Loan Spreads and Credit Cycles: The Role of Lenders' Personal Economic Experiences

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ABSTRACT

Do changes in lender optimism lead to excessive fluctuations in credit spreads across the credit cycle? Using unique data on the real estate properties of loan officers originating large corporate loans, we analyze the role of lenders' personal economic experiences as a mechanism driving such fluctuations. We provide evidence that lenders overweight their recent locally experienced economic conditions, captured by local housing growth, and this systematically shapes credit spreads for borrowers that own real estate assets and riskier loans. Our analysis suggests that these effects are driven by lenders' beliefs about real estate values and can have important aggregate implications.

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Credit spreads tend to be low during credit booms associated with predictable declines in economic activity and the widening of spreads (López-Salido, Stein, and Zakrajšek (2017), Mian, Sufi, and Verner (2017)). In the extreme cases of financial crises, credit spreads are unusually low during the credit expansions that precede crises, in a pattern that is sharply reversed after the start of crises (Krishnamurthy and Muir (2016)). In the wake of the recent financial crisis, there has been a renewed interest on the idea that changes in lender beliefs help amplify these patterns (Kindleberger (1978), Minsky (1986), Geithner (2014), and Gennaioli and Shleifer (2018)). According to this view, spreads become "too low" and "too high" in different phases of the credit cycle, as booms (busts) induce lenders to become overly optimistic (pessimistic). This interpretation raises fundamental questions. Do changes in lender optimism lead to significant fluctuations in credit spreads in excess of what can be rationalized by modern accounts of credit cycles focused on factors such as borrower credit risk and banks' financial conditions? What specific economic mechanisms lead to these excessive fluctuations in lender beliefs and credit spreads? Do these mechanisms also significantly change the relative borrowing costs of riskier firms across the credit cycle? Which lenders are more exposed to such effects? Despite the importance of these questions, we still have limited direct evidence on their answers.[1]

In this paper, we study the role of lenders' personal economic experiences in addressing these questions. We examine loan spreads for large corporate loans during a significant boom and bust cycle in credit markets, the period between the early 2000s and the aftermath of the subsequent financial crisis. In this context, we analyze the idea that lenders overweight their recent personal economic experiences when forming beliefs and this helps shape credit spreads. Intuitively, if lender beliefs about borrower credit risk are excessively influenced by their recent economic experiences, lenders will be overly optimistic (pessimistic) when pricing loans during booms (busts). This bias will lead credit spreads to fluctuate in excess of what can be rationalized by factors such as borrower fundamentals or bank financial conditions. Our approach builds on a growing body of research in economics on the role of personal experiences in shaping belief formation (Malmendier and Nagel (2011, 2016)).[2] In theory, the importance of these personal

[1] These effects can have important economic consequences. For example, they can amplify economic fluctuations by increasing the volatility of borrowing costs or by inducing a neglect of credit risk during credit booms.

[2] One natural explanation for these personal experience effects on beliefs is the existence of an availability bias (Tversky and Kahneman (1973, 1974)), where recent personally experienced outcomes can be recalled more easily from memory. This role of recent personal experiences in shaping beliefs has also been emphasized by a psychology literature on learning from description versus experience (e.g., Hertwig et al. (2014)). Malmendier, Pouzo, and

experience effects in our corporate credit-market setting is unclear. On the one hand, the fact that decision makers are sophisticated and face high stakes suggests that market competition might eliminate such effects (List (2003)). However, the inherent discretion that is associated with corporate lending decisions and a need to rely on intuition in these decisions can significantly expose lenders to such effects even in this environment.[3]

Our analysis examines the effects of loan officers' local economic experiences in the period leading to loan origination on the pricing of the loans they issue. To this end, we use a unique dataset which identifies individual loan officers in charge of originating large corporate loans and the location of their real estate properties. We first identify these loan officers using credit agreement documents that are filed to the Security Exchange Commission (SEC). We then follow Cheng, Raina, and Xiong (2014) in using the LexisNexis Public Records, which combine information from public record sources, to track the personal properties owned by these loan officers. We capture officers' recent experiences using the past housing price growth in the neighborhoods surrounding their real-estate properties. This is intended to measure recent local conditions in areas where loan officers own their properties, live, or are familiar with.

This focus on local economic experiences as captured by real estate prices is motivated by multiple considerations. First, local conditions surrounding officers should be an important source of personal experiences. Moreover, movements in housing prices received significant attention in the U.S. during the boom and bust cycle that we analyze and it is plausible to expect finance professionals to be particularly aware of such movements.[4] Indeed, previous evidence suggests that home owners are familiar with their local housing price growth (Case, Shiller, and Thompson (2012)) as well as their friends' housing prices (Bailey et al. (2018)). We build on the findings from Kuchler and Zafar (2019). Using expectations data, they document that individuals overweight their recent local housing price growth experiences when forming beliefs about

Vanasco (2020) analyze how this bias induced by personal experiences can lead to an excessive volatility in security prices.

[3] Kahneman (2011) emphasizes how such biases in belief formation are particularly relevant in the context of intuitive thinking, as opposed to deliberate statistical thinking. Akerlof and Shiller (2010) argue that specialists often face discretion in economic decisions and need to rely on intuition, an idea supported in the context of financial markets by both anecdotal and survey evidence (e.g., Graham, Harvey, and Puri (2015)).

[4] Local personal experiences from the recent past are highlighted in examples discussed by Tversky and Kahneman (1974) to illustrate the availability bias. For example, they explain that "it is a common experience that the subjective probability of traffic accidents rises temporarily when one sees a car overturned by the side of the road." They also state that "recent occurrences are likely to be relatively more available than earlier occurrences".

national real estate prices. This evidence suggests a natural mechanism through which local experiences with real estate prices can shape lenders' beliefs about the value of the real estate assets in the balance sheet of large non-local corporate borrowers. As lenders become more optimistic about the value of these assets, their perceived exposure to credit risk is reduced, leading to a reduction in spreads that should differentially affect riskier borrowers. [5]

Another motivation for our focus on officers' local experiences is the identification of personal experience effects. In our context, lenders' personal experiences can be related to other factors such as traditional borrower and bank conditions that should predict credit spreads. Therefore, determining the extent to which lenders overweight their personal experiences can be challenging. We design a novel approach to address this challenge that relies on the fact that we analyze experiences in relatively small areas around officers' properties. Specifically, we examine the extent to which loan spreads are more strongly associated with the local growth around officers originating the loan than the growth around other officers in the same state and time period. In our sample of large corporate loans, borrowers are typically located remotely from the properties of their loan officers.[6] Therefore, it is plausible to expect that differences in officer local experiences within a same state and time period are not systematically related to other factors also shaping loan spreads. For example, these local experiences are unlikely to capture differential economic conditions faced by their non-local corporate borrowers. As we can track individual officers across loans, we also focus on analyzing how a same loan officer prices loans differently over time and exploit the precise timing of local experiences and loan originations when estimating these effects. We rely on this approach to isolate the significance of lender personal experience effects at the micro level but also analyze the potential implications from these micro-level effects for aggregate movements in loan spreads.

We find that credit spreads on corporate loans are significantly lower when loan officers' recently experienced higher real estate price growth in their neighborhoods. This effect is significant for local experiences in the year prior to loan origination and concentrated in the quarter

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[5] These higher expected asset values should improve expected loan repayment, an effect that is particularly relevant for riskier loans, as we discuss below. While Kuchler and Zafar (2019) document effects on beliefs about housing prices, these prices are strongly connected to commercial real estate values at the national level (Gyourko (2009)). Additionally, these effects on beliefs about national real estate values are not associated with differences in beliefs about other national conditions (e.g., unemployment).

[6] The average distance in our sample between officers' properties and borrowers' headquarters is approximately 787 miles and is larger, for example, than the distance between New York City and Chicago.

right before origination. Previous and subsequent changes in officer local prices are not significantly related to credit spreads. In terms of geography, these effects are concentrated in the neighborhoods surrounding officers' properties and are not significant for shocks affecting areas that are outside these neighborhoods but in the same MSA as officers' properties. We also find evidence that the pricing of loans becomes less sensitive to credit risk when officers recently experienced higher local growth. In other words, more positive personal experiences by lenders are associated with reductions in the relative borrowing costs of riskier firms. This suggests that personal experience effects can contribute to a neglect of credit risk by lenders. As described below, we implement several refinements and robustness checks to address concerns about alternative interpretations for our results. This evidence suggests that we capture the effect of officers' personal economic experiences.

We then investigate the specific mechanism underlying these personal experience effects. Our analysis suggests that these effects are driven by lenders' beliefs about the value of borrowers' real estate assets. We first note that a significant portion of loans are backed by collateral that covers a broad range of assets (e.g., all property plant and equipment) that includes real estate assets. We show that our results are driven by these loans backed by real estate and are not significant for other loans, which can be unsecured or backed only by other types of collateral. However, one limitation of this evidence is that the choice of secured financing and the composition of collateral should be shaped by borrowers' credit risk.

As a main approach to provide evidence on this channel, we exploit differences in the composition of firms' balance sheet, and focus on the share of real estate assets in borrowers' tangible assets. This approach is motivated by previous evidence that shocks to real-estate prices have a differential effect on the borrowing capacity of firms that own more real estate (Chaney, Sraer, and Thesmar (2012), and Carvalho (2018)). Intuitively, borrowers with more valuable assets have a greater ability to repay their loans. If lenders' beliefs about real estate values drive our results, personal experience effects should be concentrated among borrowers that own more real-estate assets. Indeed, we find that our results are not significant for firms with limited real-estate holdings and are differentially important for real-estate intensive firms. We also provide evidence that other potential types of beliefs, such as beliefs about broad economic conditions, are unlikely to differentially affect real-estate intensive firms in this way. Moreover, these real-estate intensive firms have observable characteristics that predict weaker personal experience effects. For example, these firms have lower credit risk. This suggests that we do not capture selection effects associated with real-estate intensity.

We also connect our findings more closely to the evidence from expectations data in Kuchler and Zafar (2019). They document that the local personal experiences in our results shape beliefs about national real estate prices, but do not affect beliefs about other national economic conditions, such as national unemployment. This complements the previous evidence on the types of beliefs driving our results. More broadly, they find evidence that these personal experience effects on beliefs are domain specific, e.g. local employment experiences do not affect beliefs about real-estate prices. Consistent with this idea, we also find that recent local employment conditions in the areas surrounding officers do not affect the pricing of the loans they originate, and do not have a differential effect on the pricing of loans by real-estate intensive firms. These pricing effects are only present for officers' local housing price growth experiences. Another main finding of their study is that this effect of local personal experiences on beliefs is not related to their informativeness in the data. We also find that our results follow a similar pattern. For example, in both expectations data and our results, personal experience effects are not stronger when local housing price growth is more correlated with national real-estate price growth.

Our analysis focuses on loan officers from lead banks originating large syndicated loans, and suggests that the personal views of these officers can shape loan pricing. We discuss both institutional features of this market, in addition to previous research and anecdotal evidence that support this idea (see Section 1.3). Following this discussion, we examine if the effect of lender personal experiences on loan spreads is stronger when it is plausible to expect these officers to matter more for loan pricing. This should be the case when there is less public information about borrowers, as there is greater information asymmetry between borrowers and lenders, and the lead bank plays a more important role in monitoring and screening borrowers (Sufi (2007)) as well as establishes stronger lending relationships with borrowers (Bharath et al. (2011)). Indeed, we find that the results are driven by firms that are smaller and have less analyst coverage. Relatedly, our results are more pronounced when loan officers' decisions are less disciplined by market forces, as in cases where the borrower has interacted less with other banks in the past and the officer's lead bank is predicted to hold a larger portion of the loan.[7] We then consider the role of officer age. Previous research has suggested that older individuals are less likely to overweight recent experiences or data when forming beliefs (Greenwood and Nagel (2009), Malmendier and Nagel (2016)). Consistent with the importance of this idea in our setting, and the role of individual loan officers, we find that our results are stronger for younger loan officers.

Finally, we analyze whether these personal experience effects could significantly amplify aggregate movements in loan spreads across the credit cycle. We interpret our results as estimating how a component of officers' housing price growth experiences, which are not relevant or informative for the pricing of borrowers' loans, affects loan pricing. For identification purposes, the irrelevant growth experiences we analyze in our results are idiosyncratic, and might not have aggregate implications. However, outside these results, there could be common shocks to these uninformative growth experiences across officers during the credit cycle. Using a simple framework, we illustrate how the lender personal experiences we document could amplify the response of spreads to a macro-level shock such as the last financial crisis by 16 percent.

As explained above, we refine our analysis to address potential concerns that we do not capture the effect of lenders' personal economic experiences. For example, to address concerns that local experiences might capture information about borrower fundamentals, we highlight that our results are not more important when officers' local shocks are more likely to provide valuable information about borrower conditions. This evidence also helps to further address a possibility that officers are only influenced by local conditions because of costs associated with information acquisition.[8] We find that our spread results are unlikely to reflect changes in the pool of borrowers as we show that they are not sensitive to the inclusion of a range of controls for borrower credit risk.

Another concern is that we may capture the effect of changes in officer housing wealth, which could matter for loan pricing in the presence of agency problems within the bank Here, we explicitly contrast the effects of more or less recent local housing price growth. We note that, from

[7] Also in the context of corporate loans, Dougal et al. (2015) find evidence that market forces mitigate the importance of anchoring effects on credit spreads.

[8] Under this explanation, officers have fully rational expectations and should not be influenced by local conditions in the absence of these costs of acquiring information. We also argue that this explanation is unlikely in our setting as it implies that officers face significant economic costs in accessing publicly available data from local housing prices in other officer neighborhoods within their same state. We can also limit our sample to cases where there is a weaker potential link between officer conditions and borrower fundamentals.

a pure housing wealth perspective, it should not matter for loan officers when their housing wealth increased within the recent past (conditional on the same increase). In contrast with this view, the effects of local housing price shocks that we document are only concentrated in the most recent period prior to loan origination, consistent with previous studies suggesting that recent personal experiences have the greatest influence on beliefs or decisions.[9] We also address concerns that our findings could be driven by other managerial characteristics correlated with officer personal experiences. An important result addressing this last concern is the timing of the effects and the absence of any connection between loan spreads and officers' local experiences right after loan origination. If local officer growth captures other managerial characteristics, one should expect local growth experiences right after loan origination to still predict loan spreads. Finally, the analysis suggests that bank conditions are unlikely to drive our results. For example, we show that our results remain similar if we analyze the effect of differences in officer experiences within a same bank and time period. We also provide evidence against the view that recent loan losses could explain our findings.

Overall, our paper makes two main contributions. First, we provide micro-level evidence that excessive fluctuations in lender beliefs can help shape loan spreads and the pricing of credit risk during credit cycles. Second, we provide evidence on a specific mechanism driving such effects, where sophisticated lenders overweight their recent personal experiences when forming beliefs about borrower asset values. Our research complements a growing body of research on credit cycles supporting the view that the beliefs of market participants do not fully anticipate the risks associated with credit booms.[10] Our evidence suggests that these fluctuations in beliefs can help shape the pricing of loans and provides a specific channel connecting recent economic conditions to such distortions in loan spreads and beliefs across the credit cycle. Our results also relate to previous research connecting banks' past experiences with credit losses to their new lending terms, and highlighting the role of bank-wide institutional memory or learning about screening abilities (Berger and Udell (2004), Murfin (2012)). Our focus on understanding credit spreads across the credit cycle connects our work to research analyzing other drivers of these patterns such as shocks

[9] Of course, one could be concerned that recent increases could be more informative about future prices and thus wealth, but we provide direct evidence against this view in our setting.

[10] For example, see Greenwood and Hanson (2013), Cheng, Raina, and Xiong (2014), Baron and Xiong (2017), Falenbrach, Prilmier, and Stulz (2017), and Bordalo, Gennaioli, and Shleifer (2018). Gennaioli and Shleifer (2018) provide a detailed discussion of this literature.

to the balance sheet of financial intermediaries (Santos (2011), Gilchrist and Zakrajšek (2012)) or the availability of institutional funding in loan markets (Ivashina and Sun (2011)).

Our paper also contributes to the literature on the role of personal experiences in shaping financial decisions and beliefs. A growing body of evidence in this area has analyzed sophisticated professionals such as corporate managers in a market setting. However, in contrast with our paper, this literature has typically focused on the role of managers' lifetime experiences in shaping their decisions.[11] Our focus on recent economic experiences and their role during credit cycles ties our results with the evidence in Chernenko, Hanson, and Sunderam (2016) on how personal experiences shaped the decisions of mutual fund managers to invest in nontraditional securities during the 2003-2007 mortgage boom. Their finding that inexperienced managers in MSAs with higher housing price growth invested more in these new securities complements our analysis on the role of such experiences as a determinant of lender optimism and loan spreads in credit markets. Koudijs and Voth (2016) also provide evidence that lenders' recent personal experiences with losses affect their risk taking in the context of margin loans in the eighteenth century Amsterdam stock market. To the best of our knowledge, our paper is the first to illustrate the role of lender personal experiences in shaping the pricing of credit risk across the credit cycle. More broadly, our paper also provides novel evidence isolating the role of recent personal experiences in shaping the decisions of sophisticated agents in a high-stakes market setting. As we discussed above, personal experiences can be correlated with a range of factors also shaping decisions and we provide novel evidence addressing this identification challenge.

1. Data, Background, Variables, and Sample

1.1. Data and Sample Construction

We construct our sample by combining data from three sources. We start with all syndicated loan contracts in LPC Dealscan issued between 1994 and 2012. Within these loan contracts, we restrict our sample to those with available information on loan contract terms (e.g., spreads, loan

[11] For example, see Malmendier, Tate, and Yan (2011), Schoar and Zuo (2017), and the references therein on the role of lifetime experiences in shaping corporate managers' decisions. In the context of individuals, see Kaustia and Knupfer (2008), Choi et al. (2009), Chiang et al. (2011), and Malmendier and Nagel (2011, 2016) for evidence on the role of recent personal experiences in shaping beliefs and decisions.

amount, and maturity) and issued to borrowers that can be matched to the Compustat database. We further exclude loans issued to firms in financial (industry SIC codes in 6000-6999) and utility industries (industry SIC codes in 4900-4999). We then implement the following key steps that are described in greater detail below. First, we identify the loan officers responsible for originating a loan based on their electronic signatures from the borrower's SEC filings. Among all the loan officers identified, we focus on those who lead-arranged at least two loans (in separate deals taking place over different years). Next, we manually collect information regarding loan officers' real estate properties using the LexisNexis database. In this process, we also collect other background information on loan officers, such as age and past employers. Using locations of officers' real estate properties, we track the housing price growth rates in officers' local areas using housing price data at the zip code level from Zillow. Finally, we obtain additional information on zip codes, including their centroid locations from zip-codes.com and demographic information from the 2000 Decennial Census.

1.2. Loan Officer Identities

We identify the loan officers responsible for originating syndicated corporate loans from borrowers' SEC filings following the procedure outlined in Bushman et al. (2020). For each loan contract, we first match the borrower name to its CIK number and search for that borrower's SEC filings for all available loan documents around the loan issuance date. In this process, we focus on firms' 8-K's, 10-Q's and 10-K's, as loan documents are considered material public disclosures and are generally filed as Exhibits to these filings. In particular, we search for any public filing containing an appended Exhibit 10 (which relates to "Material Contracts").

We require the contract to contain either the word "loan" or "credit" followed by the word "agreement" in the title to ensure that the contract relates to a loan agreement, as opposed to another material agreement (e.g., supply agreements and executive compensation agreements). We search for all filings meeting this criterion in the 90-day window centered on the loan date observed in Dealscan. This allows us to account for potential measurement error in the Dealscan loan dates. We then match each contract to the corresponding Dealscan loan using the names of all banks in the syndicate.

From the disclosed credit agreements, we scrape the names of loan officers from signature pages attached to the end of loan agreements. Since most of these contracts are electronically filed, signatures can be identified by searching for the string "/s/", which indicates the placement of an electronic signature. We use the data surrounding the electronic signature string to extract the name of the banker, the bank in which she is employed, and her title. We then match bankers' identities to their respective Dealscan loan and ensure that the bankers signing the loan contracts are affiliated with banks that are also reported by Dealscan. As mentioned above, we only examine loan officers from lead arranger banks in syndicated loans, which are the banks primarily responsible for setting loan terms (see Section 1.3).

We further collect other information regarding the loan officers from various sources. We collect employment history, work location, and education background from their LinkedIn pages. This allows us to pin down an individual in the LexisNexis database if there are multiple people with the same name. In the cases where an officer does not have a LinkedIn webpage, or his (her) webpage is not public, we manually assemble their background information from other sources, such as FINRA and Google.

1.3. Background: Role of Individual Loan Officers in Syndicated Loans

Our analysis builds on the idea that personal views by these individual loan officers can help shape the spreads of the loans they originate. We describe both previous evidence and institutional details that support this potential role of the loan officers that we identify. Recall that we focus on loan officers from lead arrangers in the syndicated loan market.

Lead arranger banks play an important role in the syndicated loan market and the pricing of syndicated loans, despite the fact that a significant portion of these loans is allocated to other participant lenders.[12] In contrast with other participant lenders, lead arrangers are the ones that directly negotiate with borrowers, establishing and maintaining relationships with borrowers, and take on the primary information collection and monitoring responsibilities. During the loan pricing process, lead arrangers first collect information on the borrower and potential investors, and a set an initial range for the interest rate or a target spread. As lead arrangers allocate shares of the loan to different investors, they determine the actual loan spread.

[12] For example, see the discussions and references in Sufi (2007) and Ivashina and Sun (2011). Standard and Poor (2011) provides a practitioner's view of the pricing process. There is typically one lead arranger in syndicated loans.

There are several specific features of the syndicated loan pricing process that are relevant for our purposes. First, when pricing loans, lead arrangers play an important role in evaluating the risk of the loan as they have more information than other participants. Additionally, when allocating loan shares to other participants, lead banks face discretion on where to set loan spreads. For example, S&P (2011) explains that "pricing a loan requires arrangers to evaluate the risk inherent in a loan…" and that "at the end of the [loan pricing] process, the arranger will…make a call on where to price the paper". As we discuss in greater detail in Section 3.4, the value of borrowers' assets is important in this risk evaluation as it shapes lenders' protection against default. Relationships between lead arrangers and borrowers also play an important role in determining the pricing of these loans. S&P (2011) also emphasizes this point: "banks are driven by the overall profitability of the issuer relationship". Moreover, lead arrangers typically hold significant portions of syndicated loans, consistent with the idea that they need to have a large enough financial stake in the loan to mitigate potential conflicts of interest with loan participants (Sufi (2007)).[13] These considerations help explain why lead arrangers play an important role in the pricing of syndicated loans and why their beliefs about the risk in a loan matter. Previous research has provided direct support for the importance of this influence. Financial conditions of individual lead banks have significant effects on the pricing of the syndicated loans they originate (Santos (2011), Chodorow-Reich (2014)). Recent experiences with losses by lead arrangers shape lending terms on the new loans they originate (Murfin (2012)). Moreover, relationships between lead banks and borrowers and the information that these banks have about borrowers help shape loan spreads in syndicated loans (Santos and Winton (2008), Bharath et al. (2011), and Engelberg, Gao, and Parsons (2012)).

Do the loan officers that we identify play a significant role in shaping loan terms within their lead arranger banks? In this context, a key point for our analysis is that these loan officers have authority and discretion when determining corporate loan terms. This idea is supported by different sources of anecdotal evidence. In their LinkedIn profile, corporate loan officers in our sample frequently explain their authority in managing and structuring loans and their role in loan pricing. For example, some loan officers in our sample explain that they were "responsible for pricing ... loans booked on the firms' balance sheet" and that they "led loan … origination teams in the

[13] Most loans in our sample are revolving credit lines. While some lead arrangers sell their share in a secondary market, Blickle et al. (2020) estimate that 94% of lead arrangers keep their share on revolving credit lines during the entire life of the loan. Relationships between lead banks and borrowers are particularly relevant for such loans.

proposal and negotiation of all aspects of… loan structures". Syndicated loans during our sample are commonly written and monitored by small teams supervised by an officer with a position such as a managing director or senior vice president.[14] Consistent with the idea that we capture such individuals with authority, the majority of loan officers observed in our sample are associated with such job titles. This description is also confirmed in evidence from professional job postings such as Glassdoor, where we considered job postings for corporate loan officers by banks in our sample. For example, these job postings explain the need for corporate bankers that can immediately handle loan requests up to certain in-house limits and have credit expertise in structuring and pricing loans.[15]

Recent research also provides evidence that the individual loan officers identified in our analysis have influence on the pricing of the loans they originate. Bushman et al. (2020) estimate that fixed effects for these loan officers can explain a significant portion of observed variation in loan spreads, after controlling for borrower characteristics and bank conditions. Herpfer (2020) provides evidence that relationships between these individual officers and borrowers can significantly reduce loan spreads, which increase in new loans from a same bank after the departure of connected officers from the bank. Gao, Kleiner, and Pacelli (2020) find that, following the poor performance of the loans they originated, these loan officers are punished with a higher probability of dismissal by their banks, suggesting that these officers are seen as (partially) responsible for these credit decisions.

1.4. Real Estate Property Data

Using loan officers' names and other background information (such as employment history, work location, and approximate age etc.), we search for their records from LexisNexis Public Records database. In this process, we follow closely the procedures described in Cheng, Raina and Xiong (2014) to construct their data on the personal home transactions of Wall Street employees. After finding a loan officer in the LexisNexis database, we manually collect her real estate ownership information from the following steps. First, we gather all the addresses related to the

[14] We have confirmed these points with practitioners from banks that originate syndicated loans.

