Debt Overhang and the Retail Apocalypse*

Jack Liebersohn[†]
University of California Irvine

Ricardo Correa[‡]
Federal Reserve Board

Martin Sicilian[§]
Stanford University

December 31, 2021

Abstract

We estimate the effects of debt overhang in the retail property market and show that it has economically important effects on income and employment. High leverage impedes property owners' reinvestment after tenants close, reducing both property income and nearby employment. To identify the effects of leverage, we use plausibly-exogenous changes in owners' equity coming from local property price changes in the years after their commercial mortgage is originated. We estimate that, at the median, a ten percentage point leverage increase causes 22% lower employment mostly in large retail stores, and overall 15% lower operating income. Levered landlords take longer to fill vacant storefronts and employment in chain stores is lower. These effects are stronger for landlords who are close to renewing their mortgages.

JEL Classification: G32, G21, R33, L81.
Keywords: Debt overhang, commercial real estate, capital structure, commercial mortgage-backed securities, retail properties.

*The analysis and conclusions set forth are those of the authors and not necessarily those of the Board of Governors of the Federal Reserve System or its staff. Thanks to Chris Webster for excellent research research. Thanks to Tejaswi Velayudhan, Brent Ambrose, Andra Ghent, Rene Stulz, Eva Steiner, Jacob Sagi and Daniel Green for helpful comments, as well as to audience members at the 2021 CREDA conference, 2021 FMA Annual Meetings, Penn State. Special thanks to Joseph Nichols for helping us understand the data sources.

[†]cjlieber@uci.edu
[‡]ricardo.correa@frb.gov
[§]msic@stanford.edu

1 Introduction

The relation between debt overhang and corporate investment has been a staple of the capital structure literature since Myers (1977) noted that default risk may alter owners' incentives to invest, a deviation from the Modigliani and Miller (1958) theorem. The subsequent empirical work on this area has found mixed evidence on the effect of debt overhang on corporate investment (Wittry, 2019). There are several reasons that explain this weak relation. First, firms' debt capacity is affected by investors' expectations of future profits, making these expectations an omitted variable in regressions of investment on leverage. Second, debt overhang is most likely to be a problem at high levels of leverage, leading to under-powered tests. Furthermore, the prevalence of flexible debt contracts that allow for renegotiation, limits the potential detrimental effect of leverage on investment (Òscar Jordà, Kornejew, Schularick and Taylor, 2020).

This paper studies debt overhang in commercial real estate (CRE), an important asset class that accounts for a material portion of economic activity and that has relatively high debt levels.[1] The availability of CRE data and some of the market's institutional features makes it possible to use a new empirical strategy that isolates plausibly-exogenous variation in leverage, which improves on the empirical design compared to previous studies. Additionally, the CRE market has features comparable to those of residential real estate, where significant debt overhang effects have been estimated (Melzer, 2017). For example, CRE borrowers use long duration assets as the main collateral for debt transactions, but CRE properties also generate regular income and require investments as corporations. Many of these loans are securitized,

[1] As noted by Federal Reserve Board (2020), the commercial real estate sector had \$2.5 trillion in debt outstanding as of 2020:Q2 and accounted for almost 15% of nonfinancial business credit. Florance, Miller, Peng and Spivey (2010) estimate that, as of 2010, the value of commercial real estate is about half the value of all public equity in the United States.

which makes them more difficult to be restructured. We focus specifically on the effects of leverage in retail properties with securitized loans and estimate that debt overhang has a large impact on local retail vacancies and employment.

We use a difference-in-differences strategy to estimate the effects of leverage using data from records of commercial mortgage-backed securities (CMBS) since 2000. Greater leverage causes occupancy rates to fall, leading to lower property income and lower retail employment in the surrounding area. The reason is that leverage impedes adaptation when tenants leave; after an anchor tenant closes, it takes properties with more leverage longer to recover. Attracting new tenants requires reinvestment in properties (and potentially new loans), which may not be possible if a property already has high leverage or if its loans are securitized. As a result, leveraged properties have fewer tenants and generate less income. We estimate that a 10 percentage points increase in the loan-to-value (LTV) ratio of the median property causes a 15% lower net operating income and 22% lower retail employment per square foot within 100 meters of the property's main address. The effect on occupancy (i.e., the square feet occupied by any establishment) is only -4.3 percentage points (or a 4.3% decrease for the median property), suggesting that leverage also causes a change in the types of establishment that are present.

Our findings have important implications for the retail sector in the United States. The Great Recession, the rise of online retail, and the COVID-19 crisis have all reduced the demand for physical retailers since the early 2000s. This has caused falling retail employment and widespread store closures.[2] Shopping malls are vulnerable

[2]Figure 1 shows the number of of total U.S establishments and those in the retail sector. The latter fell sharply after 2008 and has continued its downward trend ever since. Dubbed the "retail apocalypse" in popular media, the changing economic environment for retailers has affected retail giants and smaller "mom-and-pop" stores alike, with the number of establishments falling for both types as also shown in the figure. Retail trade is one of the largest employment sectors in the United States, making a fall in establishment and employment in the sector an important policy issue, especially given concerns that conditions for brick-and-mortar retailers

to closures, since the closure of an anchor tenant can have spillover effects on other nearby stores (Benmelech, Bergman, Milanez and Mukharlyamov, 2018).[3] Importantly, retail properties are financed with CRE loans, which potentially generate a debt-overhang problem if these properties are materially levered. Debt concerns may incentivize owners to forgo investments that would otherwise speed adjustment to changes in the retail landscape.[4] These factors highlight the need to understand the potential amplification effects of store closings in levered properties, as well as the effect of these closings on employment.

Our empirical design is similar to the one in Melzer (2017). We rely on information from CMBS records for retail properties, which are mostly strip malls and shopping malls.[5] We match this data to information about nearby retail stores, including major chain stores. Our instrument exploits changes in property prices occurring after mortgages are originated. Conditional on the mortgage origination year, *ex post* price changes affect leverage by changing the value of owners' equity. We use this variation in owners' equity to identify the effects of leverage on employment, occupancy and income among properties located in the same ZIP code at the same time.

Our identifying assumption is that, among properties in the same ZIP code, ex

will become even worse in the future. For example, the following articles in the press have noted this trend in recent years: https://www.forbes.com/sites/pamdanziger/2019/04/10/retail-downsizing-will-accelerate-as-75000-stores-will-be-forced-to-close-by-2026/60b2073e339e; https://observer.com/2017/11/looming-retail-apocalypse-will-raise-unemployment-rates/

[3]This is a concern especially with Department stores facing competition from online retailers. See: https://www.nytimes.com/2020/07/05/business/coronavirus-malls-department-stores-bankruptcy.html

[4]For example, some mall owners have included new theme park attractions to lure customers. See: https://www.wsj.com/articles/malls-theme-park-attractions-covid-debt-11611267831?mod=hp˙lead˙pos5

[5]An advantage of focusing on this segment of loans securitized through CMBS products is that this type of debt is harder to renegotiate than bank debt, potentially exacerbating the debt overhang problem (Black, Krainer and Nichols, 2017, 2020). Although the performance of bank and securitized products appear to be equal at first hand (Ghent and Valkanov, 2016), once extensions and other renegotiation techniques are added the difference between both types of credit becomes apparent.

ante property characteristics are unrelated to ex post changes in property prices. This assumption is reasonable in the CRE market because mortgages are rolled over every ten years, so the date of mortgage origination is mostly determined by chance rather than any strategic timing by borrowers (Black et al., 2020). We also provide direct evidence for the validity of this assumption: In balance tests, we show that controlling for ZIP code fixed effects, measured ex ante observables are independent of later changes in property prices.

Our main results use this strategy to show that leverage substantially reduces occupancy and employment relative to other stores in the same ZIP code. To understand why, we decompose the results into two effects: The effects of leverage on properties with almost full occupancy that lose tenants for the first time, and the effect on properties who have already lost tenants. The effects of leverage on occupancy come entirely from buildings after their occupancy levels have already fallen from almost full occupancy (i.e.,95%). This implies that debt overhang reduces occupancy on average by prolonging the effects of tenant departures. This can be driven by the property owners' unwillingness to invest in their properties to attract new tenants due to debt overhang. We present suggestive evidence in this direction, as the negative relation between performance and leverage is more acute close to the refinancing date. At this time, the incentives spurred by the debt overhang problem may be more acute as the benefits of those investments are smaller for the current owners.

