Work-From-Home, Commercial Real Estate Risk and Credit Supply: Evidence from a Large Sample of Bank Loan Portfolios*

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Abstract

The pandemic-driven rise in work-from-home (WFH) has decreased commercial real estate (CRE) valuations, increasing credit risk in banks' CRE portfolios. Using novel data, we measure individual banks' exposure to the WFH shock based on their pre-pandemic CRE loan distribution. Banks with higher WFH exposure significantly tighten credit supply: a one standard deviation increase in exposure reduces new CRE originations by 7.3%. We find negative spillover effects, as these banks also reduce credit for risky non-CRE loans. The average zipcode experienced a 13 percentage point decrease in new origination growth rates due to banks' response to WFH shock, suggesting partial but incomplete substitution from banks to nonbanks.

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1 Introduction

A seemingly long-lasting economic consequence of the COVID-19 pandemic is an increase in work-from-home (WFH) rates. The rise in WFH has the potential to bring important economic benefits in the long run, for example through reduced commuting, improved housing affordability, or higher productivity.[1] A salient short-run consequence of the shift to WFH, however, has been a large drop in commercial real estate (CRE) cash flows and valuations (Gupta et al., forthcoming), with potentially important adverse consequences for the financial intermediaries and the real economy (Jiang et al., forthcoming; Cole and White, 2012).[2]

A key channel through which these valuation declines could have broader impacts on the economy is through banks' exposure to loans backed by distressed CRE properties. CRE loans make up about a quarter of total assets at the average bank, and as of the end of 2023, banks held about $2.8 trillion of the $5.8 trillion in CRE loans (including multifamily) outstanding. An increase in expected delinquencies and defaults in a key part of the bank's risk-asset portfolio may incentivize the bank to reduce risk by tightening CRE credit supply, which would likely have important negative effects on the real economy (Peek and Rosengren, 2000; Chaney et al., 2012). Furthermore, if banks reduce other (non-CRE) types of lending to maintain their overall capitalization ratios, real effects could be more widespread. These credit supply effects are likely to play out over the next several years, as WFH rates have stabilized well above pre-pandemic levels.[3]

In this paper, we empirically study the effects of the WFH shock on credit availability through this bank exposure channel. We begin by computing a bank's "exposure" to the

[1] See, for example, Delventhal and Parkhomenko (forthcoming), Howard et al. (2023), Delventhal et al. (2022), Davis et al. (2024). In contrast, Monte et al. (2023) argue that the rise in WFH at the onset of the pandemic acted as an equilibrium selection device and pushed many cities to a new equilibrium with lower welfare due to diminished production externalities.

[2] As of June 2025, CRE prices have fallen about 30 percent (in real terms) from their peak levels according to the CoStar repeat sales index.

[3] Barrero et al. (2023)'s Survey of Working Arrangements and Attitudes finds that about 27% of paid workdays in the U.S. in July 2025 were WFH days, and this number has remained stable since January 2022.

WFH shock for a large sample of banks using public records data from CoreLogic, which allows us to infer the detailed location of the properties collateralizing CRE loans on each bank's pre-pandemic balance sheet. Importantly, CoreLogic covers lending activities of not only large banks but also small banks, whose loan portfolios have a larger share of CRE loans. We define a county's exposure to the WFH shock as the share of pre-pandemic county employment that could possibly be done remotely, as measured by Dingel and Neiman (2020). A bank's exposure to the WFH shock is a weighted average of the county-level WFH exposure in each county in which the bank has outstanding CRE loans. For example, a bank with a high share of its pre-pandemic CRE loans collateralized by properties in San Francisco county, a well-known example of an area with an industry mix that allowed for high WFH rates once the pandemic hit, would be highly exposed under our definition.

We estimate the effect of bank exposure on bank CRE credit supply using an identification strategy similar to Khwaja and Mian (2008). Essentially, we compare levels of CRE mortgage originations among banks lending in the same market (county-year) with varying levels of exposure. Importantly, market-level credit demand shocks, which could be correlated with bank exposure, are partialed out by the county×year fixed effects. Our regression also controls for various bank characteristics such as size, the share of CRE loans in total assets, and the share of CRE loans collateralized by owner-occupied CRE properties.

We find that banks with greater exposure sharply reduced CRE lending after the pandemic. In the years leading up to the pandemic, we show no evidence of any pre-trend in the relationship between Exposure and lending. Starting in 2020, the first year of the pandemic, the relationship turns negative and stays negative throughout the post-pandemic period. On average across all post-pandemic years, a one standard deviation increase in Exposure is associated with a 7.3 percent decrease in the number of originations. We find similar but slightly noisier effects on origination volumes.

Our proposed mechanism for these results is that more-exposed banks experience larger (expected) losses in their CRE loan portfolio, causing them to reduce credit risk in their loan portfolio by tightening credit supply for new originations of credit, especially loans against riskier CRE properties. Since CRE mortgages are very illiquid assets, one of the easiest ways for banks to adjust their CRE portfolio risk is through a change in origination behavior. Consistent with our proposed mechanism, we find that more-exposed banks have a higher level of CRE loan delinquencies and loan loss provisions. We also find that more-exposed banks reduced their lending for CRE properties in counties with larger WFH shocks, which likely experienced worse declines in CRE fundamentals and are plausibly riskier counties for banks to lend in.

The incentive for de-risking is likely to be be larger if CRE loan losses can lead to a larger negative shock to bank capital. A large negative capital shock may shift banks closer to a constraint for raising new external funds as in the model of Froot and Stein (1998), and banks may especially need to de-risk to comply with regulatory requirements. Consistent with this hypothesis, we find that banks with a larger CRE loan portfolio relative to their Tier 1 capital reduce their lending by more given the same level of exposure to the WFH shock. We also show that de-risking is not confined to the CRE portfolio. More exposed banks tighten credit supply for non-CRE, risky loan types including some small business and residential real estate loans.

In the last part of our paper, we estimate the effect of the pullback in CRE credit supply by exposed banks on aggregate, market level credit supply. It is ex ante unclear whether bank exposure to the WFH shock led to a decline in the aggregate credit supply. Some CRE loan borrowers might have substituted from exposed banks to nonbanks or less-exposed banks, especially as credit from capital markets was widely available over most of our sample period. Our estimates show some evidence of substitution from exposed banks to nonbanks, but we nevertheless find substantial aggregate credit supply effects. We estimate that the average zipcode experienced a 13 percentage point decrease in the growth rate of total new originations (bank + nonbank) due to the bank lending response to the WFH shock. This represents the combined effects of a 29 percentage point decrease in the growth rate of originations by banks and a 8 percentage point increase in the growth rate of nonbank originations (again in the average zipcode).

Related literature. Our paper contributes to the growing literature on the implications of WFH for CRE valuations and the broader economy.[4] In early literature on this topic, Gupta et al. (forthcoming) use an asset pricing model to infer very large declines in office property value as a result of the WFH shock, which have largely materialized as the authors' model predicted. Gupta et al. (forthcoming) discuss damaged bank balance sheets and increased risk of financial fragility as important implications of their results, but do not present quantitative work on this topic as we do in this paper. Rosenthal et al. (2022) document how the pandemic changed the value firms place on access to city centers and estimate implications for the spatial patterns of commercial rents. Glancy and Wang (2024) show that office lease expiration has large negative effects on office property fundamentals such as occupancy and property income.

There are still relatively few works that examined the effects of CRE property value decline on banks, despite the potentially large negative consequences. Jiang et al. (forthcoming) estimate the effects of property value decline and monetary policy tightening on CRE loan value and show that CRE distress could put many smaller banks at risk of solvency runs. Crosignani and Prazad (2024) show evidence that banks extended the maturity of their impaired CRE loans and avoided writing off their capital. Glancy and Kurtzman (2024) study the determinants of post-pandemic CRE loan delinquencies, finding that small banks' comparatively modest delinquency rates mostly reflect observable portfolio characteristics (e.g. their low holdings of large-sized office loans). Consistent with their findings, we find a negative correlation between our measure of exposure to WFH and bank size. Our paper contributes to this literature by providing empirical estimates of the effects of the WFH shock on the supply of CRE loans as well as other lending products.

This paper also contributes to the literature of CRE lending more broadly. A body of

[4]There is also a related literature on implications for residential real estate. See, for example, Gupta et al. (2022) and Ramani et al. (2024).

papers in this literature study topics related to CRE loan distress or performance with a different focus than our paper, such as renegotiation or modification/sales of distressed CRE loans (Glancy et al., 2025; Flynn et al., 2024; Glancy and Kurtzman, 2024; Wong, 2018) and bank monitoring of financial performance of their CRE loans (Hughes and Nichols, 2025). Another body of papers, such as Ghent and Valkanov (2016), Black et al. (2020), and Glancy et al. (2022), study CRE loan originations, focusing on differences across intermediary types (banks, insurers, and CMBS). Many papers in this CRE literature rely on datasets that have a relatively limited coverage of activities of smaller banks, for which CRE lending is a larger share of their loan portfolio.[5] Our paper contributes to this literature by studying bank risk management of their CRE loan portfolio through loan originations, using a new data source that has a more comprehensive coverage of smaller banks.

Our paper is also related to a literature showing that shocks to bank net worth influence their choice of new risk-assets. This effect is often called the "bank lending channel" (Khwaja and Mian, 2008; Bidder et al., 2021; Chodorow-Reich, 2014; Ivashina and Scharfstein, 2010; Paravisini et al., 2023; Schnabl, 2012). Much of the recent literature on the bank lending channel focuses on corporate lending. We contribute to this literature by focusing on a shock to bank net worth coming through the bank's CRE portfolio. In addition, like Greenstone et al. (2020), we focus on shocks to bank balance sheets that occurred amid a strong macroeconomy whereas much of the existing literature focuses on times of crisis. Even during good economic times, we find evidence of a strong bank lending channel that has important consequences for aggregate credit supply.

[5] Existing loan-level data used in the literature include bank Y-14 regulatory filings, Real Capital Analytics (RCA), or Trepp. Y-14 filings are only required for the handful of banks with assets totaling more than $100 billion, so it is not a useful data source for studying small and regional banks. RCA only covers properties valued at $2.5 million or higher, which are more likely to be originated by large banks. Appendix Section A.3 compares the CoreLogic data with the data set from RCA in detail. Given a lack of loan-level data on bank balance sheet CRE loans, Jiang et al. (forthcoming) use data on loans securitized into CMBS from Trepp as a proxy to gauge potential distress in bank CRE loans.

2 Data

2.1 CoreLogic Real Estate Data

CoreLogic data cover the universe of property transactions, including information about the mortgage if there is one, in covered counties. Data are sourced from public records filed at the county deeds office. Mortgage refinancings are also recorded in this dataset, since they expunge an existing lien on a property and replace it with a new one. CoreLogic covers both residential and commercial property transactions. We only use data on commercial properties in this paper. Though it has been used extensively to study residential real estate markets, few papers use CoreLogic to study CRE.[6] An alternative CRE dataset covering property transactions used in the literature is RCA. Appendix Section A.3 compares the CoreLogic data with the dataset from RCA in detail. We find that RCA covers roughly half of CRE lending by banks in CoreLogic.

2.2 Bank Regulatory Data

We also use quarterly Call Reports and FR Y9-C filings to obtain bank-level information that is not available in CoreLogic, including bank characteristics (e.g. size) and information on loan performance. Both data sets are drawn from regulatory filings from banks or bank holding companies (BHCs). Call Reports are regulatory filings which provide information about a bank subsidiary of a BHC (for example, Wells Fargo Bank), and FR Y9-C provides information about a BHC (for example, Wells Fargo & Company). Both data sets report detailed information about bank balance sheets and incomes at a quarterly frequency. These data do not allow for geographic disaggregation, but provide a comprehensive picture of a bank's total size, capitalization, and the size and performance of its CRE portfolio, including including amounts of loans in delinquencies, amounts charged offs, and amounts in allowances

[6] Babalievsky et al. (2023) use CoreLogic's commercial tax data to estimate the impact of CRE regulations on output using a spatial general equilibrium model.

in anticipation of future losses.

2.3 Match between CoreLogic and bank regulatory filings

Although CoreLogic provides lender information for each loan, it has a few limitations. First, the same bank often appears in the data under different names – for example, "Wells Fargo Bank" and "Wells Fargo BK NA." Second, it is difficult to match the data to other lender-level data sets like Call Reports because CoreLogic does not provide additional information about lenders such as their tax IDs, which can be used to match with another data set.

Therefore, to aggregate loans to the bank level, we match CoreLogic lender names to RSSD IDs, which is a unique identifier assigned to financial institutions by the Federal Reserve. Using RSSD IDs, we can combine CoreLogic with Call Reports and FR Y9-C.

Details about the data match are described in Appendix A.1. Overall, we match about 91% of bank CRE loans in CoreLogic to unique RSSD IDs. In terms of loan amount, about 99% of bank CRE loans in CoreLogic are matched to RSSD IDs, suggesting our match rate is higher for larger banks, which are likely to originate large loans.

2.4 Summary Statistics of Loan Characteristics in CoreLogic

Table 1 presents summary statistics of characteristics of CRE loans originated by banks from 2016 to 2023 in CoreLogic.[7]

The average loan original balance is about 2 million, but the median is much smaller at around 550,000 due to a small fraction of very large loans. The information on loan maturity is non-missing for about 40% of loans in the sample, and the average loan term is 13.5 years, while the median is 10 years. Some loans have a very long maturity of up to 30 years. The average mortgage rate in our sample is 4.8%, but the information on mortgage rates is missing for a majority of the sample.

[7]The sample excludes loans originated by banks that are active in CMBS loan originations, which are defined as banks with at least 10% of their CRE loan originations securitized into CMBS in the RCA data. See Appendix Section A.4 for additional detail.

Based on the land use description associated with each property, CoreLogic assigns a type to each property (there are hundreds of CoreLogic types). We then use our judgment to classify each property into one of the eight types listed in Table 1. Many land use descriptions are generic (e.g. commercial building) and so we classify many properties as "Unspecificed" types. For our main analysis, we do not use the property type information and group all CRE property types together. However, in Section 5.3.1, we present results where we allow bank exposure to depend on the specific property type. Table 1 shows that in our sample, there is no particular concentration in a single property type. Multifamily, retail, and type unspecified properties have the highest shares of around 17%.

Lastly, 30% and 25% of originations are classified as purchase and refinance loans by CoreLogic, respectively. Junior-lien loans have a large share of 38%, and 8% of loans have unknown loan types.

2.5 Computing loans outstanding in 2019Q4

CoreLogic only provides information on loans at the time of origination, so to determine bank CRE exposure just before the onset of COVID-19 we need to infer how much principal remains on bank balance sheets at the end of 2019. This requires converting each loan's origination record into a quarterly panel tracking the amount of principal outstanding over time. Appendix Section A.2 describes the algorithm we use and the assumptions we make.

The process described in the Appendix results in a panel of loans outstanding as of 2019Q4 for each bank. We can compare some aspects of a bank's CRE loan portfolio to their counterparts in the Call Report to gauge the accuracy of our process to construct the loan portfolio with CoreLogic. Although the Call Report provides a very limited set of characteristics of a bank-level CRE loan portfolio, we can still observe the size of the CRE loan portfolio and a breakdown of loans for multifamily properties and other CRE loans. The binned scatter plot in Figure 1(a) shows that the CRE loan amount outstanding as of 2019Q4 constructed with CoreLogic is positively correlated with its counterpart in the Call

Report across banks (the correlation coefficient across banks is 0.85). The CoreLogic-based portfolio tends to underestimate the CRE portfolio size in the Call Report, in part because CoreLogic does not provide comprehensive coverage of real estate transactions and lending across the country. In addition, the binned scatter plot in Figure 1(b) shows that the share of multifamily loans in the CRE portfolio is highly correlated with CoreLogic and the Call Report. These two figures suggest that a bank's CRE loan portfolio based on CoreLogic has characteristics comparable to information in the Call Report.

In Appendix Table A.3, we regress the absolute value of the difference in CRE loans outstanding as measured by CoreLogic and the Call Report on a set of bank characteristics, including the measure of bank exposure that we introduce below for our main results. The R-squared from this regression is tiny (below one percent) and exposure is not related to the difference in CRE loans outstanding between the two datasets. Thus, while there is some measurement error in our CoreLogic-based CRE portfolio, the error appears idiosyncratic across banks. This finding suggests that measurement errors related to our process converting the loan origination record to outstanding loans as of 2019Q4 in CoreLogic are unlikely to be a major source of bias in our main analysis.

3 Empirical strategy and main results

In this section, we estimate the effect of the WFH shock on bank CRE credit supply. We compare banks with different exposures to this shock, using variation across banks in the geographic locations of properties backing pre-shock CRE loan portfolios and variation across regions in the share of employment directly exposed to the WFH shock.

3.1 Measuring bank exposure to WFH shock

We measure bank i's exposure as a weighted average of the county-level share of jobs that can be done remotely, weighted by the bank's pre-shock outstanding CRE loan share in each county (c):

$$Exposure_i = \sum_c \frac{\text{CRE loans outstanding}_{ic}}{\text{total CRE loans outstanding}_i} \times WFHshare_c \tag{1}$$

Intuitively, our exposure metric is a Bartik shock, where the share of jobs that can be done remotely is the "shock" component, and CRE loans are the weights that determine how exposed each bank is to the shock. CRE loans outstanding are as of 2019Q4 based on CoreLogic data.

$WFHshare_c$ is the share of employment in county c that can be done remotely, defined as:

$$WFHshare_c = \frac{\sum_k emp_{kc} \times WFH_k}{\sum_k emp_{kc}}, \tag{2}$$

where emp_{kc} is 2019Q4 employment in industry k in place-of-work county c from the Quarterly Census of Employment and Wages (QCEW).[8] WFH_k is the share of employment in industry k (defined by 3-digit NAICs code) that is teleworkable, as measured in Dingel and Neiman (2020) (DN). DN use surveys (conducted before the pandemic) describing the typical experience of US workers to classify each occupation as able or unable to be done entirely from home.[9] For example, if a majority of respondents to the survey in a given occupation report that they work outdoors every day, then the occupation is coded as cannot be done from home. The industry with the lowest and highest level of WFH_k is "food services and drinking place" (0.0175) and "Securities, Commodity Contracts, and Other Financial Investments and Related Activities" (0.95), respectively.

Figure 2(a) shows substantial variation in $WFHshare_c$ across counties, ranging from below 10% to more than 50%. Figure 2(b) shows that variation in $Exposure_i$ defined in equation 1 is also large across banks, suggesting substantial heterogeneity across banks in

[8] A single firm can have employees working across multiple locations and/or industries. For example, Amazon employees working in Seattle may be in the tech industry but in most other work locations could be in the warehousing industry.

[9] DN also provide a measure of WFH_k that does not use the surveys, but is based on the two authors' manual inspection of occupation descriptions. The two measures are highly correlated and our results are essentially unchanged using this alternative measure.

regions in which banks lend. Row 1 of Table 2 reports summary statistics of Exposure across the nearly 2200 banks in the sample of banks we use for estimation. Appendix Section A.4 describes the sample restrictions we impose for estimation.

We next explore correlations between Exposure and bank characteristics. Table 2 reports summary statistics of several bank characteristics measured from the Call Reports that we use as control variables in our main regressions.[10] Table 3 presents a regression of the z-score of Exposure (i.e. standardized so that it has zero mean and one standard deviation) on those bank characteristics. The regression shows that exposure is negatively correlated with bank size. Banks with less than $ 1 billion in assets have a 0.99 lower standardized exposure (i.e. 99 percent of one standard deviation of unstandardized exposure) compared to the very largest banks, those with over $ 100 billion in assets. More exposed banks also tend to have higher capital ratios; a lower share of owner-occupied CRE properties, which were likely less adversely affected by the WFH shock; and a low share of debt securities relative to total assets, all else equal.

Note that Exposure as defined in equation 1 does not differentiate between property types. Although the WFH shock most directly affected office properties, the WFH shock also likely affects other property types in the same local markets through spillovers or general equilibrium effects. In Section 5.3.1, we provide a detailed discussion on the spillover or general equilibrium effects to non-office properties. This section also considers an alternative measure of exposure that accounts for property-type-specific bank exposures to WFH shock.

3.2 Research design

We estimate the effect of a bank's exposure to the WFH shock on its county-year-level CRE purchase mortgage originations.[11]

[10]Our measure of CRE loans from the Call Report is the sum of the "owner-occupied, non-multifamily CRE", "non-owner-occupied, non-multifamily CRE", and "multifamily" categories.

[11]We focus on purchase loan originations in our main analysis because we cannot control for important drivers of refinance demand. Refinance loan origination volumes are likely to be driven by existing loan characteristics such as loan maturity and the amount of ballon payments at maturity. Unfortunately, the CoreLogic data only provide limited information on loan characteristics as discussed in Section 2.

We use a Poisson regression instead of linear regressions of log of origination volume because there are many bank-county-year observations with zero CRE loan originations, and the log of origination volume would be undefined for these observations. We assume that the count of loan originations at bank i, in market c (county), and year t follows a Poisson distribution with conditional mean

$$\lambda_{i,c,t} = \exp(\beta_t \text{Exposure}_i + X_i\gamma + \delta_{c,t}), \tag{3}$$

where β_t are year dummies interacted with the z-score of Exposure, and are the coefficients of interest. The sample period is from 2016 to 2023, and we normalize β_{2019}, the coefficient associated with the year just before the pandemic starts, to zero. These coefficients capture a percentage change in the number of CRE purchase loan originations in response to a one standard deviation increase in Exposure. Standard errors are clustered by bank.

The bank controls X_i are listed in Table 2 and are measured as of 2019Q4, right before the start of the pandemic. These control for persistent differences in CRE loan originations across banks with different characteristics. For the bank controls CRE loans outstanding and total assets, we include cubic polynomials of these variables as controls. We use the same set of bank controls in each specification described in the remainder of the paper. In Section 5.2, we consider an alternative specification that allows for banks with different characteristics to have differential time trends in CRE loan originations and show that our main estimate remain largely unchanged.

$\delta_{c,t}$ are county-by-year fixed effects, which absorb variation in CRE lending due to location-specific demand or supply factors that are common across banks. For example, heterogeneous trends in CRE demand across counties, possibly due to different degrees of WFH shocks, do not affect the identification of β_t. Instead, the variation pinning down β_t is within-county-year variation in *Exposure*, which comes from variation in the geographic composition of a bank's CRE loan portfolio as well as variation in size of the WFH shock across counties. We interpret the β_t's as reflecting credit supply responses to Exposure, not credit demand effects. We provide additional evidence to support this interpretation in Section 5.[12]

Appendix Section A.6 discusses and reports results for some specifications related to the baseline. We consider the count of refinance originations and the dollar volume of purchase originations as the dependent variable of the regression.

3.3 Effects of WFH exposure on bank CRE lending

Figure 3 presents the estimates of β_t in equation 3 for the number of new bank CRE purchase loan originations. In each of the four years leading up to the pandemic (2016-2019), there is no evidence of any pre-trend in the relationship between Exposure and lending, conditional on our controls. Starting in 2020, the first year of the pandemic, the relationship turns negative and stays negative throughout the post-period. On average across all post-pandemic years, a one standard deviation increase in Exposure is associated with a 7.3 percent decrease in originations (column 1 of Table 5). The point estimates for the post-period vary slightly from year to year, with stronger effects in the high interest rate environment of 2023.

How much did the average bank restrict credit supply in response to the increase in WFH during the pandemic? Our results in Figure 3 only identify relative effects across banks with different levels of DN-based Exposure, so to provide an answer to this question, we must make additional assumptions about how much the WFH shock affected CRE originations by the bank with the lowest level of Exposure across banks in our sample (0.184). If we assume that this bank's CRE originations were unaffected by the WFH shock, then we can calculate the effect for the average bank as $\frac{0.073\times(0.317-0.184)}{0.042} = 23\%$, where 0.073 is the point estimate from column 1 of Table 5, and 0.317 and 0.042 are the average and standard deviation of unstandardized bank exposure to WFH from row one of Table 2. In Appendix Section A.5, we show that the bank with the lowest level of DN-based Exposure had a CRE portfolio-weighted *actual* WFH share that did not change as a result of the pandemic, which supports

[12]Our identification strategy is similar to the one developed in Khwaja and Mian (2008), although they compared banks lending to the same firm, not to the same local market.

the assumption that this bank's CRE originations were unaffected by the WFH shock.

4 Mechanism

Our interpretation of the results in Figure 3 is that more-exposed banks restrict credit supply by more than less-exposed banks. The mechanism we propose is that more-exposed banks experienced a distress in their CRE loan portfolio via an increase in current or expected future losses of their CRE loans, which in turn may incentivize banks to reduce credit risk in their loan portfolio by pulling back on new CRE lending, in particular against riskier CRE properties. CRE loans on bank balance sheets are illiquid and long-term loans, so origination is an important margin through which banks can reduce risk in the portfolio over time. The incentive for de-risking is likely to be larger if CRE loan losses can lead to a larger negative shock to bank capital (Froot and Stein (1998)).

In this section, we perform several analyses to test our proposed mechanism. First, we explore the relationship between bank exposure and several direct measures of distress in banks' CRE portfolios. Second, we look at several dimensions of heterogeneity in the credit supply effects across geography and banks as supporting evidence for the de-risking channel. Third, we test for spillover effects of bank exposure to other, non-CRE types of lending.

4.1 Exposure and loan distress

The WFH shock is more likely to cause distress in bank CRE loan portfolios if CRE valuation declines are large enough to bring property values below outstanding loan balances. Several existing papers find evidence that lenders are exposed to losses as a result of property valuation declines since 2019. Glancy and Kurtzman (2024) find that CRE nonperforming rates have increased since the pandemic, especially for CMBS and loans held by large banks. Using data from the CMBS market, Jiang et al. (forthcoming) estimate that 14% of all CRE loans and 44% of office loans have property values below outstanding debt balances, exposing lenders to losses. In this section, using our Exposure metric, we test whether banks whose loan portfolios are more geographically exposed to the WFH shock experienced larger distress in their loan portfolios.

