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Loan spreads and credit cycles: The role of lenders' personal economic experiences

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ABSTRACT

We provide evidence that changes in lender optimism can lead to excessive fluctuations in credit spreads across the credit cycle. Using data on the real estate properties of loan officers originating large corporate loans, we find that credit spreads overreact to sophisticated lenders' recent local economic experiences, captured by local housing price growth. These effects are only present when borrowers own real estate assets and during times of greater uncertainty about real estate values, i.e., boom-and-bust cycles in housing prices. Our analysis suggests that recent personal experiences shape sophisticated lenders' beliefs about real estate values, which affect their pricing decisions.

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Credit spreads tend to be low during credit booms, which are followed by 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 2007-2009 global financial crisis, there has been a renewed interest in 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? Do the beliefs of sophisticated and professional lenders also contribute to these fluctuations? What specific economic mechanisms lead to these excessive fluctuations in lender beliefs and credit spreads? While previous research has suggested that some market participants have overly optimistic beliefs during credit booms, we have limited direct evidence on how such beliefs shape loan spreads and the answers to the previous questions.[2]

In this paper, we address these questions by studying the role of personal economic experiences by sophisticated lenders, who are in charge of issuing large corporate loans. We consider a mechanism where the beliefs of sophisticated lenders originating such loans directly shape loan spreads. Specifically, 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

[2] 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.

economics on the role of personal experiences in shaping belief formation (Malmendier and Nagel (2011, 2016)).[3]

While previous research has documented these personal experience effects for individuals, it is unclear whether these biases can significantly shape the beliefs of professional lenders in a high-stake market. On the one hand, these decision makers are sophisticated and are disciplined by market competition, conditions that might eliminate such effects (List, 2003). On the other hand, loan pricing may be significantly influenced by the personal experiences of individuals inside banks given that lending decisions involve discretion and lenders may need to rely on their intuition in these decisions.[4]

We examine the effects of loan officers' personal economic experiences in the period leading to loan origination on the pricing of the loans they issue. We capture officers' personal economic experiences using the recent past housing price growth in the local areas where their real estate properties are located. This is intended to measure recent local conditions in broad areas that loan officers are more familiar with or physically present in. To this end, we use a unique dataset that identifies individual loan officers in charge of originating large corporate loans and the locations 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.

Our data covers corporate loans originated between 2000 and 2018, a period that includes a significant boom-and-bust cycle in credit markets. This period was associated with unusual movements in housing prices and significant attention to real estate markets (Favilukis, Ludvigson, and Van Nieuwerburgh (2017)). It is plausible to expect finance professionals to be particularly aware of such movements. Indeed, previous evidence suggests that homeowners are familiar with their local housing price growth (Case, Shiller, and Thompson (2012)) as well as their friends'

[3] 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)).

[4] 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. This idea is supported by both anecdotal and survey evidence in financial markets (e.g., Graham, Harvey, and Puri (2015)).

housing prices (Bailey et al. (2018)) during this period. In this setting, we interpret officers' local housing price growth as capturing the recent local economic conditions experienced by them.

Our focus on local economic experiences is motivated by the following reasons. First, local conditions near officers should be an important source of personal experiences.[5] Consistent with this idea, Kuchler and Zafar (2019) document that individuals overweight their recent local economic experiences, including their local housing price growth, when forming beliefs about national economic outcomes such as real estate prices. Officers' views on such national economic outcomes should shape their beliefs about the credit risk of large non-local corporate borrowers. If officers are exposed to these local experience effects, these experiences should influence the pricing of the corporate loans they originate. Importantly, analyzing local experiences allows us to design a novel identification approach to isolate the role of lender personal experience effects.

We start by motivating our analysis with simpler sources of evidence on the link between loan spreads and local housing price growth experiences of sophisticated lenders. We exploit the fact that, in our data covering the locations of loan officers' properties, officers frequently have properties in the tri-state area, including the states of Connecticut, New Jersey, and New York. In contrast, these states represent a much smaller share of borrower states. We show that higher recent housing price growth in the tri-state area is strongly associated with lower loan spreads during loan originations. Moreover, this pattern remains stable after we control for real estate price conditions near borrower headquarters and exclude borrowers in the tri-state area. We document this relationship using all loans in the Dealscan universe. Across a range of specifications, we estimate that a one-standard-deviation shock to experiences in the tri-state area is associated with a 20-27 basis points change in loan spreads. These results are consistent with the idea that officers' recent local economic experiences help shape loan spreads.