Next, we exploit the unexpected closure of chain stores, mostly due to bankruptcies, to investigate the dynamics of store closure.[6] We hand-match chain store closings

[6] Retail chain stores could strategically close stores in areas with weak demand, which could be correlated with the overall performance of the property where it is located. To avoid this problem, we focus on large retail chain bankruptcies that force these businesses to close most or all of their stores (e.g., Blockbuster video).

to data on the properties they are located, which appear in our CMBS dataset. We find that, compared to other properties in the same ZIP code at the same time, properties with more leverage and that lose an anchor tenant perform worse. This further confirms the important effect of debt overhang on property owners to adapt in when faced with shocks to their revenue.

Our findings inform research on the effects of debt on corporate performance, since research in this area has largely focused on the ways that debt prolongs recessions or exacerbates crises, whereas we show how debt can hinder economic change even without a credit supply shock. Furthermore, our findings have direct implications for policies that could help malls and other retail properties survive despite rising competition with online stores.

In particular, our results contribute to the following research areas. First, it is related to an important literature analyzing the effect of debt overhang on employment, particularly during recessions and financial crises (Òscar Jordà, Kornejew, Schularick and Taylor, 2020). Relative to this literature, our findings show that leverage can hinder adaptation to change even during relatively "normal" times. While we find that leverage has the greatest effects during the years after the 2008 crisis, they are not linked to a reduction in lending at the aggregate level or to unusual financial circumstances. Rather, the effects occur following shocks that happen even without a shock to credit conditions.

Our findings also suggest that debt overhang does not work the same way for residential real estate and CRE. Ganong and Noel (2020) show that policies which reduce LTV ratios without changing monthly payments have no effect on residential mortgage borrowers. The changes in leverage studied in our study — which are caused by changing levels of building equity — affect LTV ratios but have no direct effect on monthly payments. Nonetheless, we estimate substantial effects on building performance and local employment. This means that policies which may be appropriate for alleviating debt overhang for residential mortgage borrowers may work differently for commercial mortgages, especially those that are securitized. Policies to reduce commercial LTV ratios could help reduce the costs of the "retail apocalypse" even though such policies may not be useful for residential mortgages.

We also contribute to research on the aggregate implications of debt. Influential research by Bernanke and Gertler (1989) and Kiyotaki and Moore (1997) show how falling asset values during recessions can increase leverage and reduce investment. We show that, in addition to this, the marginal effect of leverage on employment and income is greatest in years when the economy is doing poorly. Therefore, recessions reduce collateral needed for investment precisely when it is needed most.

More broadly, this paper is related to research about the effects of leverage on firm performance and investment. Empirical papers on this topic include Ahn, Denis and Denis (2006), Cai and Zhang (2011), Aivazian, Ge and Qiu (2005), Hennessy (2004), Lang, Ofek and Stulz (1996), and Moyen (2007). Our findings on the relationship between leverage and negative shocks is also related to a literature on levered firms' responses to distress. Most closely related is Opler and Titman (1994), who show that the performance of leveraged firms is lower during industry downturns. Other papers on distress and levered firms include Andrade and Kaplan (1998), Gilson (1997), and Titman and Tsyplakov (2010).

Finally, our findings form part of a growing literature that studies debt overhang in novel settings. In addition to Melzer (2017), Bernstein (2019) studies debt overhang in the residential real estate market. Giroud, Mueller, Stomper and Westerkamp (2011) provide evidence for debt overhang in the setting of Austrian ski lodges. Wittry (2019) shows that legal obligations create debt overhang in the mining industry. Our papers adds to this literature by focusing on an economically important sector, which has material consequences for employment dynamics at the local level.

The rest of the paper is organized as follows. In Section 2 we discuss the main data sources. Section 3 describes the empirical strategy we use to identify the effects of leverage. Sections 4 and 5 discuss our main results, while section 6 presents some instiutional features of the CMBS market and robustness checks. Section 7 concludes.

2 Data

Our main data source is records of CMBS for the retail property sector (e.g., shopping malls). The data we use is provided by the data vendor Trepp, LLC, which is the leading provider of CMBS data. Several features make CMBS data ideal for studying the relationship between debt and property performance. First, properties with CMBS mortgages must provide information about operating performance at regular intervals, often quarterly or annually. Second, CMBS data typically includes detailed informaiton about the debt contract structure and debt performance. This includes interest rates, origination information, and delinquency.

We focus on retail properties for several reasons. First, this enables us to study plausibly-exogenous shocks that come from unexpected chain store closures. We use these shocks to study how leverage mediates the effect of shocks on performance. Second, we instrument for retail CRE prices using price changes in the *housing* market, which is plausibly exogenous to events in the retail CRE market. Finally, retail property types are coded in standardized ways, which makes comparisons across time and space possible.

2.1 CMBS Data

We begin with the universe of CMBS records, and use several filters to select a standardized set of properties.

- We limit the data to mortgages originated between 1999 and 2018. CMBS mortgages were rare before the early 2000s and are likely to exist only for a highly-unusual set of properties.

- We use mortgages linked to a single retail property. We use data starting in 2001, to avoid possible data quality issues in the early years of data collection.

- We use only ten-year, fixed-rate balloon mortgages, the most common type for this type of property.

- We use only properties that were built at least 5 years before mortgage origination and which had full occupancy at the time of origination, which we define to be at least 95%. This is to ensure that our sample does not include properties that are newly built at the time of mortgage origination.

This yields a maximum of about 74,000 property-by-year observations. Taking into account missing values across some of the variables used in our specifications, we include about 60,000 observations in our main tests.

The main performance variables we study are the logarithmic transformation of net operating income (NOI), measured per square foot, and occupancy rates. We measure log NOI contemporaneously and also as changes since the time of mortgage origination. We also study several variables that are fixed as of mortgage origination: Rent area, interest rates, origination loan-to-value (LTV) ratios, origination debt service coverage ratios (DSCR) and loan balances. These variables are measured at the time the loan is securitized, which is shortly after the loan is originated. They provide information about property characteristics as of the loan start date. We use them to provide evidence on the determination of property LTVs at loan origination, and as control variables in the regressions.

We also use information on property prices as regressors in our specifications and to adjust our proxy of leverage at the property level. We combine data on housing market prices and on prices for commercial retail properties for each geographic location. The CRE price data is from the MSA-level retail index from the National Council of Real Estate Investment Fiduciaries (NCREIF). Wherever MSA-level data is not available, we measure price appreciation at the state level, or use house price appreciation as a proxy for the few properties where a state-level CRE index is not available. Our measure of house price changes comes from the Federal Housing Finance Agency (FHFA) and is available at the ZIP-code level. We use the house price index to instrument for changes in property leverage since mortgage origination.

We use information on the time-varying loan balance of each property from Trepp LLC. and information from the CRE price indexes to estimate property-level LTVs. To do so, we use the outstanding balance of the loans for each property at a given point in time in the numerator.[7] For the denominator, we use the CRE price indexes for each geographic location to update the value of the property since origination. This adjustments allow us to calculate a current LTV ratio for each time period. For a few properties, we use the change in the house price index to adjust the LTV ratio due to the lack of CRE prices for those specific locations.

[7] As noted in Booth (2018), borrowers are typically required to make monthly payments of principal and interest. Amortizations are calculated using a 25 to 30 year schedule.

2.2 Chain Store Closings

Chain store closings provide a plausibly-exogenous shock to property performance. Following Benmelech et al. (2018), we hand-collect information on large chain store bankruptcies (e.g., Blockbuster video) from news reports. Then, we use Chain Store Guide store closing data to determine the location of the stores for these businesses.These data sources provide information about the dates of store closings for our entire sample period. We hand match store closings data to the Trepp LLC. data using fields in the CMBS records providing the name of anchor tenants.

Matching data on store closings with CMBS data yields a sample of 1,198 properties matching a closed chain store. These come from 66 mass closing events. The closings that match the most properties in our sample are Blockbuster (243 locations), Hollywood Video (116 locations), K-Mart (97 locations) and Circuit City (87 locations). We match *any* property that contains one of the closing stores as an anchor tenant, regardless of whether that particular location closed or not. This avoids the endogeneity inherent in the fact that chain stores choose which locations to close based on expected future conditions. As we will show, the closings-shock is still on average a large negative shock to occupancy.

2.3 Employment Near Retail Properties

We measure employment near retail stores using data from Infogroup. Infogroup has data on the addresses of US establishments by year. Its coverage is nearly universal for the retail sector. It includes estimates of establishments' employment and includes their sector and ownership information.

We geocode the addresses of Trepp properties using the Google Maps API. We use QGis to match each retail property to all establishments located within 100 meters of its centroid and calculate total employment within a 100 meter radius. We repeat this, limiting employment to retail employment, several retail subsectors, chain retail employment, single-owner retail employment, and various other employment categories. We use 100 meters as a benchmark for our results, but our estimates do not vary much using longer or shorter distances.

2.4 Summary Statistics

Summary statistics are shown in Table 1. The median property in our sample has a rentable area of about 56,000 square feet and an annual operating income of over $600,000. The sample consists largely of shopping malls and strip malls, although there are some smaller retail properties as well. Very small retail properties are unlikely to have securitized mortgages, as securitization entails costs for borrowers and the characteristics of such properties are less likely to be standardized Black et al. (2017).

At origination, the median loan has an LTV of 71%, an interest rate of 5.8%, and a balance of over $5.4 million. For the median property, we calculate that occupancy rates do not change at all, and property prices increase by 7.5% in nominal terms. The latter is consistent with a lower median current LTV (60% for the median property), which is inversely related to the increase in property prices.

3 Empirical Strategy

This section describes the empirical strategy for estimating the effect of leverage, or debt overhang, on property performance. The central issue for estimates of the effects of leverage on performance is the presence of omitted variable bias. Creditors are willing to lend more to properties with better expected future performance, biasing upward estimates for the effect of leverage on property performance and retail employment.

We take a twofold strategy to estimate unbiased effects for leverage. First, we control for contemporaneous economic conditions using ZIP code-by-year fixed effects. Our estimates are identified by differences in leverage coming from retail properties which we observe in the same year and are located in the same ZIP code that have different leverage levels. The fixed effects control for an important variable affecting both leverage and property performance, as well as retail employment, namely the general economic conditions in a particular location.

Second, we use an instrumental variables strategy that isolates changes in leverage that are uncorrelated with expected property performance at the time of origination. At origination, leverage is typically between 60% and 80% for stabilized properties, and nearly always between 50% and 90%. However, if property real estate prices fall after mortgages are originated, leverage can rise substantially higher. Leverage rises when real estate prices fall, as the main asset of this type of legal entity is the property whose value is directly tied to real estate prices. We exploit local real estate price changes occurring after mortgages are originated as an instrument for property leverage. This means that our IV strategy comes from differences in the timing of mortgage origination: Properties observed in the same year and in the same ZIP code can have differences in leverage coming from experiencing a different history of local price movements since their mortgages were originated, and the resulting differences in leverage are uncorrelated with their other characteristics.

This section will start by discussing the source of omitted variable bias in estimates of debt overhang. Next, it we will describe the motivation and construction of the instrument in more detail and describe the assumptions needed for it to be valid.

3.1 Omitted Variable Bias in Estimates of the Effects of Leverage

Consider the following regression specification relating leverage to property performance:

$$Y_{it} = \beta Leverage_{it} + X_{it} + \varepsilon_{it} \tag{1}$$

where Y_{it} is a time-varying measure of retail property performance, such as nearby employment, occupancy rates or NOI, $Leverage_{it}$ is a time-varying measure of the property's leverage, and X_{it} is a vector of control variables, which may include regional or time fixed effects. Theory predicts a negative sign for β if ε_{it} is uncorrelated with the regressors. However, at the time of mortgage origination, credit supply may be greater for properties with stronger expected future performance. Better management, more stable tenants, and better local economic prospects all reduce the likelihood of default, increasing lenders willingness to originate large mortgages. These factors – some of which can be measured, and others which cannot – are positively correlated with $Leverage_{it}$ and with Y_{it}. Without controlling for them, they will bias estimates of β upwards. The same omitted variable bias can be present in estimates of debt overhang in the corporate sector more generally, beyond the context of commercial real estate.

Our data include several variables that correlate with leverage and influence expected property performance. In Appendix Table A.1, we estimate bivariate regressions of leverage on these variables. Specifically, we estimate:

$$Y_{it} = \beta Leverage_{it} + \alpha_{zt} + \gamma_{st} + \varepsilon_{it}$$

where Y_{it} is a property characteristic at origination, such as net operating income or square footage, $Leverage_{it}$ is the log LTV ratio at mortgage origination, α_{zt} are ZIP code-by-year fixed effects and γ_{st} are year-by-origination cohort fixed effects. The results confirm that a variety of variables are correlated with leverage at origination. Similar correlations are there in estimates without fixed effects and in estimates with more controls. Given the variety of observable variables that are correlated with leverage at origination, a natural concern is that many unobservable property characteristics are correlated as well.

One approach for reducing omitted variable bias is to control for observable proxies of expected future performance that might be correlated with leverage. In other words, we can estimate specification 1, but include as controls some of the variables used in Table A.1. Table A.2 in the Appendix shows the result of such estimates, including and excluding fixed effects. Columns (1) and (2) of this table show the bivariate relationship; when controls for α_{zt} and γ_{st} are added in columns (3) and (4), the estimates change, raising concerns about unobservables. In general, we might not trust these estimates because of continued concerns about unobservables. Depending on precisely which control variables are included, the estimated effects of leverage can be estimated as higher or lower, and may vary depending on whether we study unemployment or occupancy.

The issues raised here – that estimates of β may be biased because of unobserved correlates with property performance – is not limited to the commercial real estate market. This is one of the main reasons that the recent literature has not estimated the effects of leverage on performance directly and has preferred structural approaches instead.

3.2 Instrumental Variables Strategy

Before formally describing our instrumental variables strategy, it is helpful to consider a hypothetical example that demonstrates the main ideas. Suppose there are two physically-identical shopping malls located in the same ZIP code in Irvine, California, which we observe in 2012. Both have ten-year commercial mortgages originated at a 70% LTV ratio. Shopping Mall A's mortgage was originated in 2007 and Mall B's mortgage was originated in 2003, when prices were 14% lower.[8] If we adjust the LTV ratio by the price changes in this period, we get that Mall A's effective leverage has increased since 2012, from 70% to 98%, and Mall B's leverage has fallen from 70% to 62%. The changes in leverage are due to changes in local property prices rather than any difference in fundamental property characteristics or other differences in local economic activity.

We might worry that Mall A and Mall B have mortgages that have different characteristics because lending standards changed over time, and these different characteristics might have direct effect on property performance. In other words, we might be worried about cohort-specific differences that happen to be correlated with leverage. For example, a potential concern is that Mall A has a lower DSCR because its mortgage was originated a time when interest rates were low, and the lower DSCR gives it more financial flexibility. To deal with this concern, we can compare Malls A and B to malls from the same cohorts located in a different city. For example, malls A' and B' located in Houston have a similar difference in DSCR but do not have the same difference in prices. This means that we can calculate the difference between A and B relative to the difference between A' and B' in order to control for cohort-specific effects.

[8] Based on the Case–Shiller house price index for Orange County, CA.

Motivated by this example, our instrument will isolate the portion of leverage changes coming from property prices changes occurring in the years after mortgages are originated. Intuitively, we can compare property characteristics of properties observed in the same ZIP code and in the same year but with a different history of price changes since mortgage origination because their mortgages were originated at different times. We can use differences in real estate price movements across regions to account for cohort-specific differences in real estate prices.

Our main assumption is that the timing of mortgage origination is not correlated with property characteristics, for properties located in the same ZIP code. We discuss evidence for this assumption in Section 3.3. In particular, we show that ex post price changes are independent of ex ante property characteristics.

Formally, our instrument for leverage is the change in house prices since a mortgage was originated. We use house prices rather than commercial real estate prices because it increases the likelihood of being exogenous to circumstances in the commercial real estate market. Our first stage regression specification is:

$$Leverage_{it} = \beta PriceChange_{it} + \gamma_{zt} + \delta_{st} + \epsilon_{it} \tag{2}$$

where $Leverage_i$ is the natural log of leverage for property i in year t, γ_{zt} are year-by-ZIP code fixed effects, and δ_{st} are cohort-by-year fixed effects. $PriceChange_{it}$ is the natural log of price changes since the mortgage was originated.[9] Controlling for γ_{zt} sweeps out variables associated with contemporaneous economic conditions, such as differences in retail sector demand or labor supply in a given year. The cohort-by-year fixed effects δ_{st} control for differences in the types of mortgages that are originated

[9]We prefer specifications where both price changes and leverage are measured in natural logs because the theoretical relationship between price and leverage in these specifications is linear, which helps with the precision of the estimates.

in different years.

First stage estimates are shown in Table A.3 of the Appendix. Column (1) shows the effect of changes in the log of the house price index on the current LTV ratio measured in logs. Column (2) shows the effect of the same house price index on an indicator variable equal to one for properties with LTV ratios above 60% (about the median LTV ratio for the sample distribution). This latter specifications allow us to capture any potential non-linearities on the effect of leverage. The estimated effect is large and are "strong" instruments based on the first-stage F statistic, which is close to 60 in our main specifications and significantly different from zero.[10]

Our identifying variation comes from differences in house prices experienced since mortgage origination by properties located in different locations. It is equivalent to the cross-cohort differences-in-differences design considered by Duflo (2001). As in that paper, we consider differences in the "experiences" of cohorts that are studied in the same location at the same point in time. But instead of differences in education from school building, we are considering differences in leverage coming from a different history of house price appreciation.

3.3 Identifying Assumptions

For this instrumental variables design to be valid, two main assumptions must hold. First, local property price changes since mortgage origination should be related to variation in leverage in the current year. Given that we are studying balloon mortgages with amortization schedules that are small, this relationship is close to mechanical, and we verify it in the first-stage regression.

[10]If 1) mortgages were all originated at the same LTV ratio and 2) real estate price changes were perfectly correlated for all properties in the same ZIP code, then we would expect the sign on β to be -1. In fact, LTV ratios are somewhat higher when prices are lower. We measure $PriceChange_{it}$ using house prices rather than CRE prices because house prices are plausibly exogenous from changes to the CRE market. These differences will mean that β is closer to 0 in practice.

Second, there must not be a relationship between *ex post* house price changes and unobserved *ex ante* property characteristics which will later affect performance. Since our sample is limited to properties that were built at least five years before the mortgages were originated, and most CRE mortgages are long-term contracts with penalties for prepayment, the mortgage origination years in our sample are unlikely to be chosen with advanced knowledge of future changes in house price. This makes the assumption plausible.

To provide direct evidence for this assumption, Table A.4 shows that $PriceChange_{it}$ is independent of ex ante observables conditional on the fixed effects. To show this, we estimate Equation 2, but with property characteristics as the independent variable instead of ex post leverage. If ex ante variables were correlated with ex post price changes, we would expect to see a statistically and economically significant relationship in these specifications. There is no evidence for this.

4 Effects of Leverage

This section presents the main instrumental variables results of the effects of leverage on property performance and unemployment. We start by showing that higher leverage causes properties to have lower occupancy and lower incomes. We also show that higher leverage causes a reduction in employment in retail stores located in and nearby the properties.

Conversations with industry participants and practitioner reports describe a particular institutional interpretation of the estimates: When large tenants close, retail properties must reinvest to rebuild the space in order to find new tenants. Substantial leverage may prevent these properties from procuring the resources needed to adapt to the new environment, limiting their capacity to attract new tenants and generate employment. To explore this hypothesis, we proceed in two steps. First, we show that leverage causes an increase in the likelihood that occupancy in a property falls from an initially high level to a lower one, that NOI falls for these properties and that the inability to maintain occupancy leads to a drop in employment. Second, we show that the negative effects of leverage on income and occupancy are concentrated in the later years of the mortgage, closer to the rollover date, when banks are less likely to approve new financing.

4.1 Leverage Effects on Income, Occupancy and Employment

Table 2 shows the effects of leverage on property income and occupancy using the instrumental variables strategy described in section 3. The first two columns have the log NOI as their dependent variable, and the last two have the occupancy rate.[11] Columns (1) and (3) show the average relationship between leverage and these performance indicators. On average, a log-point increase in LTV results in about 0.9 log-point lower NOI. For a property with an LTV of 60% — close to the median — this means that 10 percentage points higher LTV results in an almost 15% lower NOI, which is economically meaningful. In column (3), the dependent variable is occupancy, measured in percentage points ranging from 0 to 100. The main coefficient, -26, means that increasing leverage from 60% to 70% (an increase of about 17%) results in 4.3 percentage points lower occupancy (or roughly half of a standard deviation for this variable). We think that the effects on NOI are greater than the effects on occupancy because debt overhang changes the number of tenants as well as the types of occupants that are present. Another reason that the effects may be

[11] NOI is calculated as the log of the ratio of net operating income over square feet. This measure is windsorized at the 1% level.

greater is that existing tenants may negotiate rent reductions if anchor tenants leave, as they may trigger provisions in co-tenancy agreements (Liu and Liu, 2013).

Debt overhang may have non-linear effects on property performance. Columns (2) and (4) present results for the same specification previously described, but using and indicator variable for leverage as the main regressor. This indicator variable is equal to one when the LTV ratio is above 60%, which is a value close to the median for the sample.[12] We find relatively strong effects for these more levered properties. For example, properties with higher LTV ratios have NOIs that are about 1.5 percent lower than similar properties with lower leverage.

One concern with these specifications is that the LTV ratio for some properties is adjusted using the house price index. This generates a direct correlation between the instrument and the LTV ratio measures. To alleviate this concern, we estimate specifications similar to those in table 2, but restrict the sample to those properties where we have CRE price indexes for the LTV ratio adjsuments. The results are presented in the Appendix in table A.5. In those specifications, we find even stronger results for both NOI and occupancy. In addition, we add covariates that capture information about the amortization schedule of the loan, which do not make a difference in the results.

Table 3 extends our estimates to assess the effects of leverage on local employment. Our main dependent variable is the log number of workers located within 100 meters of the center of the retail property.[13] We also separately measure the log number of retail workers, employment for large and small establishment and the number of workers for various retail subsectors such as: motor vehicles, furniture, and health

[12]We conduct similar tests using a cut-off of 90% for the LTV ratio, which yields similar, and even slightly stronger, results.

[13]After calculating the number of employees close to the retail property, we take the log of that number and then winsorize this value at the 5%percent level.

and personal care.[14]

The effect of leverage on total and on retail employment, reported in columns (1) and (2), are economically and statistically significant. Our benchmark estimate for total employment is around -1.3, indicating that a 10% increase in leverage is associated with 13% fewer workers. Estimates for retail employment are somewhat larger, with the same increase in leverage yielding almost a 20% drop in retail employment. This not surprising because most employees located near retail properties are themselves retail workers. This effect of leverage on employment is over twice as large as the estimated magnitude for NOI.[15]

The effects on employment come mostly from large establishment employment, where the point estimate is -2.87, as shown in column (3).[16] There is no statistically significant effect for smaller establishments. These findings are consistent with our interpretation, that leverage impedes finding tenants after large anchor tenants potentially close. The results are very similar when measuring retail employment located within a slightly wider distance from the store (e.g., 500 meters).

In table 4, we assess the effect of leverage on the share of employment by NAICS retail sector around the properties in our sample. The dependent variables in these specifications is defined as the number of employees in each 3-digit NAICS sector divided by total employment in the area. We use the same regressors as before, including the instruments for the log of the LTV ratio. We find that more leverage is negatively and significantly associated with smaller shares for furniture stores and

[14]

[15]Because we measuring employment within 100 meters of the property headquarters the estimates for employment are not directly comparable to the results for occupancy and income, which come from retailer records. Nonetheless, we think it is informative to compare the estimates. The difference in magnitudes could arise because establishments with more retail workers are more affected than establishments with fewer workers, or because new tenants in properties with more leverage which replace stores that close end up hiring fewer workers.

[16]Large establishment are those that have more than 10 employees.

more importantly, health and personal care establishments. These findings provide some evidence that levered properties may be less likely to adapt and attract services that are less likely to compete with online vendors such as establishments providing personal care services. We will return to this hypothesis when we test the impact of store closings on employment.

4.2 Heterogeneous Effects of Leverage

Far from bankruptcy, debt should have little effect on performance, because owners still have an incentive to invest in a property. This is a testable implication in this context because of the specific structure of CRE mortgages. Since the data only includes 10-year balloon mortgages, bankruptcy is unlikely during the first few years of mortgage origination, when the mortgage rollover is years in the future. In the early years, landlords have an incentive to make investments in their properties because they still have many years before properties have to be renewed.

For some context, industry writings about the retail property market argue that when anchor tenants close, landlords must make substantial new investments to attract a replacement anchor tenants. For example, a report from Green Street Advisory, a real estate trade magazine, writes that "the malls experiencing outsized national tenant vacancies are more likely to experience rapid deterioration or need significant capital investment."[17] Since debt overhang is most likely to be a problem when capital investments are needed, we expect that high leverage is particularly problematic when tenants close. Raising new capital to pay for investments may also be difficult for properties close to their debt capacity.

We provide direct evidence for this channel. We decompose the effects of leverage on occupancy into the effects on 1) new tenant closures and 2) prolonged low occu-

[17] As reported by Danziger (2018).

pancy after departures have occurred. The effect on new departures shows to what degree levered properties are likely to experience store closures in the first place. The effect on prolonged occupancy estimates the effects of leverage after closures have occurred.

To do this decomposition, we first create an indicator variable equal to one for properties whose occupancy rate is below a particular threshold (we use 95% and 90%). This level of occupancy is below long-run sustainable levels for most properties and is an indication of the loss of one or more important tenants. Next, we create two indicator variables capturing 1) the first time properties fall below the occupancy level, and 2) only equal to one in those years when the occupancy rate is already below this level on a continuing basis.

We then estimate regressions of these indicator variables on log LTV ratios using the main instrumental variables specification, with property prices as the instrument. Estimates are shown in Table 5. Columns (1) and (4) indicate that a 10% increase in the LTV ratio is associated with around a 11 and 9 percentage points higher likelihood that the occupancy rate will be below 95% or 90%, respectively. Columns (2) and (5) estimate the effect of leverage on the likelihood that the property will have low occupancy for the first time. Both columns show a small effect that is close to zero, although the result for the 90% threshold is statistically significant at the 10% level. Of more relevance, columns (3) and (6) show how leverage affects the likelihood that occupancy stays at low levels for extended periods of time. The effects are large and statistically significant, indicating that the entire effect of leverage on vacancy spells is through longer-duration periods of high vacancy.

In the Appendix, we conduct a similar exercise but focusing on the life-cycle of the mortgage. The objective is to assess whether borrowers are unable or unwilling to invest in their properties when leverage is large. We split the sample between those properties in the first five years of the mortgage term and those in the last five years. The results are presented in table A.6. As before, the performance measures are NOI and occupancy and the LTV ratio is instrumented with the house price index by location. We find that the relation between leverage and performance is negative. However, the effect is larger and consistently significant only for the period close to refinancing, that is, the last five years of the mortgage. This finding is consistent with borrowers being unwilling to invest in their properties closer to the refinancing date, as suggested by the debt overhang theory (Myers, 1977). At that time, they may get few of the benefits from investing in the property, especially if the property is closer to bankruptcy due to its larger levels of debt.

The estimates in this section support our interpretation of the results, that greater leverage impedes finding a new tenant after an anchor tenant departs. The mechanism works through the inability to adapt to the changing market conditions due to the lack of funds for investment as a result of financing constraints. A series of findings are consistent with this: Leverage reduces occupancy only after it has already fallen, the effects of leverage on employment are limited to large retail stores and the effect is larger closer to the maturity of the mortgage.

Additional supporting evidence for our hypothesis comes from portfolio commercial real estate lending originated and retained by banks. We expect the debt overhang problem to be less acute for this type of lending, as banks are able to renegotiate the terms of lending to borrowers that are materially indebted, as long as the property has a positive net present value. Thus, the combination of debt overhang and inflexibility embedded in CMBS may exacerbate the problems associated with leverage which further explain the results we report.

We use data on banks' lending to commercial real estate properties collected in from FR Y-14. This information is reported by banks as part of the Federal Reserve's stress testing process. Large banks subject to the Dodd-Frank stress tests have to report their holdings of loans on a quarterly basis with details about the borrowers and the terms of the loans. The fields collected are similar to the information on CMBS collected by Trepp, Inc. We estimate similar test to those using CMBS data and find no significant results on the effect of leverage on performance. This is consistent with the notion that debt overhang and its effect on investment and performance only binds when there is inflexibility in the contractual arrangements, a topic that we address later.

5 How Leverage Mediates Shocks

The findings in section 4 show that high leverage makes retail properties less profitable and reduces nearby employment. This finding is most likely explained by poor performance following anchor tenant closing, but we would also like to show direct evidence for the channel. We do so using event-study type designs following anchor tenant closures.

As described in section 2, we collect data on chain store closings that occur on a national basis due to the bankruptcy of the parent organization, following Benmelech et al. (2018). We create an indicator equal to one for properties that have an anchor tenant listed whose name matches a chain store that closes due to bankruptcy regardless of whether that particular location is actually closed. We might worry that a chain store's (or the judge's) decision of which particular branches to close might be endogenous to local economic conditions or expected future performance. So instead of measuring the particular stores that are closed, we estimate an intent-to-treat effect of store closings on property outcomes for all locations where an outlet exists.

To understand the overall effect of store closings on property outcomes, we estimate local projections of property occupancy on our indicator for chain store closings. We first attempt to understand the main effect of predicted store closings on property outcomes. One reason for this is to verify that our matching procedure is successful, i.e., stores which we predict should experience a closing actually have negative outcomes. Another reason for this is to show that occupancy for properties with store closures move in parallel relative to other properties in the same ZIP code prior to the closure. To estimate the local projection, we use the following regression specification, which we estimate from s=-2 to s=+4, where s is measured in years:

$$y_{i,t+s} - y_{i,t} = \beta_s Closing_t + \gamma_{z,t} ZIP \times Year + \varepsilon_{i,t}$$

Figure 2 shows estimates from the local projection as well as the 95% confidence interval. Overall, closings have a sharp and persistent negative effect on property occupancy, as expected.

Our main object of interest is the differential effect of store closings on properties that experienced price increases since their mortgages were originated versus those that have experienced price decreases. To identify this, we divide the sample between those properties that have chain store closures and those that do not have any large closures. Then, we proceed to estimate the instrumented version of equation 1 using occupancy, NOI and employment as dependent variables.

Table 6 shows the results. We focus only on the results for properties with closures, as the findings for stores without closures are not statistically significant.[18] In contrast, as shown in columns (1) and (3), the relation between leverage and occupancy and employment is negative and significant, underlying the impact of leverage on performance, especially for properties that experience sudden shocks due to store

[18]We omit the *Cohort X YearFE*, as the sample drops substantially when we focus on properties with store closures.

closures. We do not find a significant results for NOI, which could be driven by the smaller sample size.

The estimates in Table 6 provide direct evidence that leverage mediates the response to store closures. We next proceed to explore some institutional features that may prevent properties with CMBS from mitigating the debt overhang problem.

6 Institutional considerations and additional robustness checks

The premise of our analysis focuses on the fact that CMBS arrangements are somewhat inflexible and may exacerbate the debt overhang problem for CRE borrowers. In this section, we discuss some of these arrangements that may limit borrowers from acquiring more debt or from renegotiating their outstanding liabilities. In addition, we report results from additional robustness tests that confirm our results.

First, borrowers hit by a shock could potentially renegotiate or acquire new debt to minimize the debt overhang problem as it is usually the case in the corporate sector (Òscar Jordà, Kornejew, Schularick and Taylor, 2020). However, there is hardly any mechanism that allows CMBS lenders to provide additional credit to borrowers. In a traditional bank-lending setting, financial institutions may be reluctant to lend more to a borrower if it suffers a shock, but it may agree to do so if it believes that more credit will help the borrower to adapt the property and stabilize its cash flow. In the CMBS context, once a loan has been securitized, additional loan funds cannot be procured because there is no lender available to provide them. There are only the servicers and the bondholders, neither of which has the funds or authority to make any additional loans. And even if a borrower wanted to get additional loans from a third-party, CMBS terms prohibit taking second-lien mortgages to reinvest in the property (Booth, 2018).

Second, CMBS loans are typically non-recourse, even large borrowers may have the incentive to default on unprofitable properties. This arrangement may exacerbate the impact of debt overhang on investment, as the current debtors may become unwilling to borrow more, if it were feasible, to make improvements in the property to increase its performance.

Third, there are provisions in CMBS contracts that require borrowers to set aside funds as reserves, some of which can be used for investment purposes. This arrangement may potentially reduce the impact of the debt overhang problem on performance. However, the use of these funds may be restricted and the purpose of their use may have to be prespecified, making it difficult to divert funds for one purpose to another. In table A.7 in the Appendix, we present additional tests to assess whether leverage is associated with measures that capture expenses, renovation costs, or the value of reserves of the property. We do not find evidence relating leverage to expenses and renovations. Unfortunately, the measure for expenses does not include capital expenditures and the indicator for renovations is sparsely populated. As an alternative, we assess whether occupancy differs for properties that maintain reserves in contrast to those that do not. Most properties collect some type of reserves, but those reserves are not enough to mitigate the relation between leverage and performance. Only those properties that set aside reserves at origination may be able to mitigate the negative effect of leverage on occupancy. This may be a mechanism through which the debt overhang probelm is reduced, but it is only applicable in a small set of properties.

These institutional features underline some of the factors in CMBS arrangement that may explain the findings that we are reporting on the effect of leverage on performance. However, there may still be concerns about our empirical tests, especially related to the data. One of those problems is potential attrition in the data due to bankruptcies, prepayments or other features. Tables A.8 and A.9 in the Appendix provide evidence that attrition is not a material concern in our setting. First, we find no evidence that leverage is associated with any potential sources of attrition. Second, we assess best and worst case scenarios for properties leaving the sample and assess whether they differ substantially. We do not observe any meaningful differences, which hints at attrition not being a major concern.

In sum, our results seem to be consistent with some of the institutional features present in the CMBS market. Moreover, these findings are likely not driven by issues with the sample especially with regards to attrition.

7 Conclusion

Using exogenous variation in leverage in the retail CRE market, we calculate that debt overhang has large effects on property performance. We also provide evidence regarding the channel whereby debt overhang affects performance. We decompose the effects on occupancy into the role of initial occupancy declines, and the extent of declines conditional on occupancy having initially fallen. Because capital investments are needed to replace anchor tenants which close their stores, we would expect debt overhang to be particularly severe following these episodes. Consistent with this hypothesis, we find that the entire effect of leverage on occupancy comes after initial declines in occupancy. Evidence from plausibly-exogenous bankruptcies of anchor tenants confirm our findings.

These results have implications for both fiscal and for macroprudential policy. Policy actions that reduce leverage or support investment to facilitate the transformation of retail properties could have especially large benefits during times when the sector is doing poorly.[19] Our findings could also help provide targeted policies to regions or sectors that need it most. These implications are particularly salient given the negative effects of the COVID-19 crisis on the retail sector and local employment.

[19]Some retail property owners with deeper pockets may be able to transform their business to accommodate new clients after the departure of anchor tenants (e.g., Simon Property Group and Amazon). However, others may need support to finance this transformation.

References

Ahn, Seoungpil, David J Denis, and Diane K Denis, "Leverage and investment in diversified firms," *Journal of financial Economics*, 2006, *79* (2), 317–337.

Aivazian, Varouj A, Ying Ge, and Jiaping Qiu, "The impact of leverage on firm investment: Canadian evidence," *Journal of corporate finance*, 2005, *11* (1-2), 277–291.

Andrade, Gregor and Steven N Kaplan, "How costly is financial (not economic) distress? Evidence from highly leveraged transactions that became distressed," *The Journal of Finance*, 1998, *53* (5), 1443–1493.

Benmelech, Efraim, Nittai Bergman, Anna Milanez, and Vladimir Mukharlyamov, "The Agglomeration of Bankruptcy," *The Review of Financial Studies*, 2018, *32* (7), 2541–2586.

Bernanke, Ben and Mark Gertler, "Agency Costs, Net Worth, and Business Fluctuations," *The American Economic Review*, 1989, *79* (1), 14–31.

Bernstein, Asaf, "Negative Equity, Household Debt Overhang, and Labor Supply," *Working Paper*, 2019.

Black, Lamont, John Krainer, and Joseph Nichols, "From origination to renegotiation: A comparison of portfolio and securitized commercial real estate loans," *The Journal of Real Estate Finance and Economics*, 2017, *55* (1), 1–31.

Black, Lamont K, John R Krainer, and Joseph B Nichols, "Safe Collateral, Arm's-Length Credit: Evidence from the Commercial Real Estate Market," *The Review of Financial Studies*, 03 2020, *33* (11), 5173–5211.

Booth, Susan J., "The Unique Aspects of CMBS Loans: A Primer for Borrower's Counsel," *The Real Estate Finance Journal*, 2018, *34* (3), 35–59.

Cai, Jie and Zhe Zhang, "Leverage change, debt overhang, and stock prices," *Journal of Corporate Finance*, 2011, *17* (3), 391–402.

Danziger, Pamela N., "Future Of Malls? The Price For Overbuilding Is Coming Due," 2018.

Duflo, Esther, "Schooling and labor market consequences of school construction in Indonesia: Evidence from an unusual policy experiment," *American economic review*, 2001, *91* (4), 795–813.

Federal Reserve Board, "Financial Stability Report," Publication, Washington, D.C. November 2020.

Florance, Andrew, Norm Miller, Ruijue Peng, and Jay Spivey, "Slicing, dicing, and scoping the size of the US commercial real estate market," *Journal of Real Estate Portfolio Management*, 2010, *16* (2), 101–118.

Ganong, Peter and Pascal Noel, "Liquidity versus Wealth in Household Debt Obligations: Evidence from Housing Policy in the Great Recession," *American Economic Review*, October 2020, *110* (10), 3100–3138.

Ghent, Andra and Rossen Valkanov, "Comparing Securitized and Balance Sheet Loans: Size Matters," *Management Science*, 2016, *62* (10), 2784–2803.

Gilson, Stuart C, "Transactions costs and capital structure choice: Evidence from financially distressed firms," *The Journal of Finance*, 1997, *52* (1), 161–196.

Giroud, Xavier, Holger M Mueller, Alex Stomper, and Arne Westerkamp, "Snow and leverage," *The Review of Financial Studies*, 2011, *25* (3), 680–710.

Hennessy, Christopher A., "Tobin's Q, Debt Overhang, and Investment," *The Journal of Finance*, 2004, *59* (4), 1717–1742.

Kiyotaki, Nobuhiro and John Moore, "Credit Cycles," *Journal of Political Economy*, 1997, *105* (2), 211–248.

Lang, Larry, Eli Ofek, and RenéM Stulz, "Leverage, investment, and firm growth," *Journal of financial Economics*, 1996, *40* (1), 3–29.

Liu, Crocker and Peng Liu, "Is What's Bad for the Goose (Tenant), Bad for the Gander (Landlord)? A Retail Real Estate Perspective," *Journal of Real Estate Research*, 2013, *35* (3), 249–282.

Melzer, Brian T, "Mortgage debt overhang: Reduced investment by homeowners at risk of default," *The Journal of Finance*, 2017, *72* (2), 575–612.

Modigliani, Franco and Merton H. Miller, "The Cost of Capital, Corporation Finance and the Theory of Investment," *The American Economic Review*, 1958, *48* (3), 261–297.

Moyen, Nathalie, "How big is the debt overhang problem?," *Journal of Economic Dynamics and Control*, 2007, *31* (2), 433 – 472.

Myers, Stewart C, "Determinants of corporate borrowing," *Journal of financial economics*, 1977, *5* (2), 147–175.

Opler, Tim C and Sheridan Titman, "Financial distress and corporate performance," *The Journal of finance*, 1994, *49* (3), 1015–1040.

Titman, Sheridan and Sergey Tsyplakov, "Originator Performance, CMBS Structures, and the Risk of Commercial Mortgages," *The Review of Financial Studies*, 08 2010, *23* (9), 3558–3594.

Wittry, Michael, "(Debt) Overhang: Evidence from Resource Extraction," *Working Paper*, 2019.
ΩÒscar Jordà et al.

Òscar Jordà, Martin Kornejew, Moritz Schularick, and Alan M. Taylor, "Zombies at Large? Corporate Debt Overhang and the Macroeconomy," Staff Reports 951, Federal Reserve Bank of New York December 2020.

8 Figures

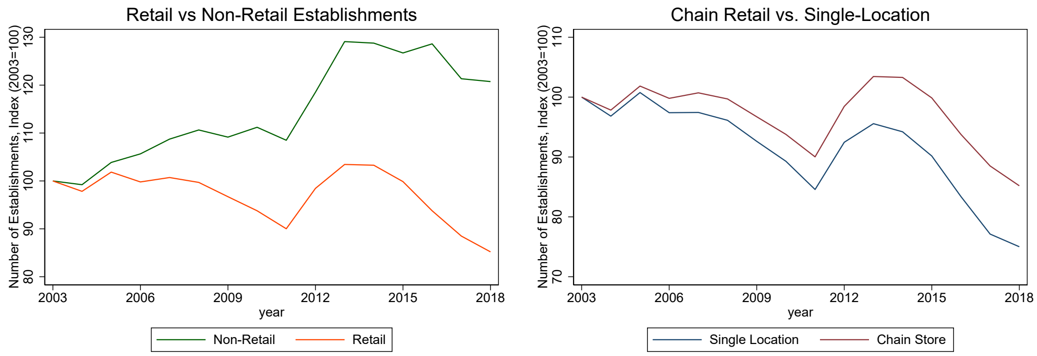

Figure 1: Counts of Establishments by Type. This figure tracks the evolution of the number of establishments by type over time. The panel on the left contrast the evolution of the counts of establishments for retail and non-retail establishments. The panel on the right tracks the same evolution withing retail establishments, contrasting single location vs. chain store establishments.

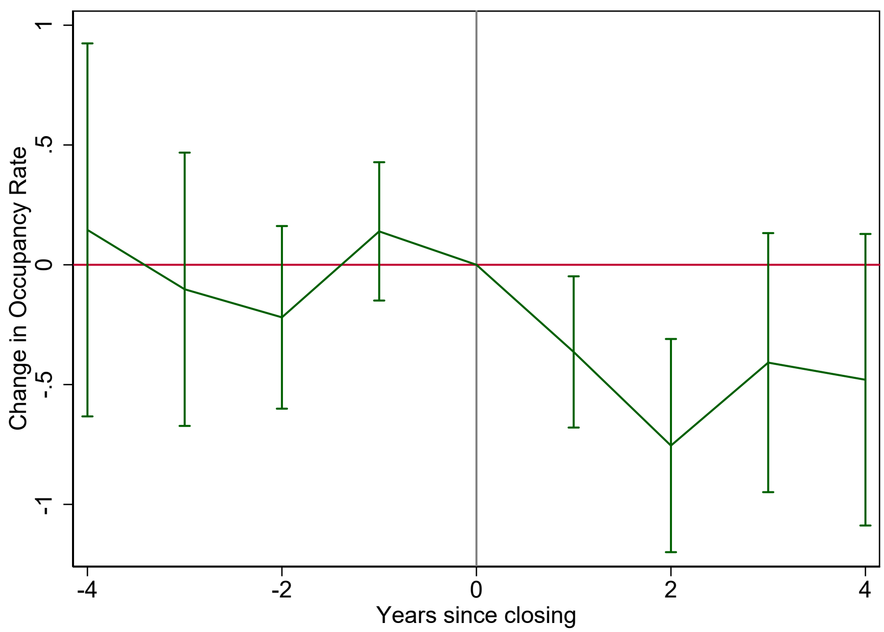

Figure 2: Effect of Store Closing. Local Projections of the effect of predicted chain store closings on the occupancy rate of properties. Year 0 is the year in which the chain store closing is announced.

9 Tables

Table 1: Summary Statistics

	Mean	Std Dev	Count	25th Pct	Median	75th Pct
NOI (Mil.)	1.46	3.95	58526	0.34	0.63	1.22
Occupancy	95.5	9.50	60008	95.1	100	100
Occupancy Chg	-3.48	9.41	57732	-2.72	0	0
Area (sq ft, 1000's)	102.6	168.8	59977	24.5	56.1	110.8
Originating Balance (Mil.)	10.9	19.6	60008	2.85	5.42	11.0
Orig Interest Rate	6.04	1.10	58459	5.41	5.81	6.43
Orig LTV	68.2	10.6	60008	63.5	71.1	75.3
Current LTV	60.4	18.7	60008	47.6	59.9	71.4
HPI Change	0.078	0.31	60008	-0.072	0.048	0.23
Price Change	0.10	0.23	60008	-0.014	0.075	0.24
Anchored	0.45	0.50	60008	0	0	1
Single Tenant	0.28	0.45	60008	0	0	1
Observations	60008					

Annual property-by-year summary statistics from the sample of properties used in estimation. Sample is limited to retail properties with an occupancy of at least 95% at mortgage origination, constructed at least 5 years before origination, with data used from 2001-2020. Occupancy change, HPI Change, and Price Change are calculated between the year the property is observed and the mortgage origination year. Other variables are calculated as of origination year. "Anchored" and "Single Tenant" are indicators for properties that are anchored and have a single tenant respectively. Source: Trepp.

Table 2: Leverage and Property Performance

	(1) NOI	(2) NOI	(3) Occupancy	(4) Occupancy
Log LTV	-0.88***		-26.0***	
	(0.27)		(5.02)	
LTV >60%		-1.53**		-42.4***
		(0.61)		(12.7)
Observations	58570	58570	60008	60008
Zip × Year FE	X	X	X	X
Cohort × Year FE	X	X	X	X

Instrumental Variables estimates of the effect of log LTV on property performance. The LTV in each year is calculated by multiplying the origination LTV by changes in the local house price index. Log LTV is instrumented for with the change in the log house price index since origination. See Table A.3 for first-stage estimates. NOI is in logs and occupancy varies from 0-100. Standard errors clustered by ZIP code.

Table 3: Leverage and Local Employment

	(1) Tot Employment	(2) Retail Employment	(3) Large Employment	(4) Small Employment
Log(LTV)	-1.29*	-1.91*	-2.87**	-1.31
	(0.72)	(0.99)	(1.34)	(1.21)
Observations	39440	33544	22103	20808
Zip × Year FE	X	X	X	X
Cohort × Year FE	X	X	X	X

Instrumental Variables estimates of the effect of log LTV on the log of nearby employment. The LTV in each year is calculated by multiplying the origination LTV by changes in the local house price index. Log LTV is instrumented for with the change in the log house price index since origination. See Table A.3 for first-stage estimates. NOI is in logs and occupancy varies from 0-100. Standard errors clustered by ZIP code.

Table 4: Leverage and Employment by NAICS-3 Retail Sector

	(1) Motor Vehicle	(2) Furniture Furnishings	(3) Electronics Appliance	(4) Building Garden	(5) Grocery	(6) Health Personal Care	(7) Gas Stations	(8) Clothing	(9) Sports, Hobby, Music, Books	(10) General Merchandise	(11) Miscellaneous	(12) Non-Store (e.g., Vending)
Log(LTV)	0.091	-0.14*	-0.020	-0.043	-0.081	-0.16**	0.021	0.076	0.052	0.028	0.057	0.0015
	(0.056)	(0.083)	(0.049)	(0.050)	(0.15)	(0.071)	(0.021)	(0.073)	(0.072)	(0.13)	(0.060)	(0.0073)
Observations	44226	44226	44226	44226	44226	44226	44226	44226	44226	44226	44226	44226
Zip $\times$ Year FE	X	X	X	X	X	X	X	X	X	X	X	X
Cohort $\times$ Year FE	X	X	X	X	X	X	X	X	X	X	X	X

Instrumental Variables estimates.

Table 5: Decomposition of the Effect of Leverage on Occupancy: Initial vs Continuing Effects

	(1) ≤95	(2) ≤95 (First)	(3) ≤95 (Subsequent)	(4) ≤90	(5) ≤90 (First)	(6) ≤90 (Subsequent)
Log(LTV)	1.07***	0.026	1.05***	0.92***	0.094*	0.82***
	(0.21)	(0.057)	(0.19)	(0.18)	(0.053)	(0.16)
Observations	61815	61815	61815	61815	61815	61815
Zip $\times YearFE$	X	X	X	X	X	X
Cohort $\times YearFE$	X	X	X	X	X	X

Estimates of the effect of LTV on property vacancy, decomposing the effect on new vacancies and the effect on existing vacancies. Columns (1) and (4) are IV estimates of the effect of LTV on low property occupancy, where low property occupancy is measured with an indicator equal to 1 for properties below 95% and 90% occupancy respectively. The instrument is the log change in the house price index since mortgage origination. In Columns (2) and (5) these indicators are equal to 1 only when a property has low occupancy for the first time and in Columns (3) and (6) the indicators are equal to 1 only when a property has low occupancy at subsequent times. Estimates are clustered by ZIP code.

Table 6: Heterogeneous Effects of Chain Store Closures

	(1)	(2)	(3)
	Δ Occupancy	Δ NOI	Δ Employment
Log LTV	-5.47***	0.0057	-0.32**
	(2.09)	(0.037)	(0.16)
Observations	2347	2325	5546
ZIP $\times$ Year FE	X	X	X

Estimates of the effect of chain store closings on property performance for more- and less-levered properties.

A Appendix

Table A.1: Correlates of LTV at Mortgage Origination

	(1) Anchored	(2) Unanchored	(3) Single-Tenant	(4) Log NOI	(5) Log Rentable Area	(6) Revenue	(7) Net Cash Flow
Origination LTV	0.62***	-0.0012	-0.23***	0.99***	0.81***	0.98***	0.62**
	(0.072)	(0.0073)	(0.072)	(0.16)	(0.16)	(0.15)	(0.25)
Observations	58541	58541	58541	52346	58570	57004	28319
R^2	0.017	0.000	0.003	0.012	0.007	0.012	0.004
Zip × Year FE	X	X	X	X	X	X	X
Cohort × Year FE	X	X	X	X	X	X	X

Linear regression estimates showing the relationship between property LTV as of mortgage origination, and other property characteristics as of property origination. Standard errors clustered by ZIP code.

Table A.2: Endogenous Estimates

	(1) NOI	(2) Occupancy	(3) NOI	(4) Occupancy	(5) NOI	(6) Occupancy
Log LTV	-0.051**	-0.55**	-0.083***	-1.22***	-0.038***	-2.23***
	(0.021)	(0.22)	(0.0072)	(0.30)	(0.013)	(0.58)
Anchored			-0.013**	-1.12***	-0.012	-1.01***
			(0.0054)	(0.26)	(0.0079)	(0.35)
Grocery Store			0.16***	1.57***	0.066***	1.38*
			(0.010)	(0.39)	(0.020)	(0.80)
Single-Tenant			-0.040***	-2.83***	-0.039***	-2.70***
			(0.0058)	(0.26)	(0.0083)	(0.39)
securlognoi			0.95***	-4.73	0.91***	-9.37**
			(0.041)	(2.89)	(0.063)	(3.95)
logrentarea			-1.00***	-1.13***	-1.00***	-0.71**
			(0.0043)	(0.20)	(0.0072)	(0.32)
securlognet			0.048	7.27***	0.053	10.9***
			(0.038)	(2.72)	(0.057)	(3.65)
securlogrev			-0.0019	-1.96***	0.025	-1.58***
			(0.0077)	(0.30)	(0.018)	(0.53)
Constant	2.73***	97.7***	0.42***	107.7***	0.28***	116.2***
	(0.088)	(0.88)	(0.047)	(1.81)	(0.081)	(2.96)
Observations	58570	58259	32160	32052	25789	25675
R^2	0.001	0.000	0.881	0.031	0.933	0.483
Zip × Year FE					X	X
Cohort × Year FE					X	X

OLS Estimates showing the relationship between property LTV and performance. The LTV in each year is calculated by multiplying the origination LTV by changes in the local house price index. NOI is in logs and occupancy varies from 0-100. Standard errors clustered by ZIP code.

Table A.3: First stage

	(1) Log LTV	(2) LTV >60%
HPI Change	-0.23***	-0.14***
	(0.030)	(0.038)
Observations	60008	60008
R^2	0.010	0.001
Zip × Year FE	X	X
Cohort × Year FE	X	X

Estimates of the first stage regression. Column 1 shows regression estimates of the Log(LTV) on the log house price index change. Column 2 shows regression estimates of the LTV on the log house price index change. House price index is a ZIP code level index from the FHFA. Standard errors clustered by ZIP code.

Table A.4: Balance

	(1) Anchored	(2) Grocery	(3) 1-Tenant	(4) 30-Yr Amort	(5) 25-Yr Amort	(6) 10-Yr Amort	(7) NOI	(8) Area	(9) Cashflow	(10) Rev	(11) Rate	(12) Occ
HPI Change	-0.055	0.0060	-0.016	0.11*	0.0076	-0.052	-0.13	-0.12	-0.14	-0.14	-0.28	-0.043
	(0.052)	(0.0045)	(0.050)	(0.056)	(0.037)	(0.032)	(0.11)	(0.11)	(0.10)	(0.17)	(0.18)	(0.049)
Observations	70146	70146	70146	70181	70181	70181	60803	70120	68355	33479	66222	70181
Zip × Year FE	X	X	X	X	X	X	X	X	X	X	X	X
Cohort × Year FE	X	X	X	X	X	X	X	X	X	X	X	X

Linear regression estimates showing the relationship between changes in the house price index between mortgage origination year and observation year, and other property characteristics as of property origination. Standard errors clustered by ZIP code.

Table A.5: Instrumental Variables Estimates, Price Change Robustness

	(1) NOI	(2) Occupancy	(3) NOI	(4) Occupancy
Log LTV	-1.01***	-30.9***	-0.82***	-29.0***
	(0.33)	(6.63)	(0.30)	(6.22)
Orig Occupancy			0.024***	0.61***
			(0.0046)	(0.091)
30-Yr Amort			0.0059	-0.41
			(0.019)	(0.37)
25-Yr Amort			-0.098**	-3.07***
			(0.047)	(0.97)
10-Yr Amort			-0.057	-1.47*
			(0.038)	(0.81)
Observations	51473	52898	48420	49898
Zip × Year FE	X	X	X	X
Cohort × Year FE	X	X	X	X

Supplementary instrumental Variables estimates. The LTV in each year is calculated by multiplying the origination LTV by changes in the local CRE price index. We restrict the sample in this specification only to those properties where the LTV ratio is adjusted with the CRE price index. Log LTV is instrumented with the change in the log house price index since origination. See Table A.3 for first-stage estimates. NOI is in logs and occupancy varies from 0-100. Standard errors clustered by ZIP code.

Table A.6: Effects of Leverage on Performance, by Term Remaining

	(1) NOI Early Term	(2) NOI Near Refi	(3) Occupancy Early Term	(4) Occupancy Near Refi
Log LTV	-0.78	-1.64*	-24.8***	-41.8*
	(0.55)	(0.92)	(8.18)	(21.6)
Observations	27020	15943	27690	16499
Zip × Year FE	X	X	X	X
Cohort × Year FE	X	X	X	X

Instrumental Variables estimates of the effect of log LTV on property performance. The LTV in each year is calculated by multiplying the origination LTV by changes in the local house price index. Log LTV is instrumented for with the change in the log house price index since origination. See Table A.3 for first-stage estimates. NOI is in logs and occupancy varies from 0-100. Standard errors clustered by ZIP code.

Table A.7: Instrumental Variables Estimates, Additional Variables

	(1) DSCR	(2) Expenses/NOI	(3) Renovation	(4) Occupancy Reserves Collected	(5) Occupancy No Reserves	(6) Occupancy Initial Reserves
Log LTV	-2.45***	-0.098	-0.030	-27.3***	-21.7*	-5.47
	(0.36)	(0.12)	(0.025)	(6.91)	(11.7)	(4.55)
Observations	43022	40945	61815	35926	9690	4136
Zip × Year FE	X	X	X	X	X	X
Cohort × Year FE	X	X	X	X	X	X

Supplementary instrumental Variables estimates. The LTV in each year is calculated by multiplying the origination LTV by changes in the local house price index. Log LTV is instrumented for with the change in the log house price index since origination. See Table A.3 for first-stage estimates. NOI is in logs and occupancy varies from 0-100. Standard errors clustered by ZIP code.

Table A.8: Attrition Robustness

	(1) Missing NOI	(2) Delinquent	(3) Bankrupt	(4) Prepayment	(5) Non-Missing
Log LTV	0.055	0.18***	0.0015	-0.087	0.057
	(0.10)	(0.063)	(0.026)	(0.069)	(0.039)
Observations	54381	54381	54381	54381	55819
Zip × Year FE	X	X	X	X	X
Cohort × Year FE	X	X	X	X	X

Instrumental variables estimates showing the effect of leverage on potential sources of attrition.

Table A.9: Attrition Robustness

	(1) NOI (Increased)	(2) Occupancy (Increased)	(3) NOI (Decreased)	(4) Occupancy (Decreased)
Log LTV	-0.77***	-20.5***	-0.66***	-18.0***
	(0.24)	(4.35)	(0.24)	(4.54)
Observations	58575	60175	58575	60175
Zip × Year FE	X	X	X	X
Cohort × Year FE	X	X	X	X

Linear regression estimates showing the relationship between instrumented leverage and occupancy/NOI, under various assumptions about attrition. Column (1) assumes that properties leaving the sample have annual NOI growth equal to the 95th percentile in the sample. Column (2) assumes that properties leaving the sample have 100% occupancy. Column (3) assumes that properties leaving the sample have annual NOI growth equal to the 5th percentile in the sample. Column (4) assumes that properties leaving the sample have 50% occupancy.