A natural measure of distress is the CRE delinquency or nonperforming loan rate. We calculate nonperforming CRE loans for each bank as the dollar volume of CRE loans at least 90 days past due or in nonaccrual in 2023Q4 as measured in the Call Report. The Call Report data also include information about loan loss provisions and chargeoffs, which can also be informative about bank distress. Loss provisions are a stock of funds set aside by banks to cover expected future credit losses. When losses are realized (for example, the property is foreclosed upon and sold), the losses are recorded as chargeoffs, and the stock of provisions decreases by the amount of chargeoffs. We measure loss provisions in 2023Q4 and cumulate all "net chargeoffs" (defined as chargeoffs net of recoveries) between 2020Q1 and 2023Q4. Because the Call Report data report CRE-specific loss provisions and chargeoffs only for banks with assets of at least $1 billion, we use loss provisions and chargeoffs as totals for the bank across all types of loan categories (CRE and others). Thus, our measures of loss provisions and net chargeoffs are noisy measures of actual loss provisions and net chargeoffs for CRE loans. We divide all of our measures of distress by the total dollar volume of CRE loans on the bank balance sheet in 2023Q4.[13]

We regress our measures of distress, each of which are measured at the bank level, on bank exposure using the following bank-level regression:

$$Y_i = \beta \text{Exposure}_i + \beta_X X_i + \epsilon_i, \tag{4}$$

where Y_i is one of our distress measures for bank i and X_i are the bank controls discussed above. For the regressions with the nonperforming rate and loss provisions as the dependent variable, we also include controls for the lagged dependent variable, measured in 2019Q4.

Results can be found in Table 4. A one standard deviation increase in Exposure is

[13] Results are similar if we normalize provisions and chargeoffs by total assets instead of CRE loan volume.

associated with a 0.07 p.p. increase in the CRE nonperforming rate (column (1)), which is about 15 percent of the average CRE nonperforming rate of 0.5 percent. Column (2) shows no significant association between exposure and chargeoffs. Column (3) shows a positive effect of exposure on loan provisions with a one standard deviation increase in exposure associated with 1.6 p.p increase in loss provisions (about 25 percent of the average loss provisions), though the estimate is only marginally significant.

Although Table 4 provides some evidence of an effect of exposure on loan distress, the results in Table 4 are unlikely to capture the full extent of distress facing exposed banks. In particular, delinquency rates are often lagging indicators of distress. For example, delinquency may not happen until the loan maturity date, as many CRE loans have balloon payments due at maturity.[14] Loss provisions are more forward-looking measures of distress. However, banks have some discretion in how much to allocate to loan loss provisions, and may have incentives to under-provision (see Crosignani and Prazad (2024), Hughes and Nichols (2025), and Correia et al. (2025)).

4.2 Heterogeneous effects

4.2.1 Effects by county's WFHshare

Under our proposed mechanism, a bank's reduced CRE loan supply is driven by its incentives to de-risk its CRE loan portfolio. Thus, we would expect a larger decline of originations of riskier CRE loans. To test this hypothesis, we estimate a specification that includes an interaction of Exposure with the county's WFHshare, as defined in equation 2. Counties with high levels of WFHshare likely experienced worse declines in CRE fundamentals and are plausibly riskier counties for banks to lend in, especially because real estate prices tend to

[14]In particular, a bank may recognize that a loan is unlikely to pay off in full due to a sharp decline in operating income at the property collateralizing the loan, but delinquency may not happen until the loan maturity date, as CRE loans are long, with low interim amortization payments and balloon payments due at maturity. Furthermore, operating income at the property may not have fallen yet but may be expected to fall in the future: office and retail leases are long (5-10 years), so landlords may still be collecting rents at pre-pandemic rates even if they expect rents on future leases to fall.

adjust slowly to changes in fundamentals. Column 2 in Table 5 presents estimates supporting the hypothesis. The point estimates imply that the marginal effect of exposure on credit supply is 8 percentage points more negative in a county with a one standard deviation higher level of WFHshare (0.07), all else equal. In addition to supporting the bank dererisking hypothesis, this result suggests that the bank credit supply channel is likely to have exacerbated the WFH shock to local markets that were more exposed to the shock.

4.2.2 Effects by bank capitalization

Under our proposed mechanism, a bank with a larger CRE loan portfolio relative to its capital base should reduce credit supply by more in response to our exposure measure in the "Post" period (i.e. years 2020 and after), all else equal. Column 3 of Table 5 provides support for this prediction. We interact Exposure with the size of the banks' CRE assets relative to Tier 1 capital, measured in 2019Q4 (pre-WFH shock). In the Post period, a bank with a high level of CRE assets relative to capital (95th percentile) lowers CRE lending by 8 percentage points more than a bank with the mean level of this ratio.

4.2.3 Effects by bank size

Column 4 of Table 5 shows the effects of Exposure on new CRE lending by bank size. The point estimates are fairly noisy since most banks in our sample are small and we cluster standard errors by bank. However, the point estimates show that the overall negative effect of Exposure on new CRE lending shown in column 1 is driven by banks in our sample with total assets less than $100 billion. For the 11 banks in our sample with over $100 billion in assets (the omitted category in the regression), the effect of Exposure on lending is actually slightly positive, but not statistically significant.

One possible explanation for the null effect for the very largest banks is that these banks are less risk averse because they can more easily cover realized losses from exposed CRE loans due to a low cost of raising external funds (Kashyap and Stein (1995)). In addition, large banks may be too big to fail and thus have a higher expected bailout probability, which may decrease their risk aversion (Naqvi and Pungaliya (2023)). Another possibility is that large banks have exposure to CRE that is not captured by our CoreLogic data. Acharya et al. (2024) show that banks with over $100 billion in assets have sizable indirect exposure to CRE through term and credit lines to REITs. As a result, we may measure exposure with more error for the largest banks, possibly leading to attenuation bias in our estimate of exposure on credit supply for the largest banks.

4.2.4 Spillovers: Effects of Exposure on small business lending and residential real estate lending

If more-exposed banks reduce CRE credit supply to de-risk their balance sheet, then they may also reduce credit supply for other types of risky non-CRE lending. Detailed micro data that can be merged to CoreLogic are available to us for small business lending and residential real estate lending, so we test for spillovers to these loan categories.

Small business lending We examine spillover effects to small business lending using the Community Reinvestment Act data, which provide information on annual small business loan originations at the bank and county level.[15] We estimate bank-county level Poisson regressions similar to equation 3. We report results for two different types of small business loan originations: total origination dollar volume and total origination volume to businesses with revenues less than $1 million.

Table 6 presents the estimates. The first and two columns present estimates for the total origination volume consistent with negative spillovers to small business lending: negative estimates for Exposure in column 1 and for the interaction term with the ratio of CRE assets to tier 1 capital in column 2. However, both estimates are statistically insignificant.

[15]Small business loans are defined as loans with size below $1 million that are reported as either loans secured by non-multifamily CRE properties or C&I loans in the call reports. We find that total non-multifamily CRE loan originations with size less than $1 million (calculated using CoreLogic) is less than 4% of the total small business loan origination amounts from the CRA data. This suggests that most of small business loan originations are C&I loans.

The third and fourth columns present estimates for the total origination volume to the subsample of businesses with revenues under $1 million. These estimates are also consistent with negative spillovers and statistically significant. Based on the estimate in column 3, a one standard deviation increase in Exposure reduces lending by 8.8 percent in the post period (2020 - 2023) relative to the pre period (2016 - 2019). Column 4 shows that in the Post period, a bank with a high level of CRE assets relative to capital (95th percentile) lowers small lending to firms with smaller revenues by 11 percentage points more than a bank with the mean level of this ratio. These more pronounced effects for smaller firms (with lower revenues) are consistent with a perception that lending to smaller businesses is typically considered risky (Federal Reserve Board, 2022). Banks wanting to de-risk their loan portfolio may be more likely to reduce lending to small businesses.

Residential real estate lending Next, we examine spillover effects to residential real estate lending using Home Mortgage Disclosure Act (HMDA) data on loan originations.[16]

We estimate bank-county level Poisson regressions similar to equation 3. We report results separately for loan origination dollar volume likely to be held on the bank balance sheet and loan origination dollar volume likely to be securitized.[17]

Table 7 presents the estimates. The first column shows that for balance sheet originations, a one standard deviation increase in Exposure reduces lending volume by 10.8 percent in the post period (2020 - 2023) relative to the pre period (2016 - 2019). The third column, however, shows a small and insignificant negative relationship between Exposure and lending volume for securitized mortgages in the post period relative to the pre period. Because bank loans originated for securitization typically leave the bank balance sheet soon after origination, securitized loans are associated with relatively low credit risk for the bank compared to loans originated to be held on balance sheet. Therefore, the results in columns 1 and 3 are

[16]We assign id rssd's to each lender in HMDA so that we can merge our bank-level variables we construct from CoreLogic and the Call Reports. Our estimation sample for the HMDA analysis has 1440 unique banks.

[17]We classify conventional loans with size greater than the national conforming loan limit and junior-lien loans as balance sheet loans. We classify all other loan types as securitized.

consistent with spillover effects from CRE WFH exposure to risky residential real estate credit supply.

Columns 2 and 4 report the results when Exposure and the Post dummy are fully interacted with bank's CRE assets-to-tier 1 capital (measured as of 2019q4). Column 2 shows that banks with high Exposure and high levels of CRE assets relative to capital reduce balance sheet lending by more, consistent with the results for CRE lending in Table 5, but the estimate is not statistically significant. For securitized lending volumes in column 4, the estimates remain close to zero.

Finally, looking across all the columns in Tables 6 and 7, there is no evidence of positive effects of Exposure on small business or residential real estate lending. These results help rule out the possibility that that more-exposed banks are derisking their CRE loan portfolio while increasing risk in other parts of their portfolio.

5 Robustness and Additional Specifications

5.1 Separating Credit Supply from Credit Demand

For the main results in Section 3, we present the results of several alternative specifications that together alleviate concerns that our estimates pick up credit demand effects in addition to or instead of credit supply effects.

5.1.1 Leave-one-out measure of Exposure

We re-estimate the Poisson model in equation 3 using a leave-one-out measure of Exposure defined as:

$$Exposure_{ic} = \sum_{j \neq c} \frac{\text{CRE loans outstanding}_{ij}}{\text{total CRE loans outstanding}_{i}} \times WFHshare_{j} \tag{5}$$

where j indexes all counties in the country other than county c, i indexes the bank, and county-level $WFHshare$ is defined as in equation 2. Leaving out county c from Exposure means that Exposure is defined without using any information from county c, making it even less likely that Exposure is correlated with the bank's demand conditions in county c.[18] Comparing Column 1 (baseline) with Column 2 (leave-one-out) of Appendix Table A.4, we see that the estimates are essentially unchanged when using the leave-one-out measure of Exposure.

5.1.2 Within-county variation in CRE demand

Our main estimates includes county-by-year fixed effects. Any within-county variation in CRE demand conditions that is correlated with Exposure could bias our results. For example, banks with higher Exposure may tend to lend within areas of a county that are more likely to experience a negative CRE demand shock during the pandemic (e.g. more urban areas of the county). In addition, banks often specialize in certain types of lending (Paravisini et al., 2023). In our context, within a county, more exposed banks could be office lending specialists, and office demand declined disproportionately after 2020.

We address these concerns with three robustness checks. First, we rerun our main reduced-form specification at the zipcode level; i.e. the location index in equation 3 represents a zipcode instead of a county and we include zipcode-by-year instead of county-by-year fixed effects.[19] Column 3 of Appendix Table A.4 shows that our results are very similar to our main specification at the county-level.

Second, because there still may be heterogeneity in demand conditions within a zipcode, we include an additional bank-county level control variable for the downtown-ness of the bank's CRE portfolio within a county. We interact this control variable with a dummy for the Post period (i.e. years 2020 through 2023). This control variable addresses a potential

[18] The leave-one-out exposure is undefined for banks with CRE loans outstanding in only a single county, and these banks are dropped from our regressions that use leave-one-out exposure.

[19] The QCEW data are only available at the county level, not the zipcode level, and so our data do not allow for a zipcode-based measure of Exposure.

concern that more exposed banks tend to lend in downtown areas, and downtown areas likely experienced a larger negative demand shock during the pandemic. To construct this control variable, we first compute the distance of each CRE property from the central business district (CBD) of its metropolitan area.[20] Then, for each bank-county pair (i, c), we compute the average distance-to-CBD of the CRE properties in i's loan portfolio in c. Column 4 of Appendix Table A.4 presents results with this control variable added and interacted with a dummy for the Post period. The results are very similar to the results from our main specification.

We can also estimate a related specification that controls for bank-county level variables more generally by adding a set of bank-by-county fixed effects. These fixed effects cannot interact with the Post dummy because then the coefficient on Exposure would not be identified. These fixed effects absorb the bank controls and so X_i is dropped from the regression. Column 5 of Appendix Table A.4 shows that the results with these fixed effects added are similar to the results from our main specification.

Third, to address concerns about bank-property-type specialization, we estimate our main Poisson lending model at the bank-county-property type-year level. That is, we estimate

$$\lambda_{i,c,z,t} = \exp(\beta_t \text{Exposure}_i + X_i \gamma + \delta_{c,z,t}) \tag{6}$$

where z indexes the 8 different property types in our sample shown in Table 1. Column 6 of Appendix Table A.4 shows the results with the property-type fixed effects are very similar to the results from our main specification, suggesting that bank-property type specialization is not a main source of bias for our main results.

[20]We do this using the CoreLogic-provided latitude and longitude of the property. For micropolitan statistical areas, we do not have CBD coordinates and we set CBDness equal to missing for properties in such areas. We include a dummy variable equal to one if the CBD variable is missing for the bank-county.

5.2 Controlling for time-varying bank heterogeneity

Although we include bank characteristics in the main regression, our estimate could be biased if banks with different characteristics have differential time trends in CRE loan originations. For example, Table 3 shows that Exposure is lower for small banks and banks with more owner-occupied CRE properties in their portfolio. The CRE loan demand at such banks may have evolved differently from that at other banks even within the same county.

To control for this time-varying heterogeneity across banks, we construct propensity scores for bank WFH exposures based on their observed characteristics and interact the county-by-year fixed effect with each propensity score quintile. Each bank i's propensity score PS_i is the predicted value of the simple bank-level regression of bank WFH exposure on bank characteristics in Table 3. Then we interact the county-by-year fixed effect ($\delta_{c,t}$) in equation (3) with each quintile of PS_i, allowing for separate time trends for banks with different exposures that result from differences in their observed characteristics. This richer fixed effect controls for the possibility that local credit demand or supply might have evolved differently at banks with certain bank characteristics that are correlated with bank WFH exposure.

Column 7 in Table A.4 presents the estimate with these propensity score controls. We find that the estimate remains very similar to the baseline estimate, suggesting that this time-varying heterogeneity across banks with different exposures does not affect the estimated WFH effects.

5.3 Measurement error in Exposure

In this section, we explore two potential sources of measurement error in our Exposure measure and conclude that they are unlikely to be a large source of attenuation bias for our main results.

5.3.1 Exposure by property type

In our main specification, a bank's Exposure accounts for the geographic heterogeneity of the properties in the bank's loan portfolio, but not heterogeneity in property type conditional on the location of those properties. This may add measurement error if a bank's true Exposure depends also on the property-type mix of its loan portfolio, causing us to understate the effects of Exposure on credit supply.

To examine the importance of property-type heterogeneity, we consider property-type (p) specific exposure for bank i:

$$Exposure_{ip} = \sum_{c} \frac{\text{CRE loans outstanding}_{ipc}}{\text{total CRE loans outstanding}_{i}} \times WFHshare_{c} \tag{7}$$

where the sum of exposure across property types, $\sum_p Exposure_{ip}$, equals our baseline measure of Exposure defined in equation 1.

We estimate the following Poisson regression with the Exposure measures for different properties:

$$\lambda_{i,c,t} = \exp(\sum_{p}(\beta_{0,p}\text{Exposure}_{ip} + \beta_{1,p}\text{Exposure}_{ip} \times Post_t) + \gamma X_i + \delta_{c,t}) \tag{8}$$

where $Post_t$ is an indicator variable for the post-pandemic 2020-2023. $\beta_{1,p}$ estimates the effects of WFH exposure by property type on lending.[21] We standardize Exposure for each property type so that the mean and standard deviation of $\sum_p Exposure_{ip}$ across observations is zero and one, respectively.

Figure 4 presents the estimates of β_p for each property type. Exposure to office, multifamily, and "other" property types have relatively stronger effects on lending. However, overall the point estimates across property types are fairly similar to each other and exposure

[21]Even if the estimates of $\beta_{1,p}$ are similar across property types, it does not mean that bank exposure is invariant to the property mix in their loan portfolio. Property types vary in their propensity to be located in high WFH areas. For example, we find that across counties, the correlation between the share of outstanding CRE loans that are office properties and the WFHshare in that county is positive.

to every property type is estimated to have a negative effect on lending (though the negative effect is not always statistically significant). The results in Figure 4 suggest that heterogeneous effects of exposure by property type are not a significant source of measurement error for our main results.

One reason the estimated office effect is not more negative relative to the other property type effects in Figure 4 is that the WFH shock likely has negative effects on other property types through office spillover or more general equilibrium effects. In Appendix Section A.8, we show a strong negative correlation at the metro area level between Exposure to the WFH shock and the change in property value between 2019Q4 and 2023Q4 for office properties but also for multifamily, retail, and industrial properties (Figure A.4 and Table A.5). Appendix Table A.6 also shows that across counties, high rates of WFH are associated with lower net migration, consistent with the findings in Brueckner et al. (2023). This fact could help explain why WFH adversely affects non-office properties, even industrial ones, that do not directly depend on office workers. Lower population levels due to WFH result in lower economic activity and potentially lower demand for commercial properties (Haslag and Weagley (2024)). In addition, fewer office workers in a given location could lead to lower retail, hotel, and multifamily demand because of spillover effects such as reduced foot-traffic, business travel, or the desire to live close to the office to lower commute time.[22] Multifamily properties in high WFH areas may have also been adversely affected by a shift in demand toward larger housing units (e.g. single-family homes) for households to make space for working from home (Mondragon and Wieland (2025)).

Another reason for these relatively similar estimates across property types is that many properties have businesses that use their space for different purposes, although CoreLogic assigned a single property type. By matching a subset of CoreLogic properties to information about businesses occupying those properties from the National Establishment Time-Series (NETS) database, our analysis in Appendix A.9 (Table A.7) shows that 20% of businesses

[22]See the evidence in Van Nieuwerburgh (2023) and DeFusco et al. (2023).

occupying a property classified as industrial by CoreLogic and 40% of businesses in properties classified as unspecified, commercial by CoreLogic are likely to use their space as an office. Moreover, 25% of businesses in a property classified as office by CoreLogic are likely to use their space as an industrial property. Thus, to the extent that some space in non-office commercial properties is used as an office, we expect to find similar effects of WFH exposure on credit supply by property type. Consistent with this finding, survey evidence from Barrero et al. (2021) shows that a sizable share of workers at non-office facilities also work from home, with the WFH rate at office, retail, and factory/warehouse facilities of 45, 22, and 15 percent as of August 2025, respectively.

5.3.2 Alternative measure of the WFH shock

As discussed in Section 3, our Exposure measure is based on pre-pandemic data and does not use any information on whether workers in a particular industry actually worked from home during the pandemic. Although using pre-pandemic data helps support the exogeneity of our exposure metric, it may also add to measurement error in our measure of exposure.

As an alternative to our DN-based measure of Exposure, we use estimates on actual WFH from the Current Population Survey (CPS) using post-pandemic data. Appendix Section A.7 describes our WFH estimates using the CPS, which have a correlation of 0.7 with the DN-based estimates. Column 8 of Appendix Table A.4 reproduces our main results using the CPS-based measure of WFH, which are very similar to our main results.

6 Effects on aggregate CRE lending

In this section, we examine whether the reduction in CRE credit supply by exposed banks was associated with a decrease in credit supply at the aggregate, market level. It is ex ante unclear whether banks' exposure to the WFH shock would lead to a decline in the aggregate credit supply because nonbanks are significant CRE lenders, and CRE loan borrowers might have substituted from banks to nonbanks when banks began to reduce their CRE lending.[23] However, there is important intermediary segmentation in CRE lending (Glancy et al. (2022)), which may act as a barrier to substitution across lender types.

Our CoreLogic data allows us to observe all CRE lending, from both banks and nonbanks such as insurance companies, mortgage REITs, pension funds, CMBS lenders, or others.[24] We use this feature of the data to test for aggregate effects using the cross-sectional regression

$$\frac{\text{Post-pandemic originations}_j - \text{Pre-pandemic originations}_j}{.5 \times (\text{Post-pandemic originations}_j + \text{Pre-pandemic originations}_j)} = \beta \text{BankExposure}_j + X_j \gamma + \epsilon_j \tag{9}$$

where j indexes the market, defined as a zipcode for these results. The dependent variable is zipcode-level centered growth rate in total CRE originations (bank + nonbank) in the post-pandemic period (2020-2023) relative to pre-pandemic period (2016-2019). We use the centered growth rate rather than a standard growth rate because it is bounded between -2 and 2, and so reduces the influence of outliers. We restrict the sample to zipcodes that have at least one origination in both the pre and post pandemic period. BankExposure for each zipcode is defined as

$$\text{BankExposure}_j = \frac{\sum_i \text{Exposure}_i \times \text{Pre-pandemic originations}_{i,j}}{\sum_i \text{Pre-pandemic originations}_{i,j}} \tag{10}$$

where Exposure is defined as in equation 1 and i indexes banks. BankExposure_j is the weighted-average exposure among banks that originate in zipcode j in the pre-pandemic period. We define Exposure this way because bank-zipcode relationships tend to be persistent so that the identities of banks lending in the pre-pandemic period is relevant for determining credit supply in the post-pandemic period. The mean and standard deviation of BankExposure across zipcodes are 0.33 and 0.03, respectively; Figure 5 shows the full distribution of

[23]Nonbanks had a 40 percent market share of new CRE originations in the years right before and after the pandemic in our data, suggesting that they may be viable substitutes for banks.

[24]We do not, however, measure the exposure of these nonbanks to the WFH shock in the way we do for banks.

BankExposure across zipcodes. There is wide variation in BankExposure even after controlling for the actual WFH share in 2023 in each zipcode, suggesting that BankExposure is not simply driven by the size of WFH shock to local markets. If banks that are more exposed to the WFH shock reduce credit supply and that reduced supply cannot be offset by less exposed banks or nonbanks, then we would expect $\beta < 0$.

For this specification, we define a market as a zipcode rather than a county as in Section 3.3. Observations in this specification are at the market level, not the bank-market level. We use a smaller definition of a market to increase sample size and provide precise estimates even once we include a large set of variables in X to control for differential demand conditions across markets (including state fixed effects).

X includes controls for other zipcode-level variables that may be correlated with the demand for CRE credit in the zipcode. For example, we include the actual WFH share in 2023 and the share of population in the zipcode living in an urban area.[25] We also include the bank market share of new originations in the pre-pandemic period for each zipcode, calculated using the CoreLogic data. The full set of controls are listed in Table 8. Because the unit of observation is a market, we cannot flexibly control for changes in loan demand in a market by adding market fixed effects as we did in the bank-level regression in Equation 3. However, our control variables address some important threats to identification.

Table 8 reports the results. We estimate that bank exposure to the WFH shock induced by the pandemic is associated with a 13 percentage point decrease in the growth rate of total new CRE originations. We estimate this as -0.911*(0.330 - 0.184), where -0.911 is the coefficient on BankExposure in column 1, 0.33 is the average value of BankExposure across zipcodes, and 0.184 is the minimum level of the DN-based Exposure across banks in our sample. As in Section 3.3, we assume that CRE originations by the bank with DN-based Exposure of 0.184 were unaffected by the WFH shock for the calculation of the BankExposure

[25] These control variables come from the ACS. Using the 5-year 2023 survey, we calculate the actual WFH share using the share of workers aged 16 and over with at least $10,000 in earnings that reported "worked at home" as their usual mode of transportation to work.

effect on the average zipcode.[26]

In columns 2 and 3, we decompose the effect of local bank exposure on aggregate credit supply into two parts: the effect of bank exposure on CRE originations by banks and the effect of bank exposure on CRE originations by nonbanks. Column 2 uses the growth rate in bank CRE originations in each zipcode as the dependent variable (instead of the growth rate of all zipcode CRE originations). We estimate the average zipcode experienced a 29 percentage point decrease in the growth rate of new bank originations due to the BankExposure effect (-1.999*(0.33 - 0.184)). The average zipcode experienced 13 percentage points of bank lending growth (bottom panel of Table 8). Therefore, the BankExposure effect decreased the level of bank originations by about 16 percent (0.13 - 0.29). This estimate is comparable to, but a little closer to zero than, the 23 percent decline implied by the bank-level estimates described in Section 3.3.

Our finding that bank exposure has a larger negative effect on bank origination growth (column 2) than on total origination growth (column 1) suggests the pull back in credit supply from banks was larger than for nonbanks in high bank-exposure counties. This result is shown directly in column 3, where we redefine the dependent variable to be the growth rate in nonbank originations. Bank exposure is positively associated with nonbank lending growth, suggestive of some substitution from exposed banks to nonbanks. However, the estimate in column 1 for total loan growth is still negative and large, suggesting that this substitution was somewhat incomplete. Overall, we find the aggregate credit crunch is driven by banks and while nonbank lenders increase the pace of origination growth to fill this gap, they do not fully offset the reduction in CRE lending by banks.