In our context, lenders' personal experiences can be related to other factors such as traditional borrower and bank conditions that should predict credit spreads. To determine the extent to which lenders *overweight* their personal experiences, we design a novel approach where we analyze local differences in real estate price growth across officers. Specifically, we examine the extent to which differences in the local conditions faced by officers within the same state and time shape the

[5] 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."

spreads in the loans they originate. We construct officer local areas as areas centered around their properties with a similar size to the average U.S. county (20-mile radius). We also use alternative definitions for officers' local areas such as 10- or 30-miles radius areas centered around properties. 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 these local differences in officers' past experiences are not systematically related to other factors also shaping loan spreads, such as economic conditions faced by borrowers. We also track how a loan officer prices loans differently over time, exploiting changes over time in the recent experiences of a given individual when analyzing these results. Using this identification approach, we find that credit spreads on corporate loans are significantly lower when loan officers recently experienced higher real estate price growth in their local areas. We estimate that a one-standard-deviation shock in officers' local, idiosyncratic experiences leads to a 7-13 basis points change in loan spreads, depending on the specification. Our evidence suggests that officers' personal economic experiences systematically affect loan pricing.

We find that these effects are concentrated during the significant boom-and-bust cycle in real estate prices during our sample period but are limited after this episode. Our analysis suggests that personal experience effects by sophisticated lenders are mostly relevant during periods with unusual movements in asset prices. This finding is consistent with the ideas that professionals are more limited in their ability to rely on historical data or existing methods to make decisions during such rare episodes (Greenwood and Nagel (2009)) and that excessive fluctuations in lender optimism mostly matter during such events (e.g., Kindleberger (1978), Minsky (1986)). Across different specifications, we estimate that a one-standard-deviation shock to officers' local experiences leads to a 12-25 basis points change in loan spreads during this period. Since we analyze idiosyncratic conditions faced by officers, we argue that the aggregate implications of these effects are likely to be economically stronger.[7]

We then investigate the specific mechanism through which local economic experiences shape lenders' beliefs and loan spreads and consider two alternatives. In the first mechanism, the personal

[6] The average distance in our sample between officers' properties and borrowers' headquarters is approximately 728 miles and is larger, for example, than the distance between New York City and Chicago.

[7] Intuitively, shocks to the experiences of all professionals can shift the policies of banks and market pricing of loans, conditions that should constrain the decisions of individual officers. We discuss how the magnitudes we estimate are important relative to the ones in other studies analyzing idiosyncratic shocks to lenders and loan pricing (Section 4.5).

experience effects we capture are *domain specific* to real estate: local experiences with real estate prices primarily shape lenders' beliefs about the value of borrowers' real estate assets. As lenders become more optimistic about the value of these assets, their perceived exposure to credit risk is reduced, i.e., they expect smaller losses given default. In the second mechanism, officers' local economic experiences affect lenders' beliefs about a broader range of economic conditions faced by borrowers (e.g., unemployment). Our results could capture the effect of broader local economic experiences or local experiences with real estate could influence officers' beliefs about a broader range of economic conditions. These beliefs could then influence how lenders perceive other determinants of credit risk, such as borrower cash flows.

Our analysis suggests that personal experience effects are domain specific and 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), including real estate assets. We find that our results are driven by 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 secured loans backed by real estate could be loans to riskier borrowers. To address this concern, we exploit differences in the composition of firms' balance sheets and focus on the share of real estate assets in borrowers' tangible assets. If lenders' beliefs about real estate values drive our results, personal experience effects should be concentrated among borrowers that own more real estate 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)). Indeed, our results are insignificant for firms with limited real estate holdings and are differentially important for real estate-intensive firms.

As an additional source of evidence on this mechanism, we consider the role of officers' local employment experiences (in the counties where they own properties). We document that our results remain stable as we control for these employment experiences. Importantly, we do not find an effect of local employment experiences on the loan spreads of real-estate intensive firms. This mechanism is consistent with direct evidence on the effect of local experiences on beliefs about national real estate values (Kuchler and Zafar (2019)).

Our results examine loan officers from lead banks originating large, syndicated loans, and suggest that the personal views of these officers can shape loan pricing. We discuss institutional features of this market, previous research, and anecdotal evidence that support this idea (see Section 1). Following this discussion, we examine if the effect of lenders' personal experiences on loan spreads is stronger in cases when lead loan officers should matter more for loan pricing. Consistent with this idea, we find that our results are only important for officers from lead banks and are not present for officers from participant banks, which are not in charge of setting loan terms. Officers should also matter more when there is less public information about borrowers. When there is greater information asymmetry between borrowers and lenders, the lead bank plays a more important role in monitoring and screening borrowers (Sufi (2007)) and establishing lending relationships with borrowers (Bharath et al. (2011)). Indeed, we find that the results are stronger for smaller firms and firms with less analyst coverage. Relatedly, our results are more pronounced when loan officers' decisions are less disciplined by market forces, as in cases where the officer's lead bank is predicted to hold a larger portion of the loan. In addition, we find that our results are stronger for and concentrated among riskier borrowers. This supports the intuition that lenders' beliefs about borrowers' assets shape their perceived loss given default and should mostly matter for borrowers with a significant risk of default. Finally, we 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 this idea, our results are stronger for younger loan officers.

We refine our analysis to address potential concerns that we do not capture the effect of lenders' personal economic experiences. First, we address concerns that local experiences might capture information about borrower and bank fundamentals. We highlight that our results remain similar if we focus on cases where there is a weaker potential link between officer conditions and borrower fundamentals. This evidence also helps to further address the possibility that officers are only influenced by local conditions because of costs associated with information acquisition.[8] Relatedly, we show that our spread results are unlikely to reflect changes in the pool of borrowers as they are not sensitive to the inclusion of a range of controls for borrower credit risk. Moreover, our results remain similar if we analyze the effect of differences in officer experiences within the same bank and time period. Lastly, we provide evidence that our findings are unlikely to capture

[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. 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.

changes in officer housing wealth, which could matter for loan pricing in the presence of agency problems inside banks, or other managerial characteristics correlated with officers' personal experiences.

Our paper 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 (Greenwood and Hanson (2013), Cheng, Raina, and Xiong (2014), Baron and Xiong (2017), Falenbrach, Prilmier, and Stulz, (2017), Bordalo, Gennaioli, and Shleifer, 2018). Our analysis makes two main contributions to this literature. First, we provide micro-level evidence that these fluctuations in beliefs help shape loan spreads. Second, we provide evidence on a specific channel connecting recent economic conditions to such distortions in loan pricing and beliefs, where sophisticated lenders overweight their recent personal experiences when forming beliefs about borrower asset values. Our results also relate to previous research connecting the past experiences of banks with credit losses to their new lending terms and highlighting the role of bank-wide factors such as institutional memory (Berger and Udell (2004), Murfin (2012), and Koudijs and Voth (2016)). More broadly, we also complement previous work analyzing the drivers of credit spreads across the credit cycle (e.g., Santos (2011), Gilchrist and Zakrajšek (2012), Drechsler, Savov, and Schnabl (2018), Ivashina and Sun (2011)).

We also contribute to the literature on the role of personal experiences in shaping financial decisions and beliefs. Our main contribution to this literature is to provide evidence on the role of recent economic experiences by finance professionals in shaping their beliefs and decisions in a high-stake market setting. Additionally, our paper contributes by documenting that these effects are domain specific and that past experiences with real estate prices mostly matter during periods with price instability and uncertainty. Our evidence complements previous research on the effect of recent economic experiences for individuals. While some research in this area has analyzed professionals such as corporate managers, in contrast with our paper, this literature has typically focused on the effect of managers' lifetime experiences in shaping their decisions.[9] Our focus on recent economic experiences of professionals and their role during credit cycles ties our results with the evidence in Chernenko, Hanson, and Sunderam (2016) on how personal experiences

[9] On the role of recent economic experiences for individuals, see Kaustia and Knupfer (2008), Choi et al. (2009), Chiang et al. (2011), Malmendier and Nagel (2011, 2016) and the references therein. Malmendier, Tate, and Yan (2011) and Schoar and Zuo (2017) provide evidence on the effect of managers' lifetime experiences on their decisions.

shaped the decisions of mutual fund managers to invest in nontraditional securities during the 2003-2007 mortgage boom. Their finding that inexperienced managers in areas with higher housing price growth invested more in these new securities complements our finding that personal experiences affect lender optimism and loan spreads in credit markets. Our findings also relate to the evidence in Chen (2017), which suggests that corporate managers overweight economic conditions around firm headquarters when forming expectations about national outcomes and making decisions in other establishments. As we discussed above, personal experiences can be correlated with a range of factors also shaping decisions. An important aspect of our analysis is our new empirical approach that allows us to further address such identification concerns while isolating the incremental contribution of the personal experiences from finance professionals on their decisions.

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

Our analysis builds on the idea that personal views by individual loan officers 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. We focus on loan officers from lead arrangers in the syndicated loan market. Lead arranger banks play an important role in the pricing of syndicated loans, despite the fact that a significant portion of these loans is allocated to other participant lenders. In contrast to participant lenders, lead arrangers directly negotiate with borrowers, establish and maintain relationships with borrowers, play an important role in evaluating the risk of the loan, and are primarily responsible for collecting information and monitoring borrowers. During the loan pricing process, lead arrangers first collect information on the borrower and potential investors and 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 and face some discretion in this process.[10] These considerations are particularly relevant in our sample, where most loans are revolving lines of credit and the participation of institutional

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

investors in syndicates is more limited.[11] Consistently, numerous studies document that shocks to lead arrangers significantly affect contract terms (see, e.g., Sufi (2007), Santos (2011), Murfin (2012), Chodorow-Reich (2014)). The relationships between lead banks and borrowers are also an important determinant of loan spreads (Santos and Winton (2008), Bharath et al. (2011), 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 some authority and discretion when determining corporate loan terms. This idea is supported by multiple sources of anecdotal evidence. In their LinkedIn profiles, 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 proposal and negotiation of all aspects of… loan structures." Syndicated loans in our sample are commonly written and monitored by small teams supervised by an officer with a position such as managing director or senior vice president.[12] Consistent with the idea that we capture individuals with authority, the majority of loan officers 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 consider 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.[13]

Recent research also provides evidence that the individual loan officers identified in our analysis have influence over 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 the observed variation in loan spreads, after controlling for borrower characteristics and bank conditions. Herpfer (2021)

[11] Lead arrangers typically hold significant portions of syndicated loans. 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. Moreover, only 10% of the loans in our sample are term loan B. As emphasized by Ivashina and Sun (2011), the presence of institutional investors in this loan market is largely concentrated in such loans.

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

[13] Section 2 in 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.

provides evidence that relationships between these individual officers and borrowers can significantly reduce loan spreads, which increase in new loans from the 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.

2. Data, Sample, and Variables

2.1. Data Sources and Sample Construction

We construct our sample by combining data from several sources. The sample period is from 2000 to 2018. We start with 24,459 syndicated loan contracts in LPC Dealscan with available information on loan contract terms (e.g., spreads, loan amount, and maturity) that are issued to U.S. public firms outside of financial and utility industries (SIC codes in 6000-6999 or 4900-4999) with available firm characteristics. We then identify loan officers responsible for originating syndicated corporate loans following the procedure outlined in Bushman et al. (2020). For each loan, we search for the borrower's SEC filings (8-K's, 10-Q's, and 10-K's) around the loan issuance date and identify credit agreements from exhibits attached to these filings. Credit agreements are available from SEC filings since 1994 but are sparse prior to 2000 (this leads to our 2000 start date). We then scrape the signature panel at the end of the credit agreements to identify the names of bankers underwriting the loan. Bankers' employment affiliations are mapped to Dealscan data to ensure that those institutions are also reported by Dealscan. We focus on loan officers from lead arranger banks (i.e., "lead bankers"), who are primarily responsible for setting loan terms (Section 1). Our search results in 3,291 lead bankers jointly underwriting 6,332 loans.

Next, we identify the property ownership records for these loan officers, which allow us to pin down the location and ownership dates of their real estate properties. Similar to the literature studying the effect of shocks to banks (e.g., Amiti and Weinstein (2018)), we examine how shocks to individual officers change their lending terms over time, and this requires more than one loan per officer. We thus focus on the 992 officers who lead-arranged at least two loans (in separate deals taking place over different years). Specifically, we search for bankers' property ownership records in LexisNexis Public Records database and 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 gather all the addresses related to the loan officer and then collect all the deed transfer records, mortgage records, and tax assessment records of those addresses. Using this information, we determine the dates when the officer gains and releases control of each property as well as the locations of these properties. We track the housing price growth rates in officers' local areas by combining this information with housing price data at the zip code level from Zillow. We also obtain additional data on the zip codes located in officers' local areas from zip-codes.com and demographic information from the 2000 Decennial Census.

Our final sample covers all loans where we can locate officers' properties using this approach. This sample includes 1,737 loans during 2000-2018 by 485 unique loan officers. Our matching rate in each of the two steps above is similar to the one in previous studies using these data sources (e.g., Bushman et al. (2020), Herpfer (2021), and Pool, Stoffman and Yonker (2012)). Appendix A provides a table listing the steps that lead to our final sample. Internet Appendix Section 1 describes in greater detail these data collection steps. Internet Appendix Section 4 shows that our sample loans have similar characteristics as loans in the Dealscan universe.

2.2. Final Database and Main Variables

With the above-listed data, we create a final database linking each loan contract to its lead officer(s). The unit of observation is a loan contract-lead officer. This database tracks individual loan officers and their local experiences over time across the different loans they originate. This allows us to contrast decisions by the same officer before and after shocks to these local experiences. Importantly, this database allows us to use all information on the experiences of different officers and their link with loan pricing. Given that some syndicated loans have more than one lead officer, this database includes 2,590 unique loan contract-lead officer observations. This approach follows previous research analyzing how shocks to individual lenders (lead arranger banks) shape lending terms in the syndicated loan market (Santos (2011), Murfin (2012), and Chodorow-Reich (2014)). We label this sample as the lender-loan sample. In our main results, we capture links across these observations by clustering our standard errors at both the borrower and officer levels. Additionally, we construct a loan-level sample, where there is a single observation for each loan. For each loan contract with more than one lead officer in our main sample, we select the officer that issues the largest number of loans.[14]

Our identification approach compares the local economic experiences of different officers within a same state and time period. We measure officers' local economic experiences using the housing price growth in a local area around their real estate properties. These local areas are intended to capture broad areas that loan officers are more familiar with or physically present in. In our main results, we construct local areas with a (geographic) size comparable to an average U.S. county, which has an area equivalent to a circle with about a 20-mile radius. We thus define local areas as zip codes located within a 20-mile radius circle centered on the property's zip code. Distance between two zip codes is measured based on their centroids. Using these circles around properties, as opposed to counties, has two important advantages. First, these areas are centered around the officers' location, which could be near a border of a county. Second, there are significant differences in the geographic area of counties across U.S. regions and states. In the Internet Appendix (Section 5), we show that our results are robust to defining local areas as a 10- or 30-miles radius around the property.

When constructing these local areas, we only keep properties owned by the loan officer during the 12 months leading to loan origination. For each local area of an officer-loan, we first compute local housing prices as the average value of housing price index across all zip codes in the local area. We then measure the local housing price growth in the period (year, semester, or quarter) leading up to loan origination. Finally, we calculate the average value for these local growth rates for the loan contract-lead officer. In the few cases where an officer owns more than one property in a state, we compute the average across all local areas associated with the officers' properties. In the rare cases where the officer owns properties in more than one state, we select the state where there is the largest number of observations in our data.[15]

This approach leads to the main independent variable used in our analysis: *Past Local HPGrowth.* Housing price growth is measured in different time horizons: year, semester, and quarter. If a loan is originated in month t, we measure the local growth in the year prior to

[14] Since we analyze shocks to individual loan officers, our results rely on variation over time in the local experiences of a same officer across multiple loans.

[15] Our results remain similar with alternative approaches to select one state such as selecting the state with the longest distance to the borrower (Internet Appendix Section 6). Focusing in one state is important to match officer experiences within a same state.

origination using the log difference of housing prices over a 12-month horizon: $Log(Local\ HPI)_{t-1} - Log(Local\ HPI)_{t-13}$, where $Log(Local\ HPI)_k$ is the log of the previous local price in month *k*. Similarly, we measure the local growth in the semester (6 months) and quarter (3 months) prior to origination using $Log(Local\ HPI)_{t-1} - Log(Local\ HPI)_{t-7}$ and $Log(Local\ HPI)_{t-1} - Log(Local\ HPI)_{t-4}$, respectively. In some results, we also measure this local growth in previous periods in an analogous way.[16] We note that the month of loan origination is 90 days prior to the Dealscan reported start date (Murfin (2012)). This accounts for the time lag between the loan 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, which is two to three months after the contract date.[17]

We also control for the average housing price growth in other areas inside the same state or Census division as officers' properties. *Adjacent Areas HPGrowth* measures the average housing price growth across all zip codes that are located *outside* of an officer's local area but are in the same MSAs as her properties. This captures the growth in adjacent, non-local areas (similar to "other counties") within the same MSA. *Matched Officer Growth – State* measures the average housing price growth in all local areas in the officer's state containing properties from other officers at any point in our sample. We exclude matched areas that overlap with the officer's own area(s). If the officer owns multiple properties in a state, we compute an average across these matched housing price growth rates across those properties.

2.3. Summary Statistics

Table 1 provides summary statistics on the main variables used in the analysis. We report these statistics both for the lender-loan and the loan-level samples. *Spread* 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 the mean and median loan amount being $907M and $450M, respectively.

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[16] For example, we use $Log(Local\ HPI)_{t-4} - Log(Local\ HPI)_{t-7}$, $Log(Local\ HPI)_{t-7} - Log(Local\ HPI)_{t-10}$, and $Log(Local\ HPI)_{t-10} - Log(Local\ HPI)_{t-13}$ when determining the local growth in Quarter -2, -3, and -4 prior to loan origination.

[17] 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 the date it 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 the mandate and the date the loan becomes effective (Rhodes (2000)). Murfin (2012) finds that the latter gap is consistent with the mandate and closing dates reported by some Dealscan loans. 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.

In the Internet Appendix Section 3, we show the geographical distribution of loan officers' properties and borrower headquarters. Both loan officer properties and borrower headquarters cover a wide range of states. Yet, there is a significant geographical separation between them. Consistent with this location gap, Panel D of Table 1 shows that the average distance between officer properties and headquarters is 728 miles, a distance greater than the one 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 Appendix B.

2.4. Sample Selection

As described above, our final sample is determined by data availability constraints. In the Internet Appendix (Section 4), we explicitly discuss whether the effect of personal experiences might be different in our sample of loans with available data on loan officers compared to the broad Dealscan sample. We address this issue by showing that the link between loan spreads and important determinants of credit risk (e.g., profitability and leverage) is very similar in our sample and in the Dealscan universe. Moreover, across a range of characteristics, borrowers and loan terms in our sample are comparable to the ones in the average Dealscan loan. The main difference is that borrowers in our sample are slightly larger and safer, as larger borrowers are more likely to disclose loan information, and our analysis in Section 5.3 suggests that this would lead us to underestimate the effects of interest. As discussed in Section 6, we also construct extended samples including officers who are renters.

3. Initial Evidence

We start our analysis by examining simpler sources of evidence on the link between loan spreads and real estate price growth experiences of sophisticated lenders. Figure 1 shows the time series variation in corporate loan spreads and loan officers' recent housing price growth experiences during our sample period (2000-2018). The figure documents that broad increases (decreases) in the recent economic experiences by lenders are associated with lower (higher) loan spreads. An important limitation of these patterns is that other macroeconomic factors changing over time could drive this link.

To better capture the role of officers' recent experiences in shaping loan spreads, we examine the link between the recent housing price growth in the tri-state area (states of Connecticut, New Jersey, and New York) and corporate loan spreads. We exploit the fact that, in our data covering the locations of loan officers' properties, these three states capture approximately half of the officer states in our main sample (45 percent) but represent a much smaller share of borrower states (12 percent). Consequently, recent real estate growth experiences in the tri-state area should affect the experiences of many officers but might have a limited link to the real estate values and economic conditions of many borrowers. Motivated by this idea, we predict loan spreads using the recent housing price growth in the tri-state area in the period prior to loan origination, after controlling for the price growth in the borrower's own state. We capture this tri-state growth using *TriState Past HPGrowth*, the equally weighted average growth rate (log difference) in the state HPI index across the three states (CT, NJ, and NY) prior to loan origination.

We measure recent housing growth in tri-state areas in a way that is analogous to our main analysis, using different time horizons (year, semester, and quarter). If a loan is originated in month *t*, we measure the state growth in the year prior to origination using the log difference in state HPI between months *t*-13 and *t*-1. Similarly, we measure the state growth in the semester (quarter) prior to origination using the log difference in state HPI between month *t*-7 and *t*-1 (month *t*-4 and *t*-1).[18] One concern with this analysis is that we might capture a link between housing prices in the tri-state area and borrower fundamentals such as housing prices in their own areas. To address this issue, we further control for the housing price growth in borrowers' headquarter states, measured over the same period (past year, semester, or quarter) as the tri-state growth. We also exclude borrowers headquartered in the tri-state area. Since we do not rely on information about the officer originating each loan, this test includes all Dealscan loans to public firms during 2000-2018 with non-missing data on loan terms and firm characteristics.

Table 2 shows the results. We find a strong link between higher recent housing price growth in the tri-state area and lower loan spreads. This effect is present for the last year, semester, and quarter prior to loan origination (Panels A, B, and C). Importantly, these results remain stable after we control for the price growth in the borrower's state and when we exclude borrowers in the tri-

[18] We use monthly data and rely on housing price index data at the state level from Zillow.

state area (headquarter in one of the three states). This suggests that we are not capturing the correlation between real estate conditions around borrowers and in the tri-state area. To evaluate the magnitude of the results, we focus on coefficients from columns (2) and (4) in each panel, where we control for the housing growth in the borrower's state. We then consider the effect of an idiosyncratic shock to tri-state past housing price growth that is unrelated to borrower state growth. To isolate the idiosyncratic component of tri-state growth, we regress *TriState Past HPGrowth* on the growth in the borrower's state and extract the residual. We estimate that a one-standard-deviation increase in this residual growth is associated with 20-27 basis points across different results in Panels A, B, and C.[19]

These findings suggest an important negative link between officer recent economic experiences and loan spreads. However, when interpreting this evidence, one might still be concerned that officers' experiences might capture other factors also shaping loan spreads such as borrower fundamentals. We design a novel identification strategy to address this type of concern.

4. Main Results

4.1. Identification Strategy

Our main empirical approach refines the previous analysis to better address identification issues. We relate differences in officers' past experiences within the same state and time period to the credit spreads in the corporate loans that they originate. Our identification strategy relies on two assumptions. First, officers overweight their recent local economic experiences when forming beliefs about national conditions. Second, differences in officers' local experiences within the 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 2.3). Therefore, differences in officer local experiences within the 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 individual officers respond to shocks to their recent experiences by tracking them over time. Of course, in principle, recent differences in officer local

[19] For example, this standard deviation is 1.3 percent for the last-quarter result in column (4) of Panel C and the average loan spread in this sample is 224.5. This implies an increase equal to (1.3%) × (-9.138) × (224.5bp) = - 27.3bp.

experiences within the same state and time could potentially capture other factors also shaping loan spreads. We discuss and address these possibilities as we present our analysis.

Figure 2 illustrates our empirical strategy with an example of two officer local areas (20-mile radius) in our sample, centered around Huntington (Suffolk County) and Cold Spring (Putnam County), both in New York State. The borrowers of those officers are headquartered in Atlanta and Miami, respectively. 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 area closer to Huntington relative to the area closer to Cold Spring predict gaps between the spread for the Atlanta-based borrower versus the Miami-based borrower. As we contrast such loