters do not hold mortgages whose terms may be affected by monetary policy, their landlords do. A natural question thus arises: does the refinancing channel of monetary policy work through the commercial mortgage market and, if so, to what extent are policy rate changes passed through to renters as changes in rent prices?

These questions are particularly relevant today in our current inflationary environment. Housing costs make up a large part of common inflation measures, such as the Consumer

[1] American Community Survey. 2019. "Demographic Characteristics for Occupied Housing Units—S2502." United States Census Bureau.

[2] Joint Center for Housing Studies of Harvard University. 2022. "America's Rental Housing." President and Fellows of Harvard College

Price Index[3], and as Figure 1.1 shows, rental prices have indeed risen dramatically in the past year, whether we look at data from the entire United States or from apartments in our sample.[4]



Notes: The *Monthly Rent* line in blue depicts the average monthly rents of New York City apartments, normalized to 100 as of 2008; these data are collected from a premier real estate listing platform for New York City. The *Zillow US* line in red depicts the Zillow Observed Rent Index (ZORI) from Zillow, a rent index that reflects the monthly rents of apartments across the entire U.S.; the *Zillow US* line is normalized to 100 as of 2015, the start of the sample.

Figure 1.1: Rent Indices Over Time

Thus, to tackle high inflation, the Federal Reserve has increased the federal funds rate from near-zero to 4% in just the past year. However, note that if higher rates are passed through to renters as increased rents through commercial mortgage refinances, these higher housing costs would directly increase inflation, rendering the Federal Reserve's strategy of increasing interest rates to lower inflation less effective. This potential feedback loop is made more problematic by the fact that housing costs make up a large part of common inflation measures. In addition, because housing is a significant expenditure category for most households, these increased costs could adversely affect how individuals develop their own inflation expectations.

[3]Economic News Release. 2022. "Table 2. Consumer Price Index for All Urban Consumers (CPI-U): U.S. city average, by detailed expenditure category." U.S. Bureau of Labor Statistics. https://www.bls.gov/news.release/cpi.t02.htm

[4]Zillow Observed Rent Index (ZORI). 2022. Zillow. https://www.zillow.com/research/data/

Moreover, due to differences in contract terms between commercial and residential mortgages, commercial property owners often have less control over when to refinance relative to homeowners. For instance, a vast majority of commercial mortgages are partially amortizing loans with large balloon payments at maturity, often leading property owners to refinance even if doing so leads to higher rates. As a result, during tightening periods, higher rates are more likely to be passed through to renters as higher housing costs than to homeowners. As households with certain demographics—racial minorities, young people, lower income—are more likely to be renters, such dynamics raise questions about how monetary policy may affect inequality.

Therefore, in this paper, we investigate whether and to what extent monetary policy is transmitted to rent prices through the refinancing of commercial mortgages. Relying on a novel dataset with apartment-level rent listing prices matched to building-level refinancing data, we employ a difference-in-differences design to measure differential rent changes between buildings whose loans were refinanced versus those whose loans were not. In order to control for differential trends in apartment demand and to account for the staggered timing of treatments (as we have buildings refinancing throughout our sample), we construct cohorts that are characterized by a Neighborhood and a "refi-month" (i.e. a month in which buildings in the given Neighborhood refinance). Each cohort includes buildings in the same neighborhood that either refinanced in the refi-month (i.e. treatment) or had no loan transactions at all in the 24 month period around the refi-month (i.e. control). Creating these cohorts allows us to run a "stacked" regression that aligns all refinancing events (i.e. refi-months) by event-time and matches only "clean" controls to treated buildings, making our estimates more robust to potential problems that may arise with staggered treatments (Roth et al., 2023, Cengiz et al., 2019). Moreover, we restrict comparisons to within a single apartment type, defined by the number of bedrooms and bathrooms, in the same neighborhood to further control for any differences in rent prices that could be driven by changes in demand for different apartment sizes and for different neighborhoods. With this stacked difference-in-differences approach, we find that after refinancing, buildings reduce their rents by 10 basis points, relative to buildings in the same neighborhood that do not refinance. As our final sample includes rents and transactions between the years 2006 to 2021, 72% of the refinances are done in a lower interest rate environment[5], so our results suggest that the average reduction in interest expenses after a refinancing leads to an average reduction in rents to tenants.

However, one may be concerned that a building's decision to refinance may be endogenous, and therefore, alternative channels could explain the changes in rental prices that we observe. We believe that the balloon payments and prepayment restrictions common in most commercial multifamily loans help reduce this concern, as they help remove control over when and whether property owners refinance. Nevertheless, we construct a novel instrument that exploits the prepayment restrictions for each loan to explicitly isolate the

[5]That is, 72% of the refinances occur when market rates are lower than the prevailing rates when the outstanding loan was originated.

portion of the refinancing decision that is driven by the loan's contract terms rather than any unobserved factor that affects rent prices. This instrument is a building-cohort-time dummy variable that equals 1 if a building has entered the "open" period of its commercial mortgage, when there are no prepayment penalties. As property owners are more likely to refinance if they have no penalties for doing so and need to refinance to cover the looming balloon payments, our instrument strongly predicts the decision to refinance. We also see this empirically, as our first stage results show that buildings' propensity to refinance rises sharply after they enter the open periods of their commercial mortgages. Moreover, as these restrictions are quite standard in our sample, selection bias into the restrictions is not a concern, and therefore, we are confident that the exclusion restriction is satisfied.

Therefore, we rerun our analysis, using a building's entrance into its open period as an instrument for each building's decision to refinance. We find that our results actually become stronger as a result. Under our instrumental variable (IV) analysis, after buildings refinance, their rent prices drop about 3%, relative to those buildings that did not refinance. This translates into a reduction of approximately $90 in monthly rent for the median apartment in our sample, an economically significant difference.[6] Again, as the vast majority of refinances in our sample occur in a low interest rate environment, these results suggest that landlords pass through interest savings to rental tenants in the form of lower monthly rents. Therefore, as commercial mortgage rates are highly influenced by changes in the policy rate, we find compelling evidence that the refinancing channel of monetary policy does have spillovers to renters.

We next attempt to specifically measure how changes in interest rates, rather than simply the act of refinancing, affect rent prices. To do so, we examine how a building's interest rate, as determined by its current loan, affects the listed rent prices for the building's apartments. However, as interest rates depend on building characteristics and property owners' refinancing decisions, we instrument the building's interest rate with an interaction between the previously mentioned "has-opened" indicator variable and the predicted change in the interest rate had the building refinanced at the start of its open period. The timing of a building's open period is plausibly exogenous, and if the open period induces property owners to refinance, the change in the building's interest rate had it refinanced in the open month should predict the path of interest rates for buildings that enter the open period. Our first stage results confirm this to be the case. With this instrument, we find an interest rate elasticity of 0.11, which means that growth in the building's interest rate by one percent leads to an 11 basis point increase in its listed rent prices. Equivalently, we find that a level increase in the building's interest rate by one percentage point leads to a 2.2% increase in rent prices. Note that as interest rates increased by nearly 4 percentage points this past year, our estimates imply that this refinancing channel would have caused rents to increase by 9%. Thus, these results provide further evidence that when commercial mortgage rates

[6]According to the Economic Well-Being of U.S. Households in 2021, a survey conducted by the Federal Reserve Board, 68% percent of adults reported having the ability to cover a $400 emergency expense exclusively using cash or its equivalent. This proportion is likely smaller for the renter subpopulation and helps to contextualize the relative magnitude of the $90 figure.

change, property owners pass some of the additional interest expenses or savings through to their tenants by adjusting rental prices.

The rest of the paper proceeds as follows. Section 1.2 contains a brief review of the literature and contextualizes our contribution. In Section 1.3, we discuss the data, and in Section 1.4, we describe the main empirical methodology and present our base refinancing results. Section 1.5 presents our interest rate results, while Section 1.6 provides a discussion of our results. Section 1.7 concludes.

1.2 Related Literature

We contribute to a long literature that studies the refinancing channel of monetary policy. First, a subset of papers in this literature focuses on documenting the effects of monetary policy on RMBS rates and, therefore indirectly, residential mortgage rates. For instance, Krishnamurthy and Vissing-Jorgensen, 2011 measure the effect of QE announcements on a variety of long-term interest rates, including MBS rates, and discuss the different channels through which QE may affect these rates. Gagnon et al., 2011 also use QE announcements to study interest rate responses and conclude that interest rates fell because of changes in risk premiums, rather than changes in expectations of future short-term rates. Gilchrist et al., 2015 compare the effects of conventional versus unconventional monetary policy on interest rates, again including MBS rates. A second set of papers studies how changes in mortgage rates affect homeowners' propensity to refinance. Green and Shoven, 1986 is a classic paper that uses a hazard model to predict how the prepayment probability of FRMs changes in response to interest rates. Fuster and Willen, 2010 use mortgage offer data to study how mortgage rates and loan volumes responded to the first QE program. Bhutta and Keys, 2016 use credit record panel data to study the effect of mortgage rates on home equity extraction and the interaction of that relationship with housing prices. Third, a few recent papers have documented how changes in mortgage rates affect homeowners' consumption. Di Maggio et al., 2017 use variation in the timing of ARM resets to find that a decline in mortgage payments leads to an increase in auto purchases. Di Maggio et al., 2020 exploit the market segmentation between conforming and jumbo mortgages to study the effect of QE on refinancing and durable consumption. Agarwal et al., 2022 study how an unexpected interest rate decrease affected credit card spending by customers of a Chinese commercial bank. They find that households with mortgages significantly increased their spending and delinquency rates decreased. We contribute to this large literature by studying how the refinancing channel affects the commercial mortgage, rather than residential mortgage, market and by analyzing the consequent monetary policy passthrough to renters, rather than homeowners.

There are a few recent papers that also analyze monetary policy transmission to renters. For instance, Wong, 2021 studies heterogeneous consumption responses to monetary policy, focusing particularly on how these responses depend on age and mortgage decisions, and finds that much of the aggregate consumption response to policy rate shocks flows to homeowners who refinance their mortgages. Renters, on the other hand, have no significant consumption response. Koeniger et al., 2022 study the transmission of monetary policy to the housing market in three European countries and find that rents decrease in response to a negative monetary policy shock in just one of the three countries in their sample. They posit that their results may be attributed to differences in public ownership of housing and the indexation of rents. Cloyne et al., 2020 use household survey data to find that in response to surprise interest rate reductions, renters significantly increase their spending but not by as much as homeowners with mortgages. They suggest that differences in household balance sheets could explain their results. Unlike these three papers, we choose to focus completely on renters and provide strong evidence for how one particular channel, involving the refinancing of multifamily mortgages, transmits monetary policy through to renters.

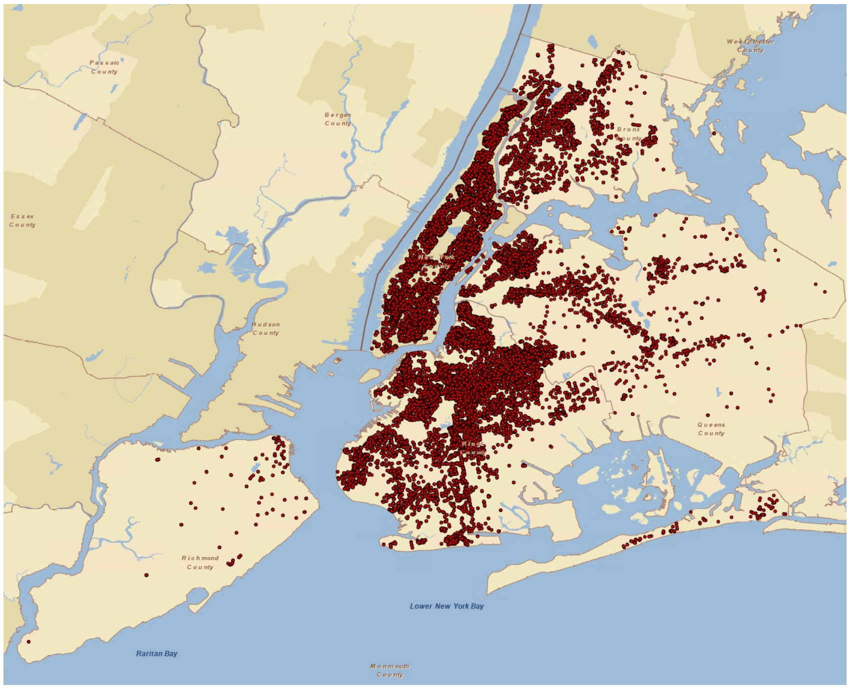
We also contribute to a literature that studies how the credit choices of landlords can affect the lease terms offered to tenants. Ambrose et al., 2019 study how the capital structures of both landlords and tenants affect the eventual contracting price, i.e. the rent. Ambrose et al., 2021 examine how landlords' financing decisions affect their tenants' eviction risks. We also study how property owners' financing affects rents but do so in the context of interest rate changes due to monetary policy and property owners' decision to refinance. Finally, our paper is most similar to Hughes, 2022, which studies how plausibly exogenous changes in mortgage payments and interest rates affect landlord behavior and property-level financial outcomes. Like our paper, Hughes, 2022 exploits the schedule of prepayment restrictions common in commercial mortgage contracts as an exogenous shock that pushes property owners to refinance, but while we look directly at the effects of refinancing on rent prices, Hughes, 2022 focuses primarily on financial variables, such as revenues, which indirectly provide information about rent prices.[7] Hughes, 2022 does include a few minor analyses that use rent data directly, from an annual survey of property owners, but the relative infrequency and survey nature of that data means that we are able to more precisely trace out the effects of refinancing using our apartment-level, daily rent listing data. As such, we focus more on understanding how the rent prices tenants pay change as a result of property owners' interest rate changes.

1.3 Data

We construct a novel dataset that contains apartment-level rent prices from over 24,000 buildings in the city of New York between the years 2006 to 2022. The data are collected from a leading real estate listing platform for New York City. Our sample only includes rental buildings where all units are rented out by the property owner/manager and excludes condo or co-op buildings where individual units may be rented out by their owners. This restriction allows us to focus specifically on how the commercial mortgage market transmits monetary policy to renters through the refinancing channel. The dataset includes buildings from all five boroughs although, unsurprisingly, not uniformly. Figure 1.2 has a map that

[7] Note that changes in financial variables like revenues do not correspond one-for-one with changes in rent prices, as other variables like occupancy rates are also inputs.

depicts the geographical distribution of the buildings in our rental dataset, and Table 1.1 includes more detailed borough-level summary statistics about the building distribution in our final sample. [8]



Notes: This figure depicts the distribution of New York City buildings present in our sample. As shown, there is vast coverage of buildings across New York City, particularly in the boroughs of Manhattan and Brooklyn.

Figure 1.2: Distribution of NYC Buildings

[8] The numbers in Table 1.1 do not necessarily match those in Figure 1.2, as not all buildings in our rental data are included in our analysis, given how we construct our cohorts and our restriction to non-rent stabilized apartments. See Section 1.4.1 for more details.

Table 1.1: Borough-Level Summary Statistics

Variable	Manhattan	Brooklyn	Queens	Bronx	Staten Island
No. of Buildings	6824	4626	808	146	4
Start Year	2006	2007	2008	2012	2012
End Year	2022	2022	2022	2022	2020
No. of Neighborhoods	50	49	19	21	1
No. of Buildings that refi	2889	1839	185	69	2
No. of Refis	3992	2159	208	83	2

Notes: This table presents summary statistics at the borough level. *No. of Buildings* refers to the number of buildings in each borough. *Start Year* is the first year in which we see buildings in the corresponding borough in our sample. *End Year* is the last year in which we see buildings in the corresponding borough in our sample. Our sample thus primarily covers the post-crisis period. *No. of Neighborhoods* refers to the number of neighborhoods in each borough that is present in our sample. *No. of Buildings that refi* gives the number of buildings in each borough that go through a refinancing transaction. *No. of Refis* gives the number of refinancing transactions that occur in each borough; this value is weakly larger than *No. of Buildings that refi* as there are few buildings in our sample that refinance more than once.

For each building in our sample, we obtain its address, its neighborhood, and its entire history of apartment rental listings on the platform. Each rental listing includes details about the specific apartment unit advertised to prospective customers; these details include the unit number, number of bedrooms, number of bathrooms, rent price, and the listing date.[9] We may see the same apartment listed multiple times throughout the sample, as tenants move in and out. As we do not see rent prices in between listing dates (which can be anywhere from 1 to 2 years apart, depending on the lease term), and given our goal of tracing out the effects of refinancing on rent prices over a year, we collapse our data to the apartment-type level (e.g. 1 bed, 1 bath), since prices of a given apartment type are more frequently updated over time. We then impute prices in between any two dates for which we have price data by linearly interpolating. See Section A.1 in the Appendix for further details on sample construction.[10]

Table 1.2 provides summary statistics on the apartments in our sample. The average apartment in our sample has two bedrooms and one bathroom, and the mean rent ranges from $1883 in the Bronx to $3700 in Manhattan.

[9]The listing date is the date on which the apartment was taken off the rental market. The rent price is as of the listing date.

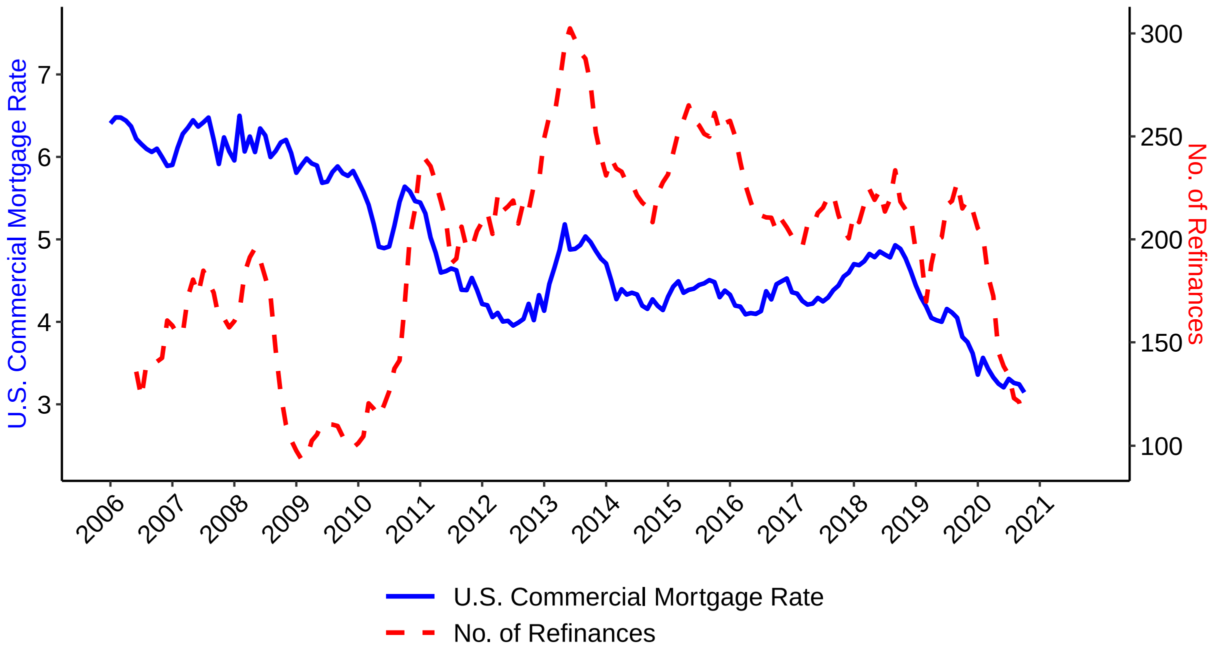
[10]Also, results are broadly similar without interpolating.

Table 1.2: Building-Apartment Type-Year-Month Level Summary Statistics

Borough	N	Variable	Min	Q1	Q2	Mean	Q3	Max
		No. of Beds	0	1	2	2	2	4
Bronx	7429	No. of Baths	1	1	1	1	1	4
		Price	500	1500	1701	1883	2103	6000
		No. of Beds	0	1	2	2	3	6
Brooklyn	493332	No. of Baths	1	1	1	1	1	4
		Price	500	2143	2664	2810	3234	14663
		No. of Beds	0	1	2	2	2	6
Manhattan	1202435	No. of Baths	1	1	1	1	2	4
		Price	500	2542	3130	3700	4201	22922
		No. of Beds	0	1	2	2	2	4
Queens	65798	No. of Baths	1	1	1	1	1	4
		Price	500	1850	2281	2474	2821	7968
		No. of Beds	1	1	2	2	2	6
Staten Island	140	No. of Baths	1	1	2	2	2	3
		Price	1278	1968	2289	2401	2828	3949

Notes: This table presents summary statistics at the building-apartment type-year-month level. In particular, it presents distributions of the number of bedrooms, number of bathrooms, and monthly rent across building-apartment type-year-month observations in each borough. An apartment type is characterized by the number of bedrooms and bathrooms (e.g. 1 bed, 1 bath is an apartment type). Aggregating to the apartment-type level gives similar distributions compared to leaving the data at the apartment level, as there are not many apartment units within each building-apartment type-year-month cell.

We connect our apartment-level rent data to two loan-level datasets. The first dataset is ATTOM, which records assessments and transactions for a near-universe of properties in the United States. We use ATTOM to construct, for each building in our sample, a timeline of refinancings between 2006 and 2021. Figure 1.3 plots the number of refinancings in New York City over time, plotted against the median U.S. commercial mortgage rate.



Notes: This figure plots, in blue, the median U.S. commercial mortgage rate over time, across all fixed rate commercial mortgages for multifamily properties. The number of refinances occurring in New York City each month is plotted in the dashed red line. As depicted in the figure, the vast majority of refinances in New York City occurred in a period of decreasing interest rates.

Figure 1.3: Commercial Mortgage Rates & Refinancing Transactions

For each loan, we have information on the transaction date, the size of the loan, and the relevant parties involved (e.g. property owner, lending banks).

While ATTOM excels in the breadth of properties and transactions covered, it lacks further information about the contract terms of each loan. For instance, we do not know the loans' interest rates, term lengths, or amortization schedules. Therefore, we supplement ATTOM with our second loan-level dataset, Trepp, which contains detailed contract information for every securitized commercial mortgage in the United States. Therefore, the loans covered in Trepp make up only a subset of the loans covered in ATTOM, but for that subset, Trepp provides very rich data.

We use Trepp in multiple ways. First, we use the sample of Trepp loans issued to buildings in our rent data to gain a better understanding of the loan terms faced by our sample of New York City properties. For example, we find that 95% of loans in our Trepp sample are fixed rate mortgages (FRM), 97% have balloon payments, and 97% have some sort of prepayment restrictions. These institutional details end up being essential for our identification strategy.

Second, we estimate a monthly time-series of the U.S. commercial mortgage rate for the "median loan"; this is the interest rate plotted in Figure 1.3. Specifically, we estimate the following regression on the sample of all FRM for multifamily properties in Trepp:

$$\begin{aligned} r_{it} = \alpha_t &+ \beta_1 \left(\text{Term}_{it} - \widetilde{\text{Term}}\right) + \beta_2 \left(\text{LoanSize}_{it} - \widetilde{\text{LoanSize}}\right) \\ &+ \beta_3 \left(\text{DSCR}_{it} - \widetilde{\text{DSCR}}\right) + \beta_4 \left(\text{LTV}_{it} - \widetilde{\text{LTV}}\right) \end{aligned} \tag{1.1}$$

where r_{it} is the interest rate of loan i issued in month t. We control for the difference between loan i's term length, size, LTV, and DSCR from their respective median values in the Trepp sample.[11] Thus, the estimated time effects α_t capture the interest rate charged over time to the representative borrower with median term length, loan size, LTV, and DSCR. We use this α_t series to approximate the interest rate change due to refinancing. Although Trepp contains information about the initial rate applied to each loan in our sample, we approximate a building's interest rate change due to refinancing by subtracting the prepaid loan's original rate from the market interest rate (i.e. α_t) on the building's refinancing month.

Finally, we use the prepayment restrictions for each loan in the Trepp sample to construct our instrument, the details of which are in Section 1.4.3.

All three datasets are linked using property addresses, which we geocode with ArcGIS to match on coordinates.

In addition to the main datasets, we also use the list of rent stabilized buildings from the NYC Rent Guidelines Board to determine which apartments in our data are rent stabilized. We find that approximately 36% of units in our sample are rent stabilized. Moreover, in 2021, the share of rent-stabilized apartments in our sample is 52%, which is comparable to the 44% reported in the 2021 New York City Housing and Vacancy Survey.[12] Our analyses in this paper are run on the sample of non-rent-stabilized apartments. We do this to ensure that rent regulations are not confounders in our estimation of the interest rate passthrough and to increase external validity, as apartments in other cities are not rent-controlled to the same extent as those in NYC.

[11] DSCR stands for debt service coverage ratio. DSCR is calculated by dividing a property's net cash flows or net operating income by its debt service (i.e. mortgage payments) and is a very common metric used for assessing risk in commercial mortgages.

[12] New York City Housing and Vacancy Survey. 2021. "Rental Units by Type of Housing." United States Census Bureau and NYC Department of Housing Preservation and Development.

1.4 Empirical Strategy & Effect of Refinancing on Rents

1.4.1 Empirical Strategy: Cohorts & Stacked Difference-in-Differences

We aim to investigate whether and to what extent monetary policy is transmitted to rent prices through the refinancing of commercial mortgages. One way to study this would be to use a difference-in-differences design to measure differential rent changes between buildings whose loans were refinanced versus those whose loans were not. However, there are a few concerns we'd need to address to ensure our estimates accurately capture the effect of refinancing.

First, as refinances can occur throughout our sample, we have a staggered treatment design. In this situation, using a simple difference-in-differences model with two-way fixed effects may result in treatment overlaps and the negative weighting of some effects, both of which could lead to biased estimates. Second, as demand for apartments may vary widely across New York City at any point in time, we want to ensure that we restrict all comparisons to apartments facing similar demand trends. Therefore, to address these issues, we construct cohorts, indexed by neighborhood and refi-month, that only include buildings in the same neighborhood that either refinanced in the refi-month (i.e. treatment) or had no loan transactions at all in the 24 month period around the refi-month (i.e. control).[13] These cohorts are created using the ATTOM data.[14] See Figure 1.4 for a graphical representation of an example cohort and Table 1.3 for some summary statistics on the cohorts.[15]

[13]When constructing cohorts, we exclude buildings that switch ownership in the 24 month period around the refi-month, buildings that refinance in a month other than the refi-month in the 24 month period around the refi-month, and buildings that refinance in the year prior or the year after the 24 month period around the refi-month. We do so to prevent treatment overlap.

[14]As robustness, we also create cohorts in which control buildings are within a 0.25 mile radius of treated buildings (and are thus not necessarily in the same neighborhood, though they remain close in proximity). Results remain robust to this alternative cohort definition. See Figures A.2 and A.3 and Table A.1 for details.

[15]The strategy of forming cohorts and running a stacked difference-in-difference is not new. See Cengiz et al., 2019 and Gormley and Matsa, 2011.

Notes: This figure shows an example cohort (i.e. the Yorkville/October 2013 Cohort) in our sample. Green dots represent buildings in Yorkville that refinanced in October 2013 (i.e. treated). Purple dots represent buildings in Yorkville that had no refinances between October 2012 and October 2014 (i.e. control).

Figure 1.4: Example Cohort - Yorkville/October 2013

Table 1.3: Cohort-Level Summary Statistics

Variable	N	Min	Q1	Q2	Mean	Q3	Max
No. of Controls	3422	1	25	57	93	124	553
No. of Treated	3422	1	1	1	2	2	34
Obs.	3422	37	1165	2599	4301	5685	28591

Notes: This table presents summary statistics at the cohort level. *No. of Controls* refers to the number of buildings in the cohort that serve as control. *No. of Treated* refers to the number of buildings in cohort that are treated (i.e. buildings that went through a refinancing transaction). *Obs.* refers to the number of building-apartment type-year-month observations in the cohort.

We then stack these cohorts on top of each other and run a stacked difference-in-differences regression, where each cohort is composed of panel data on treated and control buildings by event-time ($T = -12, ..., 12$). This design aligns all refinancing events by event-time, preventing the negative weighting issue in two-way fixed-effects regressions in staggered settings, and allows us to match only "clean" controls to treated buildings, alleviating concerns about treatment overlaps. Moreover, as cohorts only include buildings within a single neighborhood, we control for any differences in rent prices that could be driven by changes in demand across different neighborhoods. We also control for apartment type, defined by the number of bedrooms and bathrooms, in the regression, to ensure that demand for different apartment sizes are not driving our results.