Columns 4-6 show the estimates with a set of state fixed effects included. The standard errors get larger, but there is still evidence of large aggregate effects even using within state variation, and notwithstanding some substitution from exposed banks to nonbanks.

[26]As mentioned earlier in Section 3.3, we present evidence that an exposure of 0.184 is associated with no change in the actual WFH share post relative to pre-pandemic in Appendix Section A.5.

7 Conclusion

We show that the WFH shock had large, negative effects on bank CRE credit supply. Our results are consistent with qualitative evidence of bank tightening over the pandemic period from the Senior Loan Officer Opinion Survey on Bank Lending Practices (SLOOS).[27] Our estimation approach allows us to quantify the magnitude of the tightening.

In addition, we find that the WFH shock had negative spillover effects on other risky bank lending and tighter bank CRE credit supply led to large, negative aggregate credit supply effects, accounting for possible substitution to nonbanks. These results suggest that the effects of the WFH shock on bank credit supply likely had important negative effects on the real economy, though showing this directly remains an important question for future research.

Our results in this paper use data through 2023, but tight CRE credit supply from WFH is likely to persist beyond our sample period. Although WFH rates have declined from their pandemic peaks, survey evidence from Barrero et al. (2021) shows that they have stabilized well above pre-pandemic levels. Higher rates of WFH have likely continued to affect CRE credit supply, particularly as banks' CRE portfolios are made up predominantly of loans originated at pre-pandemic valuations.

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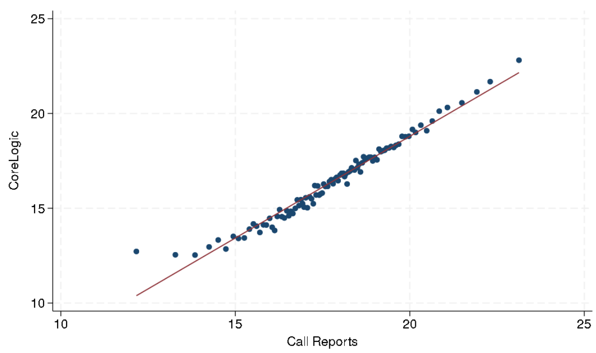
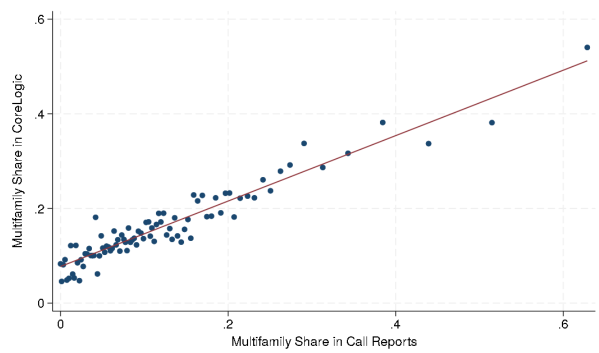
Figure 1: Benchmarking CoreLogic against the Call Report



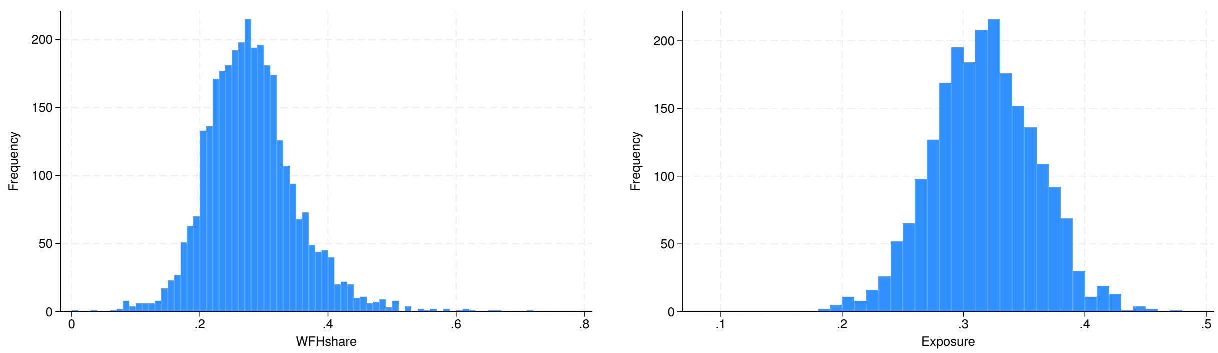
(a) Log of CRE loans outstanding as of 2019Q4



(b) Ratio of multifamily loans outstanding to CRE loans outstanding as of 2019Q4

Note: Shows binned scatter plot of bank-level relationship. CRE loans outstanding includes both multifamily and non-multifamily loans.

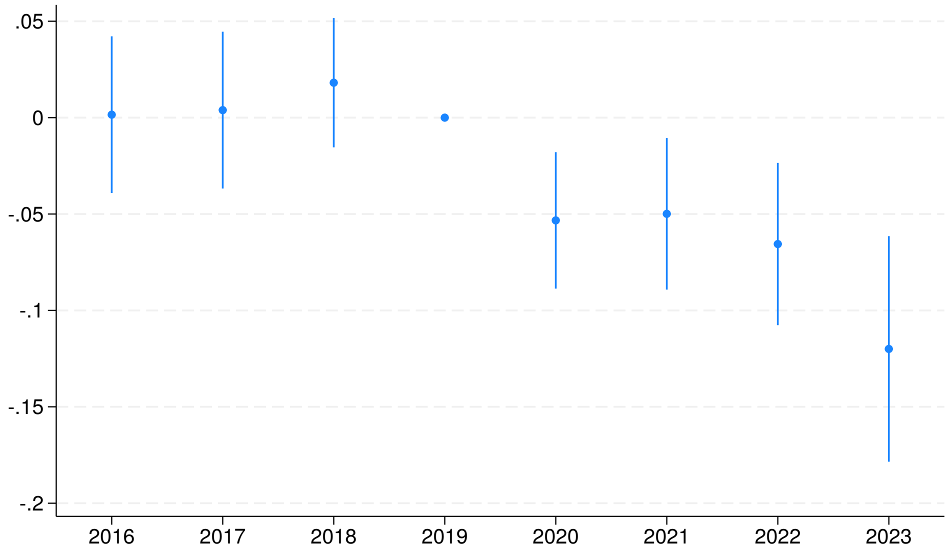
Figure 2: Heterogeneity in WFHshare and Exposure



(a) Variation in $WFHshare_c$ across counties (b) Variation in $Exposure_i$ across banks

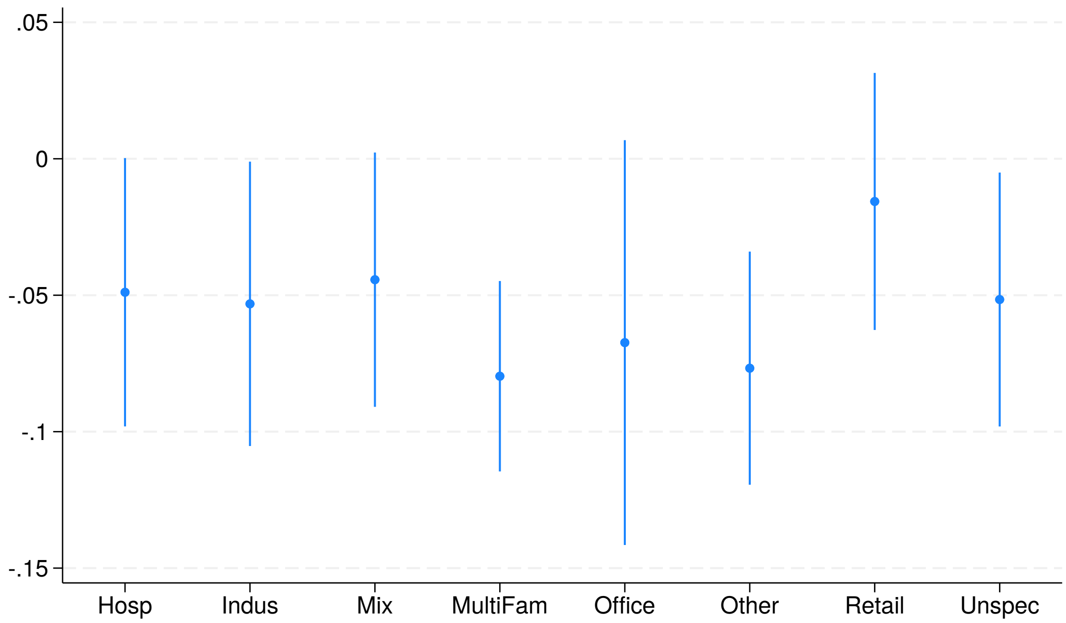
Note: WFHshare is the share of employment in a county that can be done remotely, as estimated using data from the QCEW and Dingel and Neiman (2020). A bank's Exposure is a weighted average of WFHshare in each county in which the bank has outstanding CRE loans. The weight for each county equals the share of the bank's outstanding CRE portfolio backed by properties in that county computed from the CoreLogic data, all measured in 2019Q4.

Figure 3: Effects of Exposure on bank CRE purchase loan originations



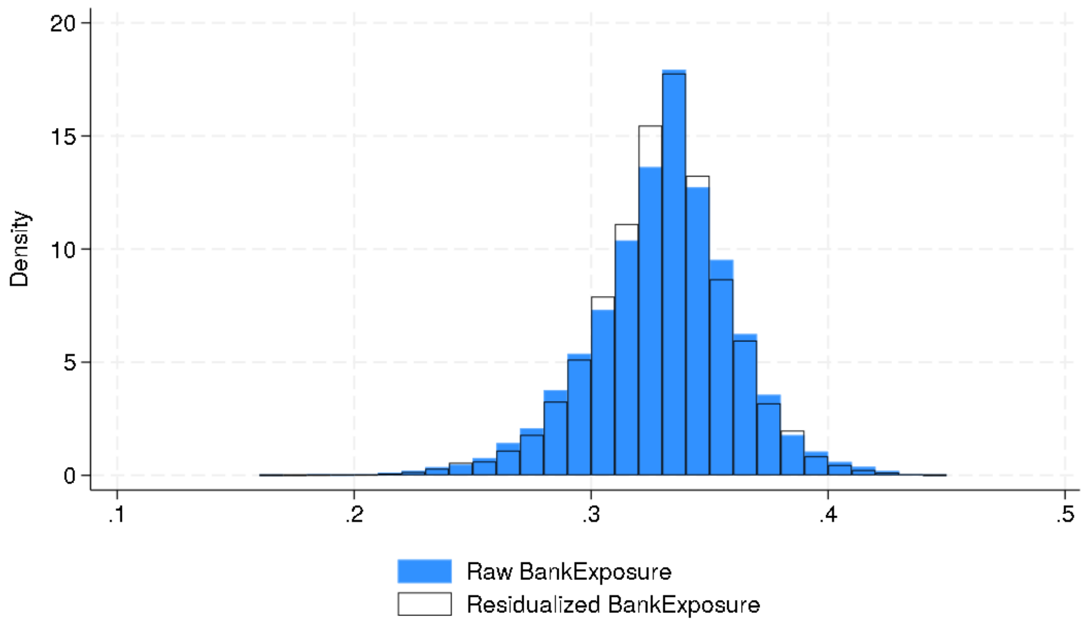
Note: This figure presents the estimates and the standard errors of β_t from the Poisson model in Equation 3. Exposure is standardized to have mean of zero and standard deviation of one so these estimates show a percentage change in the number of CRE loan originations in response to a one standard deviation increase in Exposure. The standard errors are clustered by bank.

Figure 4: Effects of Exposure by property type on bank CRE purchase loan originations



Note: This figure presents the estimates and the standard errors of $\beta_{1,p}$ from the Poisson model in Equation 8 for each property type p. The standard errors are clustered by bank.

Figure 5: Distribution of Bank Exposure Across Zipcodes



Note: This figure presents the distribution of zipcode-level BankExposure, as defined in equation 10. "Residualized BankExposure" refers to BankExposure residualized by the actual zipcode-level WFH share in 2023.

Table 1: Loan-Level Summary Statistics

	Mean	SD	p5	p50	p95	Count
Original Loan Balance (millions)	2.04	4.67	0.06	0.55	8.87	618,837
Mortgage Term (years)	13.50	9.36	3.00	10.00	30.00	250,376
Mortgage Rate (%)	4.77	1.61	2.62	4.60	7.50	29,570
Property Type:						
Office	0.10	0.30	0.00	0.00	1.00	641,290
Multifamily	0.17	0.37	0.00	0.00	1.00	641,290
Industrial	0.13	0.34	0.00	0.00	1.00	641,290
Retail	0.17	0.37	0.00	0.00	1.00	641,290
Hotel	0.02	0.13	0.00	0.00	0.00	641,290
Mixed Use	0.10	0.31	0.00	0.00	1.00	641,290
Other Types	0.15	0.36	0.00	0.00	1.00	641,290
Types Unspecified	0.16	0.37	0.00	0.00	1.00	641,290
Loan Type:						
Purchase	0.30	0.46	0.00	0.00	1.00	641,290
Refi	0.25	0.43	0.00	0.00	1.00	641,290
Junior-Lien	0.38	0.48	0.00	0.00	1.00	641,290
Unknown	0.08	0.27	0.00	0.00	1.00	641,290
Number of Observations	641,290					

Note: Characteristics of CRE loans originated by banks in the period from 2016 to 2023. The sample exclude loans originated by banks that are active in CMBS securitization. Source: CoreLogic.

Table 2: Bank-Level Summary Statistics

	mean	sd	p5	p50	p95	count
Exposure	0.32	0.04	0.25	0.32	0.39	2197
CRE loans outstanding (billions of dollars)	0.47	3.28	0.00	0.04	1.32	2197
Total assets (billions of dollars)	3.13	19.35	0.09	0.45	8.44	2197
CRE assets share of Tier 1 capital	2.39	1.36	0.41	2.26	4.89	2197
Owner-occupied share of CRE assets	0.48	0.21	0.12	0.47	0.87	2197
Tier 1 capital ratio	0.16	0.06	0.11	0.14	0.27	2197
Debt securities share of total assets	0.16	0.12	0.01	0.14	0.39	2197
Number of counties with CRE loans	25.53	55.10	2.00	11.00	95.00	2197

Note: Bank characteristics measured as of 2019Q4 using Call Reports and CoreLogic data.

Table 3: Exposure and Bank Characteristics

	(1) Exposure Coef.	SE
10 - 100 billion dollars in assets	-0.614*	(0.321)
1 - 10 billions dollars in assets	-1.022***	(0.347)
$<$ 1 billion dollars in assets	-1.401***	(0.353)
CRE loans outstanding (billions of dollars)	0.003	(0.008)
CRE assets share of Tier 1 capital	0.189***	(0.017)
Owner-occupied share of CRE assets	-0.697***	(0.096)
Tier 1 capital ratio	2.631***	(0.353)
Number of counties with CRE loans	-0.000	(0.001)
Debt securities share of total assets	-1.280***	(0.179)
R-squared	0.188	
N	2,197	

Note: Omitted group is banks with over 100 billion dollars in assets. Bank characteristics measured as of 2019Q4. Exposure is standardized to have zero mean and standard deviation equal to one.

Table 4: Effects of Exposure on Measures of Distress

	(1) Nonperf. Rate	(2) Chargeoffs	(3) Provisions
Exposure	0.0741**	-0.463	1.632*
	(0.0350)	(0.740)	(0.990)
Bank Controls	✓	✓	✓
Dep. Variable Mean	.497	4.405	6.79
R-squared	0.0522	0.0551	0.291
N	1942	1942	1942

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Nonperforming CRE loan rate is the dollar volume of CRE loans at least 90 days past due or in nonaccrual in 2023 Q4 divided by total CRE loans in 2023 Q4. Net chargeoffs is cumulative net chargeoffs (gross chargeoffs net of recoveries) across all types of loans between 2020 Q1 and 2023 Q4, normalized by the bank's CRE assets in 2019Q4. Provisions is loan loss provisions across all types of loans as of 2023 Q4, normalized by the bank's CRE assets in 2023Q4. All three dependent variables are multiplied by 100 so they are expressed in percentage terms. In addition to bank controls, the regressions shown in columns 1 and 3 include a control for the lagged dependent variable (measured in 2019Q4). Standard errors are heteroskedasticity robust.

Table 5: Effects of Exposure on bank CRE purchase loan originations, various interactions

	(1) Loan Orig	(2) Loan Orig	(3) Loan Orig	(4) Loan Orig
Post x Exposure	-0.0734***	0.302***	0.00759	0.0739
	(0.0171)	(0.0678)	(0.0326)	(0.0554)
Post x Exposure x WFHshare		-1.115***		
		(0.201)		
Post x Exposure x CRE assets / Tier 1 Capital			-0.0318***	
			(0.0116)	
Post x Exposure x 10 - 100 billion in assets				-0.150
				(0.110)
Post x Exposure x 1 - 10 billion in assets				-0.160***
				(0.0599)
Post x Exposure x $<$ 1 billion in assets				-0.0994*
				(0.0562)
Bank Controls	✓	✓	✓	✓
Post-by-County Fixed Effects	✓	✓	✓	✓
N	360,984	360,984	360,984	360,984

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Poisson regression estimates. Post is a dummy variable equal to one if the origination year is 2020 or later. Each regression includes the complete set of interactions between the variables shown in the table (i.e. all main effects and two-way interactions). Standard errors are clustered by bank.

Table 6: Spillover effects to small business loan originations

	Total originations		Total originations to business with revenue $\leq$ \$1 mil	
	(1)	(2)	(3)	(4)
Post x Exposure	-0.0440	0.0176	-0.0876**	0.0695
	(0.0394)	(0.0962)	(0.0393)	(0.100)
Post x Exposure x CRE assets / Tier 1 Capital		-0.0293		-0.0622**
		(0.0336)		(0.0305)
Bank Controls	✓	✓	✓	✓
Post-by-County Fixed Effects	✓	✓	✓	✓
N	1,144,497	1,144,497	1,141,182	1,141,182

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Poisson regression estimates where observations are at the bank-county level. Data on small business loans is from the Community Reinvestment Act data for the years 2016-2023, with "Post" denoting the years 2020-2023. Each regression includes the complete set of interactions between the variables shown in the table (i.e. all main effects and two-way interactions). Standard errors are clustered by bank.

Table 7: Spillover effects to residential real estate originations

	Balance Sheet		Securitized	
	(1)	(2)	(3)	(4)
Post x Exposure	-0.1081***	-0.0366	-0.0285	-0.0332
	(0.0324)	(0.0734)	(0.0268)	(0.0614)
Post x Exposure x CRE assets / Tier 1 Capital		-0.0308		0.0017
		(0.0267)		(0.0255)
Bank Controls	✓	✓	✓	✓
Post-by-County Fixed Effects	✓	✓	✓	✓
Observations	1,684,566	1,684,566	1,711,665	1,711,665

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Poisson regression estimates where observations are at the bank-county level. Data on residential real estate originations is from HMDA for the years 2016-2023, with "Post" denoting the years 2020-2023. We classify conventional loans with size greater than the national conforming loan limit and junior-lien loans as balance sheet loans. We classify all other loan types as securitized. Each regression includes the complete set of interactions between the variables shown in the table (i.e. all main effects and two-way interactions). Standard errors are clustered by bank.

Table 8: Growth in zipcode-level CRE purchase originations

	(1) Total Orig.	(2) Bank Orig.	(3) NonBank Orig.	(4) Total Orig.	(5) Bank Orig.	(6) NonBank Orig.
BankExposure	-0.911***	-1.999***	0.515**	-0.733*	-1.450***	0.115
	(0.212)	(0.237)	(0.250)	(0.374)	(0.372)	(0.523)
Bank Orig. Share	-0.302***	-3.210***	0.725***	-0.295*	-3.559***	0.819***
	(0.100)	(0.110)	(0.149)	(0.152)	(0.177)	(0.220)
WFH share	0.0338	-0.702***	0.475***	0.223	-0.219	0.362**
	(0.124)	(0.142)	(0.127)	(0.133)	(0.155)	(0.156)
Log(median income)	-0.205***	-0.0232	-0.225***	-0.173***	0.0231	-0.196***
	(0.0223)	(0.0246)	(0.0253)	(0.0343)	(0.0371)	(0.0415)
Urban share	-0.0442*	-0.0629**	-0.0838***	-0.0177	-0.0322	-0.0696
	(0.0227)	(0.0258)	(0.0276)	(0.0402)	(0.0534)	(0.0450)
College share	0.158**	0.384***	-0.102	0.0474	0.188**	-0.138
	(0.0646)	(0.0734)	(0.0723)	(0.0944)	(0.0817)	(0.111)
Log(population)	0.0721***	-0.00609	0.00807	0.0844***	0.0171	0.00602
	(0.00733)	(0.00848)	(0.00877)	(0.0150)	(0.0131)	(0.0198)
Change in Log(population)	0.0830*	-0.00470	0.0341	0.0709	-0.0152	0.00528
	(0.0452)	(0.0521)	(0.0577)	(0.0688)	(0.0785)	(0.0822)
Office share	0.122**	0.182***	0.0467	0.0766	0.174**	-0.0223
	(0.0499)	(0.0580)	(0.0591)	(0.0494)	(0.0661)	(0.0698)
Bank Orig. Share squared	0.769***	2.221***	0.769***	0.788***	2.423***	0.879***
	(0.0857)	(0.0949)	(0.157)	(0.120)	(0.146)	(0.226)
Dep. variable mean	.08	.13	.01	.08	.13	.01
State Fixed Effects				✓	✓	✓
R-squared	0.0721	0.115	0.170	0.107	0.170	0.206
N	11570	10399	9488	11570	10399	9488

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Origination growth is the centered growth rate of total purchase originations in 2020-2023 relative to total purchase originations in 2016-2019 for each zipcode. All control variables are measured at the zipcode level and are based on pre-pandemic data, except for the WFH share, which is measured in 2023. Change in population is 2023 relative to 2019. Bank origination share is the share of total pre-pandemic originations that are bank originations, estimated using the CoreLogic data. Office share is the share of pre-pandemic originations that are associated with office properties, estimated using the CoreLogic data. Standard errors are heteroskedasticity robust.

A.1 Details on the match between CoreLogic and bank regulatory filings

We match CoreLogic lender names to RSSD IDs using the following algorithm:

1. **Name cleaning**: we clean the names of CoreLogic lenders and the names of institutions with RSSD IDs. This involves removing punctuation, removing auxiliary words like "national association", "NA", "INC", etc. We also replace common words with their abbreviations, for example, replacing "bank" with "bk" and replacing "California" with "CA".

   One of the most common punctuation marks used in CoreLogic lender names is "/ ", which seems to have two different meanings depending on context. In some cases, "/" seems to refer branch locations of a bank. For example, names such as "Wells Fargo BK/AZ" and "Wells Fargo BK/TX" seem to point to particular Wells Fargo branch locations in Arizona and Texas. In other cases, "/" seems to be used in place of "of" in names such as "First State BK/ Gainesville". Because it is difficult to know what "/" means without investigating each specific case, we initially replace "/" with "of" in all cases at the cleaning stage. Although this change is not correct for all names containing "/", we make sure to assign correct RSSD IDs for examples like "Wells Fargo/AZ".

2. **Exact name match**: we assign RSSD IDs to CoreLogic lenders by finding CoreLogic-RSSD pairs that have the exactly identical clean names in both data sets.

3. **One name includes the other as a substring**: among CoreLogic lenders that fail to match exactly to any RSSD names, we find potential RSSD IDs for these CoreLogic lenders by finding CoreLogic-RSSD pairs such that the shorter name of a pair is included in the longer name of the pair. For example, the RSSD ID of "Wells Fargo BK" will be matched to CoreLogic lender "Wells Fargo BK/TX" in this step.

4. **Identifying duplicate matches**: there are many legally distinct banks with different RSSD IDs, but with the exactly same names like "Centennial BK" or nearly identical names like "Centennial Bank and Trust". Thus, we identify cases where a single CoreLogic lender name is matched to multiple RSSD IDs and address this issue at later steps.

5. **Use lender location information**: CoreLogic provides information on lender addresses, and this field is populated for roughly half of observations. Whenever a CoreLogic lender name is matched to multiple RSSD IDs and has its location information, we choose the RSSD ID with at least one branch at the same zipcode (or the same city if the zipcode information is missing). We use the Summary of Deposits (SOD) for the information on bank branch locations.

6. **Use property location information**: Using matched pairs from the previous steps, we can identify which RSSD IDs typically lend against properties in certain locations (zipcode, city, or state). This allows us to drop false matches when multiple RSSD IDs are matched to the same Corelogic name, and ensure that the loan is assigned to the most likely RSSD ID. For example, we identify that Centennial BK's lending against properties in Oregon is exclusively done by only RSSD ID 1029071 although there are multiple RSSD IDs with names identical or almost identical to Centennial BK. Then, whenever we see Centennial BK's lending against other properties in Oregon with the lender address missing, we assign 1029071 as that loan's RSSD ID.

After these steps, we match about 91% of bank CRE loans in CoreLogic to unique RSSD IDs in term of the number of loans. 70% of bank CRE loans are matched with the exact name match (step 2); 11% are matched using lender addresses (step 5); 8.7% are matched using property addresses (step 6); and less than 1% of loans are uniquely matched using the shorter name match (step 3).

In terms of loan amount, the overall match rate is about 99 percent among bank CRE loans, and 94 percent of bank CRE loan amounts are matched with the exact name match (step 2).

A.2 Algorithm to compute CRE loans outstanding in 2019Q4 from CoreLogic

Our first step is to filter out loans which are likely no longer on banks' books at the end of 2019. This is difficult for two main reasons. First, refinancing prior to maturity seems quite common, so relying on the loan's maturity date to filter out old loans does not remove all loans which are no longer on banks' books. Second, many commercial properties have both first and second lien mortgages which we can observe in the data. This makes it hard to determine whether a new loan on the same property is a refinancing of a preexisting mortgage or adds an additional lien. We keep only loans which have a maturity date after 2019Q4 and which are the last mortgage originated with its own seniority level. For example, if a property has a senior and junior lien outstanding in 2017Q4 and then we observe a new senior mortgage in 2018Q1, we drop the original senior mortgage.

Next, we compute the amortization schedule of each loan to determine the amount of principal outstanding in 2019Q4. For fixed-rate mortgages, we apply the standard amortization formula to compute quarterly mortgage payments, and then break out the interest and principal components of that payment. Since the mortgage rate is often missing in CoreLogic, we impute the average mortgage rate for other fixed-rate mortgages originated in the same quarter. Moreover, we replace missing maturity information with the most common maturity among loans with the same property type.[1] All the data fields required to calculate exact quarterly payments and amortization schedules for adjustable-rate mortgages (the length of the locked-rate period, the frequency of rate adjustments, minimum and maximum adjustment intervals, and minimum and maximum rates) are not usually available. Instead, we approximate the amortization schedule of adjustable-rate loans with a fixed-rate amortization schedule based on the starting mortgage rate.

Finally, we account for lender exit, whether through bankruptcy or merger. If the lender exits at any point between loan origination and 2019Q4, we reassign the loan to the acquiring firm. If there are multiple acquiring firms (as sometimes occurs after a bank failure), we assign each loan to each acquirer proportionally to the number of acquirers.

A.3 Comparing CoreLogic and RCA

One of our contributions is to shed light on CRE lending activities by smaller banks using the CoreLogic data, in contrast to other CRE data sets covering only a certain segment of the CRE lending market. CoreLogic is the natural data source for our analysis, since we are interested in measuring small bank exposure to commercial real estate in different geographic areas. This requires data with sufficient coverage of both small banks and small loans, which other datasets lack.

In particular, the CRE loan origination data set from Real Capital Analytics (RCA) has been used by many previous papers studying CRE (Büchler et al., 2024; Ghent and Valkanov, 2016; Glancy et al., 2022). However, the RCA dataset only covers CRE properties with valuations of at least 2.5 million dollars. Figure A.1 shows that RCA covers roughly

[1] For most property types, this is a fairly long loan term of 10-30 years. However, our measure of outstanding balances is not terribly affected by imputing a loan term which is too long for two main reasons. First, most commercial mortgages have amortization periods which are longer than the term of the loan itself. Though data on amortization periods is not always available, industry professionals usually cite a standard amortization period of 30 years. This is exactly the loan term we impute for most property types. Second, quarter-to-quarter changes in the dollar value of loans outstanding are second-order in magnitude compared to adding and removing loans from the balance sheet in their entirety. Most properties either refinance or sell well within the imputed term of the loan, and we observe both types of transactions in CoreLogic.

half of CRE lending by banks in CoreLogic. For this calculation, we exclude banks that are heavily involved in CMBS loan originations (those with at least 10% of their originations for CMBS in the RCA data) to focus on the difference in data coverage on loans funded by bank balance sheets. Because the geographical coverage of CoreLogic has expanded over time, we also restricted the RCA sample to county-years that are also covered by CoreLogic.

A comparison between the CoreLogic and the Real Capital Analytics datasets sheds light on how much smaller banks' activities are missing in non-comprehensive data sets. In recent years, the aggregate lending amount against CRE properties by banks in RCA is roughly a little more than half of that in CoreLogic. Although smaller-sized CRE loans (especially below $2 million) account for the vast majority of CRE lending in terms of number of loans in CoreLogic, many of these loans are missing in RCA because it covers relatively large transactions.

Figure A.1(a) shows that aggregate bank lending volume in RCA is about 55% to 90% of that in CoreLogic depending on the years. The average ratio of the RCA volume to the CoreLogic volume between 2015 and 2023 is about 71%. The ratio is even smaller in terms of loan count (Figure A.1(b)), suggesting that RCA's the minimum transaction size threshold limits its coverage of smaller loans.

Figure A.2 shows more directly that many small loans, especially below $2 million, are missing in RCA, but not in CoreLogic. These relatively small loans are much more likely to be originated by smaller banks. Banks with total assets below $1 billion account for 32% of loans below $2 million and 18% of loans above $2 million in 2022. In contrast, large banks with more than $10 billion in their assets account for 31% of loans below $2 million and 45% of loans above $2 million in 2022.

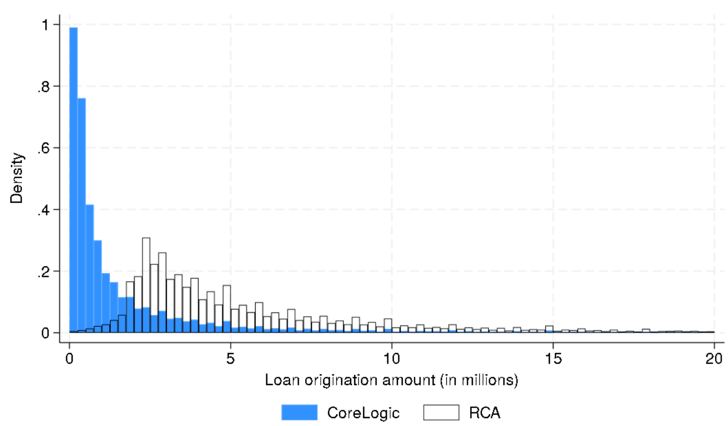
Figure A.1: Aggregate bank CRE loan originations

(a) Aggregate amount of loans originated

(b) Aggregate number of loans originated

Note: Bank loans only. Dropped CMBS lenders from both data sets. The RCA sample was restricted to to county×years that are also covered by CoreLogic. Authors' calculations based on CoreLogic and RCA.

Figure A.2: Distributions of bank loan size in 2022 in CoreLogic and RCA



Note: Bank loans only. Dropped CMBS lenders from both data sets. The RCA sample was restricted to to county×years that are also covered by CoreLogic. Truncated loans larger than 20 million dollars, which account for about 1% of the sample. Authors' calculations based on CoreLogic and RCA.

A.4 Sample Restrictions

This section describes the sample restrictions made to arrive at the final sample of banks used for estimation in Section 3 through Section 5. For the results in Section 6, the restrictions are different and these differences are discussed at the end of this section.

Table A.1 shows each sample restriction and its effects on the bank sample size. As the starting point, the top row of the Table shows that there are 4782 unique banks with nominal CRE balances in 2019Q4 ($>$ \$50k) according to the Call Report. The final sample used for estimation has 2197 unique banks (bottom row of Table).

From the original sample of 4782 banks, we first drop 22 banks that we classify as CMBS lenders – i.e. those banks with at least 10% of their CRE loan originations securitized into CMBS as computed from the RCA data. These tend to be very large banks such as Wells Fargo and JP Morgan Chase as well as investment banks such as Goldman Sachs and Morgan Stanley. CMBS loans can be originated by banks, and then leave the bank balance sheet soon after origination for securitization. Unfortunately, CoreLogic does not allow us to determine which loan originations were securitized into CMBS. Our algorithm described in Section 2.5 erroneously keeps these CMBS loans on the bank balance sheet until a subsequent loan origination associated with the property occurs or the loan matures, creating noise in our measure of exposure for banks that are active in CMBS lending.

We drop the 907 banks in the Call Report for which we are unable to compute Exposure using our CoreLogic data. Missing exposure may arise for a few reasons. First, the bank may lend exclusively in areas of the country that are not covered by CoreLogic. Second, our match between the CoreLogic and Call Report data is not perfect as we describe in Section 2.3, and this could cause missing exposure for some banks.

Next, we drop 63 banks that have a very small imputed CRE loan portfolio balance in the CoreLogic data ($<$ \$50k). Our measure of exposure may be especially noisy for these banks.

Next, we drop 157 banks that have zero total purchase loan originations in the CoreLogic data over our sample period from 2016-2023.

Next, we drop 1422 banks that have no loan purchase originations in 2016, the first year of our sample period. One reason we impose this restriction is because the coverage of CoreLogic increases over our sample period. By requiring banks to show up with originations in our data in 2016, we ensure that banks are not entering our sample simply because of expansion in CoreLogic's coverage.

Finally, we drop 14 banks that have missing covariates due to missing values in the Call Reports data.

To see how the full sample of 4782 banks from the Call Report compares with the restricted sample of 2197 banks we use for estimation, Table A.2 reports marginal effects from a probit regression using the full sample of banks. The dependent variable is a dummy equal to one if the bank is in the restricted sample and zero otherwise. The independent variables are various bank characteristics measured from the Call Report as of 2019Q4. The results show that our final, restricted estimation sample tends to have larger banks (i.e. those with larger total assets). However, the effect is somewhat nonlinear because we drop CMBS lenders. The very largest banks (i.e. those with over $10 billion in total assets) are less likely to be in our final sample. In addition, banks with more CRE, a higher share of their CRE portfolio in owner-occupied properties, and lower capital ratios are more likely to be in our restricted sample.

For the results in Section 6 that compare lending across zipcodes rather than banks, the full sample of CoreLogic lenders is used and the restrictions discussed above are not imposed. More specifically, CMBS lenders are not dropped, rather they are coded as nonbank lenders. Banks with missing exposure are in the sample and contribute to zipcode-level originations, but they do not enter the calculation of zipcode-level bank exposure in Equation 10. Banks with small CRE balances are kept in the sample. Banks with no purchase originations over the entire sample period or in the first year of the sample period are also kept in the sample.

Table A.1: Number of Banks by Sample Restriction

Sample Restrictions	Number of Banks
2019Q4 Call Report with CRE balance > $50k	4782
Dropping CMBS lenders	4760
Dropping with missing exposure	3853
Dropping with CRE balance in CoreLogic < $50k	3790
Dropping with no originations over sample period	3633
Dropping with no originations in first year of sample period	2211
Dropping with missing covariates	2197

Table A.2: Bank Characteristics in Restricted Compared to Full Sample

	I[In Restricted Sample]
Log total assets	0.127***
	(21.43)
Total assets > 10 billion dollars	-0.478***
	(-10.53)
CRE share of total assets	0.752***
	(13.30)
Owner-occupied share of CRE assets	0.123***
	(4.87)
Tier 1 capital ratio	-0.328***
	(-3.64)
Debt securities share of total assets	-0.00986
	(-0.19)
CRE Delinquency Rate	-0.159
	(-0.66)
Observations	4765

t statistics in parentheses

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

Note: Shows marginal effects from a Probit model with robust standard errors.

Table A.3: Difference in CRE loans outstanding in CoreLogic relative to Call Report and Bank Characteristics

	(1) abs(CoreLogic CRE - Call Report CRE) / Call Report CRE
10 - 100 billion in assets	-1.584
	(1.311)
1 - 10 billion in assets	-1.733
	(1.340)
$<$ 1 billion in assets	-1.697
	(1.370)
CRE share of total assets	-0.608
	(0.972)
Owner-occupied share of CRE assets	2.091*
	(1.218)
Tier 1 capital ratio	10.49*
	(5.448)
Debt securities share of total assets	-1.605
	(2.117)
Exposure	0.241
	(3.339)
R-squared	0.00520
N	2195

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: CoreLogic CRE is the size of the bank's CRE portfolio based on the CoreLogic data and our algorithm described in Section 2.3. Call Report CRE is the size of the bank's CRE portfolio based on the Call Report data. "abs" denotes the absolute value. Standard errors are heteroskedasticity robust.

A.5 Details on aggregate effect calculation

This section provides support for the assumption made in Section 3.3 that the bank with the lowest level of Exposure had a CRE portfolio-weighted WFH share that did not change as a result of the pandemic.

For each county c, we calculate $\Delta WFH_c^{ACS} = WFH_{2023,c}^{ACS} - WFH_{2019,c}^{ACS}$ where WFH_{2023}^{ACS} and WFH_{2019}^{ACS} are the actual county-level WFH shares from the 5-year 2023 and 2019 American Community Surveys (ACS), respectively. The ACS-based measure of WFH primarily captures full-time WFH (Buckman et al. (2025)). Mondragon and Wieland (2025) show that the ACS-based measure is strongly correlated with other measures of remote work that capture hybrid work arrangements and conclude that the ACS gives a very accurate measure of remote work in the cross-section.[2]

For each bank, we then calculate exposure to the actual WFH shock as

$$Exposure_i^{ACS} = \sum_c \frac{\text{CRE loans outstanding}_{ic}}{\text{total CRE loans outstanding}_i} \times \Delta WFH_c^{ACS}. \tag{11}$$

This is the same as equation 1, except we multiply the portfolio weights by the change in the actual WFH share instead of the DN-based measure $WFHshare_c$.