[15] The Internet Appendix shows specific examples. Nathenson (2004) provides a practitioner's view on the role of loan officer's personal views on commercial lending. He explains that "over time, the performance of the loan portfolio reflects the intelligence and philosophy of the banker" and advises bankers to "maintain an independent point of view regarding risk profile… and pricing". His experience is based on one of the main banks in our sample and he explained that his points were relevant in the large corporate environment.

loan officer. We then collect all the deed transfer records, mortgage records, and tax assessment records of those addresses. Using these records, we are able to determine the dates when the officer gains and releases control of each property. The Internet Appendix describes in greater detail our data collection procedure from the LexisNexis database.

1.5. Final Sample, Main Variables, and Summary Statistics

After following the steps outlined above, we create an initial database with all the real estate properties associated with each loan contract-officer pair that we identify, matched with information on the lending terms and borrower characteristics for the loan contract, and other information on the loan officer. For each loan contract-officer pair, we only keep properties that are owned by the loan officer both at the time of loan origination and during the previous 12 months. Given that we require officers in our sample to lead-arrange at least two deals, our analyses are able to exploit variation over time in the decisions made by a same loan officer when that officer faces different local economic experiences.

We measure local economic experiences around each property using the housing price growth in the neighborhoods around its location. These neighborhoods are intended to capture areas where loan officers own their properties, are more physically present, or are more familiar with. We define these neighborhoods using a 10- or 20-mile radius centered in the property's zip code. We measure the distance between two zip codes using data on the location of their centroids and include all zip codes within each of these circles. In additional analyses, we also define the neighborhood surrounding an officer's property using all zip codes in the same county as the property. For each neighborhood and loan contract-officer, we construct measures of past housing price growth in the periods leading to loan origination. We label these measures as local housing price growth and compute them using the average of housing price growth rates across all the zip codes in the neighborhood.[16]

[16] Our main sample only includes property owners. We have also identified loan officers that are renters using the LexisNexis database, and constructed an analogous measure of housing price growth for them. Due to data limitations, we do not include them in our main sample and only use them only in robustness checks (Section 5). The Internet Appendix provides more details.

Our final sample is a loan contract-officer-state panel, which allows for a separate observation for each state where an officer owns properties at the time of a given contract.[17] This data structure is motivated by our empirical strategy where we match and compare loan officers who own properties in the same state during the same period of lending decisions. This strategy is discussed in greater detail in Section 2. Our final sample contains 971 unique loans extended by 239 unique loan officers during the years of 1998 to 2012 and covers real-estate properties in 32 states. The syndicated loans in our sample can have multiple lead banks, leading to a total of 1,330 unique loan-lead bank pairs. Moreover, in some cases there is more than one loan officer associated with a loan for a given lead bank, leading to a total of 1,440 unique loan contract-lead arranger-officer observations. Finally, given that some officers in our sample own properties in more than one state, the final sample contains 1,613 loan contract-officer-state observations. This sample is concentrated during a significant credit boom and bust cycle as 94% of its observations are between 2004 and 2012.

Table 1 provides summary statistics on the main variables used in the analysis. Our main outcome of interest is the log of loan spreads (*Log of Spread*), whereby *Spreads* refers to the all-in-drawn interest rate spreads in basis points over the LIBOR. Note that the typical corporate loan in our sample is large, with mean and median loan amount being $760M and $400M, respectively.

Our main independent variables of interest measure the recent local housing price growth in the officers' neighborhoods defined above. *Local HPGrowth (Quarter -1)* is the average housing price growth (the change in log housing price) across officers' neighborhoods in a state during the three months prior to loan origination. In general, we define *Local HPGrowth (Quarter -k)* as the average housing price growth across officers' neighborhoods in a state during the k^{th} quarter prior to loan origination.[18] Analogously, *Local HPGrowth (Year -1)* and *Local HPGrowth (Year -2)* are defined as annual housing price growth rates, i.e. average changes in log of housing prices over each of the two years prior to loan origination (12-month windows). These variables capture the average housing price growth in neighborhoods surrounding an officer's properties in a state.

[17] Note that this scenario is limited in our sample, as 84 percent of the officers have properties in only one state and only one officer has properties in more than two states.

[18] More precisely, if a loan starts during month *t*, *Local HPGrowth (Quarter -1)* is the housing price growth between month t -1 and t - 4. In each zip code, this growth is calculated as log (housing price on month t -1) – log (housing price on month t - 4). *Local HPGrowth (Quarter -k)* is calculated in an analogous way using previous quarters.

We also construct variables capturing the average housing price growth in other regions near officers' properties. *Own HPGrowth* measures the average housing price growth in an officer's own zip codes, i.e. the zip codes where officers' properties are located. *MSA HPGrowth* measures the average housing price growth across all zip codes that are located outside of an officer's neighborhoods but are in the same MSAs as her properties. Finally, *Matched HPGrowth* measures the average housing price growth in a set of matched neighborhoods from other loan officers. We construct this variable in two different ways using state or bank-region matches. First, we define matched neighborhoods as all neighborhoods in the same state from other officers at any point in our sample. Alternatively, we define matched neighborhoods as all neighborhoods in the same Census region from other officers in the same bank at some point in our sample. We exclude matched areas that overlap with the officer's own neighborhood in both cases. In a case where the officer owns multiple properties in a same state, we compute an average across the previous matched housing price growth rates across all of those properties.

When constructing the variables above, we define the month of loan origination as the month in which the initial structure of loans is determined (contracting date), and focus on economic experiences in the period prior to this date. In the syndication process, after the initial structure of loans is announced, the process of determining credit spreads starts, and there is time lag between the contracting date and the date in which the loan becomes effective (Ivashina and Sun (2011)). The reported facility start date in Dealscan captures this effective date. Following Murfin (2012), we set the contracting date of a loan as 90 days prior to the Dealscan reported start date.[19]

Table IA.1 and Figure IA.1 show the geographical distribution of loan officers' properties and borrower headquarters (see the Internet Appendix). Both loan officer properties and borrower headquarters cover a wide range of states. Yet, there is a significant geographical separation between them.[20] Consistent with this location gap, Panel D of Table 1 shows that the average distance between officer properties and headquarters is 786 miles, a distance greater than the one

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[19] This 90-day gap can be decomposed into two parts. First, there can be a delay of up to one month between the date a bank approves a term sheet with the deal structure and receives a mandate (a contract to act as a lead arranger). Second, practitioners estimate a two-month gap between the date the lead arranger receives this mandate and the loan becomes effective (Rhodes (2000)). Murfin (2012) finds that the latter gap is consistent with data from Dealscan on the subset of loans reporting both mandate and closing dates. He also finds that the 90-day lag is consistent with the connections between loan terms and aggregate defaults, stock returns, and credit spreads, which show a similar lag.

[20] For example, New York, Connecticut, and New Jersey represent 52 percent of officers' states and only 11 percent of borrower states. The main results in this paper remain similar if we exclude all properties located in New York State.

between New York City and Chicago. Our analyses also include controls for borrower characteristics, conditions of officer neighborhoods, and loan contract terms. Borrower-level controls include *Equity Volatility*, *Size*, *Firm Age*, *Profitability*, *Tangibility*, *M/B* (market-to-book), *Leverage*, and *Rated* (indicator for rated firms), all measured in the year prior to loan origination. Detailed definitions of these variables are provided in Tables 1 and 2.

2. Empirical Methodology

2.1. Aggregate Patterns and Motivation

We start by motivating our analysis with broad aggregate patterns on credit spreads and officers' recent economic experiences. Figure IA.2 plots the time series variation in corporate loan spreads and loan officers' recent housing price growth experiences between 2004 to 2012, a period in which most of our sample is concentrated (see the Internet Appendix). The figure shows that broad increases (decreases) in aggregate credit spreads are associated with more negative (positive) recent economic experiences by lenders. We also investigate this time-series pattern using regression analyses in a broad sample of Dealscan loans. Specifically, we predict loan spreads using the average housing price growth rates in officers' local areas during the year prior to loan origination, and include controls for borrower and loan characteristics, and macroeconomic conditions. The regressions also include borrower fixed effects and loan type fixed effects to track the variation in credit spreads for the same borrower over time. The Internet Appendix reports the results and provides more details. We find a strong negative connection between measures of aggregate lender recent economic experiences and credit spreads. This connection is economically significant and implies that the drop between the top and the bottom of these experiences in Figure IA.2 predicts a decline of 68 bp in loan spreads, approximately 32 percent of the mean value of spreads in our data.

These broad patterns in the data are consistent with the argument that lenders' recent economic experiences help shape credit spreads. However, changes in these personal experiences over time can be related to a range of factors shaping spreads and might provide informative signals about borrower and bank conditions. Therefore, it is challenging to determine the extent to which lenders *overweight* their recent personal economic experiences using these aggregate patterns. We propose and implement an approach to study this issue at the micro level.[21]

2.2. Identification Strategy

Our analysis examines the extent to which corporate credit spreads overweight officers' own recent local conditions relative to the recent local conditions faced by other officers in the same state and time period. Our identification strategy relies on two assumptions. First, an officer's personal economic experiences should be differentially shaped by the conditions in her own neighborhoods as she is more likely to be physically present in or familiar with these local areas. Additionally, officers' housing wealth experiences could also be differentially affected by these conditions.[22] This captures the previous idea that local conditions should be an important source of personal experiences. Second, differences in officers' recent local experiences within a same state and time period are not systematically related to other factors also determining loan spreads. In our sample of large corporate loans, borrowers are typically located remotely from the properties of their loan officers (see Section 1.4). Therefore, differences in officer local experiences within a same state and time period are unlikely to capture differential economic conditions faced by their non-local corporate borrowers.

Another central aspect of our identification strategy is that we analyze how a same officer prices loans differently over time depending on her recent experiences, and exploit the precise timing of officers' experiences and loan pricing decisions. We focus on the link between spreads and local conditions in the period right before loan origination and contrast this link with the connection between spreads and local conditions in these same areas during other periods. Of course, in principle, recent differences in officer local experiences within a same state and time could potentially capture other factors also shaping loan spreads. We discuss and address these possibilities as we present our analysis.

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[21] Aggregate patterns on forecasts for credit spreads and their revisions are consistent with the idea that expectations about credit spreads overreact to past conditions (Bordalo, Gennaioli, and Shleifer (2018)). This evidence does not examine the mechanism driving such overreaction effects and the extent to which they shape market spreads.

[22] One possibility is that officers are more familiar with information on local housing price growth in their neighborhoods, and their beliefs are shaped by this local information that they personally observe. Another possibility is that personal experiences with housing wealth shape officers' beliefs about real estate values. This last effect is different from a traditional wealth effect, where there are no changes in beliefs and wealth matters by changing the incentives of officers' subject to agency problems. We address concerns that we could capture such wealth effects.

Figure 1 illustrates our empirical strategy with an example of two officer neighborhoods (20-mile radius) in our sample, Huntington and Cold Spring, in the state of New York. The figure shows the headquarter locations of their respective borrowers, which are in Atlanta and Miami. The borrower-lender distance in these examples is representative of the sample average distance between officer properties and borrower headquarters. Our analysis examines whether differences in the recent conditions in the Huntington area relative to the Cold Spring area predict gaps between the spread for the Atlanta-based borrower versus the Miami-based borrower. As we contrast such loans over a same time period, we are connecting the spreads on these loans to the recent idiosyncratic conditions in their officers' neighborhoods (within the state). An important motivation for our approach is the idea that these idiosyncratic conditions surrounding officers are unlikely to be informative about their borrowers' fundamentals. Intuitively, information relevant for these non-local borrowers should be related to conditions affecting officers' neighborhoods as a whole in the state and not the specific area of the officer originating the loan.

2.3. Empirical Specification

We implement the identification strategy discussed above using a matched sample of officer local experiences prior to loan origination. Intuitively, we track local experiences of officers prior to each loan origination and match experiences in a same state by officers originating loans during the same time period. In this sample of matched experiences, we relate differences in spreads in loan originations to variation in the recent local experiences of officers within a same state and time period. As we track individual officers, we also ensure that we capture changes in the pricing of credit risk by a same officer over time. More precisely, we use the loan contract-officer-state sample described in Section 1.4 and estimate the following specification:

$$log(Spread)_{kist} = \eta_i + \lambda_{st} + \sum_{\tau=1}^{T} \beta_{-\tau} LocalHPGrowth(-\tau)_{ist} + \delta' X_{kist} + \epsilon_{kist}, \qquad (1)$$

where $log(Spread)_{kist}$ is the log of the loan spread for facility k issued by officer i, with a property located in state s, in time t (e.g., year or quarter); η_i denotes officer fixed effects; λ_{st} denotes state-time fixed effects; $LocalHPGrowth(-\tau)$ measures the local housing price growth for the officer's properties in state s during period $-\tau$, the number of quarters or years prior to the loan origination date; and X is a vector of controls. These controls include the borrower characteristics measured in the year prior to loan origination, variables capturing the loan structure (e.g. amount and maturity), and characteristics of officer's neighborhoods (demographics and home values).

The coefficients of interest are the values for $\beta_{-\tau}$, which connect loan spreads to the recent history of local economic experiences for the officer prior to the loan. There are two important features of this specification that capture the points above. First, we include state-time fixed effects to match and only contrast experiences faced by officers within a same state and time. This contrast isolates the sensitivity of credit spreads to recent idiosyncratic conditions within the state faced by the officer originating the loan. In addition, we include loan officer fixed effects and exploit only variation in the pricing of loan originations by a same officer over time. To ensure that we are matching officers' local experiences by state, this analysis examines the local economic experiences by a loan officer across multiple states at the time of a given loan as different observations. Our results should be interpreted as capturing the average effect of these local experiences and we take this into account when we estimate our standard errors.[23]

As we match officers' experiences by state and time, one challenge is that local economic experiences can change monthly.[24] If we include state-month fixed effects, only variation in officer experiences within a same state and month will be used to estimate our results. Only a limited number of loans in our data fit into a same officer state and month of origination, and this approach would lead to imprecise estimates. Alternatively, one can reduce the periodicity of the fixed effects and, for example, include state-year fixed effects. The issue here is that the results could potentially capture the effect of within-year changes in the aggregate experiences of officers in a state, as opposed to officer idiosyncratic conditions.

In our main specification, we include state-year fixed effects but propose a simple approach to ensure that we isolate the effect of officer idiosyncratic experiences. Specifically, we control for

[23] We double cluster our standard errors at the officer and borrower level borrower and, in robustness checks, also estimate our results using only officers with properties in a single state (84 percent of officers). As we need to analyze officers separately to track them over time, in principle, we could have different officers managing a same loan (e.g., multiple lead arrangers) with properties in a same state. This would lead us to underestimate the effects as these officers can have different local experiences but mechanically will be associated with the same loan spread. To consider the potential relevance of this issue for our results, we examine the share of the overall variation in officer local experiences driven by this within loan and officer state variation. In our sample, this share is only 3%, suggesting the limited importance of this issue. There is a single loan officer in 92 percent of the loan-state pairs in our data.

[24] Recall that these experiences are measured using the history of local housing price growth up to the month of origination (Section 1.4).

the average experience of a set of matched officer neighborhoods in the same state (*Matched HPGrowth (State)*). Recall that this measures the average recent experiences in all neighborhoods from officers in our sample before loan origination (Section 1.5). While most of the officers associated with these neighborhoods are not originating loans in the current month, they appear in our sample in different periods, we can still track the recent conditions around their properties in this month. Intuitively, this control captures common shocks affecting all officer neighborhoods in the state in the month of loan origination. After the inclusion of this control, we are using only differences in local experiences that are uncorrelated with such common officer shocks to estimate our results. In other words, we are analyzing the effect of officer idiosyncratic experiences. We also include an analogous control using all neighborhoods in the same Census region from officers in the same bank at any point in our sample (*Matched HPGrowth (Bank-Region)*). This allows us to analyze the effect of experiences that are idiosyncratic within officers' bank and region. [25]

While we use this approach in our main specification to have more precise estimates, we also document our main results using finer fixed effects (e.g., state-quarter) as well as implement falsification tests to ensure that we capture the effect of officer idiosyncratic conditions. Additionally, we also include the recent housing price growth experiences in the rest of officers' MSAs (outside their neighborhoods) in our main specification (*MSA HPGrowth*). This allows us to contrast the effect of recent conditions in officer neighborhoods with the one from other parts of their MSAs and further ensure that we capture the effect of officer idiosyncratic conditions.

3. Results

3.1. Credit Spreads and Officers' Local Economic Experiences

We examine whether recent local economic experiences by officers are associated with significant differences in corporate loan spreads. Table 2 reports the results based on the estimation of Equation (1). Panel A reports the results from our main specification. The results show that higher officer local growth during the year right before loan origination (*Past 1-Year Local*

[25] More formally, after the inclusion of these matched controls, one can use the residual regression result (Goldberger (2003, Chapter 17)) to interpret the coefficient on $LocalHPGrowth(-\tau)$ as the outcome of two stages. In the first stage, one determines the residual of $LocalHPGrowth(-\tau)$ in a linear regression predicting this variable with the matched controls. In the second stage, one uses only this residual local experience (idiosyncratic component) to predict loan spreads.

HPGrowth) is associated with significantly lower credit spreads. This link is present across the different definitions of officer neighborhoods (10- and 20-mile radius), and remains similar as we control for this officer local growth in the previous year (*Local HPGrowth (Year -2)*). We estimate this effect separately for the last quarter and the previous three quarters, and find that this effect is economically much stronger and only statistically significant in the last quarter. To evaluate the economic magnitude of our results, we focus on the idiosyncratic component of officer local experiences (experience variable demeaned by state-year), as our micro-level approach is designed to isolate the extent to which officers overweight this idiosyncratic component of their local experiences. The economic magnitudes of these last-quarter and last-year effects are significant and economically similar: a one-standard-deviation increase in the idiosyncratic component of officers' local growth experiences in the last quarter (year) predicts a 15-basis (16-basis) point decrease in loan spreads.[26] One interpretation for this concentration of the effects in the last quarter is that only sufficiently strong effects can be captured by our research design, which isolates the effect of officer idiosyncratic experiences. As these personal experience effects are particularly strong in the last quarter, only these last-quarter effects are detected. This pattern is consistent with previous evidence on the role of recent bank-wide experiences with loan losses in shaping new lending terms (Murfin (2012)) as well as with evidence on individuals' belief formation (Fuster, Laibson, and Mendel (2010)).[27]

We notice that these effects for officers' neighborhoods contrast with effects for areas in the rest of their MSAs in these same specifications (Panel A). We do not find last-year or last-quarter effects for the housing price growth experiences in the rest of officers' MSAs (*MSA HP Growth*). This suggests that our effects are driven by the specific conditions surrounding loan officers, as opposed to broader conditions in their MSAs. This contrast between neighborhood and MSA effects help address a potential concern that our results capture within-year changes in conditions affecting all officers in a state, as opposed to the effect of idiosyncratic conditions faced by the officers originating the loan. Recall that we include monthly matched officer growth controls, in

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[26] Table 1 shows that this standard deviation for the last-quarter experience is 0.6% and implies an effect equal to 12.1 × 0.006 × 212.7bp = 15bp, where 12.1 is the coefficient in column (2) of Panel A in Table 2 and 212.7bp is the average loan spread in Table 1. The analogous calculation for the last-year effect is 3.9 × 0.019 × 212.7bp = 16bp.

[27] Using the same empirical setting in our paper, Murfin (2012) studies how bank-wide experiences with loan losses affect new lending terms and also finds last-year experience effects that are concentrated in the last quarter. Fuster, Laibson, and Mendel (2010) explain that "studies in a wide variety of contexts suggest actual people's forecasts place *too much weight on recent changes*, like the most recent quarterly growth rate on … home prices" (their emphasis).

addition to state × year fixed effects, to ensure that our results capture such officer idiosyncratic experiences (see Section 2.3).

We further address this concern using different sources of evidence. In Panel B of Table 2, we show that the results remain similar as we add more controls for state conditions surrounding officers. We start by including additional controls for recent state employment and housing price growth in each of the four quarters prior to loan origination (columns (1) and (2) of Panel B). We then increase the periodicity of the fixed effects to check how this affects our main findings. Specifically, we refine these controls by progressively including state × semester, state × quarter, and state × time (bimonthly) fixed effects. We do not include state × month fixed effects because of the limited number of observations in a same state and month in our data (Section 2.3). If our approach captures the effect of officer idiosyncratic conditions, our estimates should not become systematically weaker as we use finer fixed effects. Instead, these effects should only become less precisely estimated. Panel B confirms these predictions. For example, our point estimates remain very similar as we use state × quarter fixed effects (columns (5) and (6) of Panel B). These effects are still statistically significant, but their standard errors become significantly larger than the ones in Panel A (columns (1) and (2)).

Additionally, we implement an intuitive falsification test to address this concern. If our previous results capture within-year changes in conditions affecting all loan officers in a state, they should be associated with lower spreads on the loans originated by other officers in the same state and time period. We examine this possibility by estimating our specification in Panel A (columns (1) and (2)) with the log of the average loan spread for matched officers as the outcome variable. Matched officers include all other officers in the same state that originate loans in the same time period, with neighborhoods that do not overlap with the neighborhood from the officer of interest. We define this time period for matched officers as a 6-month window ("semester"), quarter, or 2-month window ("bimonthly"). In each of these three definitions, spreads for matched officers can change within a state-year and detect potential shifts in conditions affecting officers as a whole in a state. Panel C of Table 2 report the results. Across different specifications, this link between officers' local experiences and loan spreads for matched officers is never statistically significant, has a limited economic magnitude, and does not have a consistent sign. This provide further evidence that we capture officer idiosyncratic conditions.

Overall, our main results suggest a significant, negative association between lenders' local idiosyncratic housing price growth experiences and the spreads on the loans they originate. This effect is concentrated on the most recent period prior to loan origination and the nearby neighborhoods surrounding their properties.

3.2. Credit Spreads and Officers' Local Economic Experiences: Additional Facts

We document important additional facts on the previous relationship between officers' local economic experiences and loan spreads. We start by showing that our results remain similar if we do not include controls for recent experiences by matched officers at the bank-region level. These controls are helpful to isolate idiosyncratic officer conditions within a bank and region, but significantly reduce the precision of our estimates as they limit both our sample size and the variation in the data used to estimate our effects. Panel A of Table 3 replicates our results from Table 2 (Panel A) without these controls and confirms this point. The estimated effects remain with a similar magnitude but are more precisely estimated. [28]

An important issue is the timing of our effects, which we now analyze in greater detail. In principle, it is possible that officers with different characteristics sort into neighborhoods with different average growth, and such a sorting could explain the link between loan spreads and local housing growth. The concentration of these effects in the quarter before origination helps address this concern. We further address this concern by examining the link between spreads and officers' local shocks *after* loan origination. If our results capture a contrast between officers in high versus low growth areas, we should see a significant link between spreads and officers' local growth right after loan origination. On the other hand, if our results capture the effect of local experiences on officers' decision making, this link should disappear after loan origination. Panel B of Table 2 extends the specifications in Panel A of Table 2 and 3 by including post-origination local housing price growth, i.e., *Local HPGrowth (Quarter +k)*, where k = 1, 2, 3, and 4. The results show that the link between loan spreads and officer local shocks disappears after loan origination. The coefficient of *Local HPGrowth (Quarter +1)* is statistically insignificant, economically small, and

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[28] Our sample size is affected by these controls because we cannot always construct the group of matched officers in the same Census region and bank as the officer of interest. In the Internet Appendix (Table IA.3), we implement our falsification test in Panel C of Table 2 using this specification in Panel A of Table 3. This test also provides direct evidence that we capture officer idiosyncratic conditions.

has the opposite sign of the coefficient of *Local HPGrowth (Quarter -1)*. We find this same pattern with insignificant results when we estimate the average effect across the four quarters after loan origination (*Future 1-Year Local HPGrowth*). These results are consistent with the explanation that lenders' recent experiences affect their lending decisions. In the Internet Appendix (Table IA.4), we also expand the specification in Equation (1) by adding officers' recent housing growth experiences at the zip code level, and continue to find that the effect of local housing growth is concentrated at the neighborhood level.[29]

Finally, we examine the importance of these effects in different subperiods. One potential concern is that we capture patterns driven by a short window during our sample, such as the recent financial crisis. We expand Equation (1) by including interactions of both *Local HPGrowth* and *MSA HPGrowth* variables with indicator variables for different subperiods. As these subsamples cover our entire data, these interactions show the importance our effects in each of these samples. Table 4 reports the results. We examine different periods associated with the last financial crisis such as 2007-2010 and 2008-2010. We also follow Kahle and Stulz (2013) and define the financial crisis as the period between 2007Q3-2010Q1. Across all these definitions, we find that our effects are not driven by the financial crisis period. Our results have a similar magnitude and remain statistically significant outside of the crisis period. Of course, our focus on housing price experiences is motivated by the importance of real estate prices during the specific credit cycle that we analyze. But these patterns suggest that we capture a systematic effect throughout this credit cycle that is not concentrated over a narrower window of time. In the Internet Appendix (Table IA.5), we further illustrate this point by showing that our results remain similar and significant as we drop different combinations of year pairs from our sample. We also separately estimate our effects for neighborhoods with positive and negative housing price growth in the quarter prior to loan origination and find that they are important in both cases. In sum, our results capture a robust pattern during the credit cycle that we analyze between loan spreads and recent officer idiosyncratic experiences.

[29] However, this contrast between different geographic levels can be affected by measurement error, which could be greater for smaller areas, and it possible that these experience effects operate at a more local level than officers' neighborhoods.