1.4.2 The effect of refinancing on monthly rents: OLS

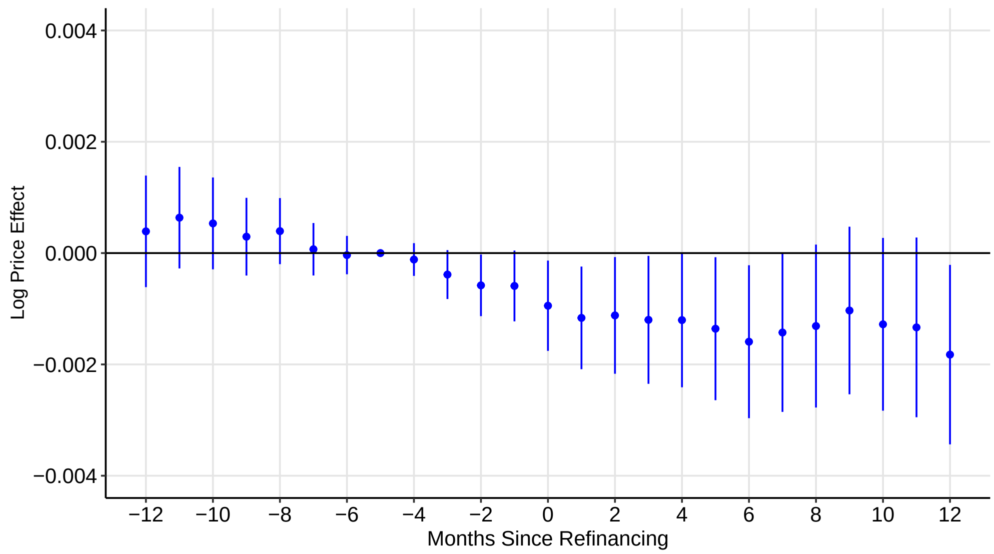
With the data in cohorts, we run the following dynamic difference-in-differences specification:

$$\text{Log Price}_{cbat} = \sum_{T} \beta_T \left(\mathbf{1}_{t=T} \cdot \text{DoesRefi}_{cb}\right) + \alpha_{cba} + \alpha_{cat} + \epsilon_{cbat} \tag{1.2}$$

where Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c. $\mathbf{1}_{t=T}$ is an indicator variable that equals one if month t is equal to T, and DoesRefi_{cb} equals 1 if building b refinances (i.e. is treated) in cohort c. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. These fixed effects mean that we control for time-invariant differences in rent prices set across buildings in the cohort, as well as the trend of rent prices for the given apartment type in the cohort. Finally, the coefficients of interest are the series of β_T, which measure the differential change in rental prices between month T and the base month, for apartments in buildings that refinanced versus those that did not. Figure 1.5 plots the point estimates and the confidence intervals for the coefficients of interest. The figure shows that after refinancing, treated buildings in the cohort have lower rent prices relative to control buildings with no transactions. As the vast majority of our refinances occur when rates are falling, this suggests that the average reduction in interest expenses are passed through to renters as reduced rents. This regression is run on the sample of non-rent-stabilized apartments for reasons explained above. In Appendix Figure A.1, we find similar results, and a sharper effect post-refinancing, on the sample of all apartments, rather than just those that are not rent-stabilized.

We do see some anticipatory behavior by buildings prior to refinancing; beginning a few months before, treated buildings in the cohort start reducing their rent prices relative to control buildings. We believe that this is due to property owners recognizing that much of the interest rate change over the loan term (on average 10 years in our sample) has already occurred, so with an approximation of the expected interest expense change, they begin smoothing their revenue stream a few months early.

We are not too concerned about the anticipatory reduction of rents for a few reasons. First, it is important to note that the rent prices for treated buildings not only differentially fall after a refinancing but also remain at lower levels. This observation rules out the possibility that property owners only reduce rents prior to refinancing as a way to boost occupancy to improve the loan terms for their new loan; if this were the case, rent prices should immediately rise back up once the refinance goes through and the new loan is secured. Moreover, the relatively lower rents for treated buildings do not extend back past these few months, which suggests that refinancing buildings are not experiencing a prolonged trend of lower rents. Finally, commercial mortgages often include balloon payments and prepayment



Notes: This figure shows how the monthly rents of buildings that refinanced evolved relative to the rents of buildings that did not refinance. Specifically, the figure plots the estimated β_T's from the following regression model: Log $\text{Price}_{cbat} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{DoesRefi}_{cb}\right) + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c. $\mathbf{1}_{t=T}$ is an indicator variable that equals one if month t is equal to T, and DoesRefi_{cb} equals 1 if building b refinances (i.e. is treated) in cohort c. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. The model is estimated on the sample of all non-rent-stabilized apartments.

Figure 1.5: Effect of Refinancing on Monthly Rents (OLS)

restrictions that make it difficult for property owners to choose whether and when to refinance, which alleviates the concern that a reduction in rents could be driving refinancing decisions.

Next, we quantify the overall extent to which rent prices decreased differentially for buildings that refinanced versus those that did not in the twelve month period after the refinancing month. Specifically, we run the following specification:

$$\text{Log Price}_{cbat} = \beta \text{hasRefied}_{cbt} + \alpha_{cba} + \alpha_{cat} + \epsilon_{cbat} \tag{1.3}$$

where hasRefied_{cbt} is equal to 1 if building b in cohort c has refinanced by month t. All other variables remain the same, and we continue to include Cohort-Building-Apartment Type and Cohort-Apartment Type-Month FE. The coefficient of interest is β, and it measures the average differential change in rents of buildings that refinanced, relative to those that did not, in the twelve months after the refinance relative to the twelve months before. As treatment is at the building-level, we report standard errors clustered by building in all of our forthcoming regressions (Abadie et al., 2022). The results are reported in Table 1.4 below.

Table 1.4: Effect of Refinancing on Monthly Rents (OLS)

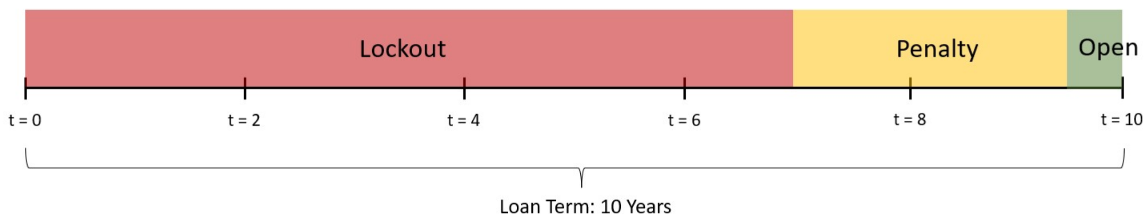
	Log Price	
	(1)	(2)
hasRefied	-0.001**	-0.001**
	(0.0006)	(0.0006)
Cohort × Building FE	✓	
Cohort × Month × Apt Type FE	✓	✓
Cohort × Building × Apt Type FE		✓
Cluster	Building	Building
R^2	0.9592	0.9918
Adjusted R^2	0.9548	0.9907
Observations	14,718,634	14,718,634

Notes: This table presents results from estimating the following regression model: $\text{Log Price}_{cbat} = \beta \text{hasRefied}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where hasRefied_{cbt} is equal to 1 if building b in cohort c has refinanced by month t, and Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. Standard errors are clustered at the building level (i.e. at the level of the treatment). The model is estimated on the sample of all non-rent-stabilized apartments.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The results in Table 1.4 further corroborate that buildings reduce their rents after refinancing, relative to those buildings that did not. Specifically, we find that in the twelve months after refinancing, treated buildings reduce their apartment rents by 10 basis points relative to control buildings with no transactions. This result is robust to variation in the specific fixed effects included in the regression specification. And, as previously mentioned, because a large proportion of refinances in our sample occur over a period of decreasing rates, these results suggest that the average reduction in interest expenses from refinancing are passed through to renters as lower rents.

1.4.3 The effect of refinancing on monthly rents: IV

Commercial mortgages, unlike residential mortgages, often include prepayment restrictions that prevent property owners from refinancing. These restrictions can be categorized into two main categories: (1) lockout periods in which borrowers are completely prohibited from prepaying and (2) prepayment penalties where borrowers can prepay early but will need to pay a penalty to do so. Figure 1.6 depicts a timeline of prepayment restrictions for a representative commercial mortgage in our sample of loans.



Notes: This figure depicts the timeline of prepayment restrictions for a representative commercial multifamily mortgage in our sample of Trepp loans. *Lockout* refers to the period in which the borrower is unable to prepay. *Penalty* refers to the period in which the borrower can prepay but only by incurring a penalty. The *Open* period is the period in which the borrower can prepay the loan without any prepayment penalties.

Figure 1.6: Prepayment Timeline of Representative Commercial Mortgage

From Figure 1.6, it is clear that prepayment restrictions are in place for much of the loan term. This implies that the period of time over which property owners can prepay freely is quite limited; in our sample, the median length of this time, which we call the "open period", is 5 months. Moreover, these restrictions are prevalent: of the loans in our sample for which we have detailed contract terms, 97% have some sort of prepayment restrictions. Moreover, 97% of these loans are non-amortizing or partially amortizing and have balloon payments at maturity. As a result, it is difficult for property owners to choose whether and when to refinance, which ameliorates endogeneity concerns surrounding the decision to refinance.

Nevertheless, in this section, we exploit prepayment restrictions to construct a novel instrument for the decision to refinance. Specifically, we instrument the timing and decision to refinance (hasRefied$_{cbt}$) with a dummy variable that equals one after a building enters its "open" period (hasOpened$_{cbt}$), during which there are no prepayment restrictions. Because the timing of the open period is driven by predetermined contract terms many years prior, it is likely that the instrument is orthogonal to any unobserved contemporaneous factors that may affect rent prices. Furthermore, it is likely that the instrument is relevant, as buildings are more likely to refinance if, contractually, they have the option to do so without any penalties.

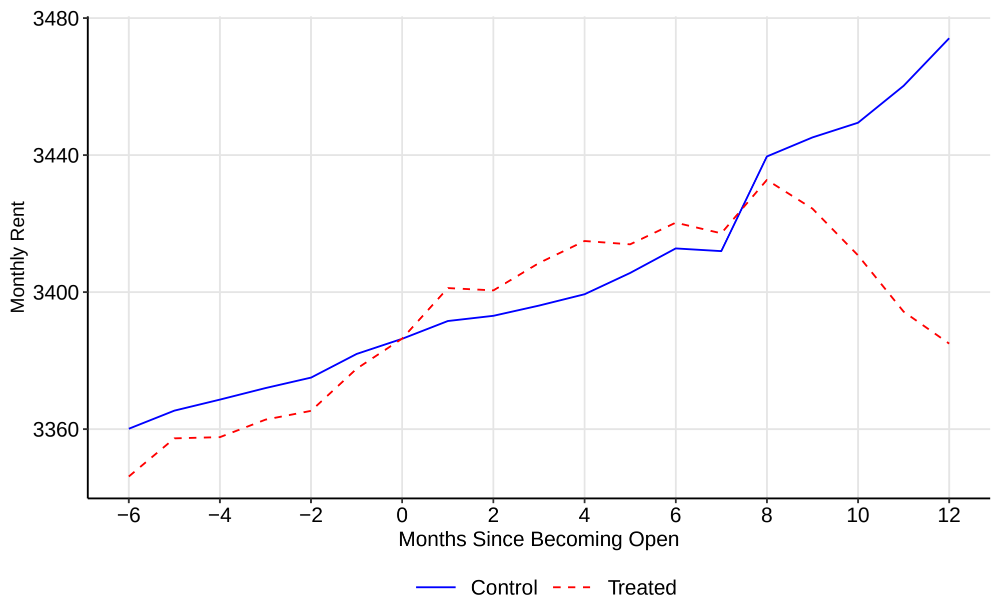
To create this instrument, we reconstruct our cohorts so that each cohort is now indexed by a neighborhood and open-month (the first month of a building's open period). Thus, each cohort now only includes buildings that either become open in the open-month (i.e. treatment) or do not enter open periods in the months around the open-month (i.e. control).[16]

Note that because the instrument requires information on prepayment restrictions, our

[16]Similar restrictions about changes in ownership and open-months outside of the cohort's open-month continue to apply.

sample now only includes loans from Trepp, which decreases our sample size by a nontrivial amount[17]. As a result, we now allow our "open cohorts" to include buildings from across New York City, rather than from a single neighborhood. To ensure that the parallel trends assumption is still satisfied, we keep, for each treated building in an open cohort, the ten control buildings that have the most similar rent growth trends in the twelve months prior to each treated building's open-month (i.e. the pre-period).[18] As before, each of these open cohorts is composed of panel data on treated and control buildings by event-time.

Even in aggregate, we see stark differences in the evolution of monthly rents between treated and control buildings in our open cohorts after the open-month. Figure 1.7 below plots the average monthly rent of treated and control buildings against event-time.



Notes: This figure plots the average monthly rent of treated and control buildings against event-time. As shown, monthly rents of treated and control buildings evolve in parallel in the period leading up to the open period. Control buildings of each treated building are the ten most similar buildings (to the treated building) in terms of rent growth in the pre-period.

Figure 1.7: Monthly Rents Over Time, by Treated and Control Buildings

As shown in Figure 1.7, monthly rents in both treated and control buildings evolve in parallel leading up to the open event. However, around 7 months after the open event, rent

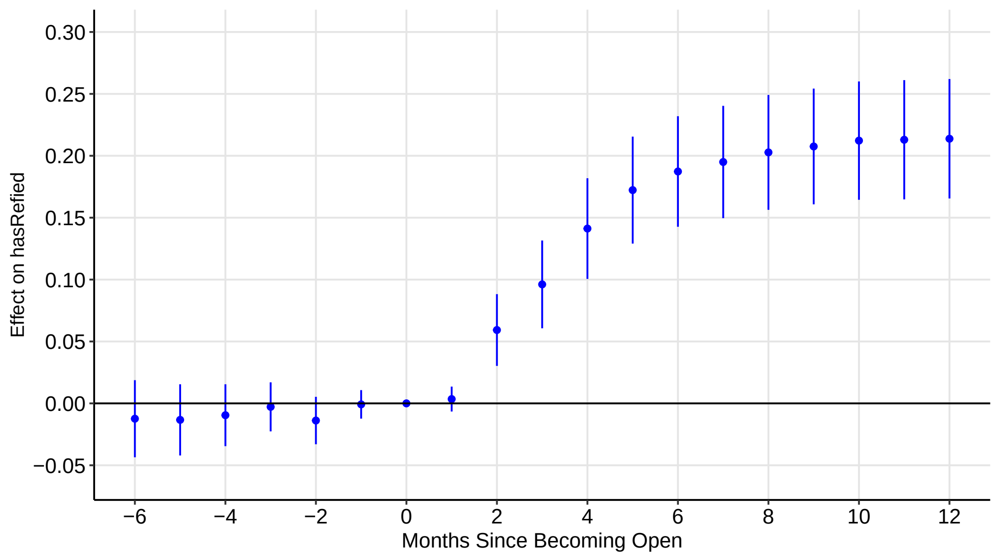
[17]We continue to have over 1,000 NYC buildings (down from about 12,000 buildings). While restricting our sample to only Trepp loans limits our sample size, doing so allows us to implement our identification strategy and more precisely estimate the causal effect of refinancing.

[18]If we include a similar restriction for the original ATTOM cohorts (i.e. for each treated building, keep as control the 10 nearest neighbors in the cohort in terms of rent growth during the pre-period), the results remain very similar as those seen in Table 1.4. See Appendix Figure A.4 and Table A.2 for more details.

prices in control buildings continue their trajectory while those of treated buildings diverge markedly from the same trend, decreasing by around 1.4% from their peak. These trends suggest that once buildings become open to refinance, they do refinance and ultimately lower their monthly rents once they experience a reduction in their mortgage interest rate. To investigate this channel more rigorously, we first test whether entering an open period predicts a refinancing event. To test this, we run the following first-stage regression:

$$\text{hasRefied}_{cbt} = \sum_{T} \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb}\right) + \alpha_{cba} + \alpha_{cat} + \epsilon_{cbat} \tag{1.4}$$

where hasRefied_{cbt} is equal to 1 if building b in cohort c has refinanced by month t, 0 otherwise; Opens_{cb} is equal to 1 if building b enters their open period (i.e. is treated) in cohort c. As before, we include the usual set of fixed effects which allow us to control for time-invariant differences in rent prices set across buildings in the cohort, as well as apartment-type-level rent price trends in the cohort. The coefficients of interest are the series of β_T, which measures the differential propensity to refinance for treated buildings, relative to control buildings, in month T relative to the base month. Figure 1.8 plots the point estimates and the confidence intervals for the coefficients of interest.



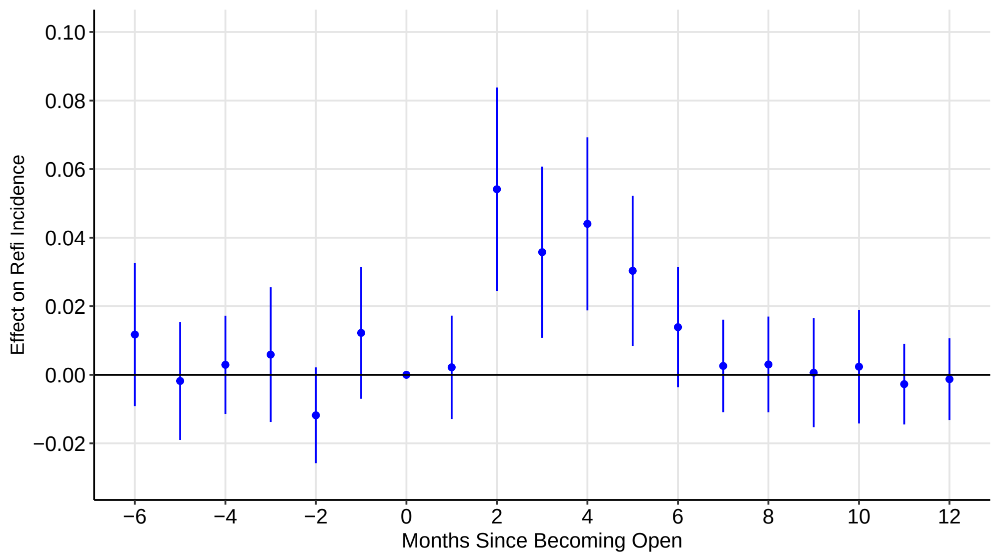
Notes: This figure gives an idea of how the probability of refinancing among treated buildings changes (relative to that of control buildings) once treated buildings enter their open period. Specifically, the figure plots the estimated β_T's from the following regression model: $\text{hasRefied}_{cbt} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb}\right) + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where hasRefied_{cbt} is equal to 1 if building b in cohort c has refinanced by month t, 0 otherwise; Opens_{cb} is equal to 1 if building b enters their open period (i.e. is treated) in cohort c; $\mathbf{1}_{t=T}$ is an indicator variable that equals one if month t is equal to T. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE.

Figure 1.8: First Stage Regressions: Have you refinanced by time t?

Figure 1.8 shows that after opening, treated buildings in the cohort begin to refinance at a much higher rate relative to control buildings who remain unable to costlessly refinance. To get a better sense of when exactly treated buildings begin to refinance at a higher rate, we run the following modified regression:

$$\text{doesRefi}_{cbt} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb}\right) + \alpha_{cba} + \alpha_{cat} + \epsilon_{cbat} \tag{1.5}$$

where doesRefi_{cbt} equals 1 if building b in open cohort c refinances in month t, 0 otherwise. All other variables remain the same. Figure 1.9 plots the point estimates and confidence intervals for the coefficients of interest, the β_T's.



Notes: This figure gives an idea of how the probability of refinancing among treated buildings changes (relative to that of control buildings) once treated buildings enter their open period. Specifically, the figure plots the estimated β_T's from the following regression model: $\text{doesRefi}_{cbt} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb}\right) + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where doesRefi_{cbt} equals 1 if building b in open cohort c refinances in month t, 0 otherwise; Opens_{cb} is equal to 1 if building b enters their open period (i.e. is treated) in cohort c; $\mathbf{1}_{t=T}$ is an indicator variable that equals one if month t is equal to T. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE.

Figure 1.9: First Stage Regressions: Have you refinanced at time t?

Figure 1.9 shows that treated buildings refinance at a higher rate starting in the second month after opening and continue to do so for the following 4 months at increasingly lower levels. The effect tapers off completely after 6 months following the open event. As a result, because we see that the propensity to refinance remains elevated for the 6 months following the open event, we set our post period to start six months after the open-month in order to increase the relevance of our instrument.

We now summarize the first-stage results from our dynamic specifications by running the following regression:

$$\text{hasRefied}_{cbt} = \beta \text{hasOpened}_{cbt} + \alpha_{cba} + \alpha_{cat} + \epsilon_{cbat} \tag{1.6}$$

where hasOpened_{cbt} is equal to 1 if building b in cohort c has entered its open period by month t. All other variables remain the same, and we continue to include the usual set of fixed effects. The coefficient of interest is β, which measures the differential change in the probability to refinance between treated and control buildings in the period starting 6 months after the open event. The results are reported in Table 1.5 below.

Table 1.5: Effect of Open Event on Refinancing Likelihood (First Stage)

	hasRefied	
	(1)	(2)
hasOpened	0.169***	0.169***
	(0.019)	(0.019)
Open Cohort × Building FE	✓	
Open Cohort × Month × Apt Type FE	✓	✓
Open Cohort × Building × Apt Type FE		✓
Cluster	Building	Building
R^2	0.8675	0.8678
Adjusted R^2	0.8432	0.8403
Observations	62,049	62,049

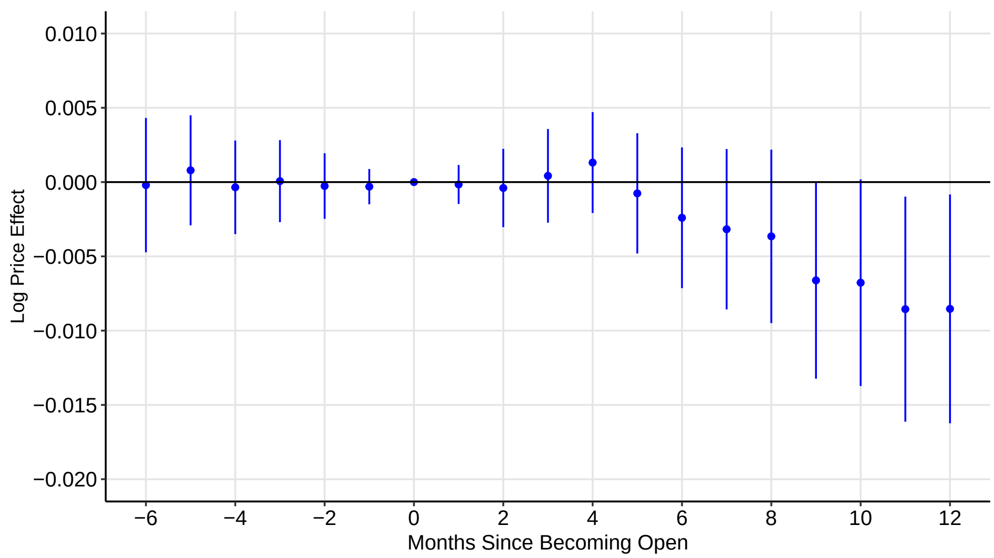
Notes: This table presents results from estimating the following regression model: $\text{hasRefied}_{cbt} = \beta \text{hasOpened}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where hasRefied_{cbt} is equal to 1 if building b in cohort c has refinanced by month t, and hasOpened_{cbt} is equal to 1 if building b in cohort c has entered its open period by month t. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. Standard errors are clustered at the building level (i.e. at the level of the treatment).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Consistent with the dynamic results from Figures 1.8 and 1.9, we find evidence that our instrument is strongly relevant. In particular, buildings who enter their open period – and are thus able to refinance without any prepayment penalty – experience an increase of 17 pp in the probability to refinance, compared to buildings that are unable to freely refinance. These results are not explained by building-level factors that could drive the decision to refinance, nor are they explained by local apartment-type-level factors that could drive certain buildings, based on the composition of their apartments, to refinance. Overall, these results speak to how property owners, by virtue of being *able* to freely refinance, ultimately end up deciding to refinance. Given that open periods are a predetermined mortgage contract feature and often begin many years after the commercial mortgage is underwritten, open events are plausibly exogenous to any unobservable, contemporaneous rent price factors. By estimating the effect of refinancing from variation in refinancing that is driven by variation in open events, we hope to estimate the causal effect of refinancing on a building's monthly rents.

Before jumping to the 2SLS results, we first show reduced form results on the effect of becoming open on monthly rents. In particular, we run the following regression:

$$\text{Log Price}_{cbat} = \sum_{T} \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb} \right) + \alpha_{cba} + \alpha_{cat} + \epsilon_{cbat} \tag{1.7}$$

where all the variable definitions remain the same. As before, the coefficients of interest, which we plot in Figure 1.10 below, are the series of β_T's, which measure the differential change in the monthly rents of treated buildings (those who become open) relative to control buildings (those who remain unopened) in the months following the open event.



Notes: This figure shows how the monthly rents of buildings that entered their open period evolved relative to the rents of buildings that did not enter their open period. Specifically, the figure plots the estimated β_T's from the following regression model: Log $\text{Price}_{cbat} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb}\right) + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c. $\mathbf{1}_{t=T}$ is an indicator variable that equals one if month t is equal to T, and Opens_{cb} is equal to 1 if building b enters their open period (i.e. is treated) in cohort c. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE.

Figure 1.10: Effect of Open Event on Monthly Rents (Reduced Form)

As depicted in Figure 1.10, once buildings enter their open period, their monthly rents experience a relative reduction compared to those in control buildings. Why would entering an open period – a predetermined feature of the mortgage contract set many years ago – have any effect on a building's monthly rents? We posit that the underlying channel operates through the decision to refinance. In particular, once property owners find themselves in their open period, they experience a reduction in the cost to refinance and, thus, decide to refinance. As our sample period primarily covers the post-crisis period, a period over which interest rates fell, property owners experience lower interest expenses upon refinancing. With interest savings in hand, property owners decide to pass those on to the rental tenants in their buildings in the form of lower monthly rents.

We now summarize the reduced-form results from our dynamic specifications by running the following regression:

$$\text{Log Price}_{cbat} = \beta \text{hasOpened}_{cbt} + \alpha_{cba} + \alpha_{cat} + \epsilon_{cbat} \tag{1.8}$$

where all variables are defined as above, and we continue to include the usual set of fixed effects. The coefficient of interest is β, which measures the differential change in the monthly rents between treated and control buildings in the period starting 6 months after the open event. The results are reported in Table 1.6 below.

Table 1.6: Effect of Open Event on Monthly Rents (Reduced Form)

	Log Price	
	(1)	(2)
hasOpened	-0.006**	-0.005*
	(0.003)	(0.003)
Open Cohort × Building FE	✓	
Open Cohort × Month × Apt Type FE	✓	✓
Open Cohort × Building × Apt Type FE		✓
Cluster	Building	Building
R^2	0.9737	0.9935
Adjusted R^2	0.9689	0.9921
Observations	62,049	62,049

Notes: This table presents results from estimating the following regression model: $\text{Log Price}_{cbat} = \beta \text{hasOpened}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where hasOpened_{cbt} is equal to 1 if building b in cohort c has entered its open period by month t, and Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. Standard errors are clustered at the building level (i.e. at the level of the treatment).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Consistent with the dynamic results from Figure 1.10, we find that monthly rents in buildings that enter their open period fall by around 6 basis points. This result is statistically significant at the 5% level and, as we will see in the forthcoming discussion of our 2SLS results, economically significant as well. We now run our 2SLS analysis,

$$\text{First Stage: } \text{hasRefied}_{cbt} = \beta_1 \text{hasOpened}_{cbt} + \alpha_{cba} + \alpha_{cat} + \epsilon_{cbat} \tag{1.9}$$

$$\text{Second Stage: } \text{Log Price}_{cbat} = \beta_2 \widehat{\text{hasRefied}}_{cbt} + \alpha_{cba} + \alpha_{cat} + \epsilon_{cbat} \tag{1.10}$$

where we estimate the effect of refinancing through variation in the refinancing event induced by the timing of open periods. The results from our 2SLS analysis are reported in Table 1.7 below.

Table 1.7: Effect of Refinancing on Monthly Rents (IV)

	hasRefied (1)	Log Price (2)	hasRefied (3)	Log Price (4)
hasOpened	0.169***		0.169***	
	(0.019)		(0.019)	
$\widehat{hasRefied}$		-0.033**		-0.029*
		(0.017)		(0.016)
Open Cohort × Building FE	✓	✓		
Open Cohort × Month × Apt Type FE	✓	✓	✓	✓
Open Cohort × Building × Apt Type FE			✓	✓
Cluster	Building	Building	Building	Building
Stage	First	Second	First	Second
R^2	0.8675	0.9735	0.8678	0.9933
Adjusted R^2	0.8432	0.9687	0.8403	0.9919
Observations	62,049	62,049	62,049	62,049
F-test (IV only)	2,886.2	16.58	2,863.2	49.32

Notes: This table presents our 2SLS results on the effect of refinancing on a building's monthly rents. The first stage regression model is: $\text{hasRefied}_{cbt} = \beta_1 \text{hasOpened}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where hasRefied_{cbt} is equal to 1 if building b in cohort c has refinanced by month t, and hasOpened_{cbt} is equal to 1 if building b in cohort c has entered its open period by month t. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. The second stage regression model is: $\text{Log Price}_{cbat} = \beta_2 \widehat{\text{hasRefied}}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where $\widehat{\text{hasRefied}}_{cbt}$ is the predicted value of hasRefied_{cbt} from the first stage. Standard errors are clustered at the building level (i.e. at the level of the treatment).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Columns (1) and (3) of Table 1.7 show first stage results with different sets of fixed effects while columns (2) and (4) show the corresponding second stage results. As shown by the first stage columns, our instrument is strong and relevant, with an F-stat much larger than 10. These first-stage results are also robust even after controlling for building-apartment-type-level factors (column 3). Turning to the second stage results, we now find stronger effects of refinancing on rent prices than in our previous OLS results. Specifically, columns (2) and (4) both show that monthly rents in buildings that refinance differentially fall by 3%, which is statistically significant at the 5% level. The estimate suggests that for the median Manhattan apartment, with a monthly rent of around \$3000, monthly rents would fall by around \$90, an economically significant amount. That these results are stronger, relative to those estimated by OLS, could indicate that there are cases when property owners endogenously choose to refinance and then raise the monthly rents afterwards. For instance, property owners with plans to renovate their buildings and thereafter increase prices may be more likely to select into refinancing, resulting in a refinancing effect on rent prices that is driven by the positive effects of renovation (as opposed to the interest rate differential) and hence biasing the OLS estimate upward. However, with the use of our instrument, we now capture effects of refinancings that are induced by predetermined prepayment restrictions, restrictions that are plausibly orthogonal to, say, a property owner's contemporaneous desire to renovate that may drive the decision to refinance and affect rental prices. As such, the negative effects of a refinancing on rental prices can now be uncovered.

1.5 The effect of interest rate changes on monthly rents

In this section, we aim to quantify the extent to which interest rate changes, rather that just the act of refinancing, affect rent prices. To do so, we need to know by how much a building's interest rate changes due to refinancing. While the Trepp data report the interest rate on current outstanding mortgages, we do not see the interest rate on the new commercial mortgage after refinancing. The reason is that, for us to see the new interest rate after refinancing, property owners would need to be given a commercial mortgage that appears in the Trepp data, meaning a commercial mortgage that ends up being securitized; this seldom is the case.[19] As a result, we use the median commercial mortgage rate at the time of refinancing, estimated from our Trepp data, to approximate the new interest rate that property owners experience after refinancing. Specifically, as discussed in Section 3, we use the Trepp data to construct a time-series of the U.S. multifamily mortgage rate for the median loan, and use this time-series to approximate the interest rates on new mortgages at the time of refinancing. This allows us to proxy for the change in interest rates experienced by property owners when they refinance their commercial mortgage. In Appendix Section A.3, as a robustness check, we use machine learning methods to predict, as a function of observables, the new interest rate that property owners experience after refinancing; our results remain little changed.

The difficulty in estimating the effect of interest rate changes on the rents property owners set in their buildings stems from the endogeneity of the interest rate. For instance, it could be the case that buildings that experience persistently high demand are able to maintain high occupancy rates and hence secure better loan terms, such as a lower interest rate. At the same time, these demand factors could also drive higher monthly rents for apartments in those buildings. In this case, a simple OLS regression would associate lower interest rates with higher monthly rents, even if the true causal effect of a lower interest rate is a relative reduction in monthly rents. As the relationship between the interest rates on property owners' mortgages and the monthly rents they set can be endogenous, we again instrument the interest rate using a similar identification strategy as that in the prior section.

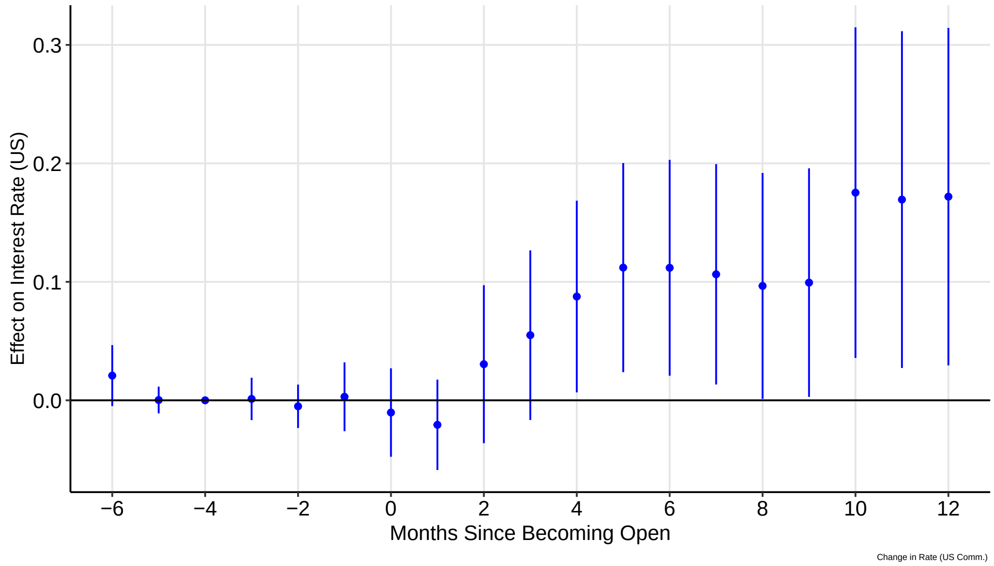
Intuitively, we instrument the change in the interest rate at the time of refinancing with

[19] While we sometimes see multiple Trepp loans for a given building, they often are not consecutive loans, as there are refinancing transactions that we see in ATTOM that fall in between the origination dates of the Trepp loans.

the timing of the open period; that is, we allow the open period to predict upward and downward changes in rates by interacting it with the counterfactual change in the interest rate that buildings would have experienced had they refinanced at the start of their open period. For the same reasons mentioned above, the timing of when an open period begins can be considered exogenous to unobservable factors (e.g. demand) driving rent price decisions, as the timing of an open event is decided at loan origination, many years in the past. Moreover, as documented in the prior section, an open event predicts a refinancing event, so an open period indicator, properly scaled by an appropriate proxy for the change in refinancing-induced rates, should also predict changes in rates as of the refinancing event. To test this, we run the following first stage regression, which continues to use the same open cohorts panel data as in the prior section.

$$\text{Interest Rate (US)}_{cbt} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb} \cdot \Delta\text{Rate}_{cb}^{\text{open}}\right) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \epsilon_{cbat} \tag{1.11}$$

where Interest Rate $(\text{US})_{cbt}$ is equal to the interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the median commercial mortgage rate in the US; Opens_{cb} is equal to 1 if building b enters its open period (i.e. is treated) in cohort c; $\Delta\text{Rate}_{cb}^{\text{open}}$ is the change in the interest rate that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period; and $\mathbf{X}_{cbt}$ includes all lower-order terms from the triple interaction in the first term. As usual, we include fixed effects to control for time-invariant differences in rent prices set across buildings in the cohort, as well as apartment-type-level rent price trends in the cohort. The coefficients of interest are the β_T's, which measure how $\Delta\text{Rate}_{cb}^{\text{open}}$ affects the difference in the interest rate between treated (those who become open) and control buildings (those who remain unopened) in month T relative to the base month. Intuitively, we allow for the effect of the open event on the interest rate to depend on the counterfactual interest rate change that would have transpired had refinancing occurred on the open event. In this way, we allow open events to predict both rate increases and decreases after a refinancing transaction. Figure 1.11 below plots the β_T's.



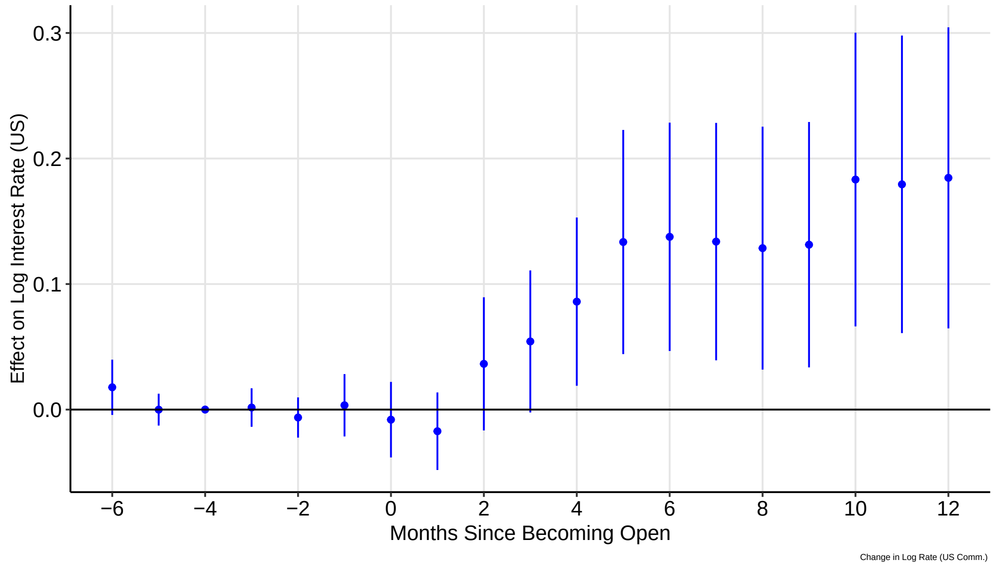
Notes: This figure shows how entering the open period affects the interest rate that property owners face after refinancing, where we allow the effect to depend on a counterfactual interest rate change that the property owner would have experienced had the refinancing occurred at the start of the open period. Specifically, the figure plots the estimated β_T's from the following regression model: $\text{Interest Rate (US)}_{cbt} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb} \cdot \Delta\text{Rate}_{cb}^{\text{open}}\right) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where $\text{Interest Rate (US)}_{cbt}$ is equal to the interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the median commercial mortgage rate in the US; Opens_{cb} is equal to 1 if building b enters its open period (i.e. is treated) in cohort c; $\Delta\text{Rate}_{cb}^{\text{open}}$ is the change in the interest rate that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period; and $\mathbf{X}_{cbt}$ includes all lower-order terms from the triple interaction in the first term. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. $\Delta\text{Rate}_{cb}^{\text{open}}$ is computed using the median commercial mortgage rate in the US at the start of the open period.

Figure 1.11: Predicting Interest Rates with Open Events (First Stage)

As depicted in Figure 1.11, once they enter their open period, buildings with a higher interest rate change as of the open event (i.e. higher $\Delta\text{Rate}_{cb}^{\text{open}}$) experience a higher interest rate after refinancing. In other words, the timing of the open event, interacted with interest rate changes as of the open event, predict similar (same direction) changes in the actual interest rate experienced after refinancing. We find similar results when looking at Log Interest Rate (US). In particular, we run the following modified first stage regression:

$$\text{Log Interest Rate (US)}_{cbt} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb} \cdot \Delta\text{Log Rate}_{cb}^{\text{open}}\right) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \epsilon_{cbat} \tag{1.12}$$

where Log Interest Rate $(\text{US})_{cbt}$ is equal to the Log interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the Log median commercial mortgage rate in the US; and ΔLog $\text{Rate}_{cb}^{\text{open}}$ is equal to the change in the Log interest rate that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period. All other variables are defined as above. Figure 1.12 below plots the β_T's, which now measure how Log $\text{Rate}_{cb}^{\text{open}}$ affects the percentage difference in the interest rate between treated (those who become open) and control buildings (those who remain unopened) in month T relative to the base month. As before, this specification allows for the effect of the open event on the Log interest rate to depend on the counterfactual Log interest rate change that would have transpired had refinancing occurred on the open event. In this way, we allow open events to predict both rate increases and decreases after a refinancing transaction.



Notes: This figure shows how entering the open period affects the interest rate that property owners face after refinancing, where we allow the effect to depend on a counterfactual interest rate change that the property owner would have experienced had the refinancing occurred at the start of the open period. Specifically, the figure plots the estimated β_T's from the following regression model: Log Interest Rate $(\text{US})_{cbt} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb} \cdot \Delta\text{Log Rate}_{cb}^{\text{open}}\right) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Interest Rate $(\text{US})_{cbt}$ is equal to the Log interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the Log median commercial mortgage rate in the US; Opens_{cb} is equal to 1 if building b enters its open period (i.e. is treated) in cohort c; $\Delta\text{Log Rate}_{cb}^{\text{open}}$ is the change in the Log interest rate that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period; and $\mathbf{X}_{cbt}$ includes all lower-order terms from the triple interaction in the first term. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. $\Delta\text{Log Rate}_{cb}^{\text{open}}$ is computed using the median commercial mortgage rate in the US at the start of the open period.

Figure 1.12: Predicting Log Interest Rates with Open Events (First Stage)

Consistent with Figure 1.11, we again see that once they enter their open period, buildings with higher interest rate growth as of the open event (i.e. higher $\Delta\text{Log Rate}_{cb}^{\text{open}}$) experience higher interest rate growth after refinancing. In other words, the timing of the open event, interacted with interest rate changes as of the open event, predict similar (same direction) changes in the actual interest rate experienced after refinancing.

We now summarize the first-stage results from our dynamic specifications by running the following regression:

$$\text{Interest Rate}_{cbt} = \beta\left(\text{hasOpened}_{cbt} \cdot \Delta\text{Rate}^{\text{open}}_{cb}\right) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \epsilon_{cbat} \tag{1.13}$$

where Interest Rate$_{cbt}$ is a placeholder for Interest Rate (US) or its Log; and hasOpened$_{cbt}$ is equal to 1 if building b in cohort c has entered its open period by month t. All other variables are defined as above, and we continue to include the usual set of fixed effects. The coefficient of interest is β, which measures the extent to which the relationship between a building becoming open and its interest rate depends on the counterfactual interest rate change as of the open event. The results are reported in Table 1.8 below.

Table 1.8: Predicting Interest Rates with Open Events (First Stage)

	Log Interest Rate (US)		Interest Rate (US)	
	(1)	(2)	(3)	(4)
hasOpened × ΔLog Rate (US Comm.)	0.129***	0.129***		
	(0.041)	(0.041)		
hasOpened × ΔRate (US Comm.)			0.110***	0.110***
			(0.039)	(0.039)
Lower Order Controls	✓	✓	✓	✓
Open Cohort × Building FE	✓		✓	
Open Cohort × Month × Apt Type FE	✓	✓	✓	✓
Open Cohort × Building × Apt Type FE		✓		✓
Cluster	Building	Building	Building	Building
R^2	0.9712	0.9712	0.9683	0.9683
Adjusted R^2	0.9658	0.9651	0.9623	0.9616
Observations	55,843	55,843	55,843	55,843

Notes: This table presents results from estimating first stage regressions of the following form: Interest Rate$_{cbt} = \beta\left(\text{hasOpened}_{cbt} \cdot \Delta\text{Rate}^{\text{open}}_{cb}\right) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Interest Rate$_{cbt}$ is equal to the interest rate (or its Log) on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the median commercial mortgage rate in the US (or its Log); hasOpened$_{cbt}$ is equal to 1 if building b in cohort c has entered its open period by month t; $\Delta\text{Rate}^{\text{open}}_{cb}$ is the change in the interest rate (or Log interest rate) that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period; and $\mathbf{X}_{cbt}$ includes all lower-order terms from the triple interaction in the first term. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. $\Delta\text{Rate}^{\text{open}}_{cb}$ is computed using the median commercial mortgage rate in the US at the start of the open period. Standard errors are clustered at the building level (i.e. at the level of the treatment).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Columns (1) and (2) are results on the Log Interest Rate (US), with different sets of fixed effects, while Columns (3) and (4) are the corresponding results on the Interest Rate (US). In particular, Columns (1) and (2) show that the effect of becoming open on a property owner's Log interest rate is greater for those who would have experienced a greater percentage change in their interest rate had they refinanced at the start of the open period. Specifically, treated buildings who enter their open period experience around 1.3% growth in their interest rate after refinancing, in response to 10% growth in the counterfactual interest rate that buildings would have experienced had they refinanced at the start of their open period. Similarly, Columns (3) and (4) show that treated buildings who enter their open period experience interest rates after refinancing that are higher by 11 basis points for every 1 pp increase in the counterfactual interest rate that buildings would have experienced had they refinanced at the start of their open period. Overall, the results of Table 1.8 show that an open period indicator, properly scaled by a counterfactual change in interest rates as of the open event, is a good predictor of the actual changes in interest rates that property owners experience once they refinance. Moreover, as discussed above, the timing of the open event is plausibly exogenous to unobservable factors driving rent prices, such as demand shocks, so this strategy ameliorates concerns of picking up a spurious relationship whereby higher prices are associated with lower interest rates, driven by a potential positive correlation between positive demand shocks and the propensity to refinance. [20]

Armed with an instrument that has a strong first stage, we now run the following 2SLS analysis:

$$\text{First Stage: Interest Rate}_{cbt} = \beta_1 \left(\text{hasOpened}_{cbt} \cdot \Delta\text{Rate}_{cb}^{\text{open}}\right) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \epsilon_{cbat} \tag{1.14}$$

$$\text{Second Stage: Log Price}_{cbat} = \beta_2 \widehat{\text{Interest Rate}}_{cbt} + \alpha_{cba} + \alpha_{cat} + \epsilon_{cbat} \tag{1.15}$$

where we estimate the effect of mortgage interest rates on a building's monthly rents by looking at variation in mortgage interest rates that is driven by the timing of open events, interacted with counterfactual changes in interest rates as of the open events. The results from our 2SLS analysis are reported in Tables 1.9 and 1.10 below.

[20] The results in Table 1.8 are also robust to the use of counterfactual interest rates that are based on other market rates (e.g. Treasury rates); see Appendix section A.4 for further analyses.

Table 1.9: Semi-Elasticity of Monthly Rents with respect to Interest Rates (IV)

	Log Price			
	(1)	(2)	(3)	(4)
Interest Rate (US)	0.003		0.003	
	(0.002)		(0.002)	
$\widehat{InterestRate}(US)$		0.022***		0.021**
		(0.008)		(0.008)
Open Cohort × Building FE	✓	✓		
Open Cohort × Month × Apt Type FE	✓	✓	✓	✓
Open Cohort × Building × Apt Type FE			✓	✓
Cluster	Building	Building	Building	Building
Stage	OLS	Second	OLS	Second
R^2	0.9748	0.9749	0.9939	0.9942
Adjusted R^2	0.9700	0.9702	0.9926	0.9930
Observations	59,446	55,843	59,446	55,843
F-test (1st stage)		1,215.2		1,212.0

Notes: This table presents 2SLS results on the effect of interest rates faced by property owners on monthly rents; in other words, this table estimates the semi-elasticity of monthly rents with respect to interest rates. The second stage regression model is: $\text{Log Price}_{cbat} = \beta \widehat{\text{Interest Rate}}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c; $\text{Interest Rate}_{cbt}$ is equal to the interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the median commercial mortgage rate in the US. $\widehat{\text{Interest Rate}}_{cbt}$ is the predicted value of $\text{Interest Rate}_{cbt}$ from the first stage; first stage regressions are shown in Table 1.8. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. Standard errors are clustered at the building level (i.e. at the level of the treatment).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.10: Elasticity of Monthly Rents with respect to Interest Rates (IV)

	Log Price			
	(1)	(2)	(3)	(4)
Log Interest Rate (US)	0.019		0.018	
	(0.012)		(0.012)	
$Log\widehat{InterestRate}(US)$		0.110**		0.105**
		(0.043)		(0.041)
Open Cohort × Building FE	✓	✓		
Open Cohort × Month × Apt Type FE	✓	✓	✓	✓
Open Cohort × Building × Apt Type FE			✓	✓
Cluster	Building	Building	Building	Building
Stage	OLS	Second	OLS	Second
R^2	0.9748	0.9749	0.9939	0.9942
Adjusted R^2	0.9700	0.9702	0.9926	0.9930
Observations	59,446	55,843	59,446	55,843
F-test (1st stage)		1,230.4		1,227.0

Notes: This table presents 2SLS results on the effect of interest rates faced by property owners on monthly rents; in other words, this table estimates the elasticity of monthly rents with respect to interest rates. The second stage regression model is: $\text{Log Price}_{cbat} = \beta \text{Log}\ \widehat{\text{Interest}}\ \text{Rate}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c; $\text{Log Interest Rate}_{cbt}$ is equal to the Log interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the Log median commercial mortgage rate in the US. $\text{Log}\ \widehat{\text{Interest}}\ \text{Rate}_{cbt}$ is the predicted value of $\text{Log Interest Rate}_{cbt}$ from the first stage; first stage regressions are shown in Table 1.8. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. Standard errors are clustered at the building level (i.e. at the level of the treatment).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Columns (1) and (3) show OLS results with different sets of fixed effects while columns (2) and (4) show the corresponding IV results. As expected from our first-stage regressions, our instruments for the interest rate are strong and relevant, with an F-stat well above 10. Overall, the IV estimates are the same sign as the OLS estimates but greater in economic and statistical significance. In particular, the IV estimates in Table 1.9, suggest that monthly rents change by around 2% for every 1 pp change in the mortgage interest rate. Turning to the results of Table 1.10, we find similar estimates.[21] In particular, we find that monthly rents change by around 11 basis points for every 1% change in the mortgage interest rate. That the OLS results are weaker in statistical and economic significance could indicate that property owners who refinance are generally those who will have better loan terms, such as a lower

[21]The results in Tables 1.9 and 1.10 are also robust to the use of counterfactual interest rates that are based on other market rates (e.g. Treasury rates); see Appendix section A.5 for further analyses.

interest rate after refinancing, and, moreover, are those who refinance when they experience positive demand shocks or when they desire to do renovations; in either case, higher prices would be associated with lower interest rates, rendering an OLS estimate that is downward biased. However, as mentioned, our IV results are estimated from variation in the mortgage interest rate induced by open events, where we allow open events to predict changes in interest rates by interacting the open event indicator with counterfactual changes in interest rates that would have occurred if a refinancing was done at the start of the open period. As the timing of the open event can be considered exogenous to contemporaneous factors that drive rent prices (e.g. demand shocks or desire to renovate), our identification strategy is able to estimate how rent prices respond to the mortgage interest rates borne by property owners. Note also that these estimates are estimated using all refinancing transactions in our sample; that is, the estimation includes refinancing transactions that led to either rate increases or rate decreases. As such, these estimates can help shed light not only on how rent prices respond over a period of decreasing market interest rates, but also on how they respond when market rates increase; recall that in the commercial mortgage market, the vast majority of loans are non-amortizing loans with balloon payments at maturity, meaning property owners are often compelled to refinance even if it leads to higher interest rates. Over the past year, the Federal Funds rate increased by 4 percentage points. Assuming commercial mortgage rates also went up by the same amount, estimates from Table 1.9 suggest that monthly rents increased by 9%[22] in buildings that refinanced and incurred a 4 pp higher interest rate on their commercial mortgage, relative to buildings that did not refinance. Over the same time period, rent prices in NYC increased by about 50%. As such, the refinancing channel for rental tenants has a non-trivial effect on rent prices and may help account for some of the rent price inflation seen in housing markets around the US.

1.6 Discussion

While our identification strategy estimates the effect of interest rates and refinancing on building rents, it's important to highlight that these effects are local average treatment effects, that is, effects for those who are affected by the instrument (Angrist et al., 1996). Operationally, this means we uncover effects for the compliers of the instrument, that is, property owners who ultimately decide to refinance because they enter their open period. But what if this subpopulation of property owners is very small? Our first stage results, which help identify the proportion of compliers, give some evidence that this subpopulation is not trivially small. Recall that estimates in Table 1.7 show that becoming open leads to a 17 pp increase in the probability to refinance, an economic and statistically significant amount. Given the strength of these first-stage results, the act of becoming open seems to

[22]For the median Manhattan apartment, with a monthly rent of around $3000, this implies an increase of $270 in monthly rent.

have a meaningful effect on the decision to refinance.[23]

It is also important to note that because our instruments are constructed using information on the timing of prepayment restrictions, which is reported solely for loans in Trepp, all our IV results are based on the subsample of securitized commercial mortgages. Naturally, this raises questions about external validity: how representative is our sample of securitized commercial mortgages of the entire commercial mortgage market? First, as of Q3 2022, approximately 51.6% of multifamily mortgage debt outstanding was securitized.[24] Second, previous papers in the literature have documented that lenders choose to focus on particular market segments within the commercial mortgage market (Griffin and Priest, 2023). This suggests that rather than certain types of loans or buildings being singled out for securitization,[25] what matters most is the lender from whom the property owner has borrowed. Does this mean we might be singling out certain lenders, which may then attract certain borrowers? This does not seem to be the case, as there is a diverse set of lenders that engage in securitization, so there is no obvious sorting of certain lenders securitizing loans. Taken together, these facts suggest that the subsample of securitized mortgages is likely rather similar to the rest of the commercial mortgage market, and therefore, we are comfortable broadly applying our results to understand how the refinancing channel works through the commercial mortgage market.

Finally, as the transmission channel of monetary policy in this paper works through refinances, a natural question is what fraction of buildings refinance in any given year. If very few buildings go through a refinancing, then the refinancing channel may not be quantitatively important. However, as Appendix Figure A.11 shows, around 10% of NYC buildings in our full sample (not restricted to Trepp) refinance every year, a sizable amount. To put the numbers into context, if we assume no other sources of rent price growth, our refinancing channel over the past year leads to a 9% growth rate in rents for 10% of NYC buildings, implying a growth in city-wide rents of 90 basis points. While this may seem small, as aggregate rent price growth in NYC has been 50% in the past year, it's important to realize that our estimates are on relative effects (e.g. buildings that refinance relative to buildings that do not), so a comparison of 90 basis points to 50% (an aggregate growth figure) is not apples-to-apples. Second, factors driving rent price inflation are in large part likely related to primitive supply and demand dynamics in the economy. That said, our estimates suggest that in buildings that refinance, tenants are likely paying over $250 dollars in rent per month, relative to tenants in other buildings, which is a non-trivial amount. These results speak to the non-uniform incidence of monetary policy on rental tenants that operates through

[23]We also run the following cohort-building regression $\text{didRefi}_{cb} = \beta \text{Treated}_{cb} + \alpha_c + \varepsilon_{cb}$, with standard errors clustered at the building level. Given dichotomous treatment (do you enter the open period or not?) and dichotomous outcome (do you refi or not?), β identifies the proportion of compliers. β is 21%, suggesting that about 1/5 of buildings in our Trepp sample ultimately refinance because they become open.

[24]Commercial/Multifamily Quarterly Databook. Q3 2022. "Multifamily Mortgage Debt Outstanding by Sector." Mortgage Bankers Association.

[25]Unlike in the residential mortgage market, there's no formulaic rule based on credit risk or loan size that makes certain loans securitized and others not.

property owners' decisions to refinance.

1.7 Conclusion

In this paper, we extend the refinancing channel of monetary policy by documenting novel spillovers to renters. Specifically, we examine whether refinancings done by property owners, in response to monetary policy, have any effect on renters in the form of rental prices. We rely on a novel dataset with apartment-level rent prices, matched to building-level refinancing data, and find, using a building's entrance into its open period as an instrument for each building's decision to refinance, that rent prices differentially fall by 3% in buildings that refinance over the post-crisis period, a period over which interest rates fell to the zero lower bound. We then measure passthrough more directly by seeing how interest rate changes, rather than simply the act of refinancing, affects rent prices. Using a similar instrumental variables strategy, we find that an increase in the building's interest rate by one percentage point leads to approximately a 2% increase in its listed rent prices. Thus, our results broadly show that the refinancing channel of monetary policy also operates through the commercial mortgage market and has passthrough effects on rent prices faced by tenants.

While our sample primarily covers a period over which interest rates fell, our results have important implications for today's inflationary environment. In particular, if property owners pass through interest savings to renters during a monetary loosening, then they might also pass through interest expenses to renters during a tightening, especially if, as is usually the case, they are compelled to refinance to fund an imminent balloon payment. Our channel thus sheds light on a potential mechanism behind today's rent price inflation, a phenomena in rental markets all around the US. Moreover, as housing costs make up a large portion of common inflation measures (e.g. CPI), our channel shows the potential difficulties the Federal Reserve may face in reducing inflation by raising interest rates.

Lastly, our results suggest a dichotomy between the effects of a monetary tightening to homeowners and renters. As commercial property owners have less control over when to refinance than homeowners, higher policy rates are more likely to be passed through to renters, who are more likely to be racial minorities, young, and have lower incomes. These dynamics point to another channel through which monetary policy may affect inequality. We leave a more thorough analysis of these dynamics to future work.

Chapter 2

Nonbank Intermediation in Times of Crisis: Evidence from Small Businesses

2.1 Introduction

While banks have historically been the main source of credit for small businesses, nonbanks, which are nondepository lending institutions, have dramatically increased their presence in the small business credit market over recent decades. For instance, since 2010, nonbanks have expanded their annual small business lending by as much as 70% (Gopal and Schnabl, 2022). Moreover, according to the 2020 Small Business Credit Survey, online lenders, a particular type of nonbank commonly referred to as "fintechs", have received a growing proportion of total small business lending applications over the past 5 years, increasing from 19% to about 33%. In short, small businesses now source funds from both banks and nonbanks in significant quantities. However, as banks and nonbanks differ in a variety of ways (e.g. regulation, funding, technology, etc.), this increased presence of nonbanks may have important effects on small business credit supply. Specifically, given the rise of nonbank institutions, what are the effects on small business lending during times of crisis, and how do these effects differ across areas with varying degrees of nonbank presence? Because small businesses play a large role in the U.S. economy–as of 2020, they accounted for over 99% of all U.S. businesses and about half of national employment–understanding the answers to these questions and the underlying mechanisms driving these effects are of crucial importance for policymakers[1].

The implications on small businesses' credit supply from increased nonbank presence are ambiguous and not clear ex-ante, not least because nonbanks and banks differ in a variety of important ways that matter for credit supply in times of crisis. On the one hand, nonbanks, especially fintech lenders, may have better technology that allow them to process loan

[1] 2020 Small Business Economic Profile, Small Business Administration

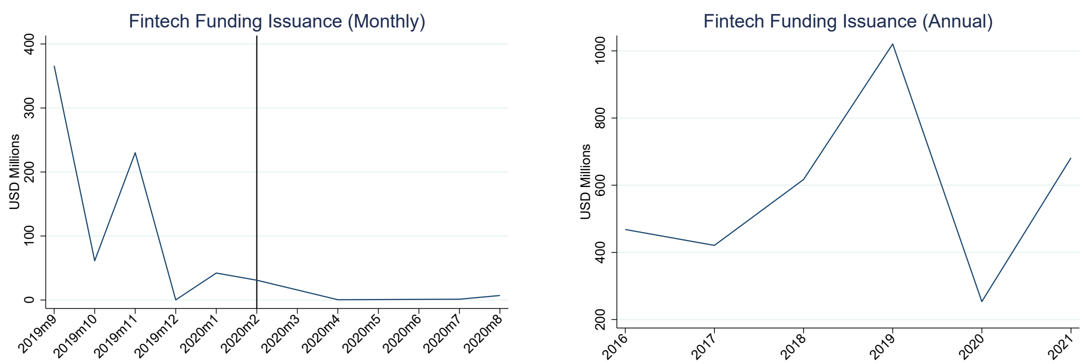
applications more quickly and better screen borrowers (Buchak et al., 2018, Fuster et al., 2019). Big data may also allow fintechs to use more information than banks in their credit approval process, arguably reducing information frictions associated with new borrowers; as a result, borrower-lender relationships, which have been documented to be important in bank lending, may be less important for nonbanks. Recent evidence from the Paycheck Protection Program (PPP) supports this phenomenon; PPP loan approval rates were roughly similar between banks and nonbanks despite the majority of nonbank loan applicants having no pre-existing relationship with the nonbank to which they applied. In contrast, the majority of bank applicants did have prior relationships with banks[2]. As a result, nonbanks may alleviate the financial accelerator mechanism (B. Bernanke et al., 1996). Specifically, during crisis periods, when borrowers lose net worth and incur adverse cash flow shocks, the banking sector could amplify these shocks if information frictions are present. However, if nonbanks can use their better technology and screening ability to help reduce information frictions between borrower and lender, they may be better able to help fund positive NPV projects in a crisis.

On the other hand, banks and nonbanks have different funding structures, which could affect their credit provision, particularly during times of crisis. For instance, while banks have access to stable deposit funding, nonbanks often fund their lending using securitization and other funding sources that can easily evaporate during times of crisis. In particular, prior literature has documented banks' superior ability to lend in times of crisis, driven by funding inflows from investors seeking safe assets and their reliance on stable deposit funding (Gatev and Strahan, 2006, Cornett et al., 2011). In contrast, nonbanks often rely on securitization markets to fund new lending, which have historically been disrupted during times of crisis, particularly in 2008 (Gorton and Metrick, 2012). As such, nonbanks have more liquid and less stable funding than banks, making them more likely to be financially constrained during times of crisis; i.e. the MM theorem fails. So if during times of crisis, investors demand safe assets and fly to quality, nonbanks may lose funding and become funding constrained. This would imply that nonbanks may be less able than banks to provide credit to positive NPV projects during downturns; as a result, areas in which nonbanks are more prevalent may actually suffer a relative reduction in credit supply. And in fact, prior research has found evidence of this differential funding effect occurring in the market for syndicated loans to larger firms (Fleckenstein et al., 2021). Specifically, the authors find that nonbank lending in the syndicated loan market is more cyclical than bank lending, and they identify that this nonbank lending cyclicality is matched by the cyclicality of their funding flows. The COVID-19 pandemic episode also provides anecdotal evidence of the potential role that nonbank funding constraints may have had in severely reducing nonbank loan originations to small businesses; for instance, in 2020, the American Banker reported that

[2] Battisto, Jessica, Nathan Godin, Claire Kramer Mills, and Asana Sarkar. 2020. "Who Received PPP Loans by Fintech Lenders?" Liberty Street Economics.

"Because [fintechs and other nonbanks] operate outside of the banking industry, they have never been able to turn to deposits as a stable source of loan funding. Instead they have relied heavily on alternative sources of liquidity – securitization markets, hedge funds and other private investors – that skeptics warned were likely to dry up during the next crisis"– American Banker article, 2020 [3]

And in fact, data on fintech funding corroborates the liquidity "dry-up" foreshadowed by the American Banker. Panel A of Figure 2.1 shows the monthly volume of fixed income, private placement, and public offering issuances by fintechs in the six months before and after March 2020. It is clear that fintech lenders' funding was very restricted during the pandemic. Moreover, Panel B of Figure 2.1 shows that this reduction in funding was significant, as 2020 levels reached even below funding levels seen in 2016.



(a) Monthly fintech funding

(b) Annual fintech funding

Notes: This figure plots monthly and annual volumes of fixed income, private placement, and public offering issuances made by fintechs. Data are sourced from Capital IQ, and fintechs are identified as firms engaging in "commercial digital lending." The monthly data range from September 2019 to August 2020, and the annual data range from 2016 to 2021.

Figure 2.1: Fintech Funding Volumes

Note that nonbank funding constraints would not be consequential if relationships were unimportant and borrowers could costlessly switch to other, unconstrained lenders. However, the literature extant has thoroughly documented the stickiness and, thus, importance of relationships in commercial lending, particularly to opaque small businesses (Petersen and Rajan, 1994, Berger and Udell, 1995, Degryse and Ongena, 2005, Chodorow-Reich, 2014).

One interesting and important interaction to consider is that areas with lower incomes

[3]Wack, Kevin. 2020. "Battered by coronavirus crisis, online lenders face reckoning." *American Banker*. May 20.

and higher minority shares have actually seen more pronounced increases in nonbank presence over time. Thus, not only are the implications of increased nonbank presence on small business supply ambiguous but also, the fact that nonbanks have focused more in certain areas raises questions about inequality. Moreover, in this paper, we focus on the COVID-19 pandemic as the most recent crisis episode, and it was exactly these areas that were differentially exposed to the economic downturn caused by COVID-19. Therefore, to the extent that nonbank funding constraints played a large role in reducing credit supply in areas where nonbanks are prevalent, nonbanks could have worsened the already severe downturn in these areas during the COVID-19 pandemic and perpetuated an uneven recovery. Shedding light on whether these channels are operative is of great interest to policymakers; Jerome Powell, the Federal Reserve Chairman, has acknowledged the uneven recovery, saying that

"The economic downturn has not fallen equally on all Americans, and those least able to shoulder the burden have been hardest hit"– Jerome Powell, 2021

Thus, in this paper, we investigate how the effects of downturns on small business credit supply differs for banks and nonbanks and what that now means for areas with varying levels of nonbank reliance, focusing primarily on the COVID-19 pandemic episode. Specifically, we test, by comparing lending within ZIP code to control for local demand and opportunities, whether banks and nonbanks change their supply of credit differentially in response to the onset of the COVID-19 pandemic. Then, to obtain local net effects, we also examine whether changes to local small business credit supply varied according to the degree of nonbank presence in that area. In doing so, we identify whether nonbanks decreased their supply of credit differentially, perhaps driven by funding constraints, and see if that has any aggregate implications to the areas that rely particularly heavily on these nonbank lenders.

To explore these questions, we use a novel dataset composed of all the Uniform Commercial Code (UCC) filings in Texas. The UCC is a set of laws in the U.S. that concern all commercial transactions. As part of the UCC, secured creditors are given the right to make a public filing at the state-level detailing the borrower's collateral to which they have a claim. These filings are used in the event of borrower bankruptcy to ascertain the priority of claims. Given their importance to creditor rights and the low cost of filing, these filings essentially cover the universe of all secured loans lent by both banks and nonbanks. The inclusion of nonbanks in this dataset differentiates it from other datasets often used in the literature for small business lending. While this dataset only has information on secured lending, we are still capturing an important subset of total small business credit, as over 85% of small business loans are secured by collateral[4]. With this data, we can thus measure the number and share of filings/loans originated by either banks or nonbanks at any geographic level, as well as the number and share of active relationships maintained by these lenders over time.

Equipped with this dataset, we first document that nonbank and fintech lenders have be-

[4]Survey of Terms of Business Lending, Federal Reserve Board, 2017; the figure rises to 93% for loan sizes under $100,000.

come increasingly important in small business credit intermediation, confirming prior findings in the literature (Gopal and Schnabl, 2022). In particular, nonbanks grow from representing less than 20% of loan originations in 2002, the first year in our sample, to representing over 60% of loan originations in 2020. Fintechs, which are online lenders and a subset of nonbanks, had almost no presence in 2002 but account for around 40% of all nonbank filings by 2020.

Then, we zoom in on the COVID-19 pandemic, the first severe economic downturn facing many of these new small business nonbank lenders, to explore whether banks and nonbanks adjust their small business credit supply differentially in response to downturns. To isolate changes in credit supply from those in credit demand, we look only at differences in lending that occur within the same ZIP code. That is, in the spirit of Khwaja and Mian, 2008, we run a differences-in-differences analysis that compares changes in credit offered by bank, fintech, and nonfintech nonbank lenders to the same zipcode, using ZIP-by-month fixed effects (FE) to control for ZIP-level trends. Doing so controls for quite local credit demand over time, allowing us to interpret any differences in lending between the different lenders to stem from their supply.

We find that, relative to banks, fintechs significantly decreased their supply of small business credit in the six months after March 2020, relative to the six months before March 2020. Specifically, fintechs differentially reduced their credit supply by 6.7%, relative to banks. Nonfintech nonbanks, however, slightly increased their supply of small business credit, by 2.4%, relative to banks after the onset of the pandemic. Thus, these results show that when looking within ZIP codes, fintechs in particular dramatically decreased their credit supply to small businesses during the pandemic.

Next, we explore whether lenders' changes in credit supply depend on the growth rate of fintech filings that ZIP codes experienced between 2016 and 2019. The idea is that if a ZIP code experienced high levels of fintech entry leading up to the downturn, it would be more likely to be affected by a decrease in fintech credit supply than another ZIP code that experienced lower levels of fintech entry. And in fact, we find that ZIP codes that had higher growth in fintech filings did indeed experience a more severe reduction of fintech credit supply between March and August 2020. Specifically, we find that ZIP codes in the 90th percentile of fintech entry experienced an additional 3% drop of credit supply from fintechs, relative to banks, than ZIP codes in the 10th percentile of fintech entry. Therefore, these results suggest that the fintech credit crunch was more severe for ZIP codes more reliant on fintech loans.

However, note that these within-ZIP results could reflect substitution away from fintechs toward other types of lenders, rather than an aggregate decrease in credit supply within the ZIP code. Therefore, to identify net effects, we examine whether there were differential changes to total credit supply across ZIP codes with varying levels of fintech entry. That is, we test whether the change in the total number of UCC filings from the six month period before March 2020 to the six month period after March 2020 varies across ZIP codes that faced different growth rates of fintech filings between 2016 and 2019. We find that ZIP codes with higher rates of fintech entry leading up to the pandemic experienced sharper declines in aggregate lending during the pandemic. Specifically, we find that ZIP codes in the 90th percentile of fintech entry experienced an 11% reduction in total credit supply relative to those in the 10th percentile. Therefore, the evidence suggests that the reduction in credit supply by fintech lenders could not be offset completely by other lenders in the ZIP code, particularly in areas that came to rely more on fintech lenders in the years prior to the pandemic.

Taken together, our results show that banks and nonbanks, but fintechs in particular, adjust their credit supply differentially in response to economic crises. Specifically, after controlling for local demand trends, fintechs reduce their credit supply significantly, relative to other lenders, and this reduction is more severe in ZIP codes that have come to rely more on fintech lenders. This reduction is not offset by compensating increases in credit supply by other lenders, as we find that total lending in these ZIP codes also decrease relatively more than in other ZIP codes less reliant on fintechs.

The rest of the paper proceeds as follows. Section 2 contains a brief review of the literature. In Section 3, we discuss the data, and in Section 4, we document the recent growth of nonbank and fintech lenders in the small business credit market. Section 5 describes the empirical methodology and presents the results of the lender-specific supply channel, and Section 6 does the same for the aggregate ZIP code-level studies. Section 7 concludes.

2.2 Related Literature

We contribute to the literature that studies the increasing role of nonbanks in various credit markets. For instance, Buchak et al., 2018 study the determinants of nonbank entry in the mortgage market and find that both new bank regulations and technological advancements played important roles. Fuster et al., 2019 also study fintechs in the mortgage market and examine the ways they differ from traditional bank lenders. In the small business credit market, Gopal and Schnabl, 2022 document the rise of fintechs and nonbanks and find evidence that new bank regulations may also have played a role in the increased importance of these new lenders. We instead focus on the differential credit supply responses by banks and nonbanks, specifically during economic crises, and the resulting implications on local credit supply. Erel and Liebersohn, 2022 use data from the Paycheck Protection Program (PPP) to document that fintech lenders seem to disproportionately lend to areas historically underserved by the banking system, i.e. areas with lower incomes and a higher minority share. Although we also use the COVID-19 pandemic to study differences in fintech lending, we do not look at the PPP, as lending through this unique government program had essentially no credit risk due to the guarantee from the Small Business Administration.

We also contribute to the growing literature that studies how the rise of nonbanks may affect financial stability. Fleckenstein et al., 2021 document that in the syndicated loan market, changes in nonbank lending drives much of the cyclicality in syndicated lending and provide evidence that this cyclicality may be driven by nonbanks' funding flows. We instead focus on the small business credit market, an economically significant sector of the economy, and find evidence that the credit supply of fintech nonbank lenders in particular is relatively more cyclical.

Finally, we also touch on the literature that examines the importance of relationships for small business borrowers, particularly in times of crisis. Because we find that reductions in fintech credit supply is not compensated by analogous increases in lending by other lenders, this indicates that borrowers cannot costlessly switch between lenders, at least during downturns. This suggests that lending relationships may be important for small business borrowers to secure credit, especially in recessions. Comparably, Chen et al., 2017 show that small business lending by large banks was particularly negatively impacted during and after the 2008 financial crisis, which had extended real effects, implying small business borrowers were not able to costlessly switch to new lenders. Chodorow-Reich, 2014 also show similar results for the syndicated loan market, using banks' exposure to Lehman Brothers as a measure of lender health. Granja et al., 2022 also document that average borrower-lender distances fall during crises, suggestive of higher risk aversion by lenders during these times.

2.3 Data

The main dataset is composed of all the Uniform Commercial Code (UCC) filings in Texas[5]. UCC filings are state-level public records that are submitted by secured lenders to detail the collateral to which they have a claim. In secured loan transactions, a borrower provides a lender with a security interest, detailing the loan collateral. The secured lender is then able to perfect her security interest (i.e. ensure priority over the collateral) by submitting a UCC filing (also called a financing statement) at the state-level, to the Secretary of State office in the state in which the borrower is located. These filings include the borrower and lender names, along with their addresses, and the collateral description. Once a filing is made, it is valid for 5 years unless (1) the lender files a continuation statement to extend the filing for another 5 years or (2) the borrower, assuming the loan is fully paid, files a termination statement. Given that lenders are considered unsecured in the eyes of the law if a UCC filing is not made, lenders are strongly incentivized to file financing statements in their secured loan transactions. The cost to file a claim is also quite low.[6] As such, the Texas UCC dataset can be thought to contain the universe of all secured lending in the state. The main advantage of this dataset is that it includes secured lending by both banks and nonbanks, which allows us to look at how the nature of intermediation in the small business credit market has changed in response to the introduction and rising importance of nonbanks. While small businesses also engage in unsecured lending (e.g. revolver lines of

[5]We focus on Texas because the data are easily accessible. In future work, we hope to gain access to proprietary data that extend to the entire U.S. as in Gopal and Schnabl (2020).

[6]The fee to file most statements online is \$5. The fee to file most statements through mail or fax is \$15. Specialty filings may cost \$60, but we specifically exclude these from our sample, as they relate to manufactured home sales, public finance transactions, or utilities providers. Source: Texas Secretary of State

credit), secured lending represents a significant fraction, over 85%, of small business lending[7]. One disadvantage of the UCC dataset is that it only provides information on the decision to file; there is no information on loan terms, such as loan size or interest rate[8]. Nevertheless, we are still able to compute lending volumes over time, using loan counts. We do so by summing the number of UCC filings made by a given lender by ZIP code and month to create a lender-ZIP-month panel, the base data on which our empirical analyses are run.

We restrict our sample of UCC filings to those secured by parties that made at least twenty filings between 2002 and 2020. Doing so allows us to check the identity of lenders carefully and, more importantly, ensures that the secured parties we analyze are in fact in the business of making loans, rather than engaging in one-off extensions of credit. We end up with 531 unique lenders that make 78% of the filings in the Texas UCC data.

We also currently focus on filings with blanket liens, which gives the lender a security interest in all assets of the borrower. We do this primarily because much of our analysis focuses on fintechs, given their rapid growth in the last decade (3000% in terms of filings) as well as their hypothesized funding constraints, and fintechs essentially only engage in secured lending with blanket liens. Moreover, blanket liens are the most common type of collateral listed in the UCC data. For instance, more than half of the filings in 2020 are those with blanket liens. Nevertheless, for future work, we do intend to look more closely at specific forms of collateral, with the understanding that doing so would mean that we would mostly be looking at lending by only banks and nonfintech nonbanks.

Our second dataset is the FDIC Summary of Deposits (SOD), which is an annual survey of banks that contains branch-level data for all FDIC-insured depository institutions. The data include the geographic distribution of the dollar amount of deposits and the number of branches. We use this dataset to identify depository institutions in the UCC data using a fuzzy merge. We define nonbanks as institutions that do not hold deposits–that is, institutions that do not match to the SOD. Because we restrict our sample to institutions that made at least 20 filings made over the sample period (2002-2020), we manually check the lender categorizations. Of the set of nonbanks, we further manually identify the set of fintech lenders, which are non-deposit-taking lenders that primarily lend online. Fintech lenders in our sample are mainly composed of Merchant Cash Advance (MCA) companies, which are lenders that take a fraction of their borrower's future business sales as loan payments. MCA companies usually require a blanket lien. In contrast to other types of lenders, fintech institutions frequently don't appear by name in the UCC data. Instead, they tend to use UCC filing agencies (e.g. Corporation Service Company), which are non-lenders who make UCC filings on behalf of lenders. These UCC filing agencies are primarily used by MCA companies (see Gopal and Schnabl, 2022), so filings submitted by UCC filing agencies in our sample are categorized as filings made by fintech lenders.

Moreover, to control for a host of ZIP-level observables, we obtain annual demographic

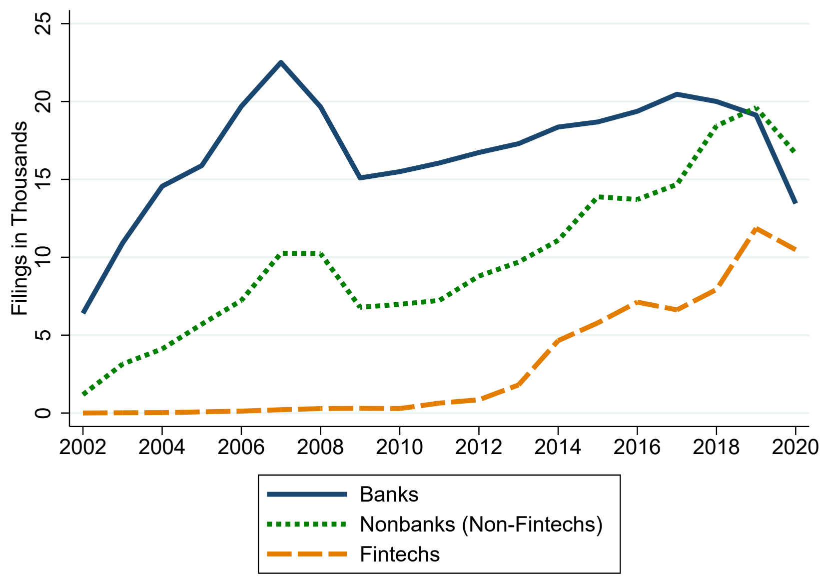
[7]Survey of Terms of Business Lending, Federal Reserve Board, 2017; the figure rises to 93% for loan sizes under $100,000.

[8]Loans backed by real estate are also not included, as these filings are filed at the local level, not at the state-level.

information from the FFIEC, which uses data from the Census Bureau. Because the data are reported at the census tract level, we aggregate up the variables to the appropriate ZIP code, weighting by tract population. Finally, we obtain information about the small businesses in our sample, namely their industry classification, from Infogroup.

2.4 Rise of Fintechs and Nonbanks

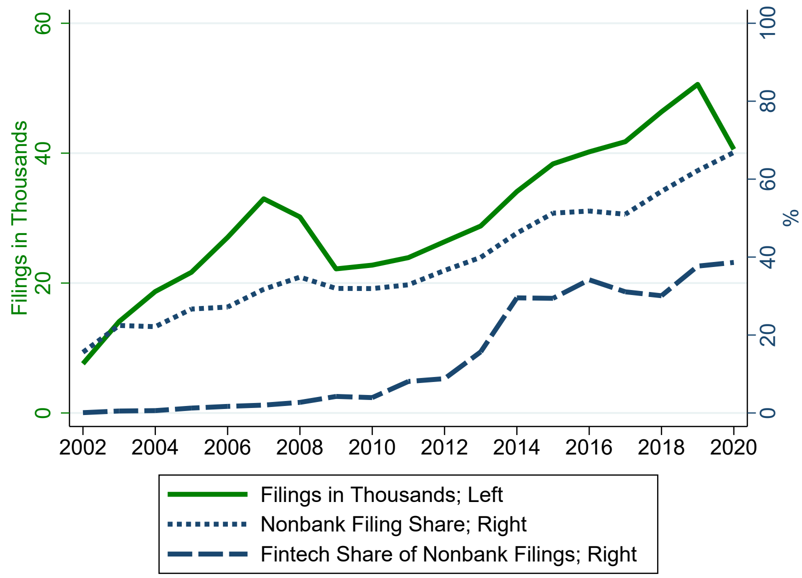
In this section, we document the rise of nonbanks and fintech lenders in the secured small business credit market, which replicates and expands on prior findings in the literature (Gopal and Schnabl, 2022).



Notes: This figure plots the annual number of UCC filings from 2002 to 2020 made by banks, nonbanks (excluding fintechs), and fintech institutions. The sample is restricted to lenders with at least twenty filings and to loans collateralized by blanket liens.

Figure 2.2: Number of UCC Filings by Lender Type

In Figure 2.2 above, we plot for each year the number of UCC filings made by lender type: banks, nonbanks (excluding fintechs), and fintech institutions. As shown in Figure 2.2, the growth rate of filings from nonbank and fintech lenders is higher than that of banks, particularly in the post-crisis period, implying an increasing nonbank share of filings. Moreover, by 2020, one sees that nonbank and fintech lenders account for the majority of UCC filings, a dramatic change from earlier in the sample period.



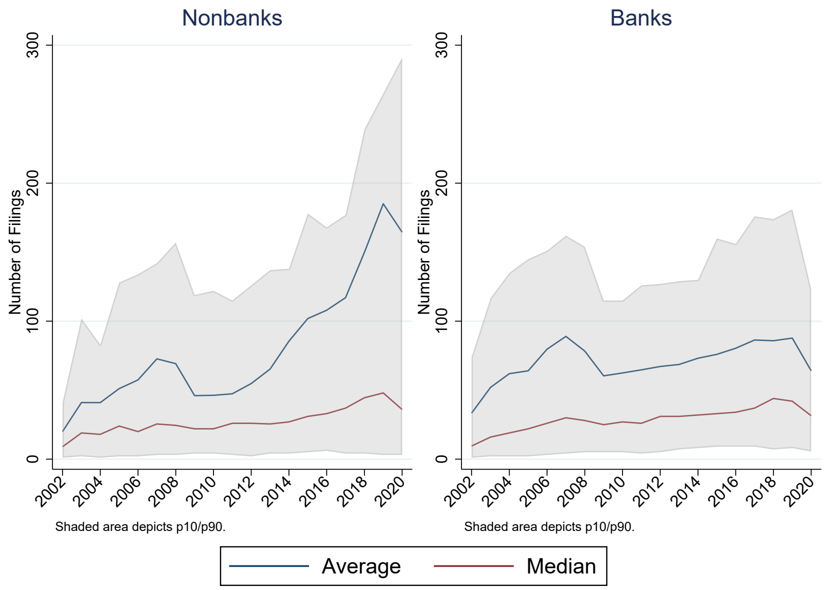
Notes: The green, solid line in the figure plots the annual number of UCC filings made by all lending institutions between 2002 and 2020. The blue, dotted line plots the share of UCC filings made by nonbank lenders during the sample period. The blue, dashed line plots the share of nonbank UCC filings made by fintechs in particular. The sample is restricted to lenders with at least twenty filings and to loans collateralized by blanket liens.

Figure 2.3: Growth of Fintechs and Nonbanks

Over the sample period, from 2002 to 2020[9], about 530,000 filings were made in total. As shown in Figure 2.3, the number of UCC filings made each year rises over time. However, the share of these filings made by nonbanks grows from under 20% to over 60%. Fintech lenders, which entered the market significantly in the post-crisis period, become increasingly important among nonbanks, accounting for around 40% of all nonbank filings by 2020. As these filings correspond to blanket liens, no two or more filings in a given year are likely to be ascribed to the same borrower. Thus, the total number of filings in a given year can be seen as a proxy for the number of businesses borrowing secured in that year. Seen in this way, the number of businesses borrowing secured in a given year grew from around 14,000 to 41,000 between 2002 and 2020. Given that the number of firms in Texas with more than 500 employees ranged from 4,941 in 2002 to 6,171 in 2018, the majority of firms borrowing secured in a given year must be small businesses[10].

[9] We restrict the sample from 2002 onwards. The reason is, prior to a regulatory change in July 2001, lenders were required to submit several UCC filings for the same loan, one for each state in which the borrower had tangible property.

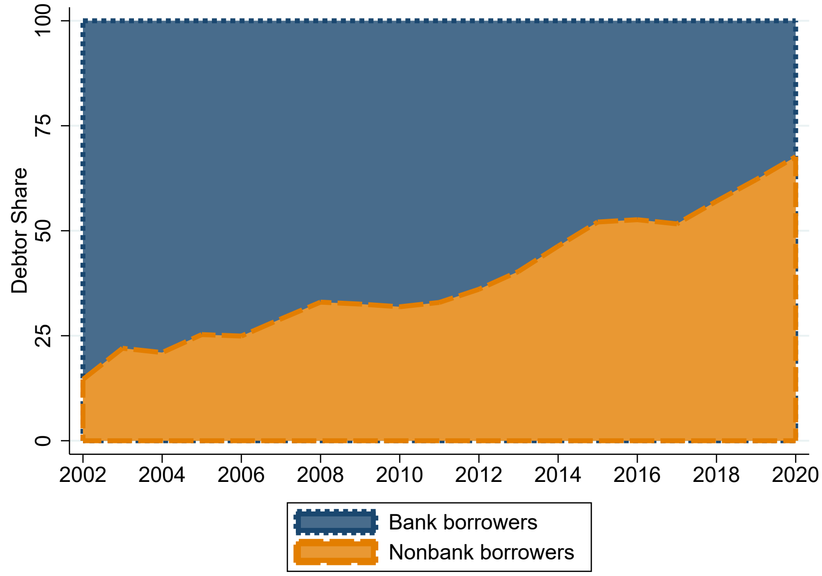
[10] 2002, 2018 (latest) Statistics of U.S. Businesses (SUSB); Cenus Bureau.



Notes: This figure plots for nonbanks (left) and banks (right), annual values for the mean and the 10th, 50th, and 90th percentiles for the number of filings made by lenders in the lender type. The blue line plots the mean value, the red line plots the median value, the bottom of the shaded area represents the 10th percentile, and the top of the shaded area represents the 90th percentile. The sample is restricted to lenders with at least twenty filings and to loans collateralized by blanket liens.

Figure 2.4: Growth of Nonbanks vs. Banks

We next look at the distribution of the number of filings done by each lender type over time. As shown in Figure 2.4, the average nonbank lender makes more UCC filings, relative to banks, over time. In particular, the number of filings that the average nonbank submits has grown such that towards the end of the sample period, the average nonbank now submits roughly twice as many UCC filings than that of the average bank. In contrast, the number of filings made by the average bank stays roughly flat.



Notes: This figure plots, in yellow and on the bottom, the share of unique borrowers that borrow only from nonbanks in a given year and, in blue and on the top, the share of unique borrowers that borrow only from banks in a given year. The plot does not include the trivial number of borrowers that borrow from both banks and nonbanks in a single year. The sample is restricted to lenders with at least twenty filings and to loans collateralized by blanket liens.

Figure 2.5: Growth of Nonbank borrowers

In a similar vein, Figure 2.5 shows that the share of total small businesses borrowing secured from nonbanks has grown, from around 15% to over 60%. In short, these figures show that small businesses now source their funds in significant quantities from both banks and nonbanks. Given the rise of nonbank institutions in the small business credit market, do nonbanks enter uniformly across all areas? Or is there concentrated presence of nonbanks in particular areas? As Table 2.1 shows, areas with a higher nonbank presence are associated with lower incomes and higher minority shares.

Table 2.1: What characterizes areas with high nonbank presence?

	(1) Log Median Income	(2) Poverty Share	(3) Minority Share
Nonbank Share of Relationships	-0.00856***	0.162***	0.655***
	(0.000521)	(0.0132)	(0.0255)
County × Year FE	✓	✓	✓
Cluster	Tract	Tract	Tract
Obs	59262	59661	59661
R^2	0.293	0.231	0.501
Adj R^2	0.262	0.197	0.479

Notes: This table presents results from the following regression model: $X_{rct} = \beta \text{Nonbank Share of Relationships}_{rct} + \alpha_{ct} + \epsilon_{rct}$, where X_{rct} is the log median income, poverty share, or minority share of census tract r in county c in year t. Nonbank Share of Relationships$_{rct}$ is the share of active lending relationships secured by nonbanks in census tract r in county c for year t, where an active relationship is defined as a non-expired filing that may have been initiated prior to year t. Moreover, α_{ct} is County-Year FE. Standard errors are clustered at the census tract level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

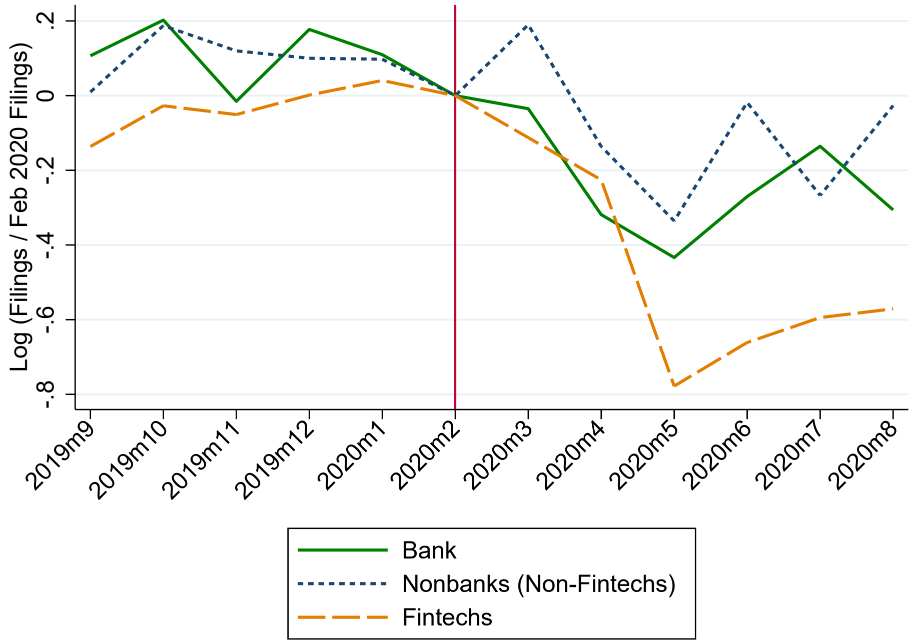
In particular, census tracts with a higher nonbank share of relationships (i.e. those tracts with a higher nonbank share of UCC filings that are still active) are those with lower incomes and higher minority shares. These correlations are further corroborated by results in the literature extant. Erel and Liebersohn, 2022, for example, find that in the context of the PPP, fintechs were disproportionately used in areas with lower incomes and higher minority shares. As mentioned, these areas were also more exposed to the pandemic. To the extent that nonbank funding constraints decreased credit supply to these areas, increased presence of nonbanks may have worsened an already severe downturn in these areas. Therefore, the increased presence of nonbanks in the small business credit market has the potential to have important effects on credit intermediation across local geographies, effects to which we will turn in the next section. In particular, credit supply during times of crisis could depend on the degree of nonbank presence in certain areas. This would imply differential exposures to credit supply shocks across geographic areas.

2.5 Differential Changes in Lenders' Credit Supply

During times of crisis, the implications of increased nonbank presence on small business credit supply are not clear ex-ante. On the one hand, nonbanks, especially fintech lenders, may have better technology and rely on big data that allow them to process loan applications quicker and better screen borrowers (Buchak et al., 2018, Fuster et al., 2019). The use of big data by fintechs have also allowed them to use more information than banks in their credit approval process, arguably reducing information frictions and increasing credit supply (Berg et al., 2020). Such evidence suggests that, in times of crisis, nonbanks may potentially increase credit supply to small businesses, given their improved ability to identify positive

NPV projects. On the other hand, banks and nonbanks have different funding structures, which may have implications on credit provision during times of crisis. In particular, while banks have access to stable deposit funding, nonbanks and fintechs, in particular, often fund their lending using securitization and other funding sources that can easily evaporate during times of crisis. Banks, for instance, have been known to possess a superior ability to lend in times of crisis, driven by funding inflows from investors seeking safe assets and a reliance on stable deposit funding (Gatev and Strahan, 2006, Cornett et al., 2011). In contrast, nonbanks often rely on securitization markets to fund new lending, markets that have historically been disrupted in times of crisis, particularly in 2008 (Gorton and Metrick, 2012). As such, nonbanks have more liquid and less stable funding than banks, making them more likely to be financially constrained during crises, as investors demand safe assets and fly to quality. This would imply that nonbanks may be less able than banks to provide credit to positive NPV projects during downturns. While one could argue that borrowers could respond by seeking credit from relatively less financially constrained lenders, this assumes that relationships are unimportant. In contrast, the literature extant has thoroughly documented the stickiness and, thus, importance of relationships between lenders and borrowers (Petersen and Rajan, 1994, Berger and Udell, 1995, Degryse and Ongena, 2005, Chodorow-Reich, 2014).

In this section, to test whether banks and nonbanks adjust their small business credit supply differentially during downturns, we focus on a window from September 2019 to August 2020, a twelve month period centered around March 2020, when COVID-19 began to affect everyday life in the United States. We use the COVID-19 pandemic because it was the first recession since the financial crisis and, thus, the first economic downturn many of these new nonbank lenders faced. To start, Figure 2.6 below plots, by lender type, the log of total filings across the state of Texas, normalized to log filings in February 2020, for the aforementioned period. Visually, the plot suggests that while all lenders decreased their lending during the COVID-19 pandemic, fintech lenders reduced their lending most significantly. However, this figure alone cannot tease out whether this reduction is due to supply-side changes from fintechs or demand-side changes from businesses that borrow from fintechs.



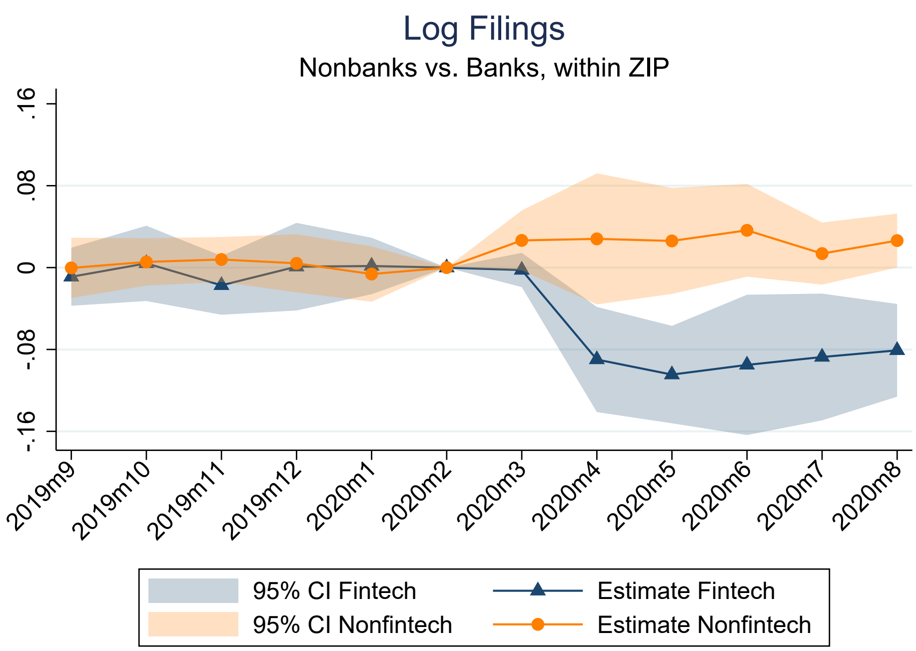
Notes: This figure plots, for each lender type and month, the log of the number of filings, divided by the lender type's number of filings in February 2020, from September 2019 to August 2020.

Figure 2.6: Changes in Lending Volume, by Lender Type

Therefore, to isolate changes in credit supply from those in credit demand, we compare only differences in lending that occur within the same ZIP code. That is, in the spirit of Khwaja and Mian, 2008, we run a differences-in-differences analysis that compares changes in credit offered by bank, fintech, and nonfintech nonbank lenders to the same zipcode, using ZIP-by-month fixed effects (FE) to control for ZIP-level trends. Doing so controls for local credit demand over time, allowing us to interpret any differences in lending between the different lenders to stem from changes in their supply. Specifically, we run the following dynamic differences-in-differences specification

$$\begin{aligned}\log(Filings_{zlt}) = \alpha_{zt} + \gamma_l + \sum_T \beta_T^F \left(\mathbf{1}_{l\in F} \times \mathbf{1}_{t=T}\right) \\ + \sum_T \beta_T^{NF} \left(\mathbf{1}_{l\in NF} \times \mathbf{1}_{t=T}\right) + \varepsilon_{zlt}\end{aligned} \tag{2.1}$$

where $\log(Filings_{zlt})$ is the log number of UCC filings in ZIP code z by lender l in month t, α_{zt} is the ZIP-by-month FE, and γ_l is the lender FE. Moreover, $\mathbf{1}_{l\in F}$ is an indicator variable that equals one if lender l is a fintech lender, $\mathbf{1}_{l\in NF}$ is an indicator variable that equals one if lender l is a nonfintech nonbank lender, and $\mathbf{1}_{t=T}$ is an indicator variable that equals one for the month t. Note that banks are the reference lender type, and February 2020 is the reference month. Therefore, the coefficients of interest are β_T^F and β_T^{NF}, which measure the differential change in credit supply for fintechs and nonfintech nonbanks, relative to that of banks, between month T and February 2020, accounting for time-varying ZIP code specific factors, such as demand shocks. Lender FEs also help control for time-invariant lender characteristics and, importantly, ensures that the estimates are not driven by a changing panel, i.e. different lenders lending in the six months following March 2020 versus in the six months prior. An identifying assumption for this specification is that relative demand for loans from the different types of lenders does not change through the sample period. Figure 2.7 plots the point estimates and the confidence intervals for the two coefficients of interest.



Notes: This figure shows the extent to which fintech and nonfintech nonbank lenders, relative to banks, changed their credit supply to small businesses in the same ZIP code after the start of the COVID-19 pandemic, using ZIP-by-month FE to control for ZIP-level demand trends. Specifically, the figure plots the estimated β_T^F's and β_T^{NF}'s from the following regression model: $\log(Filings_{zlt}) = \alpha_{zt} + \gamma_l + \sum_T \beta_T^F (\mathbf{1}_{l\in F} \times \mathbf{1}_{t=T}) + \sum_T \beta_T^{NF} (\mathbf{1}_{l\in NF} \times \mathbf{1}_{t=T}) + \varepsilon_{zlt}$, where $\log(Filings_{zlt})$ is the log number of UCC filings in ZIP code z by lender l in month t, α_{zt} is the ZIP-by-month FE, and γ_l is the lender FE. Moreover, $\mathbf{1}_{l\in F}$ is an indicator variable that equals one if lender l is a fintech lender, $\mathbf{1}_{l\in NF}$ is an indicator variable that equals one if lender l is a nonfintech nonbank lender, and $\mathbf{1}_{t=T}$ is an indicator variable that equals one for the month t. Note that banks are the reference lender type, and February 2020 is the reference month.

Figure 2.7: Changes in Nonbank Credit Supply during COVID-19

The figure shows that prior to February 2020, the credit supply of both fintechs and nonfintech nonbanks, relative to banks, did not differ significantly from February 2020, which aligns with there being parallel pre-trends for all lender types up through the start of the pandemic. However, in the months following, while the credit supply of nonfintech nonbanks continue to follow that of banks closely, credit supply by fintechs deviates substantially, with a significant decrease that remains depressed through August 2020. At its trough, in May 2020, fintech lenders' credit supply dropped by almost 8% more than banks', relative to their levels in February 2020. As we control for time-varying local demand, these results suggest that fintech credit supply did indeed differentially fall during the COVID-19 pandemic. Next, in order to quantify the overall extent to which the credit supply of the various lender types fell differentially in the entire six month period following the start of the pandemic, we run the following differences-in-differences specification

$$\log(Filings_{zlt}) = \alpha_{zt} + \gamma_l + \beta^F \left(\mathbf{1}_{l\in F} \times Post_t\right) + \beta^{NF} \left(\mathbf{1}_{l\in NF} \times Post_t\right) + \varepsilon_{zlt} \tag{2.2}$$

that replaces the individual month dummies with a single $Post_t$ indicator that equals one if month t is between March and August 2020, inclusive. All other variables remain the same, and we continue to include ZIP-by-month FEs as well as lender FEs. As before, the coefficients of interest are β^F and β^{NF}, which measure the differential change in credit supply for fintechs and nonfintech nonbanks, relative to that of banks, from before to after the start of the COVID-19 pandemic. The results are reported in Table 2.2 below.

Table 2.2: Cumulative Change in Credit Supply during COVID-19

	(1)
	Log Filings
Non-Fintech × Post	0.0242*
	(0.0141)
Fintech × Post	-0.0667**
	(0.0296)
Zip × Month FE	✓
Lender FE	✓
Cluster	Lender
Obs	32639
R^2	0.341
Adj R^2	0.151

Notes: This table quantifies the overall extent to which the credit supply of fintechs and nonfintech nonbanks, relative to that of banks, changed in the entire six month period following the start of the COVID-19 pandemic. Specifically, the table presents the results for the following regression model: $\log(Filings_{zlt}) = \alpha_{zt} + \gamma_l + \beta^F (\mathbf{1}_{l\in F} \times Post_t) + \beta^{NF} (\mathbf{1}_{l\in NF} \times Post_t) + \varepsilon_{zlt}$, where $\log(Filings_{zlt})$ is the log number of UCC filings in ZIP code z by lender l in month t, α_{zt} is the ZIP-by-month FE, and γ_l is the lender FE. Moreover, $\mathbf{1}_{l\in F}$ is an indicator variable that equals one if lender l is a fintech lender, $\mathbf{1}_{l\in NF}$ is an indicator variable that equals one if lender l is a nonfintech nonbank lender, and $Post_t$ is an indicator variable that equals one if month t is between March and August 2020. Note that banks are the reference lender type. Standard errors are clustered at the lender level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The results in Table 2.2 generally corroborate the results in Figure 2.7. For instance, cumulatively, fintech lenders decreased their credit supply significantly more than banks in the post COVID-19 period; specifically, the credit supply of fintech lenders fell by 6.7% relative to banks. Interestingly, the cumulative results pick up a slight 2.4% increase in the credit supply of nonfintech nonbank lenders, relative to banks, in the post COVID-19 period. These latter results are not necessarily incongruent with those in Figure 2.7; given that the β coefficients in equation 2.1 are estimated over a month's worth of data rather than over six months as in equation 2.2, power could have been an issue in the former results. Nevertheless, Table 2.2 confirms that when looking within ZIP codes, fintechs in particular, relative to both banks and nonfintech nonbanks, dramatically decreased their credit supply to small businesses during the pandemic. In Appendix Section B.1, we support these within-ZIP code results with within-industry and within-debtor analyses.

2.5.1 Which areas experienced greater relative cuts in fintech credit supply?

If this reduction in fintech credit supply were to decrease the aggregate supply of credit available to businesses in a local area, one would expect that areas that have come to rely more on fintech lenders would experience the largest reduction in aggregate credit supply. Thus, to begin exploring whether this is the case, we test whether lenders' changes in credit supply depend on the rate of fintech entry that ZIP codes experienced leading up to 2020. The idea is that if a ZIP code experienced high levels of fintech entry leading up to the downturn, it would be more likely to be affected by a decrease in fintech credit supply than another ZIP code that experienced lower levels of fintech entry. To test whether these interaction effects exist, we run the following triple difference specification

$$\begin{aligned}\log(Filings_{zlt}) = \alpha_{zt} + \gamma_l + \beta^F\left(\mathbf{1}_{l\in F} \times Post_t\right) + \beta^{NF}\left(\mathbf{1}_{l\in F} \times Post_t\right) \\ + \lambda^F\left(\mathbf{1}_{l\in F} \times Post_t \times FintechEntry_z\right) \\ + \lambda^{NF}\left(\mathbf{1}_{l\in NF} \times Post_t \times FintechEntry_z\right) + \varepsilon_{zlt}\end{aligned} \tag{2.3}$$

where $FintechEntry_z$ is the growth rate of fintech filings that ZIP code z experienced between 2016 and 2019. All other variables are the same as in the previous equations, and we continue to include ZIP-by-month FEs and lender FEs. The coefficients of interest are now λ^F and λ^{NF}, which measure the extent to which the differential change in credit supply for fintechs and nonfintech nonbanks, relative to that of banks, varies across ZIP codes with different levels of fintech entry. The results for this triple difference specification are reported in Table 2.3 below.

Table 2.3: Differential Change in Lenders' Credit Supply by Fintech Entry

	(1)
	Log Filings
Nonfintech × Post × Fintech Entry	0.000496
	(0.0125)
Fintech × Post × Fintech Entry	-0.0253*
	(0.0148)
Zip × Month FE	✓
Lender FE	✓
Cluster	Lender
Lower order terms	✓
Obs	32639
R^2	0.343
Adj R^2	0.153

Notes: This table shows that lenders' changes in credit supply depend on the rate of fintech entry that ZIP codes experienced leading up to 2020. Specifically, the table presents the results for the following regression model: $\log(Filings_{zlt}) = \alpha_{zt} + \gamma_l + \beta^F(\mathbf{1}_{l\in F} \times Post_t) + \beta^{NF}(\mathbf{1}_{l\in F} \times Post_t) + \lambda^F(\mathbf{1}_{l\in F} \times Post_t \times FintechEntry_z) + \lambda^{NF}(\mathbf{1}_{l\in NF} \times Post_t \times FintechEntry_z) + \varepsilon_{zlt}$, where $\log(Filings_{zlt})$ is the log number of UCC filings in ZIP code z by lender l in month t, α_{zt} is the ZIP-by-month FE, and γ_l is the lender FE. Moreover, $\mathbf{1}_{l\in F}$ is an indicator variable that equals one if lender l is a fintech lender, $\mathbf{1}_{l\in NF}$ is an indicator variable that equals one if lender l is a nonfintech nonbank lender, and $Post_t$ is an indicator variable that equals one if month t is between March and August 2020. Finally, $FintechEntry_z$ is the growth rate of fintech filings that ZIP code z experienced between 2016 and 2019. Note that banks are the reference lender type. Standard errors are clustered at the lender level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

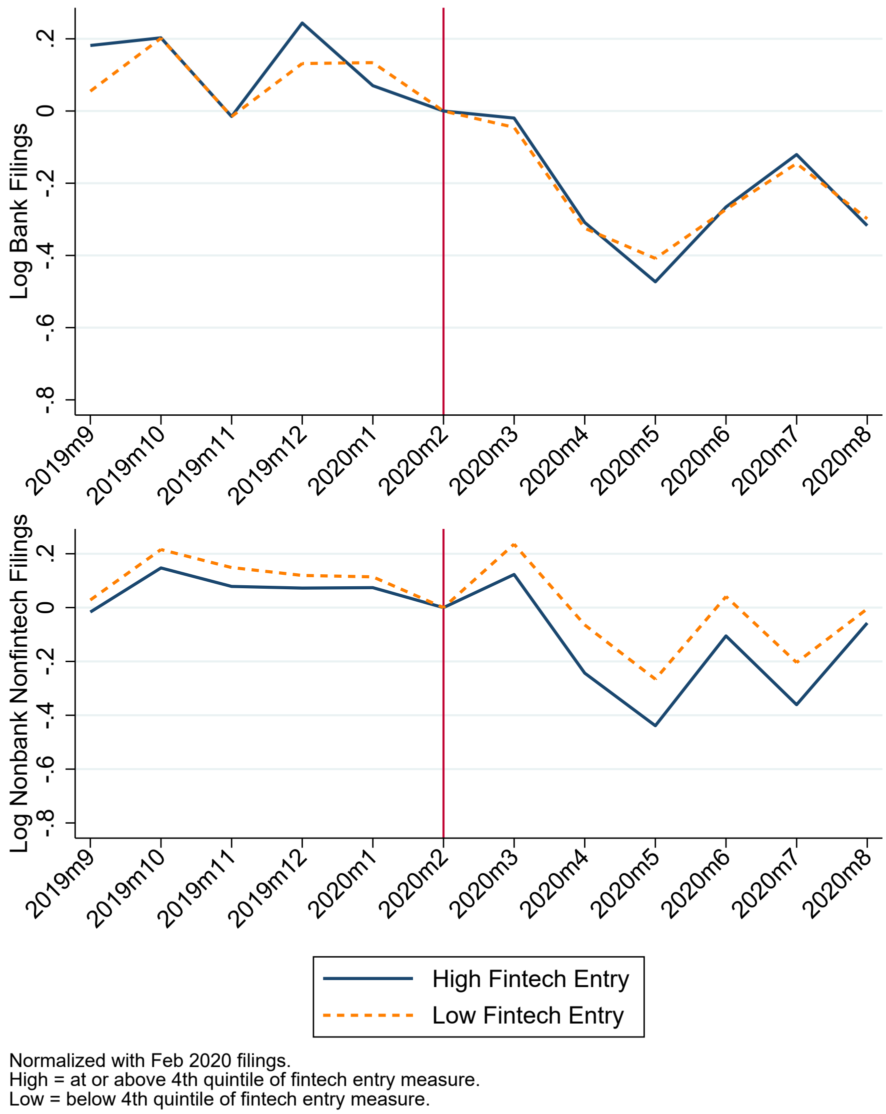
Table 2.3 finds that ZIP codes that had higher growth in fintech filings did indeed experience a more severe reduction in fintech credit supply, relative to the change in bank credit supply, between March and August 2020. Specifically, if we were to interpret the λ^F coefficient estimate, we find that ZIP codes in the 90th percentile of fintech entry experienced an additional 3% drop of credit supply from fintechs, relative to the change for banks, than ZIP codes in the 10th percentile of fintech entry. we find no such variation for the relative change in nonfintech nonbank lending. Therefore, these results show that the fintech credit crunch was indeed more severe in ZIP codes that had come to rely more on fintech lenders leading up to 2020.

2.6 Differential Changes in Local Credit Supply

The previous section showed that, fintech lenders differentially decreased their credit supply, relative to both banks and nonfintech nonbanks, during the COVID-19 pandemic. we also found that this reduction was stronger in ZIP codes that came to rely more on fintech lenders leading up to 2020. However, these within-ZIP results could potentially reflect substitution away from fintechs toward other types of lenders, rather than an aggregate decrease in credit supply within the ZIP code. For instance, the results in the previous section could be consistent with there being no change in the total credit supplied or, even, an increase, if the relative reduction by fintechs is concealing the fact that banks actually increased their lending overall. (This seems unlikely, given the trends seen in Figure 2.6.) Moreover, note that if businesses that had previously borrowed from fintech lenders were able to rather easily switch to a new lender, one would expect that the decrease in fintech credit supply should have no effect on aggregate credit supply, as the other lenders would help pick up the slack.

To first get a sense of whether there are any such patterns of substitution happening across lender types within ZIP codes, we plot in Figure 2.8 below the log of total bank and nonfintech nonbank filings in ZIP codes with high and low fintech entry, with the filings normalized to the numbers in February 2020. If these other lenders are indeed offsetting the reduction in fintech credit supply, one would expect that either or both lender types would increase their filings by larger magnitudes in ZIP codes with high fintech entry than in those with low fintech entry, given that Table 2.3 shows that the fintech credit crunch was more severe in the former areas.

However, visually, this does not seem to be the case. For banks, there seems to be almost no perceptible change in the difference in the number of filings between ZIP codes with high and low fintech entry. For nonfintech nonbanks, if anything, they seem to be moving in the opposite direction; that is, they seem to be ever so slightly decreasing the number of filings they originate in ZIP codes with high fintech entry relative to those with low fintech entry. Thus, in sum, Figure 2.8 does not seem to suggest that ZIP codes that have come to rely more heavily on fintechs receive more bank or nonfintech nonbank lending to offset the reduction in fintech credit supply they were shown to experience.



Notes: This figure plots, in panel A, the log of monthly bank filings and, in panel B, the log of monthly nonfintech nonbank filings, normalized by their respective number of filings in February 2020, in ZIP codes with high and low fintech entry. Fintech entry is defined as the growth rate of fintech filings that a ZIP code experienced between 2016 and 2019. ZIP codes are categorized as having experienced high fintech entry if their fintech entry measure is at or above the fourth quintile in the distribution; all other ZIP codes are categorized as low fintech entry.

Figure 2.8: Bank and Nonfintech Filings in ZIP codes w/ High vs. Low Fintech Entry

That being said, of course, the analysis in Figure 2.8 is merely descriptive. It is possible that, on aggregate, ZIP codes with high fintech entry do not seem to receive differentially more funding from other lenders because, perhaps, there is lower demand there. Thus, to conduct a more careful analysis, we run the following difference-in-difference specification

$$\log(Filings_{zct}) = \alpha_{ct} + \gamma_z + \beta\,(FintechEntry_z \times Post_t) + \mathbf{X}_{zt} + \varepsilon_{zct} \tag{2.4}$$

where, crucially, we include (1) α_{ct}, which is a county-by-month FE, (2) γ_z, which is a ZIP code FE, and (3) X_{zt}, which is a vector of ZIP code level demographic variables (log number of business establishments, share of households living under the poverty line, log of median income, and share of the population identifying as a racial minority) as of 2019 interacted with $Post_t$. In some specifications, X_{zt} also includes a vector of ZIP code level industry filing shares between 2016 and 2019, where industries are defined by two-digit NAICS codes, interacted with $Post_t$. $\log(Filings_{zct})$ is the log number of UCC filings in ZIP code z located in county c in month t. All other variables remain the same. Here, the coefficient of interest is β, which measures the differential change in the total number of filings that ZIP codes with higher rates of fintech entry experience during the COVID-19 pandemic, relative to ZIP codes with lower rates of fintech entry.

The inclusion of the county-by-month FEs mean that we are now comparing lending across ZIP codes in the same county within the same month. Doing so helps control for differences in loan demand because COVID-19 restrictions and mandates were often made at the county level, which should mean that ZIP codes in the same county have similar demand shocks during the pandemic. Moreover, the ZIP code FEs control for any time-invariant characteristics of the ZIP code, such as the long-term average demand for loans. Finally, the demographic and industry share controls net out any differential demand shocks across ZIP codes that may vary based on these demographics or industry composition. Thus, for instance, if one was concerned that ZIP codes with lower incomes and a higher share of racial minority residents experienced some systematic demand shock during the COVID-19 pandemic, this vector of controls would ensure that we are comparing lending in ZIP codes within the same county with similar median incomes and shares of minority residents but that vary in fintech entry leading up to 2020. Similarly, the industry share controls ensure that we only compare lending in ZIP codes within the same county that have similar industry composition, helping to control for any demand shocks driven by industry trends, such as COVID-related closures that differentially affected industries. The results are reported in Table 2.4 below.

Table 2.4: Differential Change in Total Credit Supply by Fintech Entry

	(1)	(2)
	Log Total Filings	
Fintech Entry × Post	-0.0878***	-0.0797***
	(0.0241)	(0.0240)
ZIP FE	✓	✓
County × Month FE	✓	✓
Industry Controls		✓
Demographic Controls	✓	✓
Cluster	ZIP	ZIP
Obs	8256	8256
R^2	0.652	0.654
Adj R^2	0.582	0.582

Notes: This table shows the extent to which ZIP codes with high fintech entry experienced differential changes to their aggregate credit supply after the start of the pandemic, relative to ZIP codes with low fintech entry. Specifically, the table presents the results for the following regression model: $\log(Filings_{zct}) = \alpha_{ct} + \gamma_z + \beta\,(FintechEntry_z \times Post_t) + \mathbf{X}_{zt} + \varepsilon_{zct}$, where $\log(Filings_{zct})$ is the log number of UCC filings in ZIP code z and county c in month t, α_{ct} is the county-by-month FE, and γ_z is the ZIP code FE. Moreover, $FintechEntry_z$ is the growth rate of fintech filings that ZIP code z experienced between 2016 and 2019, and $Post_t$ is an indicator variable that equals one if month t is between March and August 2020. Finally, X_{zt} is a vector of ZIP code level demographic variables (log number of business establishments, share of households living under the poverty line, log of median income, and share of the population identifying as a racial minority) as of 2019 interacted with $Post_t$. In column (2), X_{zt} also includes a vector of ZIP code level industry filing shares between 2016 and 2019, where industries are defined by two-digit NAICS codes, interacted with $Post_t$. Standard errors are clustered at the ZIP code level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The results in Table 2.4 show that ZIP codes with higher rates of fintech entry leading up to the pandemic experienced sharper declines in aggregate lending during the pandemic.[11] Specifically, if we were to interpret the β coefficient estimate in column (1), we find that ZIP codes in the 90th percentile of fintech entry experienced an 11% reduction in total credit supply relative to those in the 10th percentile. Moreover, as seen in column (2), the results remain very little changed even with the industry share controls. Therefore, the evidence suggests that ZIP codes that came to rely more on fintechs leading up to the pandemic not only experienced more severe restrictions in credit supply from fintech lenders but in aggregate as well. This implies that the reduction in fintech credit supply could not be offset completely by other lenders.

[11] See Appendix Figures B.3 and B.4 for the results of the dynamic specifications.

Although our vector of ZIP level demographic and industry share controls interacted with $Post_t$ help control for differential demand shocks across ZIP codes, we now take it one step further and modify our analysis to look not only within county but also within industry. That is, we run the following difference-in-difference specification

$$\log(Filings_{zcit}) = \alpha_{cit} + \gamma_z + \beta \left(FintechEntry_z \times Post_t\right) + \mathbf{X}_{zt} + \varepsilon_{zcit} \tag{2.5}$$

where α_{cit} is a county-by-industry-by-month FE and all other variables are the same as in equation 2.4. The inclusion of α_{cit} means that we now compare how lending to businesses in the same industry and county varies by ZIP codes' exposure to fintech entry. Thus, we control for any systematic shocks that affect the demand for loans by businesses in a given county and industry, such as COVID-related mandated closures. β remains the coefficient of interest, but it now measures the differential change in the total number of filings that businesses in industry i experience during downturns in ZIP codes with higher rates of fintech entry versus those in ZIP codes with lower rates. Results are reported in Table 2.5.

Table 2.5: Differential Change in Within-Industry Credit Supply by Fintech Entry

	(1)	(2)	(3)
		Log Filings	
Fintech Entry × Post	-0.0187***	-0.0132***	-0.0101**
	(0.00649)	(0.00489)	(0.00397)
ZIP FE	✓	✓	✓
Industry × Month FE	✓		
County × Month FE		✓	
County × Industry × Month FE			✓
Demographic Controls	✓	✓	✓
Cluster	ZIP	ZIP	ZIP
Obs	143100	142812	134556
R^2	0.093	0.120	0.283
Adj R^2	0.086	0.104	0.211

Notes: This table shows how within-industry credit supply changed differentially in ZIP codes with high fintech entry vs. low fintech entry, after the start of the COVID-19 pandemic. Specifically, the table presents the results for the following regression model: $\log(Filings_{zcit}) = \alpha_{cit} + \gamma_z + \beta\,(FintechEntry_z \times Post_t) + \mathbf{X}_{zt} + \varepsilon_{zcit}$, where $\log(Filings_{zcit})$ is the log number of UCC filings in ZIP code z and county c in industry i during month t, α_{cit} is the county-by-industry-by-month FE, and γ_z is the ZIP code FE. Moreover, $FintechEntry_z$ is the growth rate of fintech filings that ZIP code z experienced between 2016 and 2019, and $Post_t$ is an indicator variable that equals one if month t is between March and August 2020. Finally, X_{zt} is a vector of ZIP code level demographic variables (log number of business establishments, share of households living under the poverty line, log of median income, and share of the population identifying as a racial minority) as of 2019 interacted with $Post_t$. Columns (1) and (2) substitute α_{cit} for α_{it} and α_{ct}, respectively. Standard errors are clustered at the ZIP code level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The results in Table 2.5 show that even when looking within industry, businesses in ZIP codes with higher rates of fintech entry leading up to the pandemic experienced sharper declines in credit supply.[12] Specifically, if we look within county and industry, as in column (3), we find that, on average, businesses in a given industry in ZIP codes in the 90th percentile of fintech entry experienced a 1% reduction in total credit supply relative to those in the 10th percentile. Note that although the estimated change in total credit supply is still negative, the magnitude is much smaller relative to those estimated in Table 1.4. This implies that different industries experience heterogeneous changes to their credit supply. Nevertheless, on average, businesses of all industries in ZIP codes with higher fintech entry experience larger declines in total credit supply, implying that even when looking within industry, reduced fintech lending cannot be completely offset by other lenders.

[12]See Appendix Figure B.5 for the results of the dynamic within county-by-industry specification.

2.7 Conclusion

In summary, we find, using the COVID-19 pandemic as a case study, that during downturns, banks and nonbanks, but fintechs in particular, adjust their credit supply differentially. Specifically, even after controlling for local demand trends, fintechs reduce their supply of small business credit significantly, relative to other lenders. This result is consistent with fintechs being relatively more funding constrained during economic crises. Moreover, this reduction is more severe in ZIP codes that have come to rely more on fintech lenders. Finally, this reduction is not offset by corresponding increases in credit supply by other lenders, and thus, total credit supply falls in ZIP codes with high fintech entry, relative to other ZIP codes.

Although these credit supply reductions observed during the COVID-19 pandemic are already both statistically and economically significant, we expect that these effects will only become stronger from here, especially during more "normal" recessions. For one, fintechs still make up a minority share of the secured small business credit market (23% of filings in 2019), and we expect them to continue to grow over time. In fact, fintech funding issuance in 2021 is already back up to near pre-pandemic levels. Moreover, the COVID-19 recession only officially lasted for two months, so we imagine that during longer downturns, this reduction in credit supply, driven by fintechs, would be amplified.

Thus, it will continue to be important for academics and policymakers to understand the effect that fintechs may have on the cyclicality of small business credit supply, especially as there could be important welfare implications. For instance, it would be natural to assume that because businesses in ZIP codes with high fintech entry experience more severe credit crunches during downturns, real economic variables would be more harshly affected as well. It would be a useful exercise to confirm this to be true. Moreover, new papers (e.g. Erel and Liebersohn, 2022) suggest that fintech lenders in the small business credit market lend more to ZIP codes with lower incomes and a larger minority share, which bring up issues of inequality and how different segments of the population may experience macroeonomic trends differently, based on the types of lenders that concentrate in their neighborhood. Finally, given the breadth of literature that documents the importance of relationships for bank lending, the results we show here bring up some potential dynamic considerations that we plan to explore closely for the next iteration of this project. Small businesses may rely on nonbanks during good times and, in doing so, forgo the formation of bank relationships, but this may lead these businesses to suffer more in bad times if nonbank lenders are financially constrained relative to banks. We hope to explore these welfare implications in future work.

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Appendix A

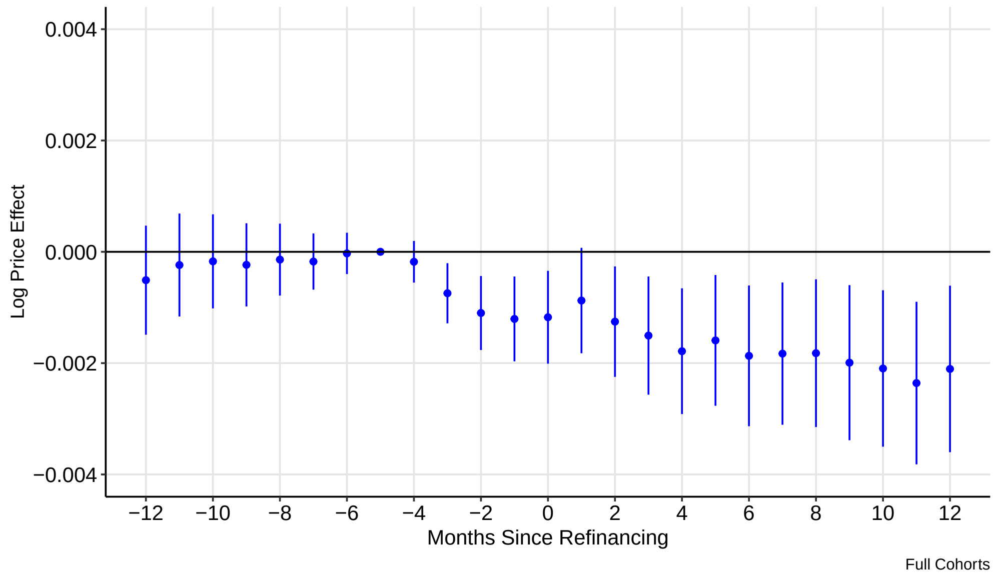
Revisiting the Refinancing Channel of Monetary Policy: Spillovers to Tenants

A.1 Details on Sample Construction

Our empirical exercise seeks to estimate the effect of refinancing on building rents in the year following the refinancing event. However, rent prices for a given apartment unit may not be updated for several years, depending on the lease term. As a result, the sparsity of the data at the unit-level would make estimation difficult. In response, we first collapse our data by averaging monthly rents across apartments of a certain type (e.g. 1 bed, 1 bath). The reason for this is that, while prices of a particular apartment unit may be few and far in between, prices for a given apartment type (e.g. 1 bed, 1 bath) would be more frequently updated. We then impute prices in the middle of two listing dates by linearly interpolating between any dates in which we have average rents of a building-apartment-type. By collapsing our data, we avoid having to interpolate over longer time horizons. Moreover, by first averaging over apartments of a certain type, we are also canceling out any idiosyncrasies in rents of a given apartment unit that would be exacerbated by the interpolation procedure. Moreover, given our difference-in-difference approach, it would have to be the case that treated buildings are systematically subject to more or less of the interpolation than control buildings for this interpolation procedure to bias our results. However, our interpolation procedure affects all buildings similarly, and we do not see economically large differences in the degree to which treated buildings are interpolated relative to control buildings.

A related concern with collapsing our data is that there might be many more apartments of a given type than other types (e.g. more 1 bed-1 bath apartments than 4 bed-5 bath apartments), which could bias our estimation procedure. In unreported results, we run weighted regressions where we weight each apartment-type by the number of apartment units comprising that type. Our results are little changed.

A.2 Effect of Refinancing: Appendix Results

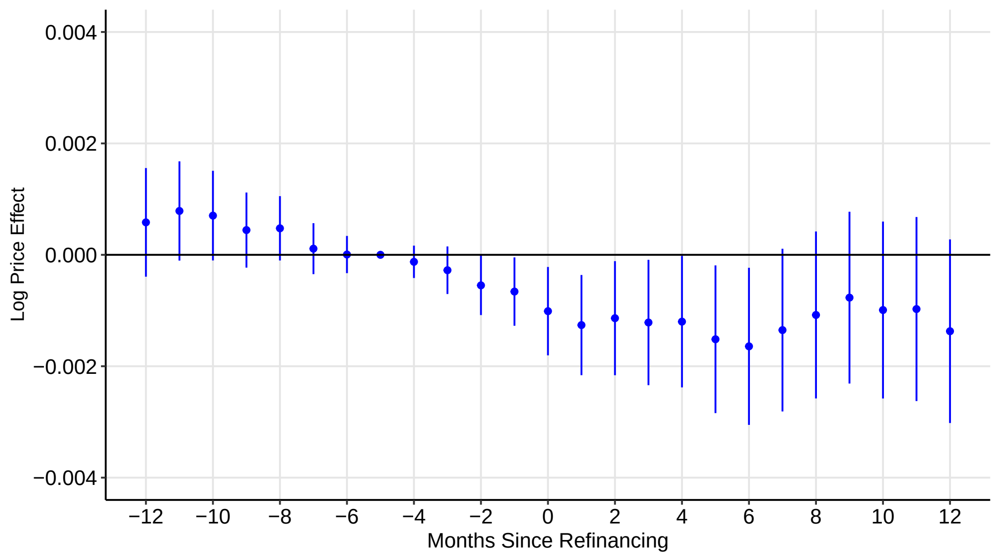


Notes: This figure shows how the monthly rents of buildings that refinanced evolved relative to the rents of buildings that did not refinance. Specifically, the figure plots the estimated β_T's from the following regression model: Log $\text{Price}_{cbat} = \sum_T \beta_T (\mathbf{1}_{t=T} \cdot \text{DoesRefi}_{cb}) + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c. $\mathbf{1}_{t=T}$ is an indicator variable that equals one if month t is equal to T, and DoesRefi_{cb} equals 1 if building b refinances (i.e. is treated) in cohort c. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. The model is estimated on the sample of all apartments, rather than just those that are not rent-stabilized.

Figure A.1: Effect of Refinancing on Monthly Rents (OLS) - All Apartments

Notes: This figure shows an example cohort (i.e. the Yorkville/October 2013 Cohort) in our sample. This cohort features an alternative construction than the one used in the main text: all control buildings are within a 0.25 mile radius of treated buildings. The green dot represents a building that refinanced in October 2013 (i.e. treated). Purple dots represent buildings in the area that had no refinances between October 2012 and October 2014. Purple dots within the shaded circle are those that lie within 0.25 miles of the green dot (i.e. control).

Figure A.2: Example Nearest Neighbor Cohort - Yorkville Building/October 2013



Notes: This figure shows how the monthly rents of buildings that refinanced evolved relative to the rents of buildings that did not refinance. Specifically, the figure plots the estimated β_T's from the following regression model: $\text{Log Price}_{cbat} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{DoesRefi}_{cb}\right) + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c. $\mathbf{1}_{t=T}$ is an indicator variable that equals one if month t is equal to T, and DoesRefi_{cb} equals 1 if building b refinances (i.e. is treated) in cohort c. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. The model is estimated on the sample of all non-rent-stabilized apartments, where the cohorts are formed by taking all control buildings within a 0.25 mile radius of treated buildings; these cohorts are deemed "Neighbor Cohorts." The results are consistent with those in the main text.

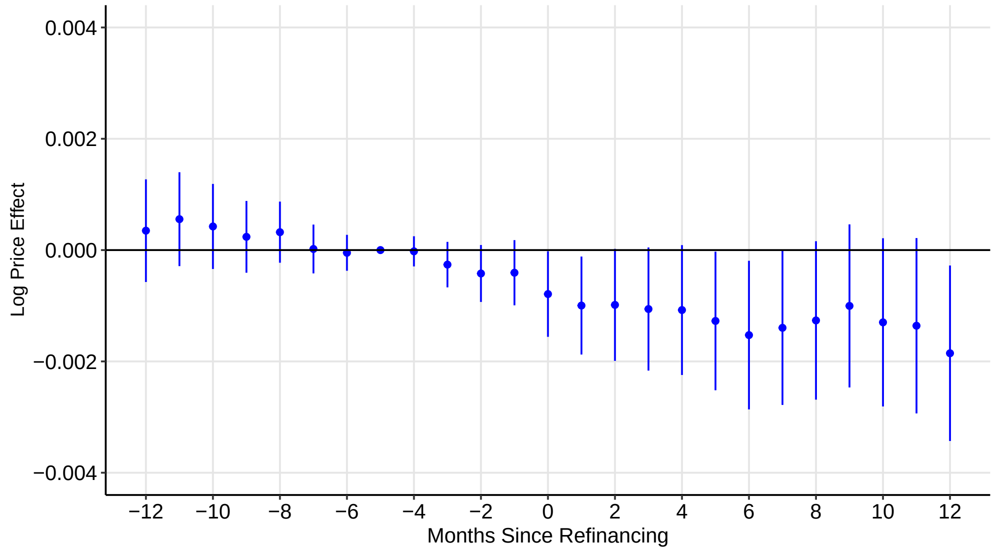
Figure A.3: Effect of Refinancing on Monthly Rents (OLS) – Neighbor Cohorts

Table A.1: Effect of Refinancing on Monthly Rents (OLS) – Neighbor Cohorts

	Log Price	
	(1)	(2)
hasRefied	-0.001**	-0.001**
	(0.0006)	(0.0006)
Cohort × Building FE	✓	
Cohort × Month × Apt Type FE	✓	✓
Cohort × Building × Apt Type FE		✓
Cluster	Building	Building
R^2	0.9544	0.9912
Adjusted R^2	0.9508	0.9903
Observations	24,329,253	24,329,253

Notes: This table presents results from estimating the following regression model: $\text{Log Price}_{cbat} = \beta \text{hasRefied}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where hasRefied_{cbt} is equal to 1 if building b in cohort c has refinanced by month t, and Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. Standard errors are clustered at the building level (i.e. at the level of the treatment). The model is estimated on the sample of all non-rent-stabilized apartments, where the cohorts are formed by taking all control buildings within a 0.25 mile radius of treated buildings; these cohorts are deemed "Neighbor Cohorts." The results are consistent with those in the main text.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$



Notes: This figure shows how the monthly rents of buildings that refinanced evolved relative to the rents of buildings that did not refinance. Specifically, the figure plots the estimated β_T's from the following regression model: Log Price$_{cbat}$ $= \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{DoesRefi}_{cb}\right) + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Price$_{cbat}$ is the log of the average rent price for all apartments of type a in building b in month t for cohort c. $\mathbf{1}_{t=T}$ is an indicator variable that equals one if month t is equal to T, and DoesRefi$_{cb}$ equals 1 if building b refinances (i.e. is treated) in cohort c. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. The model is estimated on the sample of all non-rent-stabilized apartments. For each treated building in a cohort, we keep as control the 10 nearest neighbors in the cohort in terms of rent growth during the pre-period. The results are consistent with those in the main text.

Figure A.4: Effect of Refinancing on Monthly Rents (OLS) – Refined Controls

Table A.2: Effect of Refinancing on Monthly Rents (OLS) – Refined Controls

	Log Price	
	(1)	(2)
hasRefied	-0.001**	-0.001**
	(0.0006)	(0.0006)
Cohort × Building FE	✓	
Cohort × Month × Apt Type FE	✓	✓
Cohort × Building × Apt Type FE		✓
Cluster	Building	Building
R^2	0.9795	0.9926
Adjusted R^2	0.9774	0.9918
Observations	3,653,370	3,653,370

Notes: This table presents results from estimating the following regression model: Log Price$_{cbat}$ $= \beta$hasRefied$_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where hasRefied$_{cbt}$ is equal to 1 if building b in cohort c has refinanced by month t, and Log Price$_{cbat}$ is the log of the average rent price for all apartments of type a in building b in month t for cohort c. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. Standard errors are clustered at the building level (i.e. at the level of the treatment). The model is estimated on the sample of all non-rent-stabilized apartments. For each treated building in a cohort, we keep as control the 10 nearest neighbors in the cohort in terms of rent growth during the pre-period. The results are consistent with those in the main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.3 Post-Refinancing Predicted Interest Rates

While the Trepp data report the interest rate on current outstanding mortgages, we do not see the interest rate on the new commercial mortgage after refinancing. The reason is that, for us to see the new interest rate after refinancing, property owners would need to be given a commercial mortgage that appears in the Trepp data, meaning a commercial mortgage that ends up being securitized; this seldom is the case. Therefore, our baseline results use the median commercial mortgage rate at the time of refinancing, estimated from Trepp data, to approximate the new interest rate that property owners experience after refinancing. In this section, to ensure the robustness of our results, we instead use machine learning methods to predict, as a function of observables, the new interest rate that property owners experience after refinancing; our results remain little changed when we use these predicted interest rates as a proxy for the rate on the new loan after refinancing.

We use the building's loan size, DSCR,[1] number of apartment units, year of construction, loan origination year, loan originator identity, and zipcode demographics[2] to predict the interest rate on the building's loan. Specifically, instead of predicting the absolute level of the interest rate, we instead predict the spread of the interest rate from the median commercial mortgage rate in the market. To predict the interest rate, we then take the median commercial mortgage rate in the market (which is what we use for our baseline results) and apply the predicted spread. We therefore use zipcode and building observables to predict the spread of the interest rate from the median commercial mortgage rate in the market. We directly predict the spread, as opposed to the level of the interest rate, because we do not pretend to have a model that could predict the prevailing macroeconomic conditions or monetary policy stance, which would be necessary to directly predict rates. Instead, we use the median commercial mortgage rate in the market to proxy for these macroeconomic factors, and to this median, we apply the predicted spread from our model.

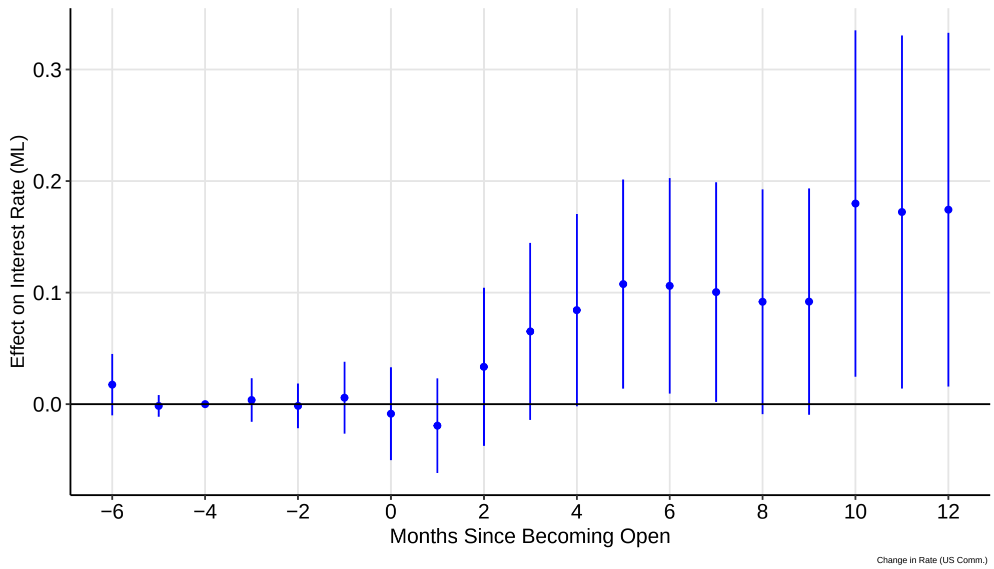
Our estimation procedure is as follows. We take the universe of Trepp loans originated in the US and fit a series of random forest and gradient-boosted regression trees to the data. To mitigate concerns of overfitting, we split the data into a training and a test sample and use cross validation procedures to select the number of regression trees that will form part of the random forest model. In a similar vein, we employ early stopping procedures while estimating our gradient-boosted regression model. Our best model has a test-sample median absolute error of about $\frac{1}{3}$ of a standard deviation of the interest rate spread.

When predicting the interest rate on a new ATTOM loan, we use information from ATTOM on the new loan's size, origination year, and originator identity. For all other building variables, we assume their values don't change from their values in Trepp[3]. We also take zipcode demographics from the Census at the time of refinancing. The results of this analysis are shown below and mirror the results shown in the main text.

[1]DSCR stands for debt service coverage ratio. DSCR is calculated by dividing a property's net cash flows or net operating income by its debt service (i.e. mortgage payments) and is a very common metric used for assessing risk in commercial mortgages.

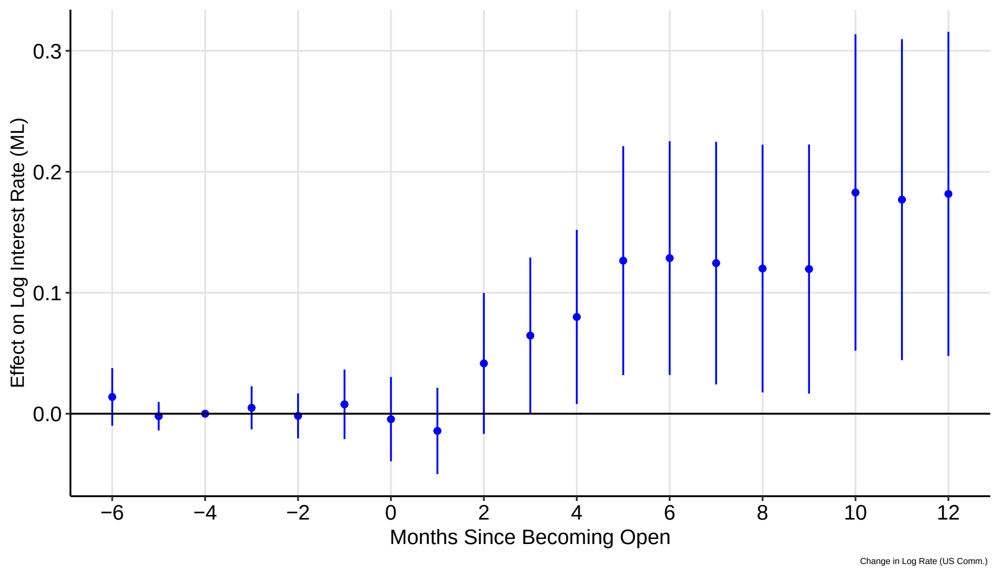
[2]These include the minority share, poverty share, median income, and population.

[3]For DSCR, we take the latest DSCR, that is, the latest DSCR prior to the refinancing event.



Notes: This figure shows how entering the open period affects the interest rate that property owners face after refinancing, where we allow the effect to depend on a counterfactual interest rate change that the property owner would have experienced had the refinancing occurred at the start of the open period. Specifically, the figure plots the estimated β_T's from the following regression model: Interest Rate $(\text{ML})_{cbt} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb} \cdot \Delta\text{Rate}_{cb}^{\text{open}}\right) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Interest Rate $(\text{ML})_{cbt}$ is equal to the interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to a predicted interest rate; Opens_{cb} is equal to 1 if building b enters its open period (i.e. is treated) in cohort c; $\Delta\text{Rate}_{cb}^{\text{open}}$ is the change in the interest rate that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period; and $\mathbf{X}_{cbt}$ includes all lower-order terms from the triple interaction in the first term. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. We use machine learning methods to predict the interest rate as a function of building, zipcode, and loan observables. The results are consistent with those in the main text.

Figure A.5: Predicting Interest Rates with Open Events (First Stage) – ML Robustness



Notes: This figure shows how entering the open period affects the interest rate that property owners face after refinancing, where we allow the effect to depend on a counterfactual interest rate change that the property owner would have experienced had the refinancing occurred at the start of the open period. Specifically, the figure plots the estimated β_T's from the following regression model: $\text{Log Interest Rate (ML)}_{cbt} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb} \cdot \Delta\text{Log Rate}_{cb}^{\text{open}}\right) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where $\text{Log Interest Rate (ML)}_{cbt}$ is equal to the Log interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to a Log predicted interest rate; Opens_{cb} is equal to 1 if building b enters its open period (i.e. is treated) in cohort c; $\Delta\text{Log Rate}_{cb}^{\text{open}}$ is the change in the Log interest rate that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period; and $\mathbf{X}_{cbt}$ includes all lower-order terms from the triple interaction in the first term. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. We use machine learning methods to predict the interest rate as a function of building, zipcode, and loan observables. The results are consistent with those in the main text.

Figure A.6: Predicting Log Interest Rates with Open Events (First Stage) – ML Robustness

Table A.3: Predicting Interest Rates with Open Events (First Stage) – ML Robustness

	Log Interest Rate (ML)		Interest Rate (ML)	
	(1)	(2)	(3)	(4)
hasOpened × ΔLog Rate (US Comm.)	0.122***	0.121***		
	(0.043)	(0.043)		
hasOpened × ΔRate (US Comm.)			0.107**	0.107**
			(0.042)	(0.042)
Lower Order Controls	✓	✓	✓	✓
Open Cohort × Building FE	✓		✓	
Open Cohort × Month × Apt Type FE	✓	✓	✓	✓
Open Cohort × Building × Apt Type FE		✓		✓
Cluster	Building	Building	Building	Building
R^2	0.9640	0.9641	0.9622	0.9622
Adjusted R^2	0.9573	0.9564	0.9551	0.9542
Observations	55,843	55,843	55,843	55,843

Notes: This table presents results from estimating first stage regressions of the following form: $\text{Interest Rate}_{cbt} = \beta\,(\text{hasOpened}_{cbt} \cdot \Delta\text{Rate}^{\text{open}}_{cb}) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where $\text{Interest Rate}_{cbt}$ is equal to the interest rate (or its Log) on the current mortgage of building b in cohort c until the building refinances, at which point it switches to a predicted interest rate (or its Log); hasOpened_{cbt} is equal to 1 if building b in cohort c has entered its open period by month t; $\Delta\text{Rate}^{\text{open}}_{cb}$ is the change in the interest rate (or Log interest rate) that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period; and $\mathbf{X}_{cbt}$ includes all lower-order terms from the triple interaction in the first term. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. We use machine learning methods to predict the interest rate as a function of building, zipcode, and loan observables. $\Delta\text{Rate}^{\text{open}}_{cb}$ is computed using the median commercial mortgage rate in the US at the start of the open period. Standard errors are clustered at the building level (i.e. at the level of the treatment). The results are consistent with those in the main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Semi-Elasticity of Monthly Rents with respect to Interest Rates (IV) – ML Robustness

	Log Price			
	(1)	(2)	(3)	(4)
Interest Rate (ML)	0.002		0.002	
	(0.002)		(0.002)	
$\widehat{InterestRate}(ML)$		0.019**		0.018**
		(0.008)		(0.007)
Open Cohort × Building FE	✓	✓		
Open Cohort × Month × Apt Type FE	✓	✓	✓	✓
Open Cohort × Building × Apt Type FE			✓	✓
Cluster	Building	Building	Building	Building
Stage	OLS	Second	OLS	Second
R^2	0.9748	0.9749	0.9939	0.9942
Adjusted R^2	0.9700	0.9702	0.9926	0.9930
Observations	59,446	55,843	59,446	55,843
F-test (1st stage), Interest Rate (ML)		1,214.3		1,212.3

Notes: This table presents 2SLS results on the effect of interest rates faced by property owners on monthly rents; in other words, this table estimates the semi-elasticity of monthly rents with respect to interest rates. The second stage regression model is: $\text{Log Price}_{cbat} = \beta \text{Interest } \widehat{\text{Rate}}\ (\text{ML})_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c; $\text{Interest Rate (ML)}_{cbt}$ is equal to the interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to a predicted interest rate. $\text{Interest } \widehat{\text{Rate}}\ (\text{ML})_{cbt}$ is the predicted value of $\text{Interest Rate (ML)}_{cbt}$ from the first stage; first stage regressions are shown in Table A.3. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. We use machine learning methods to predict the interest rate as a function of building, zipcode, and loan observables. Standard errors are clustered at the building level (i.e. at the level of the treatment). The results are consistent with those in the main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Elasticity of Monthly Rents with respect to Interest Rates (IV) – ML Robustness

	Log Price			
	(1)	(2)	(3)	(4)
Log Interest Rate (ML)	0.008		0.007	
	(0.008)		(0.008)	
$\widehat{LogInterestRate}(ML)$		0.096**		0.089**
		(0.039)		(0.037)
Open Cohort × Building FE	✓	✓		
Open Cohort × Month × Apt Type FE	✓	✓	✓	✓
Open Cohort × Building × Apt Type FE			✓	✓
Cluster	Building	Building	Building	Building
Stage	OLS	Second	OLS	Second
R^2	0.9748	0.9749	0.9939	0.9942
Adjusted R^2	0.9700	0.9702	0.9926	0.9930
Observations	59,446	55,843	59,446	55,843
F-test (1st stage), Log Interest Rate (ML)		1,222.2		1,220.3

Notes: This table presents 2SLS results on the effect of interest rates faced by property owners on monthly rents; in other words, this table estimates the elasticity of monthly rents with respect to interest rates. The second stage regression model is: Log $\text{Price}_{cbat} = \beta \widehat{\text{Log Interest Rate}}\ (\text{ML})_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c; Log Interest Rate $(\text{ML})_{cbt}$ is equal to the Log interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to a Log predicted interest rate. $\widehat{\text{Log Interest Rate}}\ (\text{ML})_{cbt}$ is the predicted value of Log Interest Rate $(\text{ML})_{cbt}$ from the first stage; first stage regressions are shown in Table A.3. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. We use machine learning methods to predict the interest rate as a function of building, zipcode, and loan observables. Standard errors are clustered at the building level (i.e. at the level of the treatment). The results are consistent with those in the main text.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.4 First Stage Regressions: Interest Rate Results

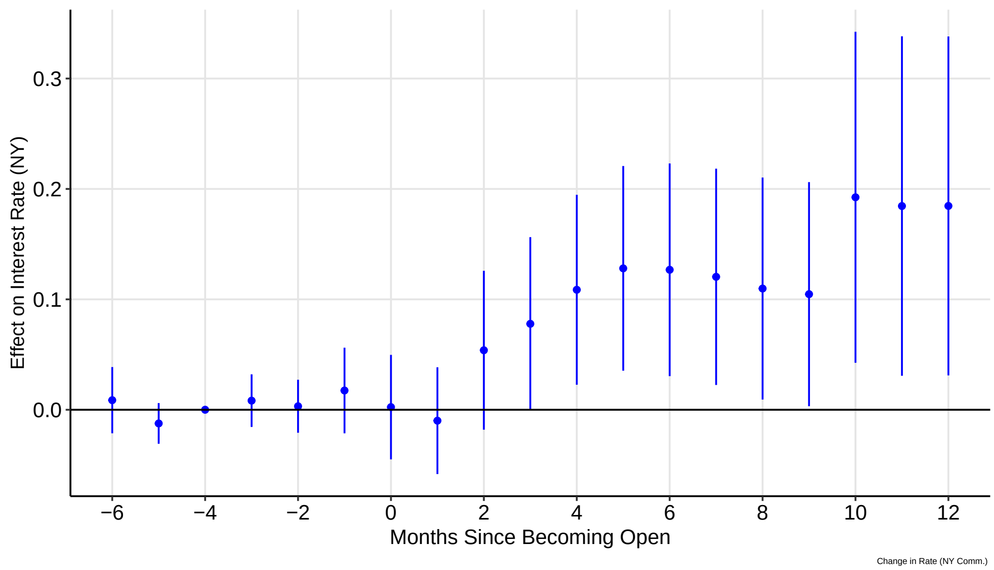
This appendix section presents further analyses on the first stage regressions that attempt to predict the interest rate with the timing of the open period, where we allow the effect to depend on a counterfactual interest rate change that the property owner would have experienced had the refinancing occurred at the start of the open period. In this section, we use counterfactual interest rates that are based on other market rates, aside from the median commercial mortgage rate in the US. We also proxy for the interest rate on the new loan using the median commercial mortgage rate in NY, rather than the US. We also look at Log specifications. The results are consistent with those in the main text.

Table A.6: Predicting Interest Rates with Open Events – Interest Rate (US)

	Interest Rate (US)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
hasOpened × ΔRate (NY Comm.)	0.112***				0.112***			
	(0.039)				(0.039)			
hasOpened × ΔRate (US Comm.)		0.110***				0.110***		
		(0.039)				(0.039)		
hasOpened × ΔRate (US 7YR)			0.103***				0.103***	
			(0.031)				(0.031)	
hasOpened × ΔRate (US 10YR)				0.104***				0.104***
				(0.034)				(0.034)
Lower Order Controls	✓	✓	✓	✓	✓	✓	✓	✓
Open Cohort × Building FE	✓	✓	✓	✓				
Open Cohort × Month × Apt Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Open Cohort × Building × Apt Type FE					✓	✓	✓	✓
Cluster	Building	Building	Building	Building	Building	Building	Building	Building
R^2	0.9680	0.9683	0.9696	0.9695	0.9680	0.9683	0.9696	0.9696
Adjusted R^2	0.9620	0.9623	0.9637	0.9637	0.9613	0.9616	0.9630	0.9630
Observations	55,501	55,843	58,323	58,323	55,501	55,843	58,323	58,323

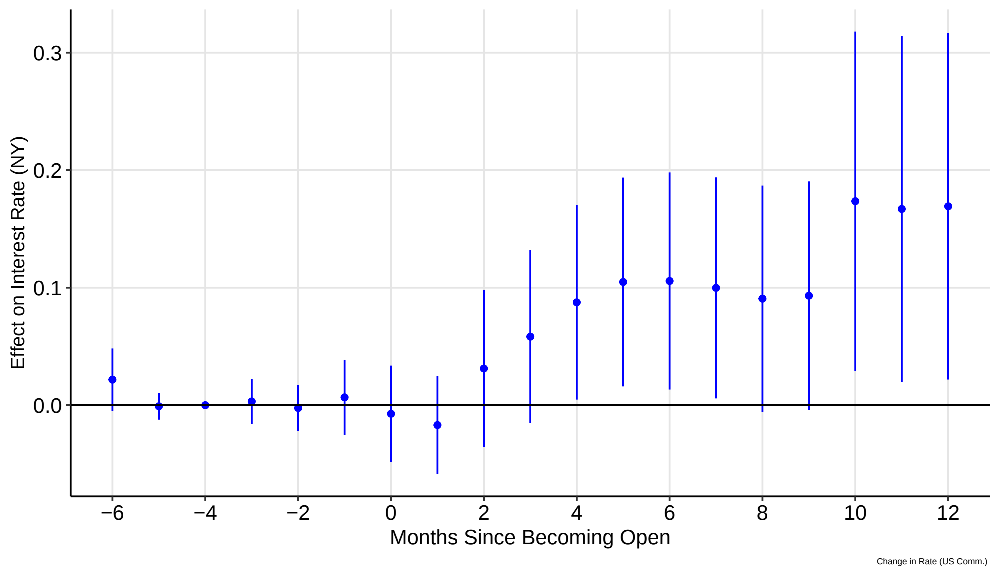
Notes: This table presents results from estimating first stage regressions of the following form: Interest Rate $(\text{US})_{cbt} = \beta\left(\text{hasOpened}_{cbt} \cdot \Delta\text{Rate}_{cb}^{\text{open}}\right) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Interest Rate $(\text{US})_{cbt}$ is equal to the interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the median commercial mortgage rate in the US; hasOpened_{cbt} is equal to 1 if building b in cohort c has entered its open period by month t; $\Delta\text{Rate}_{cb}^{\text{open}}$ is the change in the interest rate that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period; and $\mathbf{X}_{cbt}$ includes all lower-order terms from the triple interaction in the first term. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. $\Delta\text{Rate}_{cb}^{\text{open}}$ is computed using different market rates at the start of the open period: median commercial mortgage rate in the US, median commercial mortgage rate in NY only, 7-year US Treasury Rate, and the 10-year US Treasury Rate. Standard errors are clustered at the building level (i.e. at the level of the treatment). The results are consistent with those in the main text.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$



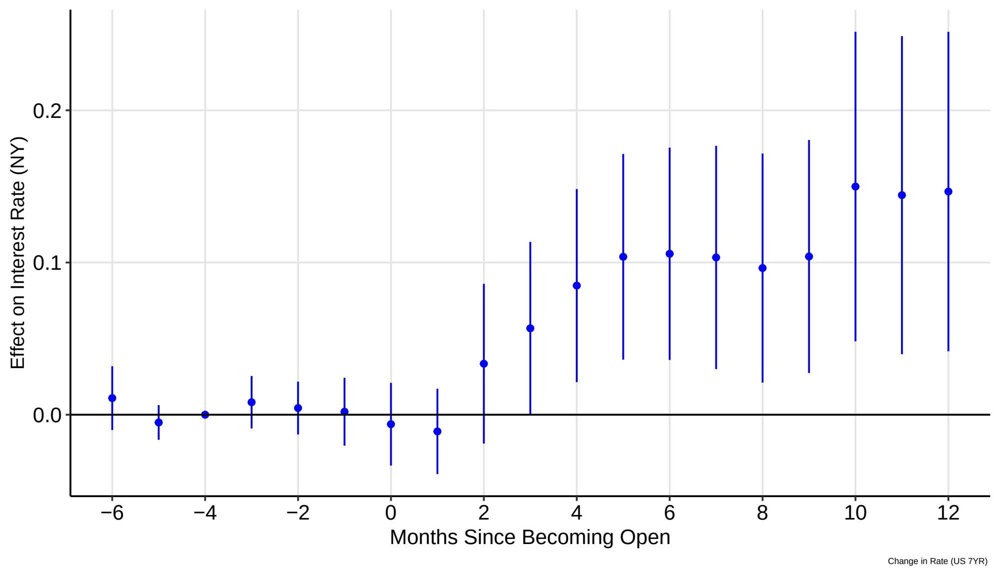
Notes: This figure shows how entering the open period affects the interest rate that property owners face after refinancing, where we allow the effect to depend on a counterfactual interest rate change that the property owner would have experienced had the refinancing occurred at the start of the open period. Specifically, the figure plots the estimated β_T's from the following regression model: $\text{Interest Rate (NY)}_{cbt} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb} \cdot \Delta\text{Rate}_{cb}^{\text{open}}\right) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where $\text{Interest Rate (NY)}_{cbt}$ is equal to the interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the median commercial mortgage rate in NY; Opens_{cb} is equal to 1 if building b enters its open period (i.e. is treated) in cohort c; $\Delta\text{Rate}_{cb}^{\text{open}}$ is the change in the interest rate that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period; and $\mathbf{X}_{cbt}$ includes all lower-order terms from the triple interaction in the first term. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. $\Delta\text{Rate}_{cb}^{\text{open}}$ is computed using the median commercial mortgage rate in NY at the start of the open period. The results are consistent with those in the main text.

Figure A.7: Predicting Interest Rates with Open Events – NY Comm. Rate Counterfactual



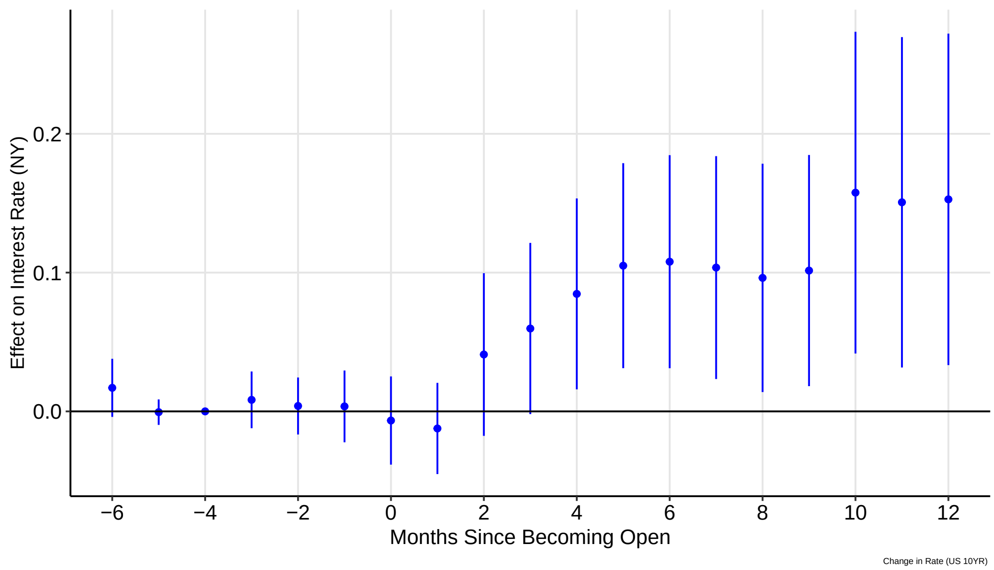
Notes: This figure shows how entering the open period affects the interest rate that property owners face after refinancing, where we allow the effect to depend on a counterfactual interest rate change that the property owner would have experienced had the refinancing occurred at the start of the open period. Specifically, the figure plots the estimated β_T's from the following regression model: $\text{Interest Rate (NY)}_{cbt} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb} \cdot \Delta\text{Rate}_{cb}^{\text{open}}\right) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where $\text{Interest Rate (NY)}_{cbt}$ is equal to the interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the median commercial mortgage rate in NY; Opens_{cb} is equal to 1 if building b enters its open period (i.e. is treated) in cohort c; $\Delta\text{Rate}_{cb}^{\text{open}}$ is the change in the interest rate that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period; and $\mathbf{X}_{cbt}$ includes all lower-order terms from the triple interaction in the first term. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. $\Delta\text{Rate}_{cb}^{\text{open}}$ is computed using the median commercial mortgage rate in US at the start of the open period. The results are consistent with those in the main text.

Figure A.8: Predicting Interest Rates with Open Events – US Comm. Rate Counterfactual



Notes: This figure shows how entering the open period affects the interest rate that property owners face after refinancing, where we allow the effect to depend on a counterfactual interest rate change that the property owner would have experienced had the refinancing occurred at the start of the open period. Specifically, the figure plots the estimated β_T's from the following regression model: Interest Rate $(\text{NY})_{cbt} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb} \cdot \Delta\text{Rate}_{cb}^{\text{open}}\right) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Interest Rate $(\text{NY})_{cbt}$ is equal to the interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the median commercial mortgage rate in NY; Opens_{cb} is equal to 1 if building b enters its open period (i.e. is treated) in cohort c; $\Delta\text{Rate}_{cb}^{\text{open}}$ is the change in the interest rate that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period; and $\mathbf{X}_{cbt}$ includes all lower-order terms from the triple interaction in the first term. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. $\Delta\text{Rate}_{cb}^{\text{open}}$ is computed using the 7-year US Treasury Rate at the start of the open period. The results are consistent with those in the main text.

Figure A.9: Predicting Interest Rates with Open Events – 7YR Treasury Rate Counterfactual



Notes: This figure shows how entering the open period affects the interest rate that property owners face after refinancing, where we allow the effect to depend on a counterfactual interest rate change that the property owner would have experienced had the refinancing occurred at the start of the open period. Specifically, the figure plots the estimated β_T's from the following regression model: Interest Rate $(\text{NY})_{cbt} = \sum_T \beta_T \left(\mathbf{1}_{t=T} \cdot \text{Opens}_{cb} \cdot \Delta\text{Rate}_{cb}^{\text{open}}\right) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Interest Rate $(\text{NY})_{cbt}$ is equal to the interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the median commercial mortgage rate in NY; Opens_{cb} is equal to 1 if building b enters its open period (i.e. is treated) in cohort c; $\Delta\text{Rate}_{cb}^{\text{open}}$ is the change in the interest rate that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period; and $\mathbf{X}_{cbt}$ includes all lower-order terms from the triple interaction in the first term. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. $\Delta\text{Rate}_{cb}^{\text{open}}$ is computed using the 10-year US Treasury Rate at the start of the open period. The results are consistent with those in the main text.

Figure A.10: Predicting Interest Rates with Open Events – 10YR Treasury Rate Counterfactual

Table A.7: Predicting Interest Rates with Open Events – Interest Rate (NY)

	Interest Rate (NY)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
hasOpened × ΔRate (NY Comm.)	0.114***				0.114***			
	(0.040)				(0.040)			
hasOpened × ΔRate (US Comm.)		0.105***				0.104***		
		(0.039)				(0.039)		
hasOpened × ΔRate (US 7YR)			0.097***				0.097***	
			(0.031)				(0.031)	
hasOpened × ΔRate (US 10YR)				0.099***				0.098***
				(0.034)				(0.034)
Lower Order Controls	✓	✓	✓	✓	✓	✓	✓	✓
Open Cohort × Building FE	✓	✓	✓	✓				
Open Cohort × Month × Apt Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Open Cohort × Building × Apt Type FE					✓	✓	✓	✓
Cluster	Building	Building	Building	Building	Building	Building	Building	Building
R^2	0.9625	0.9628	0.9642	0.9642	0.9626	0.9629	0.9643	0.9642
Adjusted R^2	0.9556	0.9558	0.9573	0.9573	0.9547	0.9550	0.9565	0.9565
Observations	55,501	55,843	58,323	58,323	55,501	55,843	58,323	58,323

Notes: This table presents results from estimating first stage regressions of the following form: $\text{Interest Rate (NY)}_{cbt} = \beta\left(\text{hasOpened}_{cbt} \cdot \Delta\text{Rate}_{cb}^{\text{open}}\right) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where $\text{Interest Rate (NY)}_{cbt}$ is equal to the interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the median commercial mortgage rate in NY; hasOpened_{cbt} is equal to 1 if building b in cohort c has entered its open period by month t; $\Delta\text{Rate}_{cb}^{\text{open}}$ is the change in the interest rate that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period; and $\mathbf{X}_{cbt}$ includes all lower-order terms from the triple interaction in the first term. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. $\Delta\text{Rate}_{cb}^{\text{open}}$ is computed using different market rates at the start of the open period: median commercial mortgage rate in the US, median commercial mortgage rate in NY only, 7-year US Treasury Rate, and the 10-year US Treasury Rate. Standard errors are clustered at the building level (i.e. at the level of the treatment). The results are consistent with those in the main text.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: Predicting Interest Rates with Open Events – Log Interest Rate (US)

	Log Interest Rate (US)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
hasOpened × ΔLog Rate (NY Comm.)	0.128***				0.128***			
	(0.040)				(0.040)			
hasOpened × ΔLog Rate (US Comm.)		0.129***				0.129***		
		(0.041)				(0.041)		
hasOpened × ΔLog Rate (US 7YR)			0.020				0.020	
			(0.013)				(0.013)	
hasOpened × ΔLog Rate (US 10YR)				0.020				0.020
				(0.015)				(0.015)
Lower Order Controls	✓	✓	✓	✓	✓	✓	✓	✓
Open Cohort × Building FE	✓	✓	✓	✓				
Open Cohort × Month × Apt Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Open Cohort × Building × Apt Type FE					✓	✓	✓	✓
Cluster	Building	Building	Building	Building	Building	Building	Building	Building
R^2	0.9710	0.9712	0.9724	0.9724	0.9710	0.9712	0.9724	0.9724
Adjusted R^2	0.9656	0.9658	0.9671	0.9671	0.9649	0.9651	0.9664	0.9664
Observations	55,501	55,843	58,323	58,323	55,501	55,843	58,323	58,323

Notes: This table presents results from estimating first stage regressions of the following form: Log Interest Rate $(\text{US})_{cbt} = \beta\,(\text{hasOpened}_{cbt} \cdot \Delta\text{Log Rate}_{cb}^{\text{open}}) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Interest Rate $(\text{US})_{cbt}$ is equal to the Log interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the Log median commercial mortgage rate in the US; hasOpened_{cbt} is equal to 1 if building b in cohort c has entered its open period by month t; $\Delta\text{Log Rate}_{cb}^{\text{open}}$ is the change in the Log interest rate that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period; and $\mathbf{X}_{cbt}$ includes all lower-order terms from the triple interaction in the first term. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. $\Delta\text{Log Rate}_{cb}^{\text{open}}$ is computed using different market rates at the start of the open period: median commercial mortgage rate in the US, median commercial mortgage rate in NY only, 7-year US Treasury Rate, and the 10-year US Treasury Rate. Standard errors are clustered at the building level (i.e. at the level of the treatment). The results are consistent with those in the main text.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: Predicting Interest Rates with Open Events – Log Interest Rate (NY)

	Log Interest Rate (NY)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
hasOpened × ΔLog Rate (NY Comm.)	0.133***				0.132***			
	(0.040)				(0.040)			
hasOpened × ΔLog Rate (US Comm.)		0.123***				0.123***		
		(0.040)				(0.040)		
hasOpened × ΔLog Rate (US 7YR)			0.011				0.011	
			(0.013)				(0.013)	
hasOpened × ΔLog Rate (US 10YR)				0.012				0.012
				(0.015)				(0.015)
Lower Order Controls	✓	✓	✓	✓	✓	✓	✓	✓
Open Cohort × Building FE	✓	✓	✓	✓				
Open Cohort × Month × Apt Type FE	✓	✓	✓	✓	✓	✓	✓	✓
Open Cohort × Building × Apt Type FE					✓	✓	✓	✓
Cluster	Building	Building	Building	Building	Building	Building	Building	Building
R^2	0.9645	0.9648	0.9661	0.9661	0.9646	0.9648	0.9662	0.9662
Adjusted R^2	0.9579	0.9581	0.9596	0.9596	0.9572	0.9574	0.9589	0.9589
Observations	55,501	55,843	58,323	58,323	55,501	55,843	58,323	58,323

Notes: This table presents results from estimating first stage regressions of the following form: Log Interest Rate $(\text{NY})_{cbt} = \beta\,(\text{hasOpened}_{cbt} \cdot \Delta\text{Log Rate}^{\text{open}}_{cb}) + \mathbf{X}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Interest Rate $(\text{NY})_{cbt}$ is equal to the Log interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the Log median commercial mortgage rate in NY; hasOpened_{cbt} is equal to 1 if building b in cohort c has entered its open period by month t; $\Delta\text{Log Rate}^{\text{open}}_{cb}$ is the change in the Log interest rate that would have occurred for building b in cohort c had the property owner refinanced at the start of the open period; and $\mathbf{X}_{cbt}$ includes all lower-order terms from the triple interaction in the first term. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. $\Delta\text{Log Rate}^{\text{open}}_{cb}$ is computed using different market rates at the start of the open period: median commercial mortgage rate in the US, median commercial mortgage rate in NY only, 7-year US Treasury Rate, and the 10-year US Treasury Rate. Standard errors are clustered at the building level (i.e. at the level of the treatment). The results are consistent with those in the main text.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.5 IV Regressions: Interest Rate Results

This appendix section presents further analyses on second stage regressions that estimate the elasticity of monthly rents with respect to interest rates, where we estimate this relationship using only variation in interest rates induced by the timing of open events, a plausibly exogenous event. In the first stage, we predict the interest rate with the timing of the open period, where we allow the effect to depend on a counterfactual interest rate change that the property owner would have experienced had the refinancing occurred at the start of the open period. In this section, we look at how sensitive the second stage results are to first stage regressions that use counterfactual interest rates that are based on other market rates, aside from the median commercial mortgage rate in the US. The results are consistent with those in the main text.

Table A.10: Semi-Elasticity of Monthly Rents with respect to Interest Rates – Interest Rate (US)

	Log Price									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Interest Rate (US)	0.003					0.003				
	(0.002)					(0.002)				
$\widehat{InterestRate}(US)$		0.020**	0.022***	0.023***	0.024***		0.019**	0.021**	0.021***	0.022***
		(0.008)	(0.008)	(0.008)	(0.009)		(0.008)	(0.008)	(0.008)	(0.008)
Open Cohort × Building FE	✓	✓	✓	✓	✓					
Open Cohort × Month × Apt Type FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Open Cohort × Building × Apt Type FE						✓	✓	✓	✓	✓
Cluster	Building	Building	Building	Building	Building	Building	Building	Building	Building	Building
Stage	OLS	Second	Second	Second	Second	OLS	Second	Second	Second	Second
Rate in IV		NY Comm.	US Comm.	US 7YR	US 10YR		NY Comm.	US Comm.	US 7YR	US 10YR
R^2	0.9748	0.9749	0.9749	0.9750	0.9750	0.9939	0.9942	0.9942	0.9938	0.9938
Adjusted R^2	0.9700	0.9702	0.9702	0.9702	0.9702	0.9926	0.9930	0.9930	0.9925	0.9925
Observations	59,446	55,501	55,843	58,323	58,323	59,446	55,501	55,843	58,323	58,323
F-test (1st stage)		1,219.2	1,215.2	1,278.4	1,253.7		1,215.6	1,212.0	1,275.5	1,250.8

Notes: This table presents 2SLS results on the effect of interest rates faced by property owners on monthly rents; in other words, this table estimates the semi-elasticity of monthly rents with respect to interest rates. The second stage regression model is: $\text{Log Price}_{cbat} = \beta \text{Interest } \widehat{\text{Rate}} \text{ (US)}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c; $\text{Interest Rate (US)}_{cbt}$ is equal to the interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the median commercial mortgage rate in the US. $\text{Interest } \widehat{\text{Rate}} \text{ (US)}_{cbt}$ is the predicted value of $\text{Interest Rate (US)}_{cbt}$ from the first stage; first stage regressions are shown in Table A.6. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. Standard errors are clustered at the building level (i.e. at the level of the treatment). The results are consistent with those in the main text.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.11: Semi-Elasticity of Monthly Rents with respect to Interest Rates – Interest Rate (NY)

	Log Price									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Interest Rate (NY)	0.002					0.003				
	(0.002)					(0.002)				
$\widehat{InterestRate}(NY)$		0.018**	0.020**	0.022***	0.022***		0.018**	0.019**	0.020***	0.021***
		(0.008)	(0.008)	(0.008)	(0.008)		(0.007)	(0.007)	(0.007)	(0.008)
Open Cohort × Building FE	✓	✓	✓	✓	✓					
Open Cohort × Month × Apt Type FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Open Cohort × Building × Apt Type FE						✓	✓	✓	✓	✓
Cluster	Building	Building	Building	Building	Building	Building	Building	Building	Building	Building
Stage	OLS	Second	Second	Second	Second	OLS	Second	Second	Second	Second
Rate in IV		NY Comm.	US Comm.	US 7YR	US 10YR		NY Comm.	US Comm.	US 7YR	US 10YR
R^2	0.9748	0.9749	0.9749	0.9750	0.9750	0.9939	0.9942	0.9942	0.9938	0.9938
Adjusted R^2	0.9700	0.9702	0.9702	0.9702	0.9702	0.9926	0.9930	0.9930	0.9925	0.9925
Observations	59,446	55,501	55,843	58,323	58,323	59,446	55,501	55,843	58,323	58,323
F-test (1st stage)		1,207.5	1,188.0	1,234.1	1,216.7		1,203.1	1,184.2	1,230.9	1,213.3

Notes: This table presents 2SLS results on the effect of interest rates faced by property owners on monthly rents; in other words, this table estimates the semi-elasticity of monthly rents with respect to interest rates. The second stage regression model is: $\text{Log Price}_{cbat} = \beta \widehat{\text{Interest Rate}}\,(\text{NY})_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c; Interest Rate $(\text{NY})_{cbt}$ is equal to the interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the median commercial mortgage rate in NY. Interest $\widehat{\text{Rate}}\,(\text{NY})_{cbt}$ is the predicted value of Interest Rate $(\text{NY})_{cbt}$ from the first stage; first stage regressions are shown in Table A.7. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. Standard errors are clustered at the building level (i.e. at the level of the treatment). The results are consistent with those in the main text.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.12: Elasticity of Monthly Rents with respect to Interest Rates – Log Interest Rate (US)

	Log Price									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Log Interest Rate (US)	0.019					0.018				
	(0.012)					(0.012)				
$Log\widehat{InterestRate}(US)$		0.104**	0.110**	0.118***	0.120***		0.099**	0.105**	0.109***	0.112***
		(0.042)	(0.043)	(0.043)	(0.044)		(0.040)	(0.041)	(0.041)	(0.042)
Open Cohort × Building FE	✓	✓	✓	✓	✓					
Open Cohort × Month × Apt Type FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Open Cohort × Building × Apt Type FE						✓	✓	✓	✓	✓
Cluster	Building	Building	Building	Building	Building	Building	Building	Building	Building	Building
Stage	OLS	Second	Second	Second	Second	OLS	Second	Second	Second	Second
Rate in IV		NY Comm.	US Comm.	US 7YR	US 10YR		NY Comm.	US Comm.	US 7YR	US 10YR
R^2	0.9748	0.9749	0.9749	0.9750	0.9750	0.9939	0.9943	0.9942	0.9938	0.9938
Adjusted R^2	0.9700	0.9702	0.9702	0.9702	0.9702	0.9926	0.9930	0.9930	0.9925	0.9925
Observations	59,446	55,501	55,843	58,323	58,323	59,446	55,501	55,843	58,323	58,323
F-test (1st stage)		1,237.2	1,230.4	1,301.9	1,279.0		1,233.2	1,227.0	1,298.9	1,275.9

Notes: This table presents 2SLS results on the effect of interest rates faced by property owners on monthly rents; in other words, this table estimates the elasticity of monthly rents with respect to interest rates. The second stage regression model is: $\text{Log Price}_{cbat} = \beta \text{Log } \widehat{\text{Interest Rate}} \text{ (US)}_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c; $\text{Log Interest Rate (US)}_{cbt}$ is equal to the Log interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the Log median commercial mortgage rate in the US. $\text{Log } \widehat{\text{Interest Rate}} \text{ (US)}_{cbt}$ is the predicted value of $\text{Log Interest Rate (US)}_{cbt}$ from the first stage; first stage regressions are shown in Table A.8. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. Standard errors are clustered at the building level (i.e. at the level of the treatment). The results are consistent with those in the main text.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

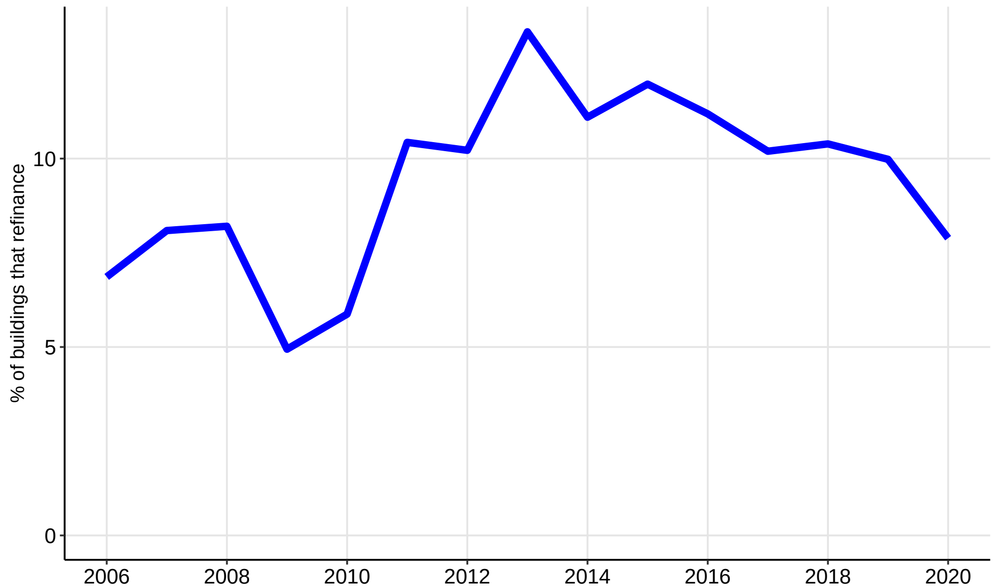
Table A.13: Elasticity of Monthly Rents with respect to Interest Rates – Log Interest Rate (NY)

	Log Price									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Log Interest Rate (NY)	0.013					0.013				
	(0.010)					(0.010)				
$\widehat{LogInterestRate}(NY)$		0.091**	0.099**	0.109***	0.109***		0.086**	0.092**	0.100***	0.101***
		(0.038)	(0.039)	(0.040)	(0.041)		(0.036)	(0.037)	(0.037)	(0.038)
Open Cohort × Building FE	✓	✓	✓	✓	✓					
Open Cohort × Month × Apt Type FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Open Cohort × Building × Apt Type FE						✓	✓	✓	✓	✓
Cluster	Building	Building	Building	Building	Building	Building	Building	Building	Building	Building
Stage	OLS	Second	Second	Second	Second	OLS	Second	Second	Second	Second
Rate in IV		NY Comm.	US Comm.	US 7YR	US 10YR		NY Comm.	US Comm.	US 7YR	US 10YR
R^2	0.9748	0.9749	0.9749	0.9750	0.9750	0.9939	0.9943	0.9942	0.9938	0.9938
Adjusted R^2	0.9700	0.9702	0.9702	0.9702	0.9702	0.9926	0.9930	0.9930	0.9925	0.9925
Observations	59,446	55,501	55,843	58,323	58,323	59,446	55,501	55,843	58,323	58,323
F-test (1st stage)		1,222.2	1,197.3	1,245.8	1,232.1		1,217.4	1,193.2	1,242.4	1,228.5

Notes: This table presents 2SLS results on the effect of interest rates faced by property owners on monthly rents; in other words, this table estimates the elasticity of monthly rents with respect to interest rates. The second stage regression model is: $\text{Log Price}_{cbat} = \beta \widehat{\text{Log Interest Rate}}\ (\text{NY})_{cbt} + \alpha_{cba} + \alpha_{cat} + \varepsilon_{cbat}$, where Log Price_{cbat} is the log of the average rent price for all apartments of type a in building b in month t for cohort c; Log Interest Rate $(\text{NY})_{cbt}$ is equal to the Log interest rate on the current mortgage of building b in cohort c until the building refinances, at which point it switches to the Log median commercial mortgage rate in NY. $\widehat{\text{Log Interest Rate}}\ (\text{NY})_{cbt}$ is the predicted value of Log Interest Rate $(\text{NY})_{cbt}$ from the first stage; first stage regressions are shown in Table A.9. Moreover, α_{cba} is Cohort-Building-Apartment Type FE, and α_{cat} is Cohort-Apartment Type-Month FE. Standard errors are clustered at the building level (i.e. at the level of the treatment). The results are consistent with those in the main text.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.6 Other Figures



Notes: This figure plots the fraction of multifamily properties in NYC, from our sample, that refinance by year. The ATTOM data is used to detect refinancing transactions.

Figure A.11: Fraction of buildings that refinance, by year

Appendix B

Nonbank Intermediation in Times of Crisis: Evidence from Small Businesses

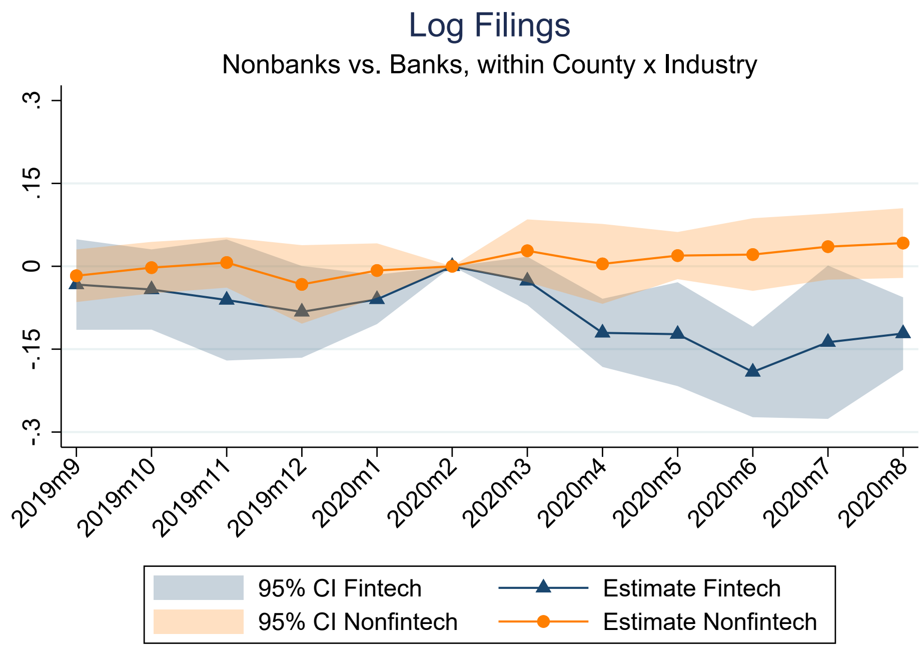
B.1 Robustness of within-ZIP code results

Our within-ZIP code results rely on the identifying assumption that relative demand for loans from the different types of lenders does not change through the sample period. In other words, we are ruling out scenarios in which, for instance, demand for fintech loans in a ZIP code falls while demand for bank loans stays the same. Such a scenario could fully account for the finding that fintech lending differentially fell during the crisis, but the cause of the lending reduction would be driven by demand as opposed to supply. Thus, one may be concerned that with our within-ZIP analysis, potential changes in the sample of borrowers over time or differential dependence on fintechs or banks by industry could lead to changes in relative demand for loans from the different types of lenders. For instance, perhaps the sample of firms seeking bank financing both before and after the crisis does not change in a given ZIP code, but the sample of firms seeking fintech financing does change in that there are fewer firms in the ZIP code seeking fintech financing. This would imply a relative reduction in the demand for fintech loans.

In this section, to mitigate some of these concerns, we run a version of equations 2.1 and 2.2 that are (1) within-industry or (2) within-debtor as opposed to within-ZIP code. In the within-industry analysis, we compare changes in credit supplied by banks, fintechs, and nonfintech nonbanks to firms in the same industry in a given county, using county-by-industry-by-month fixed effects. By looking within-industry, we control for changes in demand driven by industry-specific shocks within the county. Thus, any concerns that firms in certain industries are both differentially affected by the COVID-19 shock and differentially prefer certain lender types would be mitigated by the within-industry analysis.

Then, in the within-debtor analysis, we further narrow the scope and compare changes in credit supplied by banks, fintechs, and nonfintech nonbanks to the same debtor, using debtor-by-month fixed effects to control for debtor-level trends. Because this analysis is conducted within-debtor, we mitigate concerns that a changing sample of firm borrowers could be driving changes in relative demand and, in turn, driving the results from the above section. This analysis is, however, done at the cost of a much smaller sample of observations, as small businesses rarely borrow from multiple lenders at once. Just for the within-debtor analyses, to allow for more observations and statistical power, we include UCC filings involving any type of collateral, rather than those with only a blanket lien. The identifying assumption for the within-debtor analysis is that relative debtor-level demand for loans from the different types of lenders does not change through the sample period; this assumption is much weaker than the one required for our ZIP code results. Nevertheless, the results of this section broadly corroborate the results from the previous section.

B.1.1 Within-industry results



Notes: This figure shows the extent to which fintech and nonfintech nonbank lenders, relative to banks, changed their credit supply to small businesses in the same county and industry after the start of the COVID-19 pandemic, using county-by-industry-by-month FE to control for county-industry level demand trends. Specifically, the figure plots the estimated β_T^F's and β_T^{NF}'s from the following regression model: $\log(Filings_{cilt}) = \alpha_{cit} + \gamma_l + \sum_T \beta_T^F (\mathbf{1}_{l \in F} \times \mathbf{1}_{t=T}) + \sum_T \beta_T^{NF} (\mathbf{1}_{l \in NF} \times \mathbf{1}_{t=T}) + \varepsilon_{cilt}$, where $\log(Filings_{cilt})$ is the log number of UCC filings in county c to businesses in industry i by lender l in month t, α_{cit} is the county-by-industry-by-month FE, and γ_l is the lender FE. Moreover, $\mathbf{1}_{l \in F}$ is an indicator variable that equals one if lender l is a fintech lender, $\mathbf{1}_{l \in NF}$ is an indicator variable that equals one if lender l is a nonfintech nonbank lender, and $\mathbf{1}_{t=T}$ is an indicator variable that equals one for the month t. Note that banks are the reference lender type, and February 2020 is the reference month.

Figure B.1: Changes in Nonbank Credit Supply during COVID-19: Within-County-Industry

Figure B.1 plots the point estimates and confidence intervals for the coefficients of interest in the within-industry (and county) version of equation 2.1. The results are very similar to those seen in Figure 1.7. Although credit supply from all lender types followed broadly similar trends prior to February 2020, credit supplied by fintechs fell by up to 15% relative to that of banks after the start of the COVID-19 pandemic. We do not see any such drop for nonfintech nonbanks.

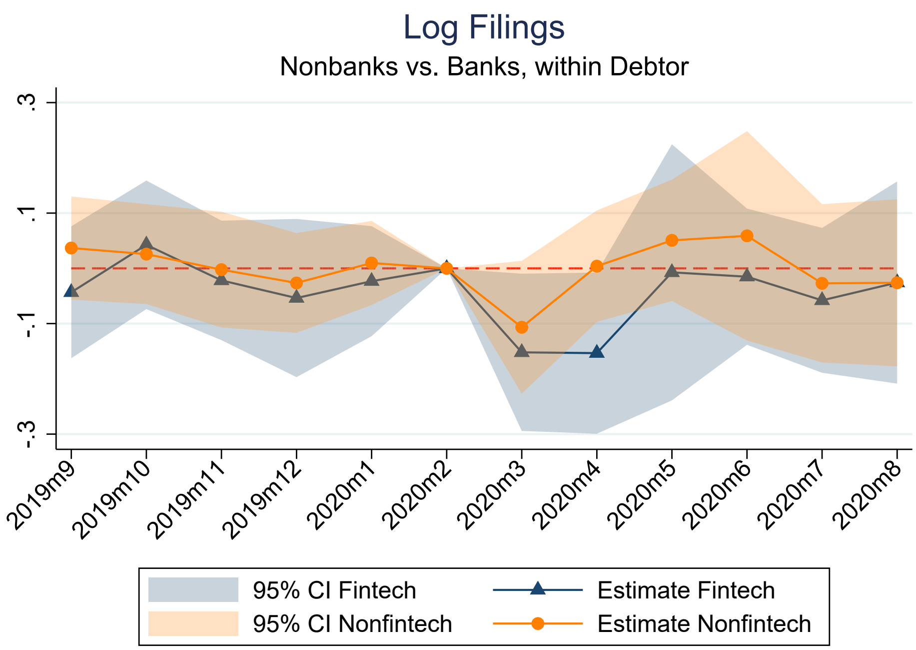
Table B.1: Cumulative Change in Credit Supply during COVID-19: Within-Industry

	(1) Log Filings	(2) Log Filings
Non-Fintech × Post	0.0219	0.0327*
	(0.0333)	(0.0169)
Fintech × Post	-0.293***	-0.0596*
	(0.105)	(0.0322)
Industry × Month FE	✓	
County × Industry × Month FE		✓
Lender FE	✓	✓
Cluster	Lender	Lender
Obs	8228	8947
R^2	0.454	0.344
Adj R^2	0.408	0.145

Notes: This table quantifies the overall extent to which the credit supply of fintechs and nonfintech nonbanks, relative to that of banks, to businesses in the same county and industry changed in the entire six month period following the start of the COVID-19 pandemic. Specifically, the table presents the results for the following regression model: $\log(Filings_{cilt}) = \alpha_{cit} + \gamma_l + \beta^F(\mathbf{1}_{l\in F} \times Post_t) + \beta^{NF}(\mathbf{1}_{l\in NF} \times Post_t) + \varepsilon_{cilt}$, where $\log(Filings_{cilt})$ is the log number of UCC filings in county c to businesses in industry i by lender l in month t, α_{cit} is the county-by-industry-by-month FE, and γ_l is the lender FE. Column (1) substitutes α_{it} for α_{cit}. Moreover, $\mathbf{1}_{l\in F}$ is an indicator variable that equals one if lender l is a fintech lender, $\mathbf{1}_{l\in NF}$ is an indicator variable that equals one if lender l is a nonfintech nonbank lender, and $Post_t$ is an indicator variable that equals one if month t is between March and August 2020. Note that banks are the reference lender type. Standard errors are clustered at the lender level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.1 reports the cumulative change in credit supply for the different lender types over the entire six-month period after the start of the pandemic, looking within industry. Column (1) reports the results with just industry fixed effects, while Column (2) reports those with county x industry fixed effects. Table B.1 shows that when looking within county and industry, fintech lenders decreased their credit supply by 5.96% relative to banks in the post-pandemic period, which is very similar to the baseline results in Table 1.2. Thus, these results confirm that even after controlling for industry-level trends, fintech lenders differentially cut their credit supply to small businesses, relative to both banks and nonfintech nonbanks.

B.1.2 Within-debtor results



Notes: This figure shows the extent to which fintech and nonfintech nonbank lenders, relative to banks, changed their credit supply to individual small businesses after the start of the COVID-19 pandemic, using debtor-by-month FE to control for debtor-level demand trends. Specifically, the figure plots the estimated β_T^F's and β_T^{NF}'s from the following regression model: $\log(Filings_{dlt}) = \alpha_{dt} + \gamma_l + \sum_T \beta_T^F (\mathbf{1}_{l\in F} \times \mathbf{1}_{t=T}) + \sum_T \beta_T^{NF} (\mathbf{1}_{l\in NF} \times \mathbf{1}_{t=T}) + \varepsilon_{dlt}$, where $\log(Filings_{dlt})$ is the log number of UCC filings to debtor d by lender l in month t, α_{dt} is the debtor-by-month FE, and γ_l is the lender FE. Moreover, $\mathbf{1}_{l\in F}$ is an indicator variable that equals one if lender l is a fintech lender, $\mathbf{1}_{l\in NF}$ is an indicator variable that equals one if lender l is a nonfintech nonbank lender, and $\mathbf{1}_{t=T}$ is an indicator variable that equals one for the month t. Note that banks are the reference lender type, and February 2020 is the reference month.

Figure B.2: Changes in Nonbank Credit Supply during COVID-19: Within-Debtor

Figure B.2 is the within-debtor analogue to Figure 1.7. Again, the results are broadly in line with Figure 1.7. Prior to February 2020, the credit supply of both fintechs and nonfintech nonbanks, relative to banks, did not differ significantly from February 2020. However, at the outset of the pandemic, credit supply by fintechs differentially falls relative to banks. In particular, in March 2020, when pandemic-related shutdowns first started occurring, fintech credit supply dropped by around 11% relative to bank credit supply. Our estimates also indicate a relative, albeit more muted, decline in the credit supply of nonfintech nonbanks.

As we're controlling for time-varying debtor-level demand, these results suggest that a differential reduction in fintech credit supply is the driver behind decreases in fintech lending.

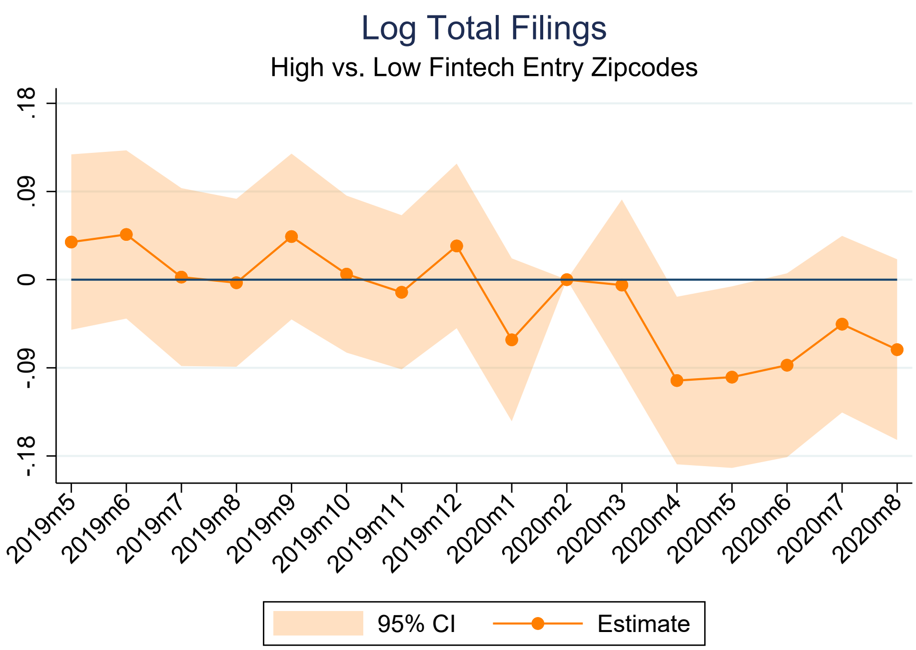
Table B.2: Cumulative Change in Credit Supply during COVID-19: Within-Debtor

	(1) Log Filings
Non-Fintech × Post	-0.0255
	(0.0254)
Fintech × Post	-0.0610*
	(0.0319)
Obs	1753
R^2	0.653
Adj R^2	0.226
Debtor × Month FE	Y
Lender FE	Y
Cluster	Lender

Notes: This table quantifies the overall extent to which the credit supply of fintechs and nonfintech nonbanks, relative to that of banks, to individual small businesses changed in the entire six month period following the start of the COVID-19 pandemic. Specifically, the table presents the results for the following regression model: $\log(Filings_{dlt}) = \alpha_{dt} + \gamma_l + \beta^F(\mathbf{1}_{l\in F} \times Post_t) + \beta^{NF}(\mathbf{1}_{l\in NF} \times Post_t) + \varepsilon_{dlt}$, where $\log(Filings_{dlt})$ is the log number of UCC filings to debtor d by lender l in month t, α_{dt} is the debtor-by-month FE, and γ_l is the lender FE. Moreover, $\mathbf{1}_{l\in F}$ is an indicator variable that equals one if lender l is a fintech lender, $\mathbf{1}_{l\in NF}$ is an indicator variable that equals one if lender l is a nonfintech nonbank lender, and $Post_t$ is an indicator variable that equals one if month t is between March and August 2020. Note that banks are the reference lender type. Standard errors are clustered at the lender level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

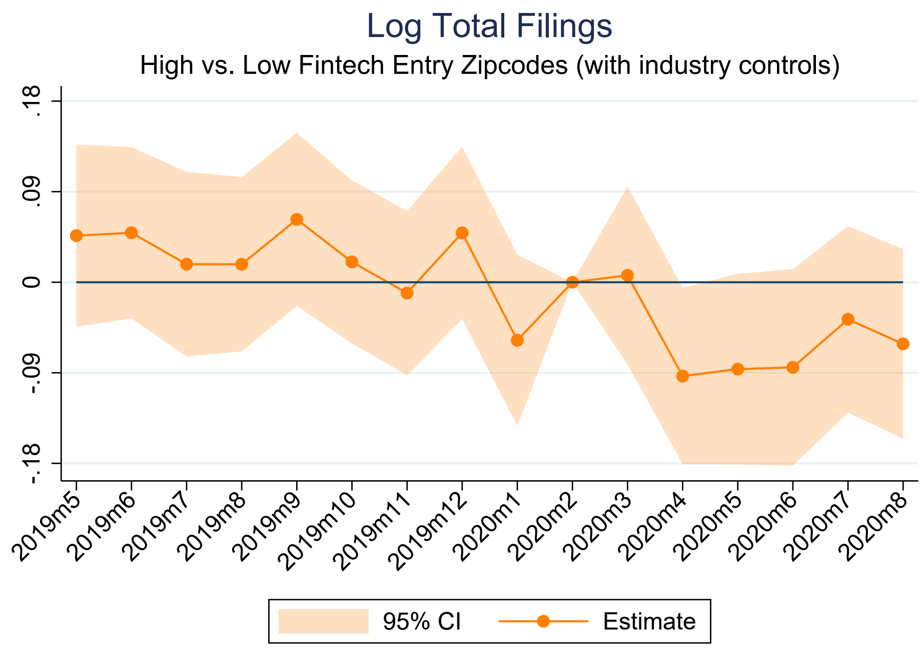
The results in Table B.2 generally corroborate the analogous results in Table 1.2. Specifically, Table B.2 shows that, cumulatively, fintech lenders decreased their credit supply by 6.1% relative to banks in the post COVID-19 period, which is in line with the 6.7% estimate found in Table 1.2. These results broadly confirm that even when looking within a debtor, fintechs in particular, relative to other types of financial intermediaries, cut their credit supply to small businesses during the pandemic.

B.2 Dynamic specifications for across-ZIP analyses



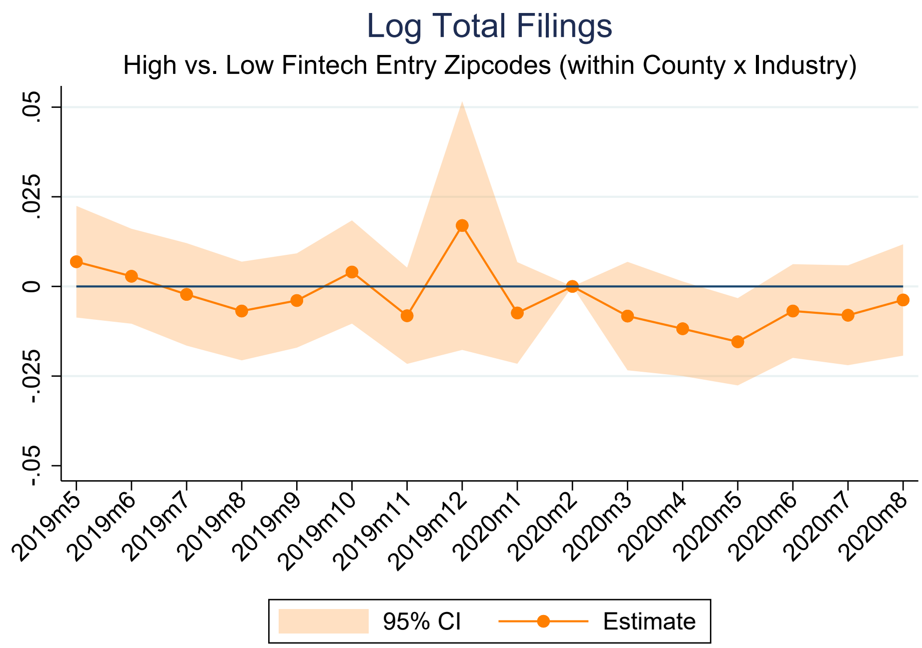
Notes: This figure shows the extent to which ZIP codes with high fintech entry experienced differential changes to their aggregate credit supply after the start of the pandemic, relative to ZIP codes with low fintech entry. Specifically, the figure plots the estimated β_T's from the following regression model: $\log(Filings_{zct}) = \alpha_{ct} + \gamma_z + \sum_T \beta_T \left(\text{HighFintechEntry}_z \times \mathbf{1}_{t=T}\right) + \mathbf{X}_{zt} + \varepsilon_{zct}$, where $\log(Filings_{zct})$ is the log number of UCC filings in ZIP code z and county c in month t, α_{ct} is the county-by-month FE, and γ_z is the ZIP code FE. Moreover, $\text{HighFintechEntry}_z$ equals 1 if the growth rate of fintech filings that ZIP code z experienced between 2016 and 2019 is at or above the fourth quintile, and $\mathbf{1}_{t=T}$ is an indicator variable that equals one for the month t. Finally, X_{zt} is a vector of ZIP code level demographic variables (log number of business establishments, share of households living under the poverty line, log of median income, and share of the population identifying as a racial minority) as of 2019 interacted with the vector of $\mathbf{1}_{t=T}$ variables.

Figure B.3: Changes in Total Credit Supply for ZIP codes with High vs. Low Fintech Entry



Notes: This figure shows the extent to which ZIP codes with high fintech entry experienced differential changes to their aggregate credit supply after the start of the pandemic, relative to ZIP codes with low fintech entry, controlling for how industry composition affected ZIP codes' demand trends. Specifically, the figure plots the estimated β_T's from the following regression model: $\log(Filings_{zct}) = \alpha_{ct} + \gamma_z + \sum_T \beta_T \left(\text{HighFintechEntry}_z \times \mathbf{1}_{t=T}\right) + \mathbf{X}_{zt} + \varepsilon_{zct}$, where $\log(Filings_{zct})$ is the log number of UCC filings in ZIP code z and county c in month t, α_{ct} is the county-by-month FE, and γ_z is the ZIP code FE. Moreover, $\text{HighFintechEntry}_z$ equals 1 if the growth rate of fintech filings that ZIP code z experienced between 2016 and 2019 is at or above the fourth quintile, and $\mathbf{1}_{t=T}$ is an indicator variable that equals one for the month t. Finally, X_{zt} is a vector of ZIP code level demographic variables (log number of business establishments, share of households living under the poverty line, log of median income, and share of the population identifying as a racial minority) as of 2019 interacted with the vector of $\mathbf{1}_{t=T}$ variables. X_{zt} also includes a vector of ZIP code level industry filing shares between 2016 and 2019, where industries are defined by two-digit NAICS codes, interacted with the vector of $\mathbf{1}_{t=T}$ variables.

Figure B.4: Changes in Total Credit Supply for ZIP codes with High vs. Low Fintech Entry (with industry share controls)



Notes: This figure shows how within-industry credit supply changed differentially in ZIP codes with high fintech entry vs. low fintech entry, after the start of the COVID-19 pandemic. Specifically, the figure plots the estimated β_T's from the following regression model: $\log(Filings_{zcit}) = \alpha_{cit} + \gamma_z + \sum_T \beta_T\,(\text{HighFintechEntry}_z \times \mathbf{1}_{t=T}) + \mathbf{X}_{zt} + \varepsilon_{zcit}$, where $\log(Filings_{zcit})$ is the log number of UCC filings in ZIP code z and county c in industry i during month t, α_{cit} is the county-by-industry-by-month FE, and γ_z is the ZIP code FE. Moreover, $\text{HighFintechEntry}_z$ equals 1 if the growth rate of fintech filings that ZIP code z experienced between 2016 and 2019 is at or above the fourth quintile, and $\mathbf{1}_{t=T}$ is an indicator variable that equals one for the month t. Finally, X_{zt} is a vector of ZIP code level demographic variables (log number of business establishments, share of households living under the poverty line, log of median income, and share of the population identifying as a racial minority) as of 2019 interacted with the vector of $\mathbf{1}_{t=T}$ variables.

Figure B.5: Changes in Total Credit Supply for ZIP codes with High vs. Low Fintech Entry (within County x Industry)