We then regress $Exposure_i^{ACS}$ on our DN-based Exposure (defined in equation 1), and generate predicted values of $Exposure_i^{ACS}$ using the regression estimates. The estimated coefficient on Exposure is 0.52 and the R-squared from the regression is 0.5. The predicted value for the bank with the smallest value of Exposure is 0.0035, or a 0.35 percentage point increase in the actual WFH share. Since this estimate is close to zero and not statistically significant, it supports our assumption that the bank with the lowest level of Exposure had a CRE portfolio-weighted actual WFH share that did not increase as a result of the pandemic.

A.6 Additional empirical results

Our main specification in Section 3 uses the count of purchase originations as the dependent variable. Column 10 of Appendix Table A.4 shows results when the dependent variable is the dollar volume of purchase originations. Overall, the results are similar to the baseline results (column1). The point estimates are a little more negative using dollar volumes, though they are more imprecisely estimated too.

Column 9 of Appendix Table A.4 shows results for the count of new refinance originations. Interestingly, there is no evidence for an effect of bank exposure on refinance credit supply. This could be because if banks reduce refinance credit supply to their existing borrowers, then they may end up creating more losses for themselves, as existing borrowers who are

[2]We calculate the actual WFH share using the share of workers aged 16 and over with at least $10,000 in earnings that reported “worked at home" as their usual mode of transportation to work. We calculate this for each place-of-work county.

unable to refinance balloon payments due at loan maturity may simply default. However, we acknowledge that this estimate for refinance originations could be biased if there are unobserved heterogeneity across banks related to demand for refinancing. Different banks may have different volumes of existing CRE loans maturing in different years with different amounts of ballon payments at the maturity.

Table A.4: Robustness and Alternative Specifications

	(1) Base	(2) Lvo	(3) ZipFE	(4) CBD	(5) BnkFE	(6) PropFE	(7) Pscore	(8) CPS	(9) Refi	(10) Dollars
Post x Exp.	-0.0734**	-0.0735**	-0.0739**	-0.0772**	-0.0740**	-0.0676**	-0.0739**	-0.0658**	-0.0163	-0.108**
	(0.0171)	(0.0136)	(0.0181)	(0.0180)	(0.0219)	(0.0176)	(0.0225)	(0.0181)	(0.0149)	(0.0322)
N	360,984	271,132	1,027,732	360,984	264,460	707,296	338,356	360,984	378,372	360,617

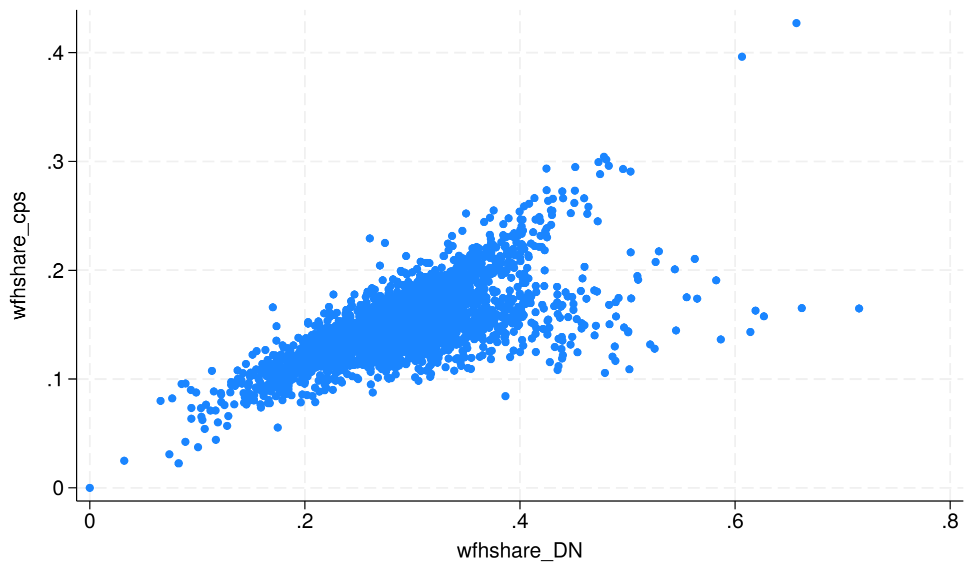
Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$

Notes: "Base" reproduces the main estimates from column 1 of Table 5. The remaining columns are variants of the baseline specification, where "Lvo" denotes the leave-one-out measure of exposure; "Zip. FE" denotes zipcode-by-year fixed effects instead of county-by-year fixed effects as in column 1; "CBD" denotes a bank-county control variable added for the CBDness (measured in hundreds of miles) of the bank's loan portfolio within a county, the CBD variable is interacted with the Post dummy; "BnkFE" denotes a set of bank-by-county fixed effects as controls; "Prop. FE" denotes property type-by-county-by-year fixed effects; "Pscore" denotes controls for the propensity score; "CPS" denotes the Current Population Survey-based measure of Exposure; "Refi" denotes Refinance loan originations as the dependent variable; and "Dollars" denotes that the dependent variable is dollars of new originations, rather than counts. All specifications include Post dummy-by-market fixed effects and bank controls (except when the bank controls are fully absorbed by the fixed effects.)

A.7 CPS-based WFH

Starting in October 2022, respondents to the CPS were asked if they teleworked or worked from home for pay at any time during the survey reference week. From October 2022 through December 2023, the share of respondents reporting "yes" to this question has been little changed at around 20 percent. The CPS also reports the industry in which the respondent

Figure A.3: WFH share by county based on Dingel-Neiman and CPS



is employed.[3] We compute the average WFH share for each industry k (WFH_k) using the share of respondents employed in industry k who report "yes" to the WFH question between October 2022 and December 2023. We then compute the county-level WFH share and $Exposure$ as in equations 2 and 1.

Appendix Figure A.3 shows a scatter plot of the DN and CPS-based measures of county-level WFH. The correlation coefficient between the two series is 0.7.

A.8 Effects of WFH on property values

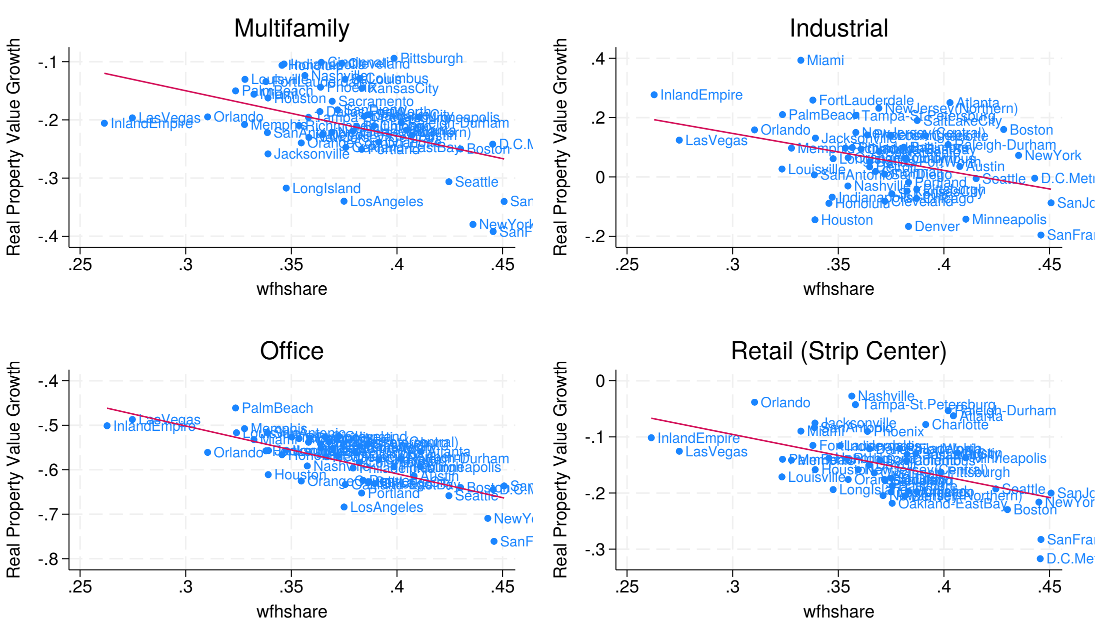
We use data on the change in real property value between 2019Q4 and 2023Q4 by market from Green Street, which is net of inflation over the same period. Green Street CRE property value estimates are based on properties that REITs hold. Green Street estimates the net asset value of the underlying properties using proprietary models that include cap rates, net operating income growth prospects as well as real estate transactions that are being negotiated and contracted. Since REITs own high-quality properties, the Green Street index

[3]The CPS uses census industry classification codes. We convert these to three-digit NAICs codes using the crosswalk provided by the CPS.

measures the value of institutional-quality CRE. Green Street provides price indexes for 50 major markets and separately by property type.[4]

We calculate the work-from-home share as in equation 2, except at the Green Street market level instead of the county level. Figure A.4 shows a negative correlation between real property value growth and work-from-home share across markets for each property type. The figure also shows that there is a lot of heterogeneity across markets and property types in property value growth. Office has the lowest growth, on average, while industrial has the highest. However, even for industrial properties, there are a number of markets with negative real property value growth such as San Francisco, Denver, and Minneapolis. Table A.5 shows that a 10 percentage point increase in the work-from-home share is associated with a 7.8, 12.5, 10.8, and 7.5 percentage point decline in real price growth for multifamily, industrial, office, and retail properties, respectively.

Figure A.4: Real Property Value Growth (2023q4/2019q4 - 1) by WFHshare



Note: Price growth is from Green Street for 50 major markets in the U.S. Price growth is inflation adjusted using the CPI. Red line shows the linear fit across markets.

[4]The Green Street retail category includes retail properties in strip centers only.

Table A.5: Real Property Value Growth (2023q4/2019q4 - 1) by WFHshare

	(1) Multifamily	(2) Industrial	(3) Office	(4) Retail (Strip Center)
Wfhshare	-0.778***	-1.248***	-1.075***	-0.749***
	(0.270)	(0.403)	(0.158)	(0.201)
R-squared	0.190	0.150	0.530	0.249
N	50	50	50	50

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Price growth is from Green Street for 50 major markets in the U.S. Price growth is inflation adjusted using the CPI. Standard errors are heteroskedasticity robust.

Table A.6: Net Migration by WFHshare

	(1) Net Migration	(2) Net Migration
Wfhshare	-0.0653***	-0.395***
	(0.0146)	(0.0332)
Weighted		✓
Dep. Variable Mean	.015	0
R-squared	0.00894	0.199
N	3132	3132

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Migration is a ratio calculated at the county level as: (net domestic migration between April 2020 and July 2024)/(population in April 2020). Column 2 shows results where counties are weighted by their April 2020 population. The migration data come from the Census and are available here: `https://www.census.gov/data/tables/time-series/demo/popest/2020s-counties-total.html`. Standard errors are heteroskedasticity robust.

A.9 Within-Property Business Types

We explore how much a CRE property assigned to a single property type by CoreLogic is utilized for purposes other than the assigned property type, by examining information about businesses occupying the property from the National Establishment Time-Series (NETS) Database. NETs provides the NAICS code of each business at a specific address. We match about 36% of property addresses in CoreLogic on the exact property address information. This matched data set provides NAICS codes of businesses located at CRE properties that are assigned to specific property types by CoreLogic. Then we determine how each business is likely to use a property based on its NAICS code. For example, businesses with the twodigit NAICS code of 54 for professional, scientific, and technical services, such as accountants and consulting offices, are likely to use their space as an office. Businesses with the three-digit NAICS code of 722 for food services and drinking places are likely to use their space as a retail. Note that we exclude multifamily properties for this analysis because many multifamily properties do not have businesses located at their locations.

Table A.7 presents a composition of NAICS-based property types within a CoreLogic property type, weighted by the number of employments at each business. It is not surprising that a majority of the businesses have the NAICS codes that are associated with CoreLogic-assigned office, industrial, and retail property types in Columns 1 to 3. However, a considerable share of businesses in CoreLogic-assigned industrial and retail properties in Columns 2 and 3 are also likely to use their space as an office (25% and 17%, respectively). In addition, nearly 40% of businesses located at CRE properties with an unspecified property type in CoreLogic are likely to use their space as an office. This result suggests that the regional WFH shock is likely to lead to a common shock to properties located in the same region through businesses using their space as an office in non-office CRE properties.

Table A.7: Business Types within Property

	(1) Office (CoreLogic)	(2) Industrial (CoreLogic)	(3) Retail (CoreLogic)	(4) Unspecified (CoreLogic)
Office (naics)	0.57	0.25	0.17	0.39
Industrial (naics)	0.20	0.55	0.15	0.22
Retail (naics)	0.10	0.14	0.58	0.27
Other (naics)	0.13	0.06	0.09	0.12
N. Obs.	73,966	65,173	87,082	73,462

Note: This table presents the share of likely property types based on NAICS codes of businesses occupying a property, weighted by the number of employments by each business. Data Source: CoreLogic Real Estate Data and National Establishment Time-Series (NETS) Database