3.3. Lenders' Local Economic Experiences and the Pricing of Credit Risk

Credit booms are not only characterized by lower average credit spreads, but also by a reduction in the relative borrowing costs of riskier firms and a deterioration of borrower quality, in patterns that are reversed during subsequent busts (Greenwood and Hanson (2013), López-Salido, Stein, and Zakrajšek (2017)). Does the mechanism we document disproportionately affect the pricing of riskier loans across the credit cycle? Intuitively, it is natural to expect shifts in lender optimism about borrower asset values or fundamentals to differentially matter for riskier borrowers.[30] Therefore, if our results capture shifts in lender beliefs, it is plausible to expect positive local experiences by lenders to disproportionately lower spreads for riskier loans.

We examine this idea by analyzing the link between the pricing of loans in our sample and measures of borrower credit risk. We measure borrower credit risk using the Merton (1974) distance-to-default (*DD*), estimated following the approach in Bharath and Shumway (2008). One issue with connecting our results to raw differences in such measures is that credit risk experienced significant aggregate changes during our sample period. Therefore, such link would largely capture differences over time in the importance of our effects. We address this issue by analyzing two measures of borrower credit risk relative to a broad sample of other loans in the same period. *DDRank* is the quintile ranking (1 to 5) of a firm's *DD* in the universe of loans in Dealscan-Compustat originated in the same quarter. A higher number means a greater value for *DD* and lower credit risk. This measure is equivalent to the main measure of borrower credit risk used in Greenwood and Hanson (2013). *DD-Demeaned* is the difference between the average *DD* in a quarter for all borrowers in the same risk quintile (defined above) minus the average *DD* in the quarter across all loans, both defined using the universe of loans in Dealscan-Compustat.

We estimate the interaction between our previous effects and such differences in borrower credit risk:

$$log(Spread)_{kist} = \eta_i + \lambda_{st} + \sum_{\tau=1}^{T} \gamma_{-\tau} LHPGrowth(-\tau)_{ist} \times CreditRisk_{kt} + \\ + \phi CreditRisk_{kt} + \sum_{\tau=1}^{T} \beta_{-\tau} LHPGrowth(-\tau)_{ist} + \delta' X_{kist} + \epsilon_{kist}, \qquad (2)$$

[30] Consider the case of optimism about the value of borrowers' assets. This value will shape creditors' recovery in default, a state of the world that will be more relevant for riskier borrowers. See Bordalo, Gennaioli, and Shleifer (2018) for a framework where lender optimism about borrower cash flows also leads to this prediction.

where $CreditRisk_{kt}$ is one of the previous credit risk measures for loan k, and the remaining variables are defined in the same way as in Equation (1). The coefficients of interest $\gamma_{-\tau}$ capture the differential importance of our effects for firms with different credit risk.

Table 5 (columns (1) and (2)) shows the results. To provide a better sense of economic magnitudes, we also report the value of γ_{-1} multiplied by the gap between the mean of *CreditRisk* for the top 50% and bottom 50% of its values. The results show that the previous effects disproportionally affect riskier borrowers. The differential effect for firms with high versus low credit risk (above and below median) has a similar magnitude to the average effect in our sample.

We also estimate this specification using the market-adjusted spread for the loan as the outcome, i.e. the difference between the log of spread for the loan and the log of the market spread. The market spread is the weighted average of spreads across all loans in the Dealscan-Compustat universe issued during the same quarter as the loan of interest. The link between the market-adjusted spread and *CreditRisk* can be interpreted as the sensitivity of loan pricing to differences in credit risk within a quarter. In this context, the value of $\gamma_{-\tau}$ captures the effect of officer experiences on this sensitivity. Columns (3) and (4) show that positive officer experiences are associated with significant reductions in this sensitivity, and these results have a similar magnitude to our previous estimates. In the Internet Appendix (Table IA.6), we also document that more positive officer experiences are associated with a weaker link between loan spreads and market benchmarks for these spreads. These market benchmarks measure the average spread for loans originated in the same period as the loan with comparable credit risk (e.g., same *DD* quintile). Intuitively, such weaker link between the spreads in our sample and market benchmarks captures a reduced sensitivity of lenders to credit risks priced by the loan market. These analyses show that the effect of lender personal experiences is concentrated on high-risk borrowers and change the sensitivity of loans spreads to credit risk.

3.4. Are the Results Driven by Officers' Beliefs About Real Estate Values?

We provide evidence on the role of officers' beliefs about real estate values in driving our results. As previously discussed, we build on Kuchler and Zafar (2019, hereafter KZ). Using expectations data, they document that individuals overweight their recent local housing price growth experiences when forming beliefs about national real estate prices. They also find that these local experiences are not associated with differences in beliefs about other national outcomes such as national unemployment. This suggests a specific mechanism for our results. Namely, local housing growth experiences shape officers' beliefs about the value of real estate assets in the balance sheet of their non-local borrowers.[31] As lenders become more optimistic about the value of their borrowers' assets, their perceived exposure to credit risk is reduced, leading to a reduction in spreads that disproportionally affects riskier borrowers. Intuitively, these assets can be used for repayment and higher expected values for them should provide greater protection against default.

We note that these personal experience effects in officer beliefs can be relevant in our setting because of the discretion associated with the loan pricing decisions we analyze (see Section 1.3) and potential need for officers to rely on intuition in such decisions.[32] We also note that, despite the presence of appraisals, there is still significant uncertainty about the *future* value of real estate assets that will matter for future loan repayment. Indeed, the evidence in KZ suggests that local experiences shape beliefs about real-estate price growth over multiple years in the future.[33] Additionally, the idea that asset values are important for loan pricing in our specific setting is also highlighted by practitioners. For example, S&P (2011) mentions the importance of collateral for evaluating loss-given-default risk in this market and explains that loans in this setting are typically secured by a broad range of assets, including tangible assets. Indeed, most loans in our sample (two thirds) are secured and 80 percent of secured loans with information on the collateral type are backed by an asset class (e.g., all assets or PPE) that covers real estate assets. The Internet Appendix (Table IA.8) provides more details and shows that this pattern is similar for the universe of Dealscan-Compustat loans. While the presence of such collateral can increase the importance of the previous mechanism, this mechanism could remain relevant even in the absence of such

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[31] Note that lender beliefs about national prices in housing and commercial real estate markets should be largely related, as these two markets are strongly interconnected (Gyourko (2009)). KZ also find that their results remain significant among more sophisticated individuals (e.g., college degree).

[32] One natural explanation for such effects is the availability bias (Tversky and Kahneman (1973, 1974)), where recent personally experienced outcomes can be recalled more easily from memory. Previous research has emphasized the idea that such biases emerge in the context of intuitive thinking, as opposed to deliberate statistical thinking (Kahneman (2011)). Many real-world economic choices are not fully structured as a pure quantitative calculation as there is discretion, and sophisticated finance professionals often need to rely on their intuition (Akerlof and Shiller (2010), and Graham, Harveyand Puri (2015)).

[33] For example, in the context of loans backed by aircraft, Littlejohns and McGairl (1998) explain: "The exact appraisal value of aircraft … is not vital because … the value will change from the outset of the deal" (quoted in Benmelech and Bergman (2009)).

collateral, as higher values for borrowers' assets can also improve the recovery in default for unsecured lenders.

Motivated by these points, we first contrast loans backed by collateral that includes real estate assets (*RESecured*) with other loans, which can be unsecured or backed only by other types of collateral such as marketable securities or working capital. Panel A of Table 6 (columns (1) and (2)) reports results separately estimating officer experience effects for these two types of loans and analyzing their differential importance. These effects are differentially important for loans backed by real estate and are not significant for other loans. This shows that our results are driven by loans where the previous channel should matter the most. However, one limitation of this evidence is that the choice of secured financing and the composition of collateral could be shaped by borrowers' credit risk, leading to potential selection issues. Specifically, riskier borrowers could be more likely to rely on secured loans.

As a main approach to isolate the importance of this channel, we exploit differences in the composition of firms' tangible assets, and focus on the share of real estate assets in borrowers' balance sheet. This approach is motivated by previous evidence that shocks to real-estate prices have a differential effect on the borrowing capacity of firms that own more real estate (Chaney, Sraer, and Thesmar (2012), and Carvalho (2018)). This captures the intuition above that more valuable assets in firms' balance sheet should provide an increased protection for lenders. Consequently, if lenders have more optimistic beliefs about real estate prices, this should differentially matter for firms that own more real estate (real-estate intensive firms). Additionally, if our results are driven by this mechanism, we should not expect significant effects for firms with limited real-estate ownership. Beliefs about real estate prices should have a limited effect on the expected value of their assets. This provides an important falsification test. As we sort firms using this real-estate share, measured as a percentage of PPE, we address the previous concern with sorting firms based using loan characteristics. Indeed, the Internet Appendix (Table IA.9) shows that there is a weak link between lower credit risk and this real-estate share, leading to selection effects in the opposite direction of the previous prediction. Indeed, this real-estate share is higher for firms that are larger, older, and more profitable (Chaney, Sraer, and Thesmar (2012)).

Panel A of Table 6 (columns (3) to (6)) reports results estimating the differential importance of officer experience effects for firms with a higher real-estate share (*RERatio*). We first use continuous differences in this ratio, but also consider groups of firms with high and low values for this share. This helps limit potential measurement error in the real estate share and check if our results are driven by firms with a high share of real estate. *REOwner* (1%) is an indicator that equals one if *RERatio* ≥ 1%. *REOwner* (5%) is defined in an analogous way. *High RERatio* is an indicator that equals one if *RERatio* is above its median value in the sample. The mean of *RERatio* in these groups with high real-estate intensity is between 29 and 40 percent. This mean value is between 0 and 6 percent for firms with low real-estate intensity.[34] Across these different approaches, we find that our results are significantly stronger for real-estate-intensive firms. This differential effect for real-estate-intensive firms is economically significant with a magnitude comparable to the one of our average effect. Moreover, we find that the experience effects are never statistically or economically significant for firms with limited real-estate intensity. This pattern is also robust across all specifications and shows that our results are only present when firms have significant real-estate holdings in their balance sheet.

One potential concern with these results is that, in principle, other types of beliefs by officers could also differentially affect borrowing terms for real-estate-intensive firms. However, recall that KZ find that local housing growth experiences are not associated with differences in beliefs about other national outcomes (e.g. unemployment). Moreover, in order to explain our results, these alternative beliefs would need to rationalize the lack of significant effects for firms with limited real-estate assets. As an additional approach to address this issue, we directly examine the differential effect of multiple types of economic shocks on the subsequent borrowing of real-estate intensive firms. Specifically, we follow the approach of Carvalho (2018), who analyzes the differential effect of predicted shocks to regional real-estate price growth on real-estate intensive firms. We first replicate this stronger effect of regional real-estate shocks on the borrowing of real-estate intensive firms, and then examine if other types of economic shocks lead to a similar pattern.[35] Panel B of Table 6 reports the results. Column (1) confirms that the borrowing (debt

[34] Following previous research (Chaney, Sraer, and Thesmar (2012), and Carvalho (2018)), we include three components of firms' PPE in our definition of real estate assets: land and improvements, buildings, and construction in progress. Because of reporting requirements, we cannot obtain *net* values for these items during our sample period. However, in our sample period, we can still measure these items at historical cost (*fatp*, *fatb*, and *fatc*), and measure their share in firms' PPE using values at their historical cost. While this can introduce some measurement error in *RERatio* if real estate assets have systematically different depreciation rates than the rest of PPE, such measurement error should have a more limited influence on the construction of these broad groups.

[35] We focus on changes on firm borrowing (net debt issuance), as these within-firm changes can be analyzed for a broad range of firms and connected to different shocks.

issuance) of real-estate intensive firms differentially increases in response to regional real-estate price shocks, predicted combining regional differences in land availability with aggregate fluctuations in real-estate markets. Columns (2) to (5) then show that a range of alternative positive shocks to economic conditions do not lead to this differential pattern. For example, we consider predicted changes in state employment, constructed using the share of each industry in the state combined with fluctuations in the national-level employment growth for each industry (i.e., the Bartik instrument). Across all specifications, we find that broad improvements in economic conditions are not associated with a greater increase in the borrowing of real-estate intensive firms. This pattern is only present for changes in real estate prices. This suggests that, even if present, a link between local experiences and lender beliefs about alternative economic conditions would not have a stronger effect on real-estate intensive firms.

We also connect our findings more closely to the evidence from expectations data in KZ, and focus on the main patterns that they document. They find that these personal experience effects on beliefs are domain specific, e.g. local employment experiences do not affect beliefs about real-estate prices. We analyze this idea by incorporating the effect of officers' local employment growth experiences in our analysis. If our results are driven by beliefs about real estate values, personal experiences with local employment should not lead to similar effects. Specifically, we now include the quarterly employment growth in officers' counties in the four quarters prior to loan origination. We also include MSA and matched officer employment growth variables. These variables are all constructed in an analogous way to our local housing price growth variables, which are now also constructed at the county level. Panel A of Table 7 reports the results. We analyze both the average effects of these local experiences across borrowers as well as their differential effect on real-estate intensive firms. The results show that positive recent local employment experiences are not significantly associated with lower spreads in both of these cases. Moreover, in both cases, the results of local housing price growth experiences remain significant as we implement our analysis at the county level and control for employment experiences. This supports the prediction that our results are uniquely driven by experiences with real-estate price growth.

Another main finding of KZ is that individuals do not rely more on local experiences when they are more informative about aggregate outcomes. We verify this finding in our setting by examining whether our results are stronger when local officer experiences are more likely to predict borrower conditions. We consider the following scenarios. Officer local conditions should be more informative about a borrower when the borrower's industry is well represented in the local area (*Ind. Representation*). Local experiences that are highly correlated with national conditions can contain more information regarding borrowers, which are large, remotely located corporations. Additionally, local conditions are more likely to be informative signals about borrower fundamentals when these signals are less volatile. We capture these ideas using the correlation between local and national housing price growth (*HP Correlation*) as well as the volatility of local housing price growth (*HP Volatility*), both measured prior to loan origination. Panel B of Table 7 reports the results, which show that our results are not significantly stronger when local conditions are more likely to predict borrower conditions. While effects seem larger when local conditions have lower volatility and greater correlation with national conditions, the interaction is statistically insignificant and economically small. These analyses confirm that our findings follow the key patterns documented on the effect of local experiences on beliefs about national real estate prices. Taken together, our collective evidence suggests that our results are driven by changes in lender beliefs about real estate values.

3.5. Additional Evidence on the Role of Loan Officers

We analyze if our results are stronger when it is plausible to expect officers from lead banks to matter more for the pricing of the loans they originate. When implementing this analysis, we build on our discussion in Section 1.3. Table 8 reports the results. The importance of the officers in our data should be greater when there is less public information about borrowers, as there is greater information asymmetry between borrowers and lenders. When this is the case, the lead bank plays a more relevant role in monitoring and screening borrowers (Sufi (2007)) and should be more important in evaluating risks and pricing the loan. Consequently, differences in beliefs by lead banks can matter more.[36] Additionally, when this information asymmetry is more pronounced, lending relationships between lead banks and borrowers are stronger (Bharath et al. (2011)), what also increases the room for lead banks to shape loan pricing. We analyze this idea by estimating the differential importance of our results for firms that are smaller and have less analyst coverage. Consistent with the view that lead arrangers are more important in such loans and need to have

[36] The information asymmetry we are considering is not necessarily about the value of the borrower's real estate assets. Our argument is that, when this issue is more relevant, lead banks will have more discretion in determining the risk in the loan and pricing, leading to a greater influence by them on this decision in general.

more “skin in the game”, we find in the Internet Appendix (Table IA.11) that lead banks hold a larger share of the loan when borrowers have these characteristics. Columns (1) and (2) of Table 8 show that our loan pricing results are also significantly stronger for these borrowers.

We then consider the number of lead arrangers that the borrower has used in the past (*NPreviousLenders*). As the borrower has less access to alternative lenders, lead banks should have greater bargaining power to set loan spreads in the loan. Column (3) confirms that our results are significantly stronger when the borrower has used fewer lead arrangers in the past. This supports the view that our results are stronger when loan officers’ decisions are less disciplined by market forces, a pattern also documented by Dougal et al. (2015) in the context of anchoring biases in lenders’ decisions. Relatedly, our effects should be stronger when lead banks are predicted to hold a larger portion of the loan and rely less on other lenders to fund it. We predict this share using the average share for the lead bank on other loans with the same structure, i.e. same number of lead banks and number participant lenders. Column (4) also confirms that this is the case and show that this effect is economically important. One issue with this last result is that the number of participants is determined during the allocation of loan shares by lead arrangers. We also estimate the differential importance of our results for loans with a single lead bank. In the Internet Appendix, we show that the presence of a single lead is associated with a large increase in the loan share of the officer’s bank. Column (5) of Table 8 then shows that our results are differentially more important when there is a single lead bank, and that this difference is economically large with a magnitude comparable to our average results.

Previous research suggests that older individuals are less likely to overweight recent experiences or data when forming beliefs (Greenwood and Nagel (2009), Malmendier and Nagel (2016)). Motivated by this evidence, we also consider the importance of officer age. Consistent with these ideas, column (6) shows that our results are mostly relevant for younger loan officers. This provides additional support for the role of loan officers in our results.

4. Alternative Explanations

We interpret our results as capturing the effect of lenders’ personal experiences on loan spreads. Here, we further address concerns that our findings might be plausibly explained by alternative considerations, including the role of borrower fundamentals, officer wealth effects, bank conditions, and alternative officer characteristics.

A first concern is that local experiences could capture borrowers' fundamentals. One possibility is that local conditions in officers' neighborhoods capture valuable information for predicting their borrowers' credit risk. Given our identification strategy (see Section 2), this concern will only be relevant if officers' idiosyncratic conditions within their state predict the fundamentals of non-local borrowers. We note that Panel B of Table 7 documents that our main results are not more important when local officer conditions are more likely to be informative about borrower fundamentals (see Section 3.4). We further address this possibility by removing cases where the distance between borrowers' headquarters and their loan officers' properties is at the bottom quartile of our sample.[37] We also exclude cases where borrowers' industries are highly represented in the local areas surrounding loan officers' properties (top quartile in our sample). Intuitively, when industries are under-represented in the officers' local areas, there is less scope for local conditions in these areas to reflect news about borrowers' industries.[38] Panel A of Table IA.12 reports the results using these alternative samples, which remain similar to our main results (see the Internet Appendix).

These additional analyses reinforce the view that our results are unlikely to be explained by a link between officers' local experiences and news about borrower fundamentals. Our findings from Panel B of Table 7 also further address a related possibility that officers have fully rational expectations but, when forming beliefs about borrower fundamentals, rely on their local experiences because of costs associated with information acquisition. This narrative predicts that officers should rely more on local conditions when these conditions provide more informative signals about borrowers. Another important challenge for this explanation is the fact that it implies significant costs for the acquisition of information about other neighborhoods in the same state.[39]

[37] This sample cut ensures that the minimum (average) distance between borrowers and officers' properties is 250 (1,124) miles, making it unlikely that the location of officers' properties within a state captures geographic proximity to their borrowers. We find similar results with alternative cutoffs.

[38] Of course, it could be the case that local conditions are not directly informative about the borrower's industry but informative about related industries (e.g., suppliers) that matter for the borrower's industry. To the extent that such industry signals drive our results, it is natural to expect direct signals about the borrower's industry to matter, which is the prediction that we consider here.

[39] The average loan amount in our sample is $750 million and our results imply changes in spreads of 14 basis points. This leads to an average dollar effect of around $1 million. In this narrative, costs of acquiring local information need

A related possibility is that recent local conditions predict differences in spreads because they affect lenders' choice of borrowers. If this selection effect drives our results, we should expect our findings to become significantly weaker after the inclusion of important controls for borrower credit risk. Panel B of Table IA.12 shows that our results remain economically similar as we drop and add important controls for credit risk (see the Internet Appendix). For example, our results remain similar in specifications where we include *DDRank* × year fixed effects, and only compare loans within a same distance-to-default quintile and year. Our results also remain stable as we drop the borrower credit risk controls used in Table 2 or include additional related controls. This evidence suggests that selection effects are not important in shaping our results.

Another potential concern is that the effect of housing price shocks on credit spreads that we capture may be explained by fluctuations in officers' wealth. As discussed in Section 2, our results could capture the effect of personal experiences with housing wealth on lenders' beliefs. In contrast, the concern here is that officers' wealth affect loan spreads by shaping the incentives of officers with rational expectations due to agency problems inside banks (simple wealth effect). It is difficult to reconcile this wealth effect with some of our results. First, our results are only important for firms that own significant amounts of real-estate assets (Panel A of Table 6). Moreover, these effects are not significantly more pronounced when local housing prices are more indicative of future wealth levels, i.e., when housing prices are less volatile (Panel B of Table 7). Additionally, in theory, is unclear why such wealth effects would be concentrated on riskier loans (Table 5). More importantly, local growth in the most recent period (quarter or year) is more influential on credit spreads than previous periods. From a pure housing wealth perspective, it should not matter for loan officers *when* their home prices increased within the recent past (conditional on a same increase).[40]

One concern remains that the most recent housing price growth may be more predictive of future prices and thus officer wealth than previous growth. We assess this argument by examining the correlation between future housing prices and past price appreciation in a loan officer's neighborhood. To do so, we regress future housing prices in a loan officer's neighborhood on the

to have this magnitude. In contrast, if lenders are exposed to biases because they rely on intuition when making decisions, these costs have to be balanced against the potential benefits from using intuition in these decisions.

[40] On the other hand, a discussed in Section 3.1, it is plausible for officer personal experience effects to be the strongest and only detected in the data in the most recent period.

previous two-year history of housing price growth in the same neighborhood (quarters -8 to -1), controlling for state-year fixed effects, as well as housing price growth in officers' MSA and matched neighborhoods as in our main results. Columns (1) and (2) of Table 9 show the results, which document that local housing growth in the last quarter (quarter -1) is not a stronger predictor of future local prices than local housing growth in the previous seven quarters (quarters -2 to -8). This indicates that an officer influenced by simple wealth effects that has rational expectations should not react more strongly to housing price shocks to in the last quarter than to shocks occurring in the previous quarters. In column (3) of Table 9, we then document that officers' local growth in the last quarter does have a differential effect on loan spreads. This differential last-quarter effect has a similar magnitude to the one in our main results. This analysis suggests that simple wealth effects are unlikely to drive our results.

An additional possibility is that our results are explained by changes in bank fundamentals. If banks' loan portfolios are concentrated in areas near the properties of their loan officers, shocks to housing prices near officers' properties could reflect changes in the balance sheet or performance of their banks. However, as explained in Section 2.3, our main results include controls for housing price in matched bank-region neighborhoods, and capture the effect of idiosyncratic officer conditions within a same bank and Census region. Moreover, our results are not sensitive to the inclusion of this control (see Tables 2 and 3), suggesting that such a link between officer local conditions and bank fundamentals is unlikely to shape our results. We also note that our results are not concentrated during the financial crisis (Table 4), when loan losses were most closely tied to housing price movements during our sample period. In Table 10, we provide two additional sources of evidence against this possibility. Panel A of Table 10 replicates our results excluding the larger officer states in terms of area (e.g., Texas and California). In order to drive our results, this link between officer local housing price growth and banks' conditions has to take within a state. As we focus on states with smaller area, this possibility becomes less plausible. In Panel B of Table 10, we estimate our main results with the addition of bank × year fixed effects and bank lending controls, which measure the total lending amount by the bank across all loans in Dealscan during the loan's quarter and the average spread across these loans. Across these approaches, our results remain statistically significant, economically similar, and typically become stronger. These findings provide additional evidence against the role of bank fundamentals in explaining our results.

Finally, we also note that our results are unlikely to be explained by alternative officer characteristics potentially correlated with their recent local housing price growth. In principle, officers with different characteristics can sort into local areas with different growth rates, and our results could reflect a link between spreads and these characteristics.[41] We addressed this concern in Section 3.2 by showing that there is no link between spreads and officer local growth immediately after loan origination. It is unlikely that these selection effects would disappear when we examine the growth in officers' neighborhoods right after loan origination (see Section 3.2).

5. Additional Evidence and Robustness

We provide additional evidence that complements our main findings as well as examine the robustness of our main results to alternative control variables and sample structures. We briefly outline some of these analyses here and defer a more detailed discussion to the Internet Appendix. First, we examine the effect of officer local experiences on additional outcomes of the loans in our data. We provide evidence that more positive officer housing price growth is associated with larger loan shares for officers' lead banks and less strict loan covenants, but these effects are economically weaker than our loan spread effects and are not statistically significant. These patterns are consistent with the view that, given the data and mechanism we analyze, our results should be particularly strong for loan spreads. Data limitations prevent us from measuring the share of syndicated loans allocated to officers' lead banks for most loans. This share has to be predicted using the loan structure (number of participants and leads) and this introduces measurement error on this outcome.[42] Additionally, in theory, increased optimism about the value of the assets in firms' balance sheets have a clearer connection with loan spreads (increased creditor protection against default) than with covenant strictness. We note that distortions in the pricing of credit risk and excessive fluctuations in credit spreads play a central role in narratives and models of distorted lender beliefs and credit cycles (e.g., Bordalo, Gennaioli, Shleifer (2018)). Additionally, previous research on credit cycles has relied on credit spreads to capture shifts lender optimism across the credit cycle (e.g., López-Salido, Stein, and Zakrajšek (2017), Mian, Sufi, and

[41] As we include officer fixed effects in our results, these officer characteristics would need to change over time and be systematically different in periods where a same officer had recently experienced more positive growth.

[42] As discussed in Section 1.3, the lead bank holds a share of the loan but plays an important role evaluating risks and setting the spread for the entire loan. Idiosyncratic shocks to the optimism of lead banks could increase the size of this share.

Verner (2017)). Our findings on loan spreads are directly connected to these important ideas and empirical patterns. Second, we also incorporate a sample of officers that are renters, and differentiate between primary and secondary properties owned by officers. Our results are robust to analyzing the local housing experiences of both renters and property owners together as well as to focusing only on primary properties owned by officers. However, because of data limitations, we cannot analyze the differential importance of local housing experience effects across these subgroups.

6. Potential Aggregate Implications

As a final step in our analysis, we examine the potential implications of the results above for aggregate fluctuations in loan spreads. We interpret our results as estimating how a component of officers' housing price growth experiences, which should not be relevant in an objective model for the pricing of borrowers' loans, affects this pricing. In our empirical results, we focus on irrelevant growth experiences that are idiosyncratic to the loan officer for identification purposes. These specific experiences might not have aggregate implications. However, if there are systematic shifts on these uninformative growth experiences by lenders, such experiences could help amplify aggregate movements in loan spreads. We present a simple framework to illustrate this possibility, and combine this framework with data on housing price growth experiences to analyze the potential magnitude of these effects during an event such as the last financial crisis. We describe here our set up, main results, and intuitions. We provide additional details in the Appendix.

The starting point of our analysis is the idea that, conditional on current real-estate prices, the last-period growth for these prices before loan origination should not significantly predict borrowers' future real estate values. If lenders respond to this uninformative recent growth, they become overly optimistic (pessimistic) after periods with high (low) recent real-estate price growth. Of course, there is evidence that future housing price growth can be forecasted using past growth (e.g., Titman, Wang, and Yang (2014)). However, over the time horizons here analyzed (average loan maturity is 4.4 years), this predictability should take the form of a reversal in housing price growth that would lead us to underestimate the effects here analyzed.

More formally, suppose loan officer i issues a loan with spread y_{it} at quarter t. Officers are matched with specific neighborhoods and the local real estate price (in logs) in these neighborhoods is denoted by p_t^i. We denote the national real-estate prices (in logs) as P_t. National and local real-estate price growth are given by $G_t = P_t - P_{t-1}$ and $g_t^i = p_t^i - p_{t-1}^i$, respectively. We assume that future values of G_t cannot be predicted using its past values or past information from local real-estate price growth. This condition captures the lack of predictability of future price growth. We consider a benchmark objective model for loan pricing that would take place under rational expectations and denote the loan spread under this model as x_{it}. In this model, the credit risk of non-local borrowers is shaped by future national real-estate prices and future values of A_t, which represent other national conditions orthogonal to real-estate prices, i.e. past real-estate prices (national and local) do not predict future values of A_t.[43] Information available to lenders when pricing loans at time *t* does not matter for loan pricing in this model if this information does not help predict future values of P_{t+k} and A_{t+k}. We label such information as (objectively) irrelevant information.

We are at the start of $t = 1$ and focus on the response of credit spreads at $t = T$ to a sequence of future shocks observed between $t = 1$ and $t = T$, a period that we interpret as part of a credit boom or bust. Imagine a lender determining spreads after observing $\{G_t\}_{t=1}^T$. Note that we can decompose this sequence of shocks as $G_t = \bar{G} + N_t$, where $\bar{G} \equiv (1/T)(\sum_t G_t)$ is the average (realized) growth during the period and $N_t = G_t - \bar{G}$ are deviations from this average growth. In this context, any sequence of shocks $\{G_t\}_{t=1}^T$ can be represented as the shocks $\{\bar{G}, N_2, N_3, \dots, N_T\}$. A same cumulative growth over the entire period can take place in different ways depending on the timing of this growth, which is captured by $\{N_2, N_3, \dots, N_T\}$. A key point is that the timing of the past growth does not help forecasting future prices and is irrelevant information. For example, imagine two countries that start with the same initial price P_0 and experience the same average growth ($\bar{G}$) up to $t = T$. This growth determines their real-estate prices $P_T = P_0 + \bar{G}T$ at the time of loan origination. But these countries could have achieved this cumulative growth in different ways, due to shocks to the timing of their growth, e.g. one country might have a higher component of its growth in the last period (N_T). Because of the lack of predictability of future real-estate price growth, this timing is irrelevant for forecasting future deviations from the price at $t = T$.

[43] One way to interpret this set up is to imagine that there are two types of alternative factors: one type that moves together with real-estate prices and another that is independent from real-estate prices and is captured by A_t. One can interpret the effect of real-estate prices on the pricing of loans as also incorporating the first factor above.

We model national real-estate price growth as an aggregation of local growth across officers' neighborhoods: $G_t = \sum_i w_i\, g_t^i$, where w_i are weights associated with each area. Officers observe all past shocks across different neighborhoods. We note that we can decompose officers' own local growth as:

$$g_t^i = \beta_0^i \bar{G} + \beta_1^i N_t + \epsilon_t^i,$$

where ϵ_t^i is an idiosyncratic residual uncorrelated with $\bar{G}$ and N_t that aggregates to zero, i.e. $\sum_i w_i\, \epsilon_t^i = 0$. One can represent the history of price shocks across neighborhoods as the combination of the previous aggregate shocks and the distribution of ϵ_t^i across areas. Here, the key point is that $n_t^i = \beta_1^i N_t + \epsilon_t^i$ captures the uninformative component for the past growth in each area. A first part of this term is given by the purely idiosyncratic officer shocks ϵ_t^i, which capture the idiosyncratic officer conditions in our empirical analysis. However, there is also the exposure of local conditions to uninformative aggregate shocks ($\beta_1^i N_t$). In contrast to idiosyncratic shocks, the aggregate value of this last component is not zero and given by $\sum_i w_i \beta_1^i N_t = N_t$.

Building on our empirical results, we assume that officers are influenced by this uninformative component of their local growth because of personal experience effects, leading to a distortion in loan pricing relative to the previous benchmark.[44] Specifically, we assume that $y_{iT} = x_{iT} + \theta n_T^i$, where n_T^i is the uninformative component of their local growth in the last period and θ captures the local personal experience effects we analyze. We focus on the aggregate distortion induced by these effects: $AD_T = \sum_i w_i (y_{iT} - x_{iT})$. This aggregate distortion is given by $AD_T = \theta N_T$. Intuitively, these aggregate distortions are determined by the aggregate shocks N_t to officers' uninformative experiences.

When the national real estate prices experience a sequence of increasingly positive or negative shocks, these aggregate distortions are particularly relevant and can amplify a credit boom or bust. Intuitively, higher (lower) recent growth is not more informative about future conditions but leads to more positive (negative) recent experiences. We illustrate the potential magnitudes of this effect by considering the sequence of bad aggregate housing price growth shocks associated with the last financial crisis. We follow the timing of the crisis in Kahle and Stulz (2013) and use the previous

[44] Our results provide direct evidence that lenders respond to the idiosyncratic component of their uninformative local growth (ϵ_t^i). The key assumption we are making here is that they would respond in a similar way to other uninformative shocks to their local real-estate price growth ($\beta_1^i N_t$).

expression to compute the distortion in aggregate response of loan spreads to the crisis in each quarter between 2007Q3 and 2010Q1. In each quarter T, we use the expression $AD_T = \theta N_T$ to compute the implied distortion, where N_T measures the gap between the last-quarter growth and the realized growth since the start of the crisis. We use the average value housing price across all officer neighborhoods (20 miles) in our sample as our empirical measure for G_t. The sequence of negative shocks to this aggregate growth implied an average value of -0.76% for N_T across these quarters. We use the average effect between columns (1) and (2) in Panel A of Table 2 (9.96) as our estimate for θ. This implies an average distortion equal to -0.76% × 9.96 × 213 = 16 bp, where 213 is the average spread in Table 1. We compare this predicted effect to the realized increase in aggregate spreads between the crisis and the previous 11-quarter period (2007Q2 and 2004Q4). This realized increase equals 107.5 bp, and is computed using the universe of Dealscan-Compustat loans originated during these periods. Therefore, the effects here analyzed represent 16/107.5 = 15 percent of the realized change in aggregate spreads.[45]

7. Conclusion

Do excessive fluctuations in lender optimism help amplify changes in credit spreads across the credit cycle? We study the role of lenders' personal economic experiences as a mechanism driving such effects. Using unique data on the location of the real estate properties of loan officers originating large corporate loans, we provide evidence that lenders overweight their recent, local economic experiences and this helps shape credit spreads. We capture officers' local experiences using the housing price growth around their real estate properties prior to loan originations. Our analysis links differences in these experiences across officers within a same state and time to the loan spreads of non-local borrowers. This approach is motivated by previous evidence on the role of these local real-estate experiences in the formation of beliefs about national real-estate prices as well as identification considerations. We find that higher recent growth in officers' neighborhoods leads to significant reductions in loan spreads that are concentrated on borrowers with significant

[45] An important condition for these effects is the fact that personal experience effects are stronger in the most recent periods, i.e. there is a recency bias in personal experience effects. As in our empirical results, the previous calculation assumes that personal experience effects for past periods are not significant. In contrast, if personal experience effects were equal to θ in all periods since the shock, the aggregate distortion would be $AD_T = \theta(\sum_t N_t) = 0$. Intuitively, if personal experiences are equally shaped by growth in all periods, they are shaped by the cumulative growth, which is informative. Distortions come from the fact that these different growth periods are treated differently. This role of a recency bias in personal experiences in driving an overreaction to shocks is also emphasized by Malmendier, Pouzo, and Vanasco (2020).

real-estate ownership and riskier loans. Our evidence suggests that these effects are driven by lenders' beliefs about real estate values and can have significant aggregate implications

Overall, we provide support for the importance of a specific micro-level mechanism inducing excessive fluctuations in lender beliefs and loan spreads across the credit cycle. Our findings also offer new evidence on the importance of such excessive fluctuations in the pricing of credit risk in the first place. In the mechanism we document, reductions on average loan spreads are associated with a neglect of credit risk and a drop on the relative borrowing costs for riskier firms. Taken together, these findings contribute to our understanding of important patterns on credit spreads during credit cycles. For example, this mechanism can help rationalize the notion that spreads appear to be "too low" or "too high" during parts of the credit cycle and the idea that spreads seem to overreact when credit booms turn into busts. An important feature of this mechanism is the central role of sophisticated lenders originating large loans, which contrasts with a view that excessive shifts in optimism in credit markets should be driven only by less sophisticated investors. The importance of personal experiences in this mechanism can also help explain which lenders are more or less affected by these considerations.

The mechanism we document could also have important implications for real economic activity. These personal experience effects could lead to an amplification of economic volatility during credit cycles by increasing the volatility of borrowing costs or by inducing greater risk taking in credit markets during booms. Understanding in greater detail the broader implications of this mechanism for aggregate fluctuations in credit markets and economic activity are important areas for future research.

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Appendix: Quantification Analysis

We provide additional details on the framework used in Section 6. The new information available for lenders between $t = 1$ and $t = T$ is denoted by I_T and given by all local price growth $\{g_t^i\}_{t=1}^T$ across the areas for all officers $i \in I$, in addition to the sequence of other national conditions $\{A_t\}_{t=1}^T$. We denote these two information subsets using IG_T and IA_T, respectively. We model aggregate price growth as $G_t = \sum_i w_i\, g_t^i$ and define $\bar{G} \equiv (1/T)(\sum_t G_t)$ and $N_t \equiv A_t - \bar{G}$. We capture the idea that A_t represents alternative factors shaping credit risk with the assumption that $E(A_{T+k}|IG_T,\ IA_T)$ does not depend on IG_T, i.e. it is not predicted by price growth information. The lack of predictability of future aggregate price growth using past growth information is captured by the assumption that $E(G_{T+k}|IG_T,\ IA_T)$ does not depend on IG_T. Our assumption on irrelevant information is that an information subset IZ_T of I_T does not matter for the pricing of credit risk in the objective model if both $E(A_{T+k}|I_T)$ and $E(P_{T+k}|I_T)$ do not depend on IZ_T. This captures the notion that future national price and non-price conditions determine the credit risk of non-local borrowers. The fact that $\{G_t\}_{t=1}^T$can be represented using the shocks $\{\bar{G}, N_2, N_3, \ldots, N_T\}$ follows from the fact that there is a one-to-one mapping between these two sets of random variables. To see that the information $\{N_2, N_3, \ldots, N_T\}$ is irrelevant, first note that this information is determined by IG_T and does not predict A_{T+k} and G_{T+k}. Since $P_T = P_0 + \bar{G}T$ and P_0 is fixed, the value of P_T is fully determined by $\bar{G}$. Additionally, note that $P_{T+k} = P_T + G_{T+1} + \cdots + G_{T+k}$. From the previous facts, it follows that $E(P_{T+k}|I_T)$ does not depend on $\{N_2, N_3, \ldots, N_T\}$. Hence, both $E(A_{T+k}|I_T)$ and $E(P_{T+k}|I_T)$ do not depend on this information subset, which is irrelevant. We define ϵ_t^i as the residual of a linear projection of the random variable g_t^i on a constant, $\bar{G}$, and N_t, where the inner product between two random variables is given by $< X, Y > = E(XY)$. From this definition, it follows that $g_t^i = \gamma^i + \beta_0^i \bar{G} + \beta_1^i N_t + \delta_t^i$, where δ_t^i and $\epsilon_t^i = \delta_t^i + \gamma^i$ are uncorrelated with $\bar{G}$ and N_t. Since $G_t = \sum_i w_i\, g_t^i = \bar{G} + N_t$, it follows that $\sum_i w_i \epsilon_t^i = 0$ and $\sum_i w_i\, \beta_1^i = 1$. Given the knowledge of $\{\beta_0^i, \beta_1^i\}$ for all $i \in I$, there is a one-to-one mapping between IG_T and $\{\bar{G}, N_2, N_3, \ldots, N_T, IL_T\}$, where IL_T denotes the combined values of $\{\epsilon_t^i\}_{t=1}^T$across all $i \in I$. In other words, IG_T can be represented using the aggregate shocks $\{\bar{G}, N_2, N_3, \ldots, N_T\}$ combined with idiosyncratic shocks. Moreover, the previous expression describes the role of each of these shocks in determining g_t^i. A key result is that $\{N_2, N_3, \ldots, N_T, IL_T\}$ is irrelevant. We described above why $\{N_2, N_3, \ldots, N_T\}$ is irrelevant. Note that $\epsilon_t^i = g_t^i - \beta_0^i \bar{G} - \beta_1^i N_t$ and, conditional on $\bar{G}$, the information from g_t^i and N_t is not relevant for predicting P_{T+k}. This follows the same logic above, since $P_{T+k} = P_0 + \bar{G}T + G_{T+1} + \cdots + G_{T+k}$ and this information does not predict the values of G_{T+1} to G_{T+k}. This same information is also not relevant for predicting A_{T+k}. Hence, $\{N_2, N_3, \ldots, N_T, IL_T\}$ is irrelevant. This implies that the component $n_t^i = \beta_1^i N_t + \epsilon_t^i$ of officers' local growth is irrelevant in the objective model. Since $\sum_i w_i \beta_1^i = 1$ and $\sum_i w_i \epsilon_t^i = 0$, it follows that $\sum_i w_i \beta_1^i N_t = N_t$ and $\sum_i w_i n_t^i = N_t$. The aggregate distortion is then given by $AD_T = \sum_i w_i (y_{iT} - x_{iT}) = \sum_i w_i \big(\theta n_T^i\big) = \theta\, N_T$.

Table 1
Summary Statistics

This table presents the summary statistics for the main variables used in our study. The unit of observation is a loan-loan officer-state, where state refers to the state in which the loan officer's properties are located. The construction of this sample is described in Section 1. Panel A shows the summary statistics for loan contract terms. *Log of Spread* is the log of all-in-drawn loan spread over LIBOR. *Loan Maturity* is the log of the loan maturity (in months). *Loan Size* is the log of the total loan amount (in U.S. dollars). *Loan Type* is a discrete variable that indicates if the loan is a term loan or if the loan is a revolving line of credit. *Secured* is an indicator variable that equals one if the loan is secured. *RESecured* is an indicator that equals one for secured loans where the collateral is an asset class that includes real estate: all assets, PPE, or real estate. When constructing *RESecured1*, this measure is only constructed for loans that either have known collateral or are unsecured. In the case of *RESecured2*, we classify unknown collateral as real estate collateral (80 percent of known collateral includes real estate). Panel B shows the summary statistics for local housing price growth variables and the matched growth variables. Adjusted variables equal the original variable minus its mean in the officer state-loan year. These variables are described in Section 1.5. Panel C shows the summary statistics for borrower characteristics, which are all measured in the year prior to the loan origination. *Equity Volatility* is the annualized standard deviation of daily stock returns. *Size* is the log of total assets (*at*). *Age* is the number of years since the firm first appeared in the Compustat database. *Profitability* is the ratio of operating income (*oibdp*) to total assets (*at*). *Tangibility* is the ratio of property, plant, and equipment (*ppent*) to total assets (*at*). *M/B* is calculated using the following expression: *M/B* = (Stock price (*prcc*) × shares outstanding (*csho*) + total assets (*at*) – book equity (*ceq*))/total assets (*at*). Leverage is the ratio of long-term debt (*dltt*) plus current debt (*dlc*) to total assets (*at*). *Rated* is an indicator that equals one if the firm has a long-term bond rating. *Distance-to-Default* is the Merton (1974) distance to default, estimated using the approach in Bharath and Shumway (2008). *RERatio* measures the borrower's ratio of real estate assets to total PPE (measured at historical costs, see Section 3.4 for details). Panel D shows summary statistics for measures of the distance between officers' properties and borrowers' headquarters. This distance (in miles) measures the distance between the zip code of the officer's property and the zip code of the borrower's headquarter.

	Mean	Median	Std. Dev.	Nobs
Panel A: Loan Terms				
Spreads (in bps)	212.658	200	128.726	1,574
Log of Spread	5.148	5.298	0.715	1,574
Maturity (in Months)	52.964	60	19.318	1,574
Loan Amount (in $Millions)	760.140	400	1,252.558	1,574
Secured	0.661	1	0.473	1,231
RESecured1	0.430	0	0.495	1,017
RESecured2	0.529	1	0.499	1,231
Panel B: Local Housing Price Growth Variables - 20 Mile Radius				
	Mean	Median	Std. Dev.	Nobs
Local HPGrowth (Quarter -1)	-0.002	-0.005	0.019	1,574
Local HPGrowth (Quarter -1) - Adjusted	-0.000	0	0.006	1,574
Local HPGrowth (Quarter -2)	-0.001	-0.005	0.020	1,574
Local HPGrowth (Quarter -2)- Adjusted	0.000	0	0.007	1,574
Local HPGrowth (Quarter -3)	-0.000	-0.004	0.020	1,574
Local HPGrowth (Quarter -3)- Adjusted	0.000	0	0.007	1,574
Local HPGrowth (Quarter -4)	0.000	-0.003	0.021	1,574
Local HPGrowth (Quarter -4)- Adjusted	0.000	0	0.006	1,574
Local HPGrowth (Year -1)	-0.003	-0.023	0.076	1,574
Local HPGrowth (Year -1)- Adjusted	0.000	0	0.019	1,574
Local HPGrowth (Year -2)	0.005	-0.018	0.086	1,574
Local HPGrowth (Year -2)- Adjusted	-0.000	0	0.020	1,574
Own HPGrowth (Quarter -1)	-0.000	-0.001	0.023	1,562
MSA HPGrowth (Quarter -1)	-0.002	-0.007	0.019	1,565
Matched HPGrowth (Quarter -1) - State	-0.001	-0.004	0.018	1,548
Matched HPGrowth (Quarter -1) - Bank × Region	0.000	-0.004	0.018	1,338
Panel C: Borrower Charateristics				
	Mean	Median	Std. Dev.	Nobs
Size	7.897	7.757	1.596	1,574
Age	21.873	15.000	16.934	1,574
Equity Volatility (Annualized)	0.425	0.350	0.222	1,574
Tangibility (Net PPE/Assets)	0.340	0.239	0.290	1,574
Leverage	0.282	0.255	0.204	1,574
Profitability	0.130	0.119	0.095	1,574
M/B	1.751	1.463	1.003	1,574
Rated	0.625	1	0.484	1,574
Distance-to-Default	5.640	5.142	5.209	1,413
RERatio	0.235	0.199	0.212	1,141
Panel D: Loan Officer and Borrower Location Characteristics				
	Mean	Median	Std. Dev.	Nobs
Distance (Miles)	786.740	659.786	722.734	1,540

Table 2
Credit Spreads and Officers' Local Economic Experiences

This table reports results connecting corporate loan spreads to the recent local economic experiences in loan officers' neighborhoods. The results are based on the estimation of Equation (1). The sample is described in Table 1. The outcome variable is *Log of Spread*, the log of the all-in-drawn interest rate loan spread (in basis points) over the LIBOR. Panel A reports the results from our main specification. *Local HPGrowth (Quarter -1)* is the local housing price growth in the quarter before loan origination. *Local HPGrowth (Quarters -2 to -4)* estimates the average across the local growth coefficients between quarters -2 and -4 (see Section 1.5 for more details). *Past 1-Year Local HPGrowth* estimates the average across the local growth coefficients for the past four quarters. Past 2-year growth control includes *Local HPGrowth (Year -2)* as an additional control. Panel B reports results with additional controls for state-level conditions using finer fixed effects or more control variables. State condition controls include the state's employment growth in each of the four quarters prior to loan origination as well as the state's housing price growth (OFHE) in each of those quarters. Panel C reports the results from a falsification test, where we estimate the specifications in Panel A (columns (1) and (2)) with a different outcome variable, the log of the average loan spread for loans originated by matched officers. Matched officers include all officers with neighborhoods in the same state as the officer of interest (that do not overlap), and originating loans at the same time period. We compute the average of loan spreads issued by matched officers during the same 6-month window (“semester”), quarter, or 2-month window (“bimonthly”). All results control for the housing price growth in loan officers' MSAs in the four quarters prior to loan origination (*MSA HPGrowth (Quarter -k)*). These variables are constructed in an analogous way to officers' local growth variables using all the zip codes in the same MSAs as officers' properties that are outside the neighborhoods (10- or 20- radius) around these properties. We also control for recent histories of housing price growth prior to loan origination in matched officer neighborhoods. These controls are also constructed for the four quarters prior to loan origination in an analogous way to the previous MSA controls, but use different sets of neighborhoods. *Matched Officer Growth - State (Bank-Region)* includes all neighborhoods by officers present at some point in our sample in the same state (bank and region) as the officer of interest (see Section 1.5). All results include the following borrower and loan controls (defined in Table 1): *Loan Size*, *Loan Maturity*, *Equity Volatility*, *Size*, *Firm Age*, *Leverage, Profitability*, *Tangibility*, and *M/B.* We also include controls for the following characteristics of officers' neighborhoods: *Population*, *Average Home Value*, *Income per Household*, *Black Share*, and *Hispanic Share*. *Population* is the log of the population. *Average House Value* is the log of the average house value. *Income per Household* is the log of the income per household. *Black (Hispanic) Share* is the percentage of black (hispanic) population. All neighborhood characteristics are measured using information from the 2000 Decennial Census and average values across the properties of the officer in the state. Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Officers' Local Economic Experiences

	Outcome: Log of Spread			
	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius
	(1)	(2)	(3)	(4)
Local HPGrowth (Quarter -1)	-7.834***	-12.086***	-7.006***	-11.380***
	(2.307)	(3.319)	(2.374)	(3.288)
Local HPGrowth (Quarters -2 to -4) - Avg Effect	-0.345	-1.225	-0.361	-1.758
	(1.093)	(1.832)	(1.138)	(1.853)
Past 1-Year Local HPGrowth - Avg Effect	-2.217**	-3.940***	-2.022**	-4.164***
	(0.882)	(1.380)	(0.905)	(1.399)
MSA HPGrowth (Quarter -1)	2.481	5.067	0.959	4.328
	(4.443)	(4.682)	(4.288)	(4.732)
MSA HPGrowth (Quarters -2 to -4) - Avg Effect	-0.766	-0.082	-2.319	-0.355
	(1.944)	(1.860)	(2.374)	(2.066)
Past 1-Year MSA HPGrowth - Avg Effect	0.046	1.205	-1.500	0.815
	(1.586)	(1.286)	(1.973)	(1.434)
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes
Matched Officer Growth Controls - Bank × Region	Yes	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Past 2-Year Growth Control			Yes	Yes
Observations	1,500	1,316	1,500	1,316
R-squared	0.609	0.592	0.611	0.594

Panel B: Additional Controls for State Economic Conditions								
	Outcome: Log of Spread							
	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Local HPGrowth (Quarter -1)	-7.028***	-10.939***	-5.629**	-10.961***	-6.517**	-11.966**	-8.475**	-16.375**
	(2.257)	(3.294)	(2.586)	(4.039)	(2.772)	(4.758)	(4.259)	(6.791)
Local HPGrowth (Quarters -2 to -4) - Avg Effect	-0.463	-1.891	-0.755	-1.347	-1.642	1.004	-2.209	0.132
	(1.158)	(1.837)	(1.418)	(1.918)	(1.923)	(2.672)	(1.711)	(2.883)
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched Officer Growth Controls - Bank × Region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Condition Controls	Yes	Yes						
State × Year FE	Yes	Yes						
State × Semester FE			Yes	Yes				
State × Quarter FE					Yes	Yes		
State × Time (Bimonthly) FE							Yes	Yes
Observations	1,500	1,316	1,500	1,316	1,500	1,316	1,500	1,316
R-squared	0.611	0.595	0.666	0.661	0.705	0.716	0.728	0.748

Panel C: Do the Results Capture Officer Idiosyncratic Conditions?						
	Outcome: Log of Matched Officer Spread					
	Matched by State-Semester		Matched by State-Quarter		Matched Bimonthly by State	
	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius
	(1)	(2)	(3)	(4)	(5)	(6)
Local HPGrowth (Quarter -1)	0.114	-3.181	-0.506	-2.300	1.880	-1.983
	(2.004)	(2.441)	(2.528)	(3.905)	(3.102)	(4.120)
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes	Yes	Yes
Matched Officer Growth Controls - Bank × Region	Yes	Yes	Yes	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,289	1,084	1,176	967	1,037	845
R-squared	0.708	0.731	0.540	0.599	0.509	0.529

Table 3
Credit Spreads and Officers' Local Economic Experiences: Additional Evidence

This table reports results connecting loan spreads to officers' local economic experiences with different specifications. The outcome variable is *Log of Spread*, the log of the all-in-drawn interest rate loan spread (in basis points) over the LIBOR. Panel A examines the effects in Table 2 (Panel A) without controls for matched officer growth within the same bank-census region. Panel B reports results analyzing the link between loan spreads and officers' past and future experiences. The results are based on the estimation of the specification in Panel A and Table 2 (Panel A) with additional local experience variables. These additional variables capture officers' local housing price growth experiences in the four quarters after loan origination. *Local HPGrowth (Quarter +k)* is defined in an analogous way to *Local HPGrowth (Quarter -k)* for k = 1, 2, 3, 4. *Local HPGrowth (Quarters +2 to +4)* estimates the average across the local growth coefficients between quarters 2 and 4 after loan origination. *Future 1-Year Avg Effect* estimates the average across the local growth coefficients for the four quarters after loan origination. Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Results without Controls for Matched Officer Growth in Bank-Region

	Outcome: Log of Spread			
	(1)	(2)	(3)	(4)
	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius
Local HPGrowth (Quarter -1)	-7.193***	-10.124***	-6.508***	-9.734***
	(2.167)	(2.495)	(2.220)	(2.505)
Local HPGrowth (Quarters -2 to -4) - Avg Effect	-0.856	-2.252	-0.790	-2.307
	(1.030)	(1.725)	(1.069)	(1.699)
Past 1-Year Local HPGrowth - Avg Effect	-2.440***	-4.220***	-2.219***	-4.164***
	(0.828)	(1.274)	(0.836)	(1.249)
MSA HPGrowth (Quarter -1)	4.273	4.502	2.479	4.328
	(4.365)	(3.697)	(4.372)	(3.701)
MSA HPGrowth (Quarters -2 to -4) - Avg Effect	-1.950	0.261	-3.393	0.278
	(2.036)	(1.703)	(2.376)	(1.815)
Past 1-Year MSA HPGrowth - Avg Effect	-0.394	1.321	-1.925	1.290
	(1.513)	(1.250)	(1.909)	(1.327)
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Past 2-Year Growth Controls			Yes	Yes
Observations	1,574	1,539	1,574	1,539
R-squared	0.610	0.610	0.613	0.611

Panel B: Officers' Past and Future Local Experiences

	Outcome: Log of Spread			
	(1)	(2)	(3)	(4)
	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius
Local HPGrowth (Quarters -2 to -4) - Avg Effect	-0.623	-1.368	-0.788	-2.293
	(1.126)	(1.889)	(1.041)	(1.700)
Local HPGrowth (Quarter -1)	-7.761***	-12.100***	-7.431***	-10.636***
	(2.264)	(3.402)	(2.094)	(2.633)
Local HPGrowth (Quarter +1)	1.707	1.385	1.705	2.107
	(2.163)	(3.657)	(1.875)	(3.231)
Local HPGrowth (Quarters +2 to +4) - Avg Effect	0.616	0.781	0.705	1.708
	(0.869)	(1.358)	(0.857)	(1.373)
Past 1-Year Local HPGrowth - Avg Effect	-2.407***	-4.051***	-2.449***	-4.379***
	(0.836)	(1.329)	(0.804)	(1.193)
Future 1-Year Local HPGrowth - Avg Effect	0.889	0.932	0.955	1.807
	(0.815)	(1.212)	(0.802)	(1.128)
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes
Matched Officer Growth Controls - Bank × Region	Yes	Yes		
MSA HP Growth Controls	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Observations	1,500	1,316	1,574	1,539
R-squared	0.613	0.600	0.613	0.613

Table 4
The Effect of Loan Officer Economic Experiences in Subsamples

This table reports the link between loan spreads and officers' local economic experiences in subsamples. The outcome variable is *Log of Spread*, the log of the all-in-drawn interest rate loan spread (in basis points) over the LIBOR. The resuts are based on an extension of the specification in Table 3 (column (2) of Panel A), which now includes interactions of both *Local HPGrowth* and *MSA HPGrowth* variables with indicators for different subsamples. In each specification, the combined subsamples cover our overall sample and the sum of their indicators equals one. For example, *Crisis Year(2008 to 10)* and *Other Year (2007-, 2011+)* are indicators that equal one for loans originated between 2008 and 2010 and other years, respectively. Other crisis indicators are constructed in an analogous way. *PosGrowth (NegGrowth)* indicates if *LHP Growth (Quarter -1)* is positive (negative). Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Outcome: Log of Spread			
	20 Miles Radius	20 Miles Radius	20 Miles Radius	20 Miles Radius
	(1)	(2)	(3)	(4)
LHPGrowth (Quarter -1) × Crisis Year(2008 to 2010)	-11.304**			
	(5.182)			
LHPGrowth (Quarter -1) × Other Year (2007-, 2011+)	-9.024***			
	(3.257)			
LHPGrowth (Quarter -1) × Crisis Year(2007 to 2010)		-9.735**		
		(4.919)		
LHPGrowth (Quarter -1) × Other Year (2006-, 2011+)		-10.121***		
		(3.600)		
LHPGrowth (Quarter -1) × Crisis Quarters (2007Q3 to 2010Q1)			-11.568	
			(7.707)	
LHPGrowth (Quarter -1) × Other Quarters			-8.226***	
			(2.820)	
LHPGrowth (Quarter -1) × PosGrowth				-20.634***
				(7.199)
LHPGrowth (Quarter -1) × NegGrowth				-9.199***
				(2.938)
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Observations	1,539	1,539	1,539	1,539
R-squared	0.611	0.613	0.618	0.611

Table 5
Officers' Local Economic Experiences and the Pricing of Credit Risk

This table examines how lenders' personal experiences affect the link between loan spreads and borrower credit risk. The results are based on the estimation of Equation (2). The sample is described in Table 1. We use two measures for *CreditRisk*: *DDRank* and *DD-Demeaned*. These measures are based on the Merton (1974) distance-to-default (*DD*), estimated following the approach in Bharath and Shumway (2008). *DDRank* is the quintile ranking of a firm's *DD* in the Dealscan-Compustat universe loans originated in the same quarter. *DDRank* has values between 1 and 5, where 5 has the highest *DD*. *DD-Demeaned* is the average value of *DD* for all borrowers in the same quintile category minus the average of value of *DD* across all borrowers. These average values are computed using all Dealscan-Compustat loans in issued during the same quarter in the relevant group. The dependent variable of columns (1) and (2) is the log of loan spread. The dependent variable of columns (3) and (4) is the market adjusted log of spread, defined as the difference between the log of spread for the loan and the log of the market spread. Market spread is the weighted average of spreads across all loans in the Dealscan-Compustat universe issued during the same quarter as the loan of interest, with the weight being the loan amount. Scaled effects are calculated as the product of the interactive coefficient and the gap between the average value of the credit risk variable in the top 50% and bottom 50% of its distribution. We include the same set of controls as in Panel A of Table 3 (column (2)) but also include interactions of all *MSA Growth* variables with *CreditRisk* as additional controls. Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Outcome: Log of Spread		Outcome: Log of Spread, Market Adjusted	
	(1)	(2)	(3)	(4)
DDRank	-0.093***		-0.096***	
	(0.022)		(0.022)	
DD-Demeaned		-0.057***		-0.066***
		(0.014)		(0.014)
Local HPGrowth (Quarter -1)	-22.564***	-8.491***	-15.890***	-4.938*
	(5.173)	(2.566)	(5.294)	(2.600)
LHPGrowth (Quarter -1) × DDRank	6.457***		4.992***	
	(1.724)		(1.832)	
LHPGrowth (Quarter -1) × DD-Demeaned		4.143***		3.193***
		(1.131)		(1.209)
Scaled Effect	15.242***	13.147***	11.783***	10.132***
	(4.069)	(3.591)	(4.325)	(3.836)
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Observations	1,419	1,419	1,419	1,419
R-squared	0.654	0.651	0.584	0.585

Table 6
Are the Results Driven by Beliefs About Real Estate Values?

This table presents evidence on the role of officer beliefs about real estate values in driving our results. Panel A reports the analysis of interactions between our main results and borrower or loan characteristics that predict the relevance of this mechanism. In the case of each interaction variable Z, we estimate specifications analogous to Equation (2), where we replace *CreditRisk* with Z. The dependent variable is the log of loan spread. *RESecured* is an indicator that equals one for secured loans where the collateral is an asset class that includes real estate: all assets, PPE, or real estate. In column (1), this measure is only constructed for loans that either have known collateral or are unsecured. In column (2), we classify unknown collateral as real estate collateral (80 percent of known collateral includes real estate). *RERatio* measures the borrower's ratio of real estate assets to total PPE in the year prior to the loan (measured at historical costs, see the text for details). *REOwner* (1%) is an indicator that equals one if *RERatio* $\geq$ 1%. *REOwner* (5%) is defined in an analogous way. *High RERatio* is an indicator that equals one if *RERatio* is above its median value in the sample. We include the same set of controls as in Panel A of Table 3 (column (2)) but also include interactions of all MSA Growth variables with Z as additional controls. In addition to reporting the coefficients for these interactions, we also report their scaled values, where the coefficient on the interaction is multiplied by the gap between Z's mean in the top 50% and bottom 50% of its distribution. Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. Panel B examines the effect of alternative economic shocks on the subsequent borrowing of real estate-intensive firms, following the analysis in Carvalho (2018). We analyze how economic shocks between years t-5 and t (*Shock*) differentially predict the borrowing of real-estate intensive firms in the post period (years t+1 to t+5). The outcome variable is the change in firms' average net debt issuance (as percentage of their total assets) in the post period, relative to this same average in the period before the shock (years t-9 to t-5). We estimate a linear regression predicting this outcome with *Shock*, *RERatio,* and *Shock* × *RERatio*, in addition to industry fixed effects (3-digit SIC code) interacted with *Shock*, and year fixed effects. In column (1), *Shock* is the housing price growth in the MSA of the firm's headquarter (*HPGrowth*). We estimate this specification using an IV approach, which combines measures of geographic-determined land unavailability in the MSA (*Uland*) with the average housing price growth across MSAs (*NPGrowth* Specifically, we use *NPGrowth* × *Uland* × *RERatio* as an instrument for *HPGrowth* × *RERatio* and control for *NPGrowth* × *Uland*, *Uland* × *RERatio*, *NPGrowth* × *RERatio*, *Uland* and *RERatio*. In column (2), *Shock* is *StateEmpGrowth*, the employment growth in the firm's state (headquarter). We also use an IV approach where we instrument for this growth using the national-level employment growth for each industry combined with the initial share of the industry in the state. See the Internet Appendix for more details on these first stages. *GDPGrowth* is the national-level GDP growth. *StatePersonalIncomeGrowth* is the personal income growth in the firm's state (headquarter). *IndSalesGrowth* is the growth of the median sales in the firm's industry. All growth rates capture the cumulative growth between years t-5 and t. Standard errors are heteroskedasticity robust and double clustered at the state (headquarter) and year level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Differential Effects for Real-Estate Intensive Firms and Loans Backed by Real-Estate Collateral

	Outcome: Log of Spread					
	20 Miles Radius	20 Miles Radius	20 Miles Radius	20 Miles Radius	20 Miles Radius	20 Miles Radius
	(1)	(2)	(3)	(4)	(5)	(6)
LHPGrowth (Quarter -1)	0.731	-0.391	-2.394	2.481	-1.285	-2.938
	(3.845)	(3.883)	(4.187)	(5.573)	(5.330)	(3.454)
LHPGrowth (Quarter -1) × RESecured	-19.206***	-14.776**				
	(6.919)	(5.818)				
LHPGrowth (Quarter -1) × RERatio			-35.495**			
			(14.995)			
LHPGrowth (Quarter -1) × REOwner(5%)				-17.376**		
				(6.778)		
LHPGrowth (Quarter -1) × REOwner(10%)					-13.906**	
					(6.386)	
LHPGrowth (Quarter -1) × High RERatio						-14.256***
						(5.289)
Scaled Effect			-11.775**			
			(4.974)			
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	994	1,201	1,119	1,119	1,119	1,119
R-squared	0.686	0.653	0.664	0.667	0.663	0.665

Panel B: Effects of Different Economic Conditions on Debt Issuance by Real-Estate Intensive Firms					
	Outcome: ΔNet Debt Issuance				
	IV	IV	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)
RERatio × HP Growth	0.138***				
	(0.034)				
RERatio × StateEMPGrowth		-0.064			
		(0.075)			
RERatio × GDPGrowth			-0.081		
			(0.070)		
RERatio × StatePersonalIncomeGrowth				-0.069	
				(0.059)	
RERatio × IndSalesGrowth					-0.021*
					(0.012)
Industry FE × Shock	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	17,408	8,935	17,408	17,408	17,408
R-squared	0.058	0.077	0.062	0.063	0.062

Table 7
Role of Lender Beliefs About Real Estate Values: Additional Evidence

This table presents additional evidence on the role of officer beliefs about real estate values in driving our results. The outcome variable is *Log of Spread*, the log of the all-in-drawn interest rate loan spread (in basis points) over the LIBOR. Panel A shows our results in Table 3 (column (2) in Panel A) and Table 6 (column (3) in Panel A) including officers' local employment growth experiences prior to loan origination. In column (1), we replicate our results from Table 2 using the county in which officers' properties are located as their neighborhoods to construct all housing price growth variables. In column (2), we estimate the same specification as in column (1) but replace all housing price growth experiences with local employment growth experiences, constructed using officers' counties in the same way as in column (1). The results in column (3) extend the specification in column (1) to also include all employment growth variables used in column (2). In column (4), we estimate the results from Table 6 with two changes. First, we define all housing price growth variables using counties as officers' neighborhoods. Second, we include all the employment growth experiences in column (2) in a symmetric way to housing price growth variables (including their interactions with *RERatio*). Panel B reports the analysis of interactions between our main results and variables indicating the informativeness of local housing price growth. In the case of each interaction variable Z, we estimate specifications analogous to Equation (2), where we replace *CreditRisk* with *Z*. The dependent variable is the log of loan spread. *Ind. Representation* is constructed using the 3-digit NAICS of the borrower's industry, and is given by the ratio of this industry's employment share in the officer's county to its employment share at the national level. *HP Volatility* is the standard deviation of the quarterly local housing price growth rate in the officer's neighborhoods. This standard deviation is measured using all data prior to the loan origination. *HP Correlation* is the correlation between the quarterly local housing price growth rate in the officer's neighborhoods and the quarterly growth rate of the national housing price index. We include the same set of controls as in Panel A of Table 3 (column (2)) but also include interactions of all MSA Growth variables with *Z* as additional controls. In addition to reporting the coefficients for interactions of experience variables with *Z*, we also report their scaled values, where the coefficient on the interaction is multiplied by the gap between *Z*'s mean in the top 50% and bottom 50% of its distribution. Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Local Real Estate and Employment Experiences

	Outcome: Log of Spread			
	(1)	(2)	(3)	(4)
County HPGrowth (Quarter -1)	-4.744***		-5.146***	
	(1.568)		(1.602)	
County HPGrowth (Quarters -2 to -4) - Avg Effect	-0.120		-0.101	
	(1.112)		(1.097)	
County EMPGrowth (Quarter -1)		-2.302	-2.085	
		(1.558)	(1.416)	
County EMP Growth (Quarters -2 to -4) - Avg Effect		-0.825	-0.240	
		(1.449)	(1.371)	
County HPGrowth (Quarter -1) × RERatio				-38.420***
				(12.249)
Scaled Effect				-12.746***
				(4.064)
County EMPGrowth (Quarter -1) × RERatio				-3.709
				(9.179)
Scaled Effect				-1.231
				(3.045)
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes
MSA HP Growth Controls	Yes		Yes	Yes
MSA Employment Growth Controls		Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Observations	1,517	1,520	1,515	1,092
R-squared	0.606	0.594	0.607	0.677

Panel B: Are the Results Stronger When Local Conditions Are More Informative?			
	Outcome: Log of Spread		
	20 Miles Radius		
	(1)	(2)	(3)
LHPGrowth (Quarter -1) × Ind Representation	0.871		
	(1.061)		
LHPGrowth (Quarter -1) × HP Volatility		-65.432	
		(463.018)	
LHPGrowth (Quarter -1) × HP Correlation			-4.574
			(10.520)
Scaled Effect	3.260	-0.634	-2.409
	(3.969)	(4.485)	(5.539)
Matched Officer Growth Controls - State	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes
Observations	1,422	1,539	1,539
R-squared	0.639	0.612	0.615

Table 8
Additional Evidence on Role of Loan Officers

This table provides additional evidence supporting the role of loan officers from lead banks in driving our results. The outcome variable is *Log of Spread*, the log of the all-in-drawn interest rate loan spread (in basis points) over the LIBOR. The results analyze interactions between our main effects and variables indicating a greater role for these officers in shaping loan spreads. In the case of each interaction variable Z, we estimate specifications analogous to Equation (2), where we replace *CreditRisk* with *Z*. *Size* is the log of total assets. *Analyst Coverage* is the number of analysts covering the borrower. *NPreviousLenders* measures the number of lead arrangers that the borrower has used in the past. *BankLoanShare* is the predicted share of the officer's bank (lead bank) in the loan. This share is predicted using the average value of this share for lead banks across all Dealscan-Compustat loans in our sample period with the same structure, i.e. same number of participants and lead arrangers. *Single Lead* is an indicator that equals one if the officer's bank is the only lead arranger in the loan. *Officer Age* is the age of the loan officer (years). All borrower characteristics are measured in the year prior to the loan. These results use the same control variables as in Panel A of Table 3 (column (2)) but also include interactions of all MSA Growth variables with Z as additional controls. In addition to reporting the coefficients for interactions of experience variables with *Z*, we also report their scaled values, where the coefficient on the interaction is multiplied by the gap between *Z*'s mean in the top 50% and bottom 50% of its distribution. Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Outcome: Log of Spread					
	(1)	(2)	(3)	(4)	(5)	(6)
	20 Miles Radius	20 Miles Radius	20 Miles Radius	20 Miles Radius	20 Miles Radius	20 Miles Radius
LHPGrowth (Quarter -1) × Size	4.341***					
	(1.585)					
LHPGrowth (Quarter -1) × Analyst Coverage		0.729*				
		(0.378)				
LHPGrowth (Quarter -1) × NPreviousLenders			0.716**			
			(0.359)			
LHPGrowth (Quarter -1) × BankLoanShare				-0.437**		
				(0.214)		
LHPGrowth (Quarter -1) × Single Lead					-10.075*	
					(5.880)	
LHPGrowth (Quarter -1) × Officer Age						0.780**
						(0.314)
Scaled Effect	10.857***	8.694*	6.637**	-8.875**		9.828**
	(3.965)	(4.510)	(3.330)	(4.340)		(3.951)
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,539	1,427	1,459	1,539	1,539	1,530
R-squared	0.651	0.643	0.632	0.622	0.616	0.614

Table 9
Further Addressing Wealth Effects

This table reports results further addressing the concern that simple housing wealth effects may explain our main findings. We analyze the role of local officer experiences in the two years prior to loan origination (quarters -8 to -1), and explicitly contrast the importance of last-quarter effects. In columns (1) and (2), we examine whether the most recent housing growth of a loan officer's neighborhood is a stronger predictor of future housing prices in the same area when compared to past growth in the officer's neighborhood. We benchmark future price levels against the price level 2 years before loan origination, thus the outcome variables are *LHP Growth (Quarter +4 to -8)* and *LHP Growth (Quarter +2 to -8)*. We predict these outcomes using *LHPGrowth (Quarter -1)*, *LHPGrowth (Quarter -2 to -8)*, state × year fixed effects, as well as the controls for housing price growth in officers' MSA and matched neighborhoods used in our main results (Table 2). We estimate these results in our main sample and focus on the differential importance of this effect in the last period. In column (3), we examine whether officer local housing growth during the most recent quarter is a stronger predictor of loan spreads when compared to officer previous local housing growth. The specification is analogous to the one in Table 2 (column (2) of Panel A). The only difference is that we now measure all housing growth variables over these two periods of interest (quarter -1 and quarter -2 to -8). In each result, we report the difference between the estimated coefficients for *Local HPGrowth (Quarter -1)* and *Local HPGrowth (Quarter -2 to -8)*. Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	20 Mile Radius		
	(1)	(2)	(3)
	Outcome: LHP Growth (Quarter +4 to -8)	Outcome: LHP Growth (Quarter +2 to -8)	Outcome: Log(Spread)
Local HPGrowth (Quarter -1)	1.190***	0.978***	-11.566***
	(0.207)	(0.114)	(3.206)
Local HPGrowth (Quarter -2 to -8)	1.258***	1.169***	-0.520
	(0.044)	(0.026)	(0.974)
Local HPG (Quarter -1) - Local HPG (Quarters -2 to -8)	-0.068	-0.191	-11.046***
	(0.212)	(0.119)	(3.629)
Matched Officer Growth Controls - State	Yes	Yes	Yes
Matched Officer Growth Controls - Bank × Region	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes
Loan Type FE			Yes
Industry FE			Yes
Loan Officer FE			Yes
Observations	1,316	1,316	1,316
R-squared	0.790	0.909	0.593

Table 10
Are the Results Driven by Bank Fundamentals?

This table reports results further addressing the concern that bank fundamentals can explain our main findings. The outcome variable is *Log of Spread*, the log of the all-in-drawn interest rate loan spread (in basis points) over the LIBOR. Panel A reports results estimating the specification in Table 2 (columns (1) and (2) of Panel A) in a sample that excludes all officer states with a total area in the top tercile across states. In Panel B, we estimate our main results (column (2) in Panel A of Tables 2 and 3) with the addition of bank × year fixed effects and bank lending controls. Bank lending controls measure the total lending amount by the bank across all loans in Dealscan during the loan's quarter and the average spread across these loans. Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Excluding Large States

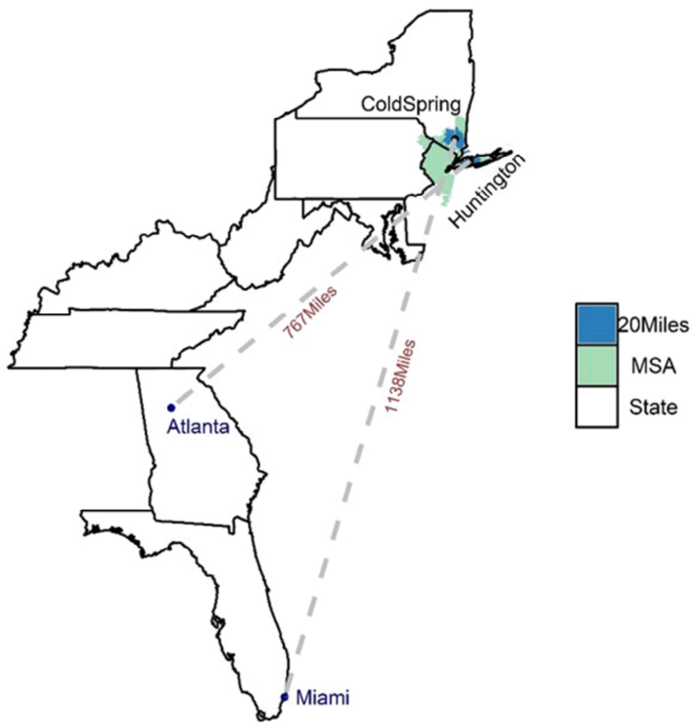
	Outcome: Log of Spread	
	Excluding Large States	
	10 Miles Radius	20 Miles Radius
	(1)	(2)
Local HPGrowth (Quarter -1)	-8.283***	-12.431***
	(2.552)	(3.587)
Local HPGrowth (Quarters -2 to -4) - Avg Effect	0.376	-1.085
	(1.264)	(2.183)
Matched Officer Growth Controls - State	Yes	Yes
Matched Officer Growth Controls - Bank × Region	Yes	Yes
MSA HP Growth Controls	Yes	Yes
Loan Type FE	Yes	Yes
Industry FE	Yes	Yes
Loan Officer FE	Yes	Yes
State × Year FE	Yes	Yes
Observations	1,226	1,044
R-squared	0.617	0.602

Panel B: Results with Additional Bank Controls

	Outcome: Log of Spread			
	(1)	(2)	(3)	(4)
	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius
Local HPGrowth (Quarter -1)	-8.018**	-9.021**	-7.305**	-12.732***
	(3.183)	(3.834)	(3.150)	(4.382)
Bank Lending Controls	Yes	Yes	Yes	Yes
Bank × Year FE	Yes	Yes	Yes	Yes
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes
Matched Officer Growth Controls - Bank × Region			Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Observations	1,477	1,446	1,418	1,256
R-squared	0.657	0.659	0.656	0.654

Figure 1
Identification Strategy: Example

This figure helps illustrate the identification strategy used in our empirical analysis. The figure shows an example of two officer neighborhoods in our sample that are located in the state of New York as well as the location of the borrowers' headquarters in the loans associated with these two officer neighborhoods.



Internet Appendix for
"Loan Spreads and Credit Cycles:
The Role of Lenders' Personal Economic Experiences"

Daniel Carvalho, Janet Gao, and Pengfei Ma

Abstract

This appendix provides additional information and analyses that complement the main results in the paper. Section 1 provides details on our data collection process. Section 2 provides anecdotal evidence that supports the discussion in our paper on the role of the loan officers we analyze. Section 3 presents the distribution of loan officer properties and borrower headquarters across states. Section 4 examines the link between aggregate housing growth experiences of loan officers and loan spreads. In Section 5, we extend a falsification test in the paper. Sections 6 and 7 examine the robustness of our results to various geographical ranges and subsample periods. Section 8 provides additional evidence on how officer personal experiences asymmetrically affect the pricing of riskier loans. In Section 9, we show that our cross-sectional results are robust to controlling for matched officer growth by bank and region. Section 10 reports the distribution of collateral types in our sample and across all Dealscan loans. Section 11 presents evidence that borrowers' real estate share is not associated with higher credit risk. Section 12 shows the first-stage results from our instrumental-variable analyses. Section 13 provides evidence on the connection between borrower and loan characteristics and the lead arranger share in syndicated loans. Section 14 further addresses the concern that borrower fundamentals could drive our results. Section 15 provides additional robustness checks regarding controls and sampling choices. Section 16 examines the effect of loan officer personal experiences on other, non-pricing loan terms and outcomes. Finally, Section 17 shows the robustness of our results to considering only primary residences, and to including renters.

1. Data Collection Process

We provide additional details on how we collect our data. Our initial sample identifies the names of loan officers by looking at the signatures of loan agreements (see Section 2). In our initial sample, we have the loan officer's name, affiliation (bank) when they signed the loan and loan deal information from Dealscan. We then basically followed Cheng, Raina and Xiong's (2014) online instructions to collect data from Lexis/Nexis Public Records. We made some minor changes to their algorithms to fit our purpose and because we believe by doing so, it is more accurate.

1.1. Procedure to Identify Target Loan Officer on LexisNexis

We first collect additional information (employment information, working location, education information, graduation year, etc.) about our sample loan officers from LinkedIn. We use a loan officer's name and affiliation when they signed the loan contracts to search for the officer on LinkedIn to get further information like employment information, education information, location information, etc. In doing so, we require the date when our target loan officer signed a loan contract under a certain bank lies in the time period listed on LinkedIn during which the target loan officer works for the same bank. If we cannot identify a loan officer on LinkedIn, we search for information regarding his (her) career path on the internet. If the target lives outside of the U.S., we classify this loan officer's location as "Foreign."[1]

We then perform the following algorithms to identify a loan officer's LexID on LexisNexis.

Algorithm 1. Search for a loan officer based on Name and Location/Address (if available from LinkedIn). Then filter through the possible matches based on (a) Age Range, (b) Employment Information, and (c) Education Information.

We start searching by an officer's name and location (if available from LinkedIn) on Lexis/Nexis. We then check manually all the research results returned on LexisNexis according to the following matching criteria:

i. "Age" on LexisNexis should be reasonably matched with the dates when the loan officer signed for loan contracts (we assume the loan officer was between 20 and 70 years old when signing for the loan contracts). Additionally, if we can find graduation year from LinkedIn, we follow the assumption used in Pool, Stoffman and Yonker (2012) that the loan officer was between 18 and 24 years old when graduating. We remove records that are not consistent with the above-listed age range. If we are able to pinpoint exactly one person on LexisNexis, we consider this as the correct match. If multiple records remain after filtering by age range, we continue to the next step.

ii. "Employment Locator" on LexisNexis should match with a loan officer's affiliation shown on the loan contract and employment information from LinkedIn. We require a potential match on LexisNexis to have at least one employer that corresponds to the employers shown in LinkedIn and SEC contract. If we are able to pinpoint exactly one person on LexisNexis, we consider this as the correct match. If we find no match of the employer or we do not have employment information on Lexis/Nexis, proceed to **Algorithm2**.

[1] We search for this person without specifying "Location" on Lexis/Nexis.

iii. "Possible Education" on Lexis/Nexis should match with education information from LinkedIn (if education is available from both data sources). If we are able to pinpoint exactly one person on LexisNexis, we consider this as the correct match. If we find no match of the employer or we do not have employment information on Lexis/Nexis, proceed to **Algorithm2**.

If we find multiple matching results following the steps above, we will search for some additional information on the internet to pinpoint the correct match. This describes **Algorithm 2**. If multiple records still remain after this procedure, we provide multiple matches.

Algorithm 2. Relax the criteria on Employment. Redo the search based on Name and Location. Filter through the matches by Other Information (collected from the internet).

i. Match by Education: Match "Possible Education" on LexisNexis with education information from LinkedIn (if we can get this data both from LinkedIn and LexisNexis). If we are able to pinpoint exactly one person on Nexis, we consider this a correct match.

ii. Match by Industry: Based on the "Employment Locator" on LexiNexis, check whether at least one employer is in the Banking/Finance industry. If we are able to pinpoint exactly one person on LexisNexis, we consider it as a correct match.

iii. If we find only one matched result and we do not have enough information to perform the above steps, then we consider it as a (roughly) correct match.

1.2. Procedure to Collect Property Ownership Information

We first collet all the deed transfer records, mortgage records, and tax assessment records of the properties related to all the address related to our identified loan officers from LexisNexis. We then follow the steps below to get the dates when the target gained or released control of the property. This procedure closely follows the instructions provided by Cheng, Raina and Xiong's (2014).

A. We first identify all the people that are once listed in the deed records, mortgage records or tax assessment records together with a loan officer in our sample (these people share ownership of a property with our target loan officer). We classify these people as "associates" of the loan officer. We refer to both the loan officer and his (her) associates as a "target" in the following.
B. Find all the properties for which a target is listed as buyer or seller in at least one deed record, whose mortgage record contains a loan officer (or associates), or for which a loan officer (or associates) is listed in the tax assessment record as an owner. We identify these properties as our target properties. We then collect purchase and sale dates for these properties.
C. Get the date when the target gained control of the property.
    1. Find the earliest deed record with the target listed as "Buyer". Use this deed record to collect the "Contract Date," if available, or the "Recording Date" as Property Purchase Date.
    2. If there is no deed record with the target listed as "Buyer", then find the earliest mortgage record with "Mortgage Type" of "Purchase Money". Use this mortgage record to collect the "Contract Date," if available, or the "Recording Date" (if deed record does not already provide this information) as Property Purchase Date.
    3. If we can't get the Property Purchase Date from deed records and mortgage records, we then try to find tax assessment records for the property with the target's name (or the name of someone associated with the target) as the "Owner".

1) If we can find "Recording Date" in these assessment records, then collect it as Property Purchase Date.

2) If we cannot find "Recording Date" in these assessment records, then we try to find a tax assessment record for a person who owns the property after our target, Often, such records also have information on "Prior Recording Date". We use the "Prior Recording Date" to record the Property Purchase Date for our target.

4. If we cannot find the date when the target gained control of the property, we leave it as missing.

D. Get the date when the target released control of the property.
   1. Find the latest deed record for the property in which the target (and/or anyone the loan officer is associated with) listed as "Seller". Use the record to collect the "Contract Date" as Property Sale Date if available. If not, use the "Recording Date".

   2. If there is no deed record in which the target released control of the property, then the property is either:

      Still owned by the target if:

      There are no assessment/deed/mortgage records with other people as owners of the property with dates that are later than the records associated with our target. And the assessment records for the property with the target's name come close to present-day (e.g., we have assessment records reaching until 2015 or so). Then we set Property Sale Date as 2019 (which indicates the property is still currently owned by the target).

      Or sold without a deed record:

      1) There are tax assessment/deed/mortgage records with other people listed as owners of the property with dates that are later than the records associated with our target; if so, then try to get the Property Sale Date by using the following two ways (If we can get the date from both, we use the earlier one):

         a. Tax assessment records: find a tax assessment record for a person who owns the property after our target. Use the "Recording Date" in the assessment records to fill in the Property Sale Date.

         b. Mortgage records: find a mortgage record for a person who buys the property after our target in the list with "Mortgage Type" of "Purchase Money". Treat this record as the document capturing data on the transaction in which the next owner gains control of the property. Use this mortgage record to collect the "Contract Date," if available, or the "Recording Date" as Property Sale Date.

      2) If we can't get the Property Sale Date by 1), and the assessment records for the property with the target's name do not come close to present-date (e.g., the last record at all for the property is in 2010 or before). Then set the Property Sale Date as missing.

   3. If we can't find the date when the target released control of the property, we leave it as missing.

E. If we cannot get the purchase and/or sale date for the property. We use the earliest tax assessment record year for the purchase date and/or the latest tax assessment record year for the sale date. The idea is that even though we do not know when our target loan officer bought or sold this property, but we are sure that during this time period, our target loan officer has the ownership for this property.

1.3. Identifying Primary Residence and Rental Property

In our final sample, there are officers own multiple properties at the time of loan origination. In order to identify their primary residences, we first search for their LinkedIn profiles and get their work locations at the time of loan origination. Then we define the primary residence as the ones that are the closest to their work locations. For other officers who own only one residence at the time of loan origination, the only properties they own are also defined as primary residence.

LexisNexis provide all addresses that are associated with a person and the time period during which each address is associated with the person. They collect this data from a variety of sources, including post office change of address records. However, how they collect and integrate all the addresses are confidential and not entirely transparent. With this information, we are able to expand our sample to include loan officers that are not property owners at the time of loan origination. These are the officers that are associated with some addresses, but we do not find any transfer deed records or tax assessment records from LexisNexis indicating the properties associated with these addresses are owned by these officers. We infer that these properties are likely to be the places where loan officers rent to live. However, we do not have additional information directly showing that officers indeed rent these properties and live there. We are fully aware that this data can be noisy and may include addresses that are not rental property. We remove addresses that are not residential like hotel or motel, U.S postal service (post office box), social services facility, etc. We also require the address is associated with the officer at the loan origination date and for at least one year.

2. Anecdotal Evidence on Loan Officers' Role

2.1. Evidence from LinkedIn Job Description

Loan officers in our sample frequently explain their authority in managing and structuring loans and their role in loan pricing in their LinkedIn profiles. Such statements also frequently suggest that they work closely with the borrower.

For example, some senior loan officers in JP Morgan Chase & Co show the following statements in their LinkedIn profiles: "*I led loan and debt securities origination teams in the proposal and negotiation of all aspects of cash flow and ABL revolving, term loan, bond and bridge loan structures for leveraged transactions. I presented structural recommendations and negotiated all aspects of debt instruments with Corporate and Private Equity Sponsor clients. I managed legal documentation negotiations, syndication communications and the operational closing process*"; and "[I am] *actively involved in significant syndicated lending activity including origination, structuring and execution in conjunction with JPMorgan's syndications Group*". Some loan officers at Bank of America claim that "[I] *manage multi-billion dollar portfolio by negotiating credit terms and advising on operational risks in credit agreements"; "[I] work with borrowers, lenders, legal counsel and bank deal teams including syndications, credit officers, analysts, and fulfillment teams to coordinate closings*"; "*I construct and advise on international syndicated credit agreements including advisement to companies at the CFO and Treasurer level for multicurrency and cross border operational needs*."

We list more examples from loan officers' LinkedIn profiles in the table below:

Officer Job Title and Affiliation	Job Statement on LinkedIn
Director Credit Suisse	Senior banker with demonstrated expertise in business development, transaction structuring and execution. Expert in loan market pricing dynamics, as well as company analysis. Solely responsible for pricing all leveraged and investment grade loans booked on the firms balance sheet accomplished by daily interaction with the secondary loan market and credit default swaps trading desks.
Director Bank of America Merrill Lynch	Underwrite and manage credit exposure for a variety of products such as multi-million-dollar syndicated loans, foreign exchange, derivatives, treasury management and trade products. Lead and manage the underwriting, structuring, negotiation and documentation of syndicated senior bank loans.
Vice President Bank of America Merrill Lynch	Experienced Credit Risk professional, with particular expertise in underwriting and structuring loans for large corporate, financial sponsors (LBO's) and middle market clients. Proficient in evaluating the risk/return of opportunities through extensive industry, competitor and external environmental analysis, advanced cash flow modeling, sensitivity analysis, key credit risk assessment, due diligence discussions and enterprise valuation.
Director SunTrust Bank	Structure and negotiate project finance transactions of \$50mm+ as Lead Arranger or Participant.

2.2. Evidence from Job Postings

We provide below some examples regarding job descriptions for corporate loan officers by banks in our sample. These examples also illustrate their role in managing, structuring, and pricing loans, as well as working closely with borrowers. They are excerpts from various lenders' job posting for commercial/corporate banker/underwriters:

Recruiting Firm	Job Description
JP Morgan	Provides credit expertise in structuring and/or pricing loan; Leads or assists in negotiating and finalizing documentation for loans; Provides a cohesive and comprehensive approach to review ratings, risk assessment, portfolios, clients, and industries in the sector Detailed client screening (GFCC, sanctions, AML, anti-corruption, PEP, news, etc.) Development of forward-looking new business plans for clients and the maintenance of existing business relationships
JP Morgan	Proven ability to create, develop and maintain trusted client relationships, as evidenced by winning networks. Strong credit skills and the ability to successfully drive relationships in the context of JP Morgan's risk appetite against each client.

	Develop and sustain relationships with senior decision-makers within client organizations and client prospects Serve as the clients' chief sponsor within J.P. Morgan to assure high levels of client services, appropriate lines of credit, and help resolve legal and documentation issues that could arise.
JP Morgan	As a Banker you will be responsible for securing new and retaining profitable relationships within the target space (companies with revenues between $20-$100 million). This role is the focal point of client acquisition and ongoing relationships. Bankers work both independently and as part of a team to introduce our comprehensive solutions. Ability to assess risks inherent in complex credit transactions and mitigate, structure and negotiate accordingly.
Bank of America	Working with commercial clients. Develops, participates and/or presents client/client team presentations (economic updates, markets forecasts, industry updates, industry valuations), pitch books and relationship reviews. Able to work with minimal supervision on assigned tasks, and is able to connect analytical work to the client needs and strategic objectives.
BB&T	Develop and manage a portfolio of large credit relationships. Responsibilities include originating, underwriting, structuring and closing direct and syndicated loans ($25MM to $150MM+) with risk profiles, structure and pricing consistent with Bank policy. Monitor credit relationships to ensure ongoing compliance with approved terms/conditions. Review existing relationships with credit administration at intervals consistent with Bank policy. Cross-sell all other Bank products and services Develop and manage BB&T agented or co-agented credit facilities. This includes originating, underwriting, structuring, closing and servicing syndicated loan relationships. Oversee all structuring aspects of syndication process and ongoing servicing of client relationships. Ensure all responsibilities associated with the agent/participant bank relationship are met.
BB&T	Highly-skilled and proficient in most aspects of finance; proficient in managing large and complex corporate relationships. Knowledgeable and experienced in complex credit products and structuring, including loan syndication and participations and capital markets solutions. Develop and execute a marketing plan focused on winning new client relationships and expanding existing client relationships. Can readily handle a loan request of $10,000,000 or more up to in-house limit. Ability to grasp complex credits clearly; is insightful in all aspects of finance; Strong interpersonal communications; can handle client relationships with borrowing clients with total debt of $10,000,000 or more

Valley National Bank	Maintain close customer contact to ensure continued satisfaction and to follow or anticipate additional financing needs. Monitor and report changes in credit quality. Negotiate to properly structured and priced credit facilities consistent with the bank's credit policies and lending practices.

3. Geographic Distribution of Officers and Borrowers

In Table IA.1 and Figure IA.1 we present the distribution of our sample loan officer addresses and borrower headquarters across states. Both loan officer properties and borrower headquarters cover a wide range of states. Yet, there is a significant geographical separation between them. For example, New York, Connecticut, and New Jersey represent 52 percent of officers' states and only 11 percent of borrower states.

4. Link Between Corporate Loan Spreads and Aggregate Fluctuations in Officer Housing Price Experiences

We examine the link between aggregate fluctuations in officers' recent housing price growth experiences and corporate loan spreads. We start with graphical evidence. Figure IA.2 illustrates loan officers' average local housing price growth (past 12 months) together with average loan spreads. The solid line represents average corporate loan spreads across all loans issued to public U.S. firms in Dealscan during our sample period. The dashed line represents the average housing price growth in the 20-mile neighborhood around our sample loan officers, and the dotted line indicates the average state-level HPI growth across all the states that have at least one loan officer real estate property. Both measures capture average experiences in the past 12 months. There is a clear negative link between local housing price growth and corporate credit spreads over time.

Second, we analyze this same link using a regression analysis. Using loan-level data, we regress log of loan spreads on the average housing price growth experiences across officers in our data over the past year. Note that this explanatory variable only changes in the time series. When computing this average, we use the housing price growth around the properties of all officers in our data. As we aggregate the past growth around these properties, we use housing price data at three different geographic levels where properties are located: 20-mile neighborhoods, county, and state. This leads to three different measures of housing price growth experiences.

Table IA.2 shows the results. In columns (1) through (3), we show the baseline results with the three measures above. In columns (4) through (6), we add controls for macroeconomic conditions such as S&P index stock returns and GDP growth. We also control for the changes in aggregate lending policies, measured by the equity growth rate and the loan losses of the banking sector.

We find a strong, negative association between aggregate fluctuations in officer housing price growth experiences and corporate credit spreads. This result shows that, during the credit cycle we analyze, corporate credit have significantly lower spreads following more positive aggregate experiences by loan officers. To see the economic importance of this link note that *Local HPGrowth (Year -1)* drops by approximately 20 percentage points between its top and bottom in Figure IA.1. Column (4) implies that this leads to a reduction in the log of loan spreads by $0.32 = 0.2 \times 1.6$, which represents approximately $213\text{bp} \times 0.32 = 68$ bp (using mean for loans in Table 1).

5. Do the Results Capture Officer Idiosyncratic Conditions?

We replicate the falsification test implemented in Panel C of Table 2 using a different specification. In this alternative specification, we exclude the controls Matched Officer Growth - Bank × Region. The exclusion of these controls allows us to have a larger sample and more precise estimates and we use this alternative specification in the paper when estimating interactions of our main effects. Here, we illustrate that this alternative specification also captures officer idiosyncratic conditions as we mention in the text. See Section 3.1 for a discussion of this test.

6. Local Experiences at Different Geographic Levels

We expand the baseline specification in Equation (1) by adding officers' recent housing growth experiences at the zip code level. Table IA.4 presents the results. We continue to find that the effect of local housing growth is concentrated at the neighborhood level.

7. Robustness of Results to Subperiods: Additional Evidence

We provide additional evidence that our results are not driven by specific subperiods. We repeat the baseline analysis by removing every two-year interval in our sample period. These intervals include 1999—2000, 2001—2002, …, 2012—2012. Table IA.5 reports the results. In each subsample, our results remain statistically significant and economically similar to our baseline result. This evidence complements our analysis in the paper showing that our effects are not concentrated during the financial crisis and remain important outside this subperiod. Overall, the results from this set of analyses suggest that our base findings are unlikely to be driven by a specific subperiod of the credit cycle we analyze.

8. Officers' Economic Experiences and the Pricing of Credit Risk: Additional Evidence

We provide additional evidence on the effect of loan officers' personal economic experiences on the pricing of credit risk. We extend our results in Table 5 and document that more positive officer experiences are also associated with a weaker link between loan spreads and market benchmarks for these spreads. These market benchmarks measure the average spread for loans originated in the same period as the loan with comparable credit risk (same DD quintile or same credit rating group). Intuitively, such weaker link between the spreads in our sample and market benchmarks captures a reduced sensitivity of lenders to credit risks priced by the loan market. In contrast with the interactions used in Table 5, these benchmarks capture both differences in credit risk across firms and the pricing of such differences by current market conditions. Table IA.6 reports the results. In columns (1) and (2), we show that benchmark spreads are powerful in predicting the loan spread for the borrower of interest. In columns (3) and (4), the results show that this link becomes significantly weaker after more positive officer local experiences or that personal experience effects disproportionally affect riskier loans. These results confirm that more positive lender personal experiences reduce the sensitivity of loans spreads to credit risk or distort the pricing of credit risk.

9. Additional Controls in Cross-Sectional Analyses

We remove the control of Matched Officer Growth - Bank × Region in our cross-sectional analyses because this allows us to have a larger sample and more precise estimates. In the paper, we show that average personal experience effects remain similar with or without these controls. We now examine whether results from the key cross-sectional analyses are robust to the addition of this control. Specifically, we focus on our key cross-sectional tests: the interactions of personal experience effects with borrowers' credit risk and real-estate holdings. Table IA.7 reports the results. Column (1) shows the results from the interactive regression of local housing price growth with DDRank. Column (2) shows the results from interactions with DD-Demeaned. Column (3) shows how officer experience effects are related to High RERatio. All of those results are robust to the addition of matched officer growth control by bank and census division. The interaction terms remain statistically significant and imply similar economic magnitudes to the results in Panel A of Tables 5 and 6.

10. Distribution of Collateral Types in Loans

We describe the distribution of collateral type in Dealscan loans in Table IA.8. We report the percentage of loans with the following types of collateral assets: (1) the entirety of borrower assets, (2) property, plant, and equipment (PP&E), (3) current assets, including accounts receivable, inventory, cash and marketable securities, (4) real estate, (5) intangibles and patents, (6) other, including ownership of options and warrants, as well as agency guarantee, and (7) unknown collateral type. We report the distribution both for our sample of loans and for the entire Dealscan universe. We also report the shares of collateral types (1) to (6) as percentage of known collateral types, i.e. excluding cases with unkown collateral.

In our sample, the most common type of collateral is all of the assets of the borrower, which capture 70 percent of the cases with known collateral. Real estate asset values could influence the collateral value of a loan when the loan is backed by all assets, PP&E, and real estate properties. These cases collectively account for 80 percent of the cases with known collateral in our sample, and 67 percent of loans with known collateral in the Dealscan universe. These patterns are consistent with industry documentation discussed in Section 3.4.

11. Real Estate Holdings and Credit Risk

We examine whether higher credit risk is associated with a larger share of real estate holdings. We regress firms' real estate ownership on DDRank, which captures firms' quintile in terms of their distance-to-default (see Section 3.3). Higher values of DDRank capture quintiles with lower credit risk.

Table IA.9 reports the results. We measure real estate ownership using the four measures in Table 6. We also test the relation between credit risk and real estate holdings in our sample and the Dealscan-Compustat universe of loans. Columns (1) to (4) report the results using the Dealscan-Compustat universe and columns (5) through (8) report the results from our sample. Across all measures of real estate ownership and both sample choices, we do not find evidence suggesting that real estate ownership is positively associated with credit risk. If anything, results from columns (1) through (4) suggests that higher credit risk is associated with a lower share of real estate holdings. Moreover, this link is economically weak. For example, in column (1), an reduction in credit risk by one quintile of *DD* predicts an increase in RERatio equal to 0.8%, which represents 0.8/24 = 3.3 percent of the average value of RERatio (see Table 1 for this average value). These patterns are consistent with the fact that real estate holdings are more important among firms that are larger, older, and more profitable (Chaney, Sraer, and Thesmar (2012)).

12. First-Stage Results from IV Regressions

In Table IA.10, we report the first-stage results from the IV regressions used in Panel B of Table 6 (columns (1) and (2)). These IV regressions analyze the differential effect of economic shocks between years t-5 and t (Shock) on the debt issuance of real-estate intensive firms, captured by higher values of RERatio (see Table 6). In these first-stage regressions, the dependent variable is Shock × RERatio, which captures this differential shock on real-estate intensive firms. In column (1), Shock is the housing price growth in the MSA of the firm's headquarter (HP Growth), and HP Growth × RERatio can be interpreted as a shock to the value of firms' real estate assets. In column (2), Shock is the county-level employment growth around the firm's headquarter. RERatio is defined as the ratio of net real estate assets (land and improvements, buildings, and construction in progress) over total net PPE. RERatio is defined as the value of this ratio at t-5 for all years up to 1998. Because of data availability, this ratio is measured at 1993 from 1999 on. The sample includes all non-financial and non-utility firms in Compustat without missing values for RERatio (firms need to be present in 1993 or earlier).

Column (1) in Table IA.10 follows Carvalho (2018) and uses the triple interaction of RERatio with measures of geographically-determined land unavailability at the MSA level (Uland1 to Uland5) and national real estate price growth (NPGrowth) as instruments for RERatio × HP Growth. These measures capture five geographic features of MSAs (based on satellite data) that limit the availability of land for construction, i.e. shares of a 50 km radius around the city's centroid covered by the ocean, covered by open water, with high slopes, with woody wetlands, and with herbaceous wetlands. The motivation for this approach is the idea that increases in real estate prices can be offset by increased construction activity, which lowers prices through housing supply, but this mechanism is muted when there is land less available. Therefore, land availability can be key for predicting which areas have a greater exposure to national-level shocks to real estate price growth. These characteristics can matter to different degrees and including them separately allows them to have different effects. In sum, this differential exposure of areas to changes over time in national conditions (Uland × NPGrowth) predicts differences in local real estate price growth (HP Growth). As we interact these predicted local shocks with firms' real estate share, we arrive at the triple interaction described above (see Carvalho (2018) for more details). All variables included as controls are listed in Table IA.10. Firm characteristics (Size, Age, and Cash flow) are measured in year t -5 and are defined in the same way as in our main results.

Column (2) applies a Bartik instrument that projects national employment growth to the state level. Bartik is given by $\sum_i s_{it} NG_{it}$, where i denotes each industry in the state, s_{it} measures the share of the industry in the state (employment) at year t-5, and NG_{it} measures the national-level growth of the industry (excluding the state) between years t-5 and t. This predicts shocks to state employment growth between years t-5 and t using national-level shocks and differences in the initial industry composition of states, i.e. differences in state exposures to national shocks. As in the case of column (1), all variables included as controls are listed in Table IA.10.

13. Link Between Borrower-Loan Characteristics and Lead Arranger Share

We complement our analysis in Section 3.5 by examining the link between share of loans retained by lead arrangers in a syndicate and the characteristics of borrowers and the lending syndicate. Table IA.11 reports these results. We regress the lead arranger share on the borrower's size, analyst coverage, DDRank, and whether the loan has a single lead arranger. We use the same sample as in our main results, with the same set of controls and fixed effects. This is intended to capture the link between each characteristic and the lead share in our analysis. When missing, we impute lead arranger shares based on syndication structure, following Chodorow-Reich (2014). Specifically, we use the average lead share in all other Dealscan loans with the same loan structure (number of participants and number of lead banks).

In a first set of results (columns (1) to (3)), we connect borrower characteristics (size, analyst coverage, and DDRank) to this lead arranger share. The goal of these results is to provide additional support for the view that lead arrangers are more important in such loans and need to have more "skin in the game". A syndicate loan can create a conflict of interest between lead arrangers and other lending participants, as lead banks need to incur costly screening and monitoring activities on behalf of all lenders but retain only a portion of the loan. Therefore, in the presence of information asymmetry between lead arrangers and other loan participants, leads should hold a larger share of syndicated loans to mitigate this potential conflict of interest. In other words, when lead banks play a more important role in these loans because of their greater ability to evaluate risks and monitor borrowers, other participants should expect them to invest more in the loan. Sufi (2007) discusses this idea and provides evidence supporting it. In our discussion in Section 3.5, we argue that the greater information asymmetry associated with smaller borrowers and borrowers with less analyst coverage should lead to an increased importance of lead arrangers. We also argue that lead arrangers should plausibly perform a more important role in riskier loans. We now confirm that these borrower characteristics are associated with larger lead shares in syndicated loans. In Section 3.5, we also argue that lead banks should be important more important in loans with a single lead. Column (4) also shows that having a single lead is a strong predictor of the lead share in these loans. Recall that all these characteristics predicting a higher lead share are associated with stronger officer personal experience effects on loan spreads in our analysis. Therefore, our collective evidence illustrates that the officer effects we analyze are stronger in contexts where their lead banks are predicted to play a more important role.

14. Further Addressing Concern About Borrower Fundamentals

We provide additional evidence addressing a concern that our results could be driven by borrower fundamentals. One possibility is that local conditions in officers' neighborhoods capture valuable information for predicting their borrowers' credit risk. Given our identification strategy (see Section 2), this concern will only be relevant if officers' idiosyncratic conditions within their state predict the fundamentals of non-local borrowers. We note that Panel B of Table 7 documents that our main results are not more important when local officer conditions are more likely to be informative about borrower fundamentals (see Section 3.4). In Table IA.12, we design two subsample tests to further address this possibility. First, we remove cases where the distance between borrowers' headquarters and their loan officers' properties is at the bottom quartile of our sample. Second, we exclude cases where borrowers' industries are highly represented in the local areas surrounding loan officers' properties (top quartile in our sample). Industry representation is defined as the ratio of the share of local county employment by the borrower's industry (defined at the 3-digit NAICS level) to this same industry share at the national level. Specifically, the ratio is defined as $(\mathrm{Emp}_{c,j,t}/\mathrm{Emp}_{c,t}) \div (\mathrm{Emp}_{US,j,t}\ /\mathrm{Emp}_{US,t})$, where j is the borrower's industry, and c is the loan officer's county, and t represents time. After calculating this ratio for every quarter, we take the average values across the four quarters prior to loan origination. Panel A of Table IA.12 shows that our results are unchanged when we use these alternative samples.

A related possibility is that recent local conditions predict differences in spreads because they affect lenders' choice of borrowers. If this selection effect drives our results, we should expect our findings to become significantly weaker after the inclusion of important controls for borrower credit risk. Panel B of Table IA.12 shows that our results remain economically similar as we drop and add important controls for credit risk. In Columns (1) and (2), we control for DD Rank × year fixed effects. In columns (3) and (4), we add the following additional credit risk controls: DDRank, the benchmark credit spread based on DD quintiles, and the benchmark credit spread based on credit rating category. In columns (5) and (6), we remove credit risk controls such as leverage and equity volatility from the baseline regressions. Our results are highly stable to the variation of these controls. This suggests that this selection effect is unlikely to be important in our setting.

15. Additional Robustness Analysis

In Table IA.13, we present results from additional robustness analyses that examine the choice of controls and sample construction. Panel A reports results when we remove controls for other loan contract terms such as loan maturity and loan size. Column (1) shows the results from the baseline specification (column (2) of Panel A in Table 2). In column (2), we examine the robustness of the interactive effect of local housing price growth and firms' real estate ownership. Our main results remain similar without these controls for other loan contract terms. Panel B reports results related to sample construction. In column (1), we restrict the sample to officers owning properties in only one state. In column (2), we restrict the sample to loans with one or two lead officers. In column (3), we retain only the largest facility in a loan deal. Our results persist across all of alternative sampling choices.

16. Additional Loan Outcomes

We examine the effect of officer local experiences on additional outcomes of the loans in our data. Panels A and B of Table IA.14 analyze our main results with these additional loan outcomes as the dependent variable. We estimate both the average effect of officer personal experience effects (Panel A) and the differential effect on real estate owners (Panel B). Our preferred specification is the one in Panel B, which focuses on the differential effect of personal experience effects on firms that hold real estate assets and is motivated by the specific mechanism we analyze.

Intuitively, increased lender optimism about the value of borrowers' assets could also increase the supply of credit by officers' lead banks. As discussed in Section 1.3, the lead bank holds a share of the loan they originate. Therefore, the idiosyncratic effects on their supply of credit should be reflected in a larger share by the lead bank on the loans they originate (other participants are not directly affected by the personal experience effects we analyze). One challenge in testing this idea is the presence of data limitations. We cannot measure the share of syndicated loans allocated to officers' lead banks for most loans. This share has to be predicted using the loan structure (number of participants and leads) and this introduces measurement error on this outcome. This same issue also limits our ability to analyze the dollar amount of the loan allocated to the officer's lead bank. On the other hand, the lead bank plays an important role evaluating risks and setting the spread for the entire loan, and this spread can be directly measured. Therefore, this measurement issue is not present for loan spreads.

We find that more positive local experiences are associated with larger loan shares for the officers' banks, but this effect is not statistically significant and is economically smaller than the one for loan spreads. For example, the magnitude of the effect in column (1) (Panel B of Table IA.14) can be directly compared to the one in Table 6 (column (6) in Panel A). One potential explanation for this weaker effect on loan shares is the measurement issue described above.

Another possibility is that increased lender optimism leads to less strict loan covenants. In theory, this connection between lender beliefs about the value of borrowers' assets and covenants is less clear than the connection between these beliefs and loan spreads. Loan spreads should be shaped by these specific beliefs because these asset values determine the recovery of lenders in case of default, and directly shape returns on the loan. This effect is intuitive and also emphasized by practitioners in this market (see Section 3.4). In principle, this increased protection against default could also reduce the need for monitoring and setting strict loan covenants, but this effect is less direct.[2] We estimate our main results using loan strictness as the outcome variable, following the measure proposed by Murfin (2012). We find that more positive officer experiences lead to less strict loan covenants but, as in the previous case, this effect is statistically insignificant and economically smaller.

Finally, we also examine effects for the loan maturity and the total loan size. In the context of our preferred specification, these results show no clear effects on loan maturity and the total loan amount (columns (4) and (5), Panel B of Table IA.14). We note that, in theory, the potential connection between

[2] Of course, there are other supply-side factors that could have a stronger effect on loan covenants (see Murfin (2012)). We are here considering these specific shifts in beliefs about the value of borrowers' assets.

lender beliefs about borrowers' asset values and loan maturity is also less clear than the connection of these beliefs with loan spreads. This can rationalize the contrast between our findings for loan spreads and loan maturity. When interpreting the total loan amount result, recall that our empirical analysis focuses idiosyncratic shocks to officers' lead banks. As other loan participants are not directly affected by this shock, the effect on officers' bank has to be sufficiently strong to be detected in the data. In contrast, as discussed above, the lead bank plays an important role evaluating risks and setting the spread for the entire loan.

As we consider the broader implications of our results, we note the following important points. First, our findings do not suggest that lenders' beliefs about borrower asset values do not significantly matter for loan amounts. We focus on idiosyncratic shocks to certain lead banks for identification reasons. More broadly, aggregate conditions affecting the beliefs of all participants in a syndicate could have stronger effects on loan amounts.

Additionally, we note that distortions in the pricing of credit risk and excessive fluctuations in credit spreads play a central role in narratives and models of distorted lender beliefs and credit cycles (e.g., Bordalo, Gennaioli, Shleifer (2018)), and that previous research on credit cycles has relied on credit spreads to capture shifts in lender optimism across the credit cycle (e.g., López-Salido, Stein, and Zakrajšek (2017), Sufi, Mian, and Verner (2017)). Our findings on loan spreads are directly connected to these important ideas and empirical patterns.

17. Local Housing Price Growth Experiences: Primary Residences and Renters

We extend our analysis of local housing price growth experiences in two ways. First, we consider only experiences associated with officers' primary residences, as opposed to secondary properties owned by officers. Second, we extend our sample to include rental properties by officers. Table IA.15 reports the results. As explained in the paper and Section 1 of the Internet Appendix, our analysis focuses on experiences around properties owned by loan officers because of data challenges associated with the measurement of rental properties. In our main results, we consider all properties owned by loan officers at the time of loan origination, and estimate an average effect of local experiences around these properties.

As a first alternative to our main results, we now restrict our analysis to properties that are more likely to be officers' primary residence. Identifying officers' primary residences in the data can be challenging and we follow a simple and intuitive approach. Using information from LinkedIn, we identify the location (state) of officers' job at the time of loan origination. We then classify properties owned by officers in this same state as primary residences. Intuitively, properties outside this state should have significantly longer commuting times and are less likely to capture locations where officers spend most of their time. If local housing experiences matter because officers are more familiar with local information around their properties, we should expect our results to be stronger for primary residences, relative to secondary properties, as officers are more likely to be present in these primary residences. Only a limited share of our data covers secondary properties and contrasting the effects for these two groups is not feasible. However, we check if our results remain important as we restrict our analysis only to primary residences. Column (1) in Table IA.15 shows that main our results remain economically similar and statistically significant when estimated using only housing experiences from primary residences.

In a second alternative to our main results, we also include renters in our sample. Here, we also face data limitations as identifying addresses associated with rental properties is also subject to challenges. As discussed in Section 1 of this Internet Appendix, we faced issues with the data used to identify the address of officers' rental properties at the time of loan origination. Because of these data considerations, we focused on properties owned by officers in our main results, and include this extension to renters as a robustness check. We measure housing price growth experiences in the same way as in the case of property owners using the location of rental properties at the time of loan origination. We combine our data on local experiences by owners and renters and start by estimating an average effect across these two groups. Column (2) of Table IA.15 shows that our results remain economically similar and statistically significant as we combine these two groups of experiences together. We then estimate separate effects for owners and renters (column (3) of Table IA.15) in this combined sample. We also estimate our effects using only the sample renters (column (4) of Table IA.15). If local housing experiences matter because officers are more familiar with local information around their properties, our results should remain relevant among renters. Alternatively, as discussed in the paper (e.g., Section 2.2), our results could capture the effect of personal experiences with housing wealth on beliefs about real estate prices. In other words, officers' might overweight changes in housing prices that affect their personal wealth when forming beliefs about national real estate prices. If this is the case, our effects should be present among renters and should be only important for property owners. We note that our effects are only statistically significant for property owners but that they have the same sign with a smaller magnitude for renters. The magnitude of the results using only renters (column (4)) is smaller but comparable to the one for primary residences (column (1)) and owners and renters combined (column (2)). As the data for renters is less accurate, this pattern could reflect greater measurement error and the smaller sample size associated with our sample of renters. In sum, our results remain similar as we include both owners and renters but our data does not allow us to differentiate in a clear way between the effects for owners and renters.

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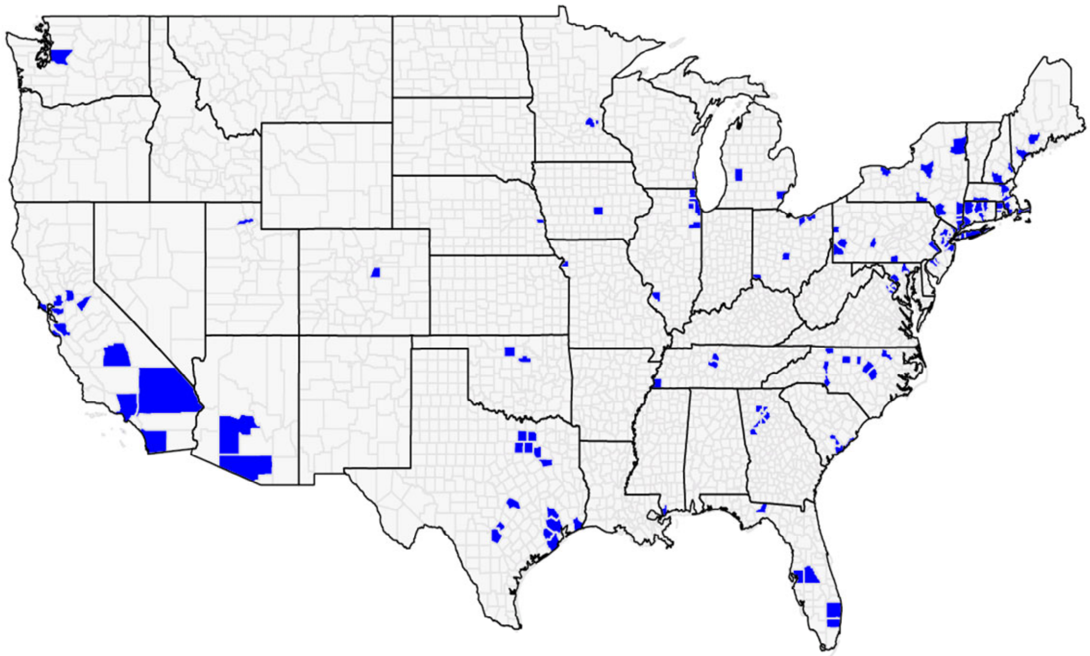
Figure IA.1
Officer Property and Borrower Headquarter Location

This figure shows the all counties with officer properties or borrower headquarters in our sample described in Table 1. Panel A shows all counties where there is at least one loan officer property in this sample. Panel B shows all counties where there is at least one borrower headquarter in this sample.

Panel A: Distribution of Officer Property Counties



Panel B: Distribution of Borrower Headquarter Counties

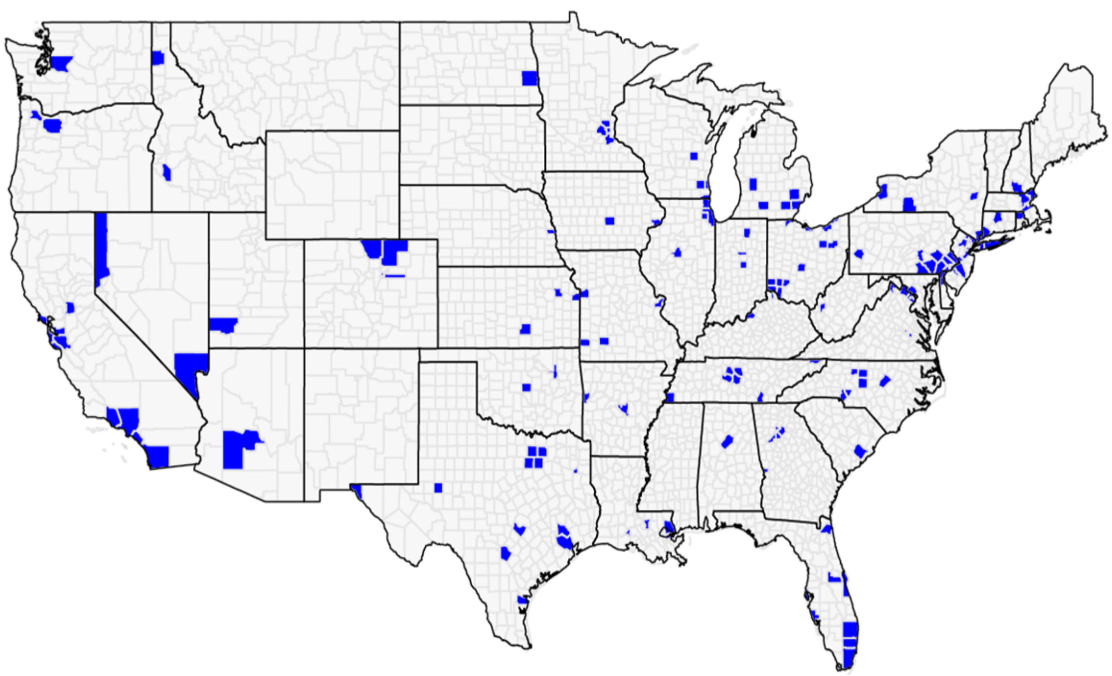


Figure IA.2

Credit Spreads and Lenders' Recent Economic Experiences: Aggregate Patterns

This figure shows aggregate patterns for corporate credit spreads on bank loans and measures of the recent economic experiences of corporate loan officers between 2004 and 2012. *Loan Spreads* measures the average value for the all-in-drawn interest rate spreads in basis points over the LIBOR in a large sample of corporate loans. This sample covers all loans in Dealscan that can be matched to Compustat. *Avg State HP Growth* and *Avg Local HP Growth* measure the weighted average of recent housing price growth in the states and neighborhoods where the properties of corporate loan officers in our data are located, respectively. These averages are computed using the share of properties in our data located in a state or zip code as weights. These shares are constant over time and based on our entire dataset. Information on state-level housing prices comes from the Office of Federal Housing Enterprise Oversight (OFHEO). Neighborhood growth is the average growth of housing prices (Zillow) across the zip codes in 20-mile neighborhoods centered around officers' properties. Both measures of recent housing price growth is calculated as the cumulative growth rates between quarters *t*-1 and *t*-5 (annual past growth).

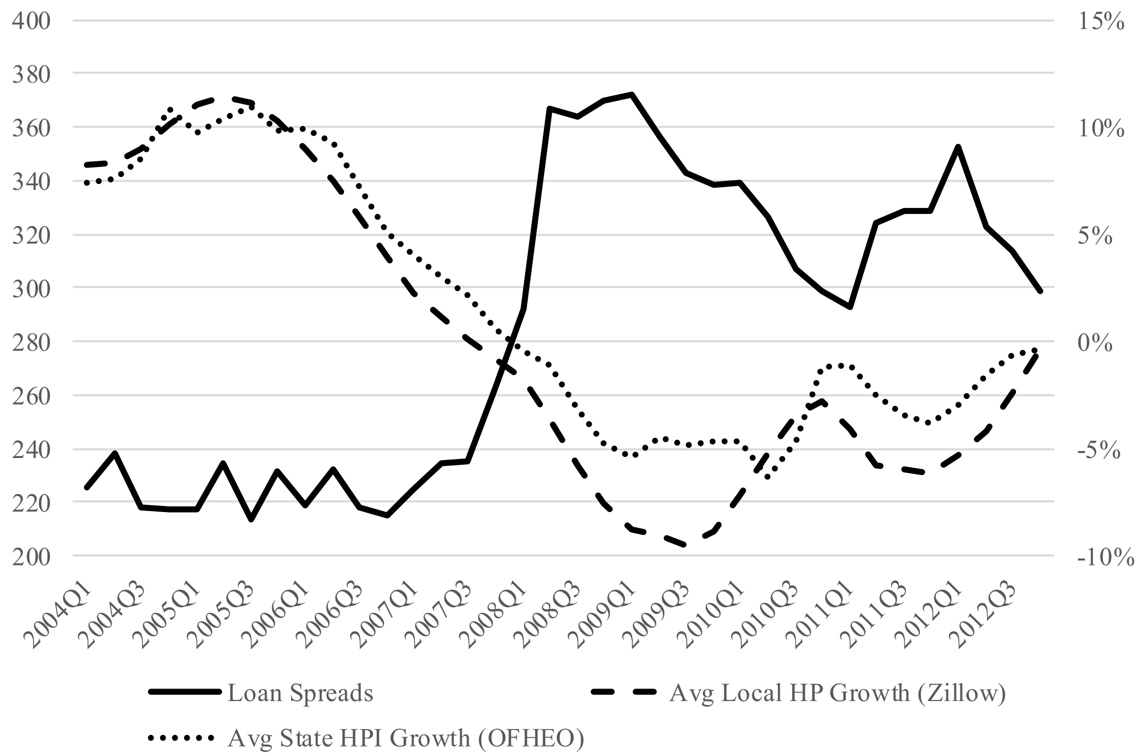


Table IA.1
Distributions of Officer and Borrower States

This table presents the distributions of officer property states and borrower headquarter states in the sample described in Table 1.

Officer States			Borrower States		
State	Nobs	%Obs	State	Nobs	%Obs
NY	381	23.6%	TX	312	19.3%
NJ	251	15.6%	CA	139	8.6%
TX	221	13.7%	MA	108	6.7%
CT	205	12.7%	NY	85	5.3%
GA	86	5.3%	FL	78	4.8%
MA	78	4.8%	GA	75	4.6%
IL	60	3.7%	IL	68	4.2%
CA	54	3.3%	PA	58	3.6%
OH	43	2.7%	CO	56	3.5%
NC	41	2.5%	CT	54	3.3%
VA	28	1.7%	TN	51	3.2%
FL	26	1.6%	MO	50	3.1%
DC	24	1.5%	VA	46	2.9%
PA	17	1.1%	OH	42	2.6%
WA	16	1.0%	NJ	41	2.5%
MN	12	0.7%	NC	39	2.4%
TN	10	0.6%	OK	32	2.0%
MD	8	0.5%	MN	29	1.8%
WI	7	0.4%	MI	27	1.7%
AZ	7	0.4%	KS	24	1.5%
MO	6	0.4%	AZ	23	1.4%
NH	5	0.3%	NV	22	1.4%
MI	4	0.2%	WI	21	1.3%
ME	4	0.2%	DC	13	0.8%
RI	4	0.2%	KY	10	0.6%
CO	3	0.2%	RI	9	0.6%
NE	3	0.2%	LA	9	0.6%
OK	3	0.2%	NE	9	0.6%
UT	2	0.1%	WA	7	0.4%
SC	2	0.1%	ID	7	0.4%
IA	1	0.1%	IN	7	0.4%
MS	1	0.1%	MD	6	0.4%
Other	0	0.0%	Other	56	3.5%
Total	1,613	100.0%	Total	1,613	100.0%

Table IA.2

Officers' Personal Economic Experiences and Loan Spreads, Time-Series Evidence

This table reports results connecting changes in corporate loan spreads over time to recent economic conditions in loan officers' neighborhoods, captured by the average local housing price growth in these areas. The unit of observation is a loan and the sample includes all Dealscan loans matched to U.S. public firms between 2004 and 2012. The dependent variable is Log of Spread, the log of the all-in-drawn interest rate loan spread (in basis points) over the LIBOR. In each specification, the recent local housing growth experience of loan officers is captured by one of the following three variables. Local HPGrowth (Year -1) is the average local housing price growth in the year prior to loan origination. This average is computed using all neighborhoods (20-miles radius) around officer properties in our data (fixed set of properties over time covering our entire sample). County HPGrowth (Year -1) is the average housing price growth in the counties of loan officers' properties in the year prior to loan origination. State HPGrowth (Year -1) is the average housing price growth in the state of loan officers' properties (OFHEO data) in the year prior to loan origination. This average across counties (states) is measured every quarter using all counties (states) with officer properties in our data. Each county (state) is weighted using the share of total officer properties in our data (constant weights over time). Neighborhood (county) growth is the average growth of housing prices (Zillow) across the zip codes in the neighborhood (county) where officers' properties are located. All results include the following controls: Loan Size, Loan Maturity, Equity Volatility, Size, Firm Age, Profitability, Tangibility, M/B, Leverage, and Rated. These borrower characteristics are measured in the year prior to the loan. The results in columns (4) to (6) also include the following additional controls: S&P Returns, GDP Growth, Banking Sector Equity Growth, and Banking Sector Loan Losses. S&P Returns is the average S&P 500 returns during the four quarters prior to loan origination; GDP Growth is the average growth rate of U.S. GDP in the past four quarters; Banking Sector Equity Growth is the average growth rate of equity to asset ratio of the U.S. banking sector in the past four quarters; and Banking Sector Loan Losses measures the average loan losses (scaled by total equity capital) of the U.S. banking sector in the past four quarters. Standard errors are heteroskedasticity robust and clustered by borrower. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Outcome: Log of Spread					
	(1)	(2)	(3)	(4)	(5)	(6)
Local HPGrowth (Year -1)	-3.454***			-1.623***		
	(0.251)			(0.369)		
County HPGrowth (Year -1)		-3.563***			-1.736***	
		(0.249)			(0.366)	
State HPGrowth (Year -1)			-5.319***			-3.383***
			(0.272)			(0.439)
S&P Returns				-0.313***	-0.311***	-0.370***
				(0.048)	(0.048)	(0.049)
GDP Growth				-0.017**	-0.015**	-0.004
				(0.007)	(0.007)	(0.007)
Banking Sector Equity Growth				-2.121***	-2.025***	-1.203*
				(0.701)	(0.693)	(0.673)
Banking Sector Loan Losses				16.405***	16.161***	12.740***
				(1.068)	(1.071)	(1.208)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,514	11,514	11,514	11,514	11,514	11,514
R-squared	0.379	0.381	0.396	0.408	0.408	0.412

Table IA.3
Do the Results Capture Officer Idiosyncratic Conditions?

This table reports the results from the falsification test in Panel C of Table 2 with a different specification. The outcome variable is the log of the average loan spread issued by matched officers. Matched officers refer to ones living in the same state, but outside the 10-/20-mile radius as the officer of interest. All regressions follow the specification in Panel C of Table 2 but remove the control for Matched Officer Growth - Bank × Region. We compute the average of loan spreads issued by matched officers during the same 6-month window ("semester"), quarter, or 2-month window ("bimonth"). Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Outcome: Log of Matched Officer Spread					
	(1)	(2)	(3)	(4)	(5)	(6)
	Semester		Quarter		Bimonth	
	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius
Local HPGrowth (Quarter -1)	0.050	0.961	-0.814	1.161	0.894	-2.139
	(1.547)	(1.929)	(2.433)	(3.550)	(3.172)	(3.271)
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,334	1,226	1,216	1,077	1,076	941
R-squared	0.699	0.712	0.540	0.579	0.484	0.514

Table IA.4
Credit Spreads and Officers' Local Economic Experiences: Additional Evidence

This table reports results analyzing the role of officers local experiences at different geographic levels. The outcome variable is *Log of Spread*, the log of the all-in-drawn interest rate loan spread (in basis points) over the LIBOR. The results are based on the estimation of the specification in Panel A and Table 2 (Panel A) with additional local experience variables (*Own HP Growth*). *Own HP Growth (Quarter -k)* are defined in an analogous way to the previous neighborhood variables using the housing price growth in officers' own zip codes. Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Outcome: Log of Spread			
	(1)	(2)	(3)	(4)
	Radius	Radius	Radius	Radius
Own HPGrowth (Quarter -1)	0.257	-0.677	0.970	0.238
	(1.218)	(1.492)	(1.206)	(1.172)
Local HPGrowth (Quarter -1)	-7.884***	-11.451***	-8.238***	-10.336***
	(2.243)	(3.318)	(2.033)	(2.689)
MSA HPGrowth (Quarter -1)	2.396	3.724	4.427	3.655
	(4.286)	(4.642)	(4.042)	(3.594)
Own HPGrowth (Quarters -2 to -4) - Avg Effect	0.114	-0.034	-0.094	-0.317
	(0.760)	(0.768)	(0.719)	(0.661)
Local HPGrowth (Quarters -2 to -4) - Avg Effect	-0.779	-1.153	-1.009	-1.883
	(1.244)	(2.123)	(1.198)	(1.990)
MSA HPGrowth (Quarters -2 to -4) - Avg Effect	-1.060	-0.620	-2.241	-0.375
	(2.005)	(2.000)	(2.028)	(1.818)
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes
Matched Officer Growth Controls - Bank × Region	Yes	Yes		
MSA HP Growth Controls	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Observations	1,481	1,298	1,555	1,521
R-squared	0.610	0.593	0.612	0.611

Table IA.5
Results Across Subperiods: Additional Evidence

This table shows the robustness of our results to removing every two-year interval from the sample. The regressions follow the baseline specification, shown in column (2) of Panel A in Table 2. Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Outcome: Log of Spread						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Years Excluded:	1999-2000	2001-2002	2003-2004	2005-2006	2007-2008	2009-2010	2011-2012
Local HPGrowth (Quarter -1)	-12.028***	-11.833***	-9.932***	-10.510***	-13.271***	-10.893***	-14.563**
	(3.325)	(3.260)	(3.521)	(3.700)	(3.463)	(4.038)	(5.620)
Local HPGrowth (Quarters -2 to -4) - Avg Effect	-1.183	-0.508	-0.305	-2.889	-3.646*	-1.688	0.765
	(1.836)	(1.841)	(1.917)	(2.164)	(1.890)	(2.536)	(2.524)
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched Officer Growth Controls - Bank × Region	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,302	1,259	1,206	1,113	1,060	1,051	906
R-squared	0.590	0.583	0.600	0.611	0.652	0.585	0.632

Table IA.6
Officers' Economic Experiences and the Pricing of Credit Risk, Additional Evidence

This table examines the interactive effect of officer local housing price growth and borrower credit risk on market adjusted log of spread. Borrower credit risk is measured by Benchmark Spread, the average spread (also market adjusted) among a group of borrowers with similar credit risk profile as the borrower of interest. The benchmark spread is calculated as the average loan spreads issued to firms in the same quintile of distance-to-default during the same quarter in columns (1) and (3); and as the average loan spreads issued to firms with the same credit rating during the same year in columns (2) and (4). Both benchmarks are defined in quarter/year prior to loan origination. In columns (1) and (2), we report results from linear regressions predicting the loan spread (market adjusted) using the benchmark spreads (also market adjusted). In columns (3) and (4), we report results from the estimation of Equation (2). Scaled effects are calculated as the product of the interactive coefficient and the average gap between the top 50% and bottom 50% values of the benchmark spreads. All regressions include the set of controls and fixed effects used in Table 3 (columns (1) and (2) of Panel A). We also control for the interaction terms of the variable of interest and the recent histories of local housing price growth as well as those of MSA housing price growth. Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Outcome: Log of Spread, Market Adjusted			
	(1)	(2)	(3)	(4)
Benchmark Defined by:	Distance-to-Default	Credit Rating	Distance-to-Default	Credit Rating
Benchmark Spread (Mkt Adjusted)	0.295***	0.420**	0.213***	0.307**
	(0.063)	(0.164)	(0.075)	(0.152)
Local HPGrowth (Quarter -1)			-6.939***	-7.295***
			(2.584)	(2.611)
LHPGrowth (Quarter -1) × Benchmark Spread (Mkt Adjusted)			-19.344***	-13.554**
			(6.595)	(5.655)
Scaled Effect			-13.720***	-10.360**
			(4.678)	(4.322)
Matched Officer Growth Controls - State			Yes	Yes
MSA HP Growth Controls			Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
Observations	1,449	1,574	1,419	1,539
R-squared	0.564	0.567	0.581	0.611

Table IA.7
Cross-Sectional Results with Additional Controls

This table shows the robustness of the cross-sectional results to adding the control for Matched Officer Growth - Bank × Region. Columns (1) and (2) correspond to column (1) and (2) of Table 5 in the paper and column (3) correspond to column (6) of Panel A in Table 6 in the paper. DDRank is the quintile ranking of a firm's distance-to-default among Dealscan-Compustat universe loans originated in the same quarter. DD-Demeaned is the average level of distance-to-default of borrowers within the same quintile category subtracting the average of distance-to-default across all Dealscan-Compustat loans issued during the same quarter. High RERatio is an indicator for whether the borrower's RERatio ranks above the sample median level. Scaled effect equals the interactive coefficient multiplied by the gap between the conditioning variable's mean in the top 50% and bottom 50% of its distribution in our sample. Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Outcome: Log of Spread		
	(1)	(2)	(3)
Local HPGrowth (Quarter -1)	-23.381***	-10.933***	-4.672
	(5.454)	(3.277)	(4.875)
LHPGrowth (Quarter -1) × DDRank	5.836***		
	(1.879)		
DDRank	-0.099***		
	(0.025)		
LHPGrowth (Quarter -1) × DD-Demeaned		3.346**	
		(1.336)	
DD-Demeaned		-0.062***	
		(0.017)	
LHPGrowth (Quarter -1) × High RERatio			-14.057**
			(6.859)
Scaled Effect	13.777***	10.619**	
	(4.435)	(4.241)	
Matched Officer Growth Controls - State	Yes	Yes	Yes
Matched Officer Growth Controls - Bank × Region	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes
Observations	1,209	1,209	974
R-squared	0.634	0.632	0.665

Table IA.8
Distribution of Collateral Type

This table presents the distribution of collateral types both in our sample and in the Dealscan-Compustat merged universe during the period of 1998 and 2012.

Collateral Type	Our Sample	Our Sample (%known collateral)	Universe (1998-2012)	Universe (%known collateral)
All Assets	47.72%	69.83%	24.23%	41.78%
PPE	6.04%	8.83%	10.18%	17.56%
Current Assets	4.78%	7.00%	8.78%	15.13%
Real Estate	0.57%	0.83%	4.17%	7.19%
Intangibles	0.23%	0.33%	3.81%	6.57%
Other	9.00%	13.17%	6.82%	11.76%
Unknown	31.66%		42.00%	
Total	100.00%	100.00%	100.00%	100.00%

Table IA.9
Firm Credit Risk and Real-Estate Ownership

This table presents results relating borrowers' real estate holding and credit risk. The results are based on a linear regression predicting measures of real estate ownership with DDRank, the quintile ranking of a firm's distance-to-default among Dealscan-Compustat universe loans originated in the same quarter. Higher values of DDRank capture quintiles with lower credit risk. Columns (1) to (4) reports the results using universe Dealscan loans between 1998 and 2012 that can be merged with Compustat firms (excluding financial and utility firms). Columns (5) to (8) reports the results using our sample described in Table 1. RERatio stands for the ratio of borrowers' real estate assets over PP&E. REOwner(5%) is an indicator for whether borrowers' real estate assets account for more than 5% of PP&E. REOwners(10%) is defined analogously. High RERatio is an indicator for whether the borrower's RERatio ranks above the sample median level. All regressions in this table control for industry and year fixed effects. Standard errors are heteroskedasticity robust and double clustered at the borrower and year level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome:	RERatio	REOwner(5%)	REOwner(10%)	High RERatio	RERatio	REOwner(5%)	REOwner(10%)	High RERatio
DDRank	0.008***	0.031***	0.031***	0.030***	0.010	0.006	-0.003	-0.019
	(0.002)	(0.004)	(0.004)	(0.004)	(0.008)	(0.024)	(0.023)	(0.019)
Ind FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	44,606	44,606	44,606	44,606	1,021	1,021	1,021	1,021
R-squared	0.196	0.160	0.173	0.157	0.349	0.346	0.349	0.351

Table IA.10
First Stage for IV Regression Results

This table presents the first stages for the IV regression results in columns (1) and (2) of Panel B, Table 6. These IV regressions analyze the differential effect of economic shocks between years t-5 and t (Shock) on the debt issuance of real-estate intensive firms, captured by higher values of RERatio (see Table 6). In these first-stage regressions, the dependent variable is Shock × RERatio, which captures this differential shock on real-estate intensive firms. In column (1), Shock is the housing price growth in the MSA of the firm's headquarter (HP Growth), and HP Growth × RERatio can be interpreted as a shock to the value of firms' real estate assets. In column (2), Shock is the county-level employment growth around the firm's headquarter. RERatio is defined as the ratio of net real estate assets (land and improvements, buildings, and construction in progress) over total net PPE. RERatio is defined as the value of this ratio at t-5 for all years up to 1998. Because of data availability, this ratio is measured at 1993 from 1999 on. The sample includes all non-financial and non-utility firms in Compustat without missing values for RERatio (firms need to be present in 1993 or earlier). Column (1) follows Carvalho (2018, cited in the paper) and uses the triple interaction of RERatio with measures of geographically-determined land unavailability at the MSA level (Uland1 to Uland5) and national real estate price growth (NPGrowth) as instruments for RERatio × HP Growth. Column (2) applies the Bartik instrument that projects national employment growth to the state level. All regressions in this panel control for industry and year fixed effects. Interactions of MSA real estate price growth with indicators for firms' industry are included as controls in column (1). Interactions of state employment growth with indicators for firms' industry are included as controls in column (2). All other variables listed below are also included as controls in each case. Firm characteristics (Size, Age, and Cash flow) are measured in year t -5. See the text for variable definitions and discussion of these approaches. Standard errors are heteroskedasticity robust and double clustered at the state and year level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Outcome: RERatio × HP Growth	Outcome: RERatio × StateEMPGrowth
	(1)	(2)
Uland_1	-0.060***	
	(0.016)	
Uland_2	-0.030	
	(0.020)	
Uland_3	-0.082	
	(0.085)	
Uland_4	0.026	
	(0.036)	
Uland_5	0.043	
	(0.039)	
NPGrowth × *Uland_1*	-0.776***	
	(0.184)	
NPGrowth × *Uland_2*	-0.584***	
	(0.168)	
NPGrowth × *Uland_3*	2.003**	
	(0.856)	
NPGrowth × *Uland_4*	-2.233***	
	(0.658)	
NPGrowth × *Uland_5*	-0.496	
	(0.440)	
NPGrowth × *RERatio*	0.595***	
	(0.113)	

Table IA.10 (Continued)		
Uland_1 × RERatio	0.135***	
	(0.030)	
Uland_2 × RERatio	0.053	
	(0.044)	
Uland_3 × RERatio	0.141	
	(0.147)	
Uland_4 × RERatio	-0.060	
	(0.071)	
Uland_5 × RERatio	-0.080	
	(0.076)	
MSA HP Growth	0.469***	
	(0.009)	
NPGrowth × Uland_1 × RERatio	1.602***	
	(0.350)	
NPGrowth × Uland_2 × RERatio	1.320***	
	(0.370)	
NPGrowth × Uland_3 × RERatio	-4.669***	
	(1.673)	
NPGrowth × Uland_4 × RERatio	4.350***	
	(1.359)	
NPGrowth × Uland_5 × RERatio	1.366	
	(0.832)	
RERatio × Bartik		0.935***
		(0.034)
Bartik		-0.458***
		(0.029)
RERatio	-0.018**	0.008
	(0.008)	(0.008)
Size	-0.000	0.000
	(0.000)	(0.000)
Age	0.000	-0.000
	(0.000)	(0.000)
CashFlow	0.000	-0.001
	(0.008)	(0.003)
StateEMPGrowth		0.448***
		(0.037)
Ind FE	Yes	Yes
Year FE	Yes	Yes
Industry-by-MSA HP Growth	Yes	
Industry-by-State Employment Growth		Yes
Observations	17,408	8,935
R-squared	0.926	0.952

Table IA.11
Lead Arranger Share and Borrower-Loan Characteristics

This table presents results relating shares retained by lead arranger to borrower and loan characteristics. %LoanShare is the share of the officer's bank (lead arranger) in the loan (in percentage points). When this variable is missing, we impute it based on the loan syndication structure (see the text). Size is the log of total assets. Analyst Coverage is the number of analysts covering the borrower. DDRank is the quintile ranking of a firm's distance-to-default among Dealscan-Compustat universe loans originated in the same quarter. Single Lead is an indicator for whether the bank is the only lead arranger bank that the borrower has had. All borrower characteristics are measured in the year prior to the loan. We control for loan characteristics (Loan Size and Loan Maturity) and local demographics (Population, Average Home Value, Income per Household, Black Share, and Hispanic Share). Scaled effect equals the product of the coefficient and variable's mean in the top 50% and bottom 50% of its distribution in our sample. All regressions in this panel control for loan type, industry, loan officer and state-year fixed effects. Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Outcome: Bank Loan Share			
	(1)	(2)	(3)	(4)
Size	-2.052***			
	(0.582)			
Analyst Coverage		-0.220***		
		(0.079)		
DD Rank			-0.860*	
			(0.487)	
Single Lead				6.453***
				(1.997)
Scaled Effect	-5.132***	-2.630***	-2.031*	
	(1.455)	(0.942)	(1.148)	
Loan and Demographic Controls	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Observations	1,574	1,458	1,449	1,574
R-squared	0.359	0.356	0.364	0.366

Table IA.12
Do Local Officer Conditions Capture Borrrower Fundamentals?

This table reports results further addressing a concern that officers' recent local conditions capture borrowers' credit risk. The outcome variable is *Log of Spread* , the log of the all-in-drawn interest rate loan spread (in basis points) over the LIBOR. Panel A reports results further addressing the possibility that officers' local conditions contain information about borrower fundamentals. We estimate our previous effects in samples without local neighborhoods that are geographically or economically close to borrowers. In columns (1) and (2), we remove cases where the distance between the loan officer's property and the borrower's headquarter is in the bottom quartile of our sample. In columns (3) and (4), we remove cases where the borrower's industry representation in their officer's county (*Ind. Representation*) is in the top quartile of the sample. *Ind. Representation* is constructed using the 3-digit NAICS of the borrower's industry and is given by the ratio of this industry's employment share in the officer's county to its employment share at the national level. In each case, we estimate the specification in Table 2 (columns (1) and (2) of Panel A) using these samples. Panel B reports results further addressing concerns about borrower selection. We address this concern by showing our results in Table 2 (columns (1) and (2) of Panel A) with varying sets of controls for borrower credit risk. In columns (1) and (2), we additionally control for *DD Rank* × year fixed effects. *DDRank* is the quintile ranking of a firm's distance-to-default (*DD*) in the Dealscan-Compustat universe loans originated in the same quarter. In columns (3) and (4), we add more controls indicating borrowers' credit risk: *DDRank* , *Benchmark Spread* (*DD*), and *Benchmark Spread* (*Rating*). *Benchmark Spread (DD)* is the average spread for loans in the same *DDRank* and quarter. *Benchmark Spread (Rating)* is the average spread for loans in the same rating group and year. These averages are computed using all Dealscan-Compustat loans in each group. In columns (5) and (6), we remove *Leverage* and *Equity Volatility* from the basic set of controls. Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Excluding Regions Geographically or Economically Close to Borrowers

	Outcome: Log of Spread			
	Excluding Regions with Low Distance		Excluding Regions with High Industry Representation	
	(1)	(2)	(3)	(4)
	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius
Local HPGrowth (Quarter -1)	-7.742***	-16.203***	-9.444***	-11.347***
	(2.838)	(4.978)	(2.915)	(3.780)
Local HPGrowth (Quarters -2 to -4) - Avg Effect	-1.170	-0.445	-0.612	-1.047
	(1.368)	(2.277)	(1.287)	(2.201)
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes
Matched Officer Growth Controls - Bank × Region	Yes	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Observations	1,129	981	1,160	1,007
R-squared	0.652	0.634	0.619	0.605

Panel B: Results with Alternative Credit Risk Controls

	Outcome: Log of Spread					
	(1)	(2)	(3)	(4)	(5)	(6)
	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius
Local HPGrowth (Quarter -1)	-5.571***	-11.205***	-6.853***	-10.575***	-7.863***	-10.909***
	(2.027)	(3.221)	(2.257)	(3.052)	(2.237)	(3.298)
DDRank × Year FE	Yes	Yes				
Additional Credit Risk Controls			Yes	Yes		
No Leverage and Equity Vol Controls					Yes	Yes
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes	Yes	Yes
Matched Officer Growth Controls - Bank × Region	Yes	Yes	Yes	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,382	1,209	1,382	1,209	1,500	1,316
R-squared	0.690	0.692	0.713	0.679	0.603	0.585

Table IA.13
Additional Robustness Analyses

This table presents additional robustness results. Panel A removes loan-level controls from the baseline specification. Column (1) follows our baseline specification (column (2) of Panel A in Table 2). Column (2) estimates interactive effect of officer local housing price growth and the relevance of borrowers' real estate value (Column (6) of Panel A in Table 6). Panel B estimates our baseline specification (column (2) of Panel A in Table 2) in different samples. Column (1) restricts the sample to officers with properties located in only one state. Column (2) restricts the sample to loans with only one or two officers. Column (3) looks at only the largest facility in each loan package. Standard errors are heteroskedasticity robust and double clustered at the officer and borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Removing Loan Controls

	Outcome: Log of Spread	
	(1)	(2)
	No Loan Controls	No Loan Controls
Local HPGrowth (Quarter -1)	-11.838***	-2.830
	(3.328)	(3.520)
Local HPGrowth (Quarters -2 to -4) - Avg Effect	-1.223	
	(1.849)	
LHPGrowth (Quarter -1) × High RERatio		-14.250***
		(5.414)
Matched Officer Growth Controls - State	Yes	Yes
Matched Officer Growth Controls - Bank × Region	Yes	
MSA HP Growth Controls	Yes	Yes
Loan Type FE	Yes	Yes
Industry FE	Yes	Yes
Loan Officer FE	Yes	Yes
State × Year FE	Yes	Yes
Observations	1,316	1,119
R-squared	0.586	0.660

Panel B: Sample Filters			
	Outcome: Log of Spread		
	(1)	(2)	(3)
	Officers in One State	Officers per Loan <= 2	Only Largest Facilities
Local HPGrowth (Quarter -1)	-9.644**	-13.295***	-12.900***
	(3.967)	(4.656)	(3.463)
Local HPGrowth (Quarters -2 to -4) - Avg Effect	-2.180	-0.755	-0.904
	(2.588)	(2.335)	(2.040)
Matched Officer Growth Controls - State	Yes	Yes	Yes
Matched Officer Growth Controls - Bank × Region	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes
Observations	1,031	1,006	924
R-squared	0.623	0.662	0.643

Table IA.14
Additional Loan-Level Outcomes

This table presents our main results with alternative (non-pricing) loan-level outcomes as the dependent variable. Log(LoanShare) is the log of share of the officer's bank (lead arranger) in the loan. %LoanShare is the share of the officer's bank (lead arranger) in the loan (in percentage points). When loan share retained by the bank is missing, we impute it based on the loan syndication structure (see text). Strictness represents covenant strictness, computed following Murfin (2012). Log(FacilityAmount) is the log of loan amount (in millions). Log(Maturity) is the log of loan maturity (in months). Panel A reports the average effects of officer local economic experiences (columns (1) and (2) in Panel A of Table 2). Panel B estimates interactive effect of officer local housing price growth and the relevance of borrowers' real estate value (column (6) in Panel A of Table 6). Standard errors are heteroskedasticity robust and double clustered by officer and borrower. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: Effect of Local Housing Price Growth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Outcome:	Log(LoanShare)		%LoanShare		Strictness		Log(FacilityAmount)		Log(Maturity)	
	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius	10 Miles Radius	20 Miles Radius
Local HPGrowth (Quarter -1)	2.181	1.953	64.732	34.092	-0.616	-4.543	-2.678	-4.520	0.031	0.336
	(4.006)	(5.647)	(80.292)	(105.618)	(2.227)	(3.554)	(4.278)	(6.890)	(1.961)	(3.585)
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matched Officer Growth Controls - Bank × Regi	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,500	1,316	1,500	1,316	1,099	961	1,500	1,316	1,500	1,316
R-squared	0.397	0.432	0.324	0.347	0.410	0.419	0.484	0.470	0.313	0.326

Panel B: Interactive Effect of Local Growth and Borrower Real-Estate Ownership					
	(1)	(2)	(3)	(4)	(5)
Outcome:	Log(LoanShare)	%LoanShare	Strictness	Log(FacilityAmount)	Log(Maturity)
LHPGrowth (Quarter -1) × High RERatio	4.772	125.622	-4.878	-0.801	-1.049
	(6.682)	(150.313)	(6.124)	(8.983)	(6.224)
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes	Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes	Yes
Observations	1,119	1,119	795	1,119	1,119
R-squared	0.425	0.402	0.484	0.510	0.287

Table IA.15
Local Housing Price Growth Experiences: Primary Residences and Renters

This table extends our analysis of local housing price growth experiences by considering experiences around officers' primary residences and rental properties. Column (1) restricts our main sample to properties that are the officers' primary residences. Columns (2) and (3) expand our sample described in Table 1 to include officers that do not own a property and rent properties at the time of loan origination. Local housing price growth experiences around these rental properties are constructed in the same way as in our main results. Column (3) estimates the interactions of recent housing price growth around loan officer's neighborhood (i.e., LHP Growth (Quarter -1)) with separate indicators for each subsample. Column (4) focuses on effects only for renters. Owner (Renter) is an indicator that equals one for officers who are property owners (renters) at the time of loan origination. The results in columns (1), (2), and (4) are estimated using the same specification used in Panel A of Table 2 (column (1)). The results in column (3) are estimated using a specification analogous to Table 4 (column (1)) with different interactions (indicators for subsamples). Standard errors are heteroskedasticity robust and double clustered by officer and borrower. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Outcome: Log of Spread			
	20-miles radius			
	Primary Residence Only (1)	Owners and Renters (2)	Owners and Renters (3)	Renters Only (4)
Local HPGrowth (Quarter -1)	-9.175**	-8.613***		-7.427
	(3.571)	(2.384)		(6.035)
Local HPGrowth (Quarters -2 to -4) - Avg Effect	-2.641	-1.854		-5.067
	(2.118)	(1.413)		(3.848)
Local HPGrowth (Quarter -1) × Owner			-8.024***	
			(2.521)	
Local HPGrowth (Quarter -1) × Renter			-4.520	
			(3.561)	
Matched Officer Growth Controls - State	Yes	Yes	Yes	Yes
Matched Officer Growth Controls - Bank × Region	Yes	Yes		Yes
MSA HP Growth Controls	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Observations	1,172	2,255	2,634	815
R-squared	0.602	0.578	0.559	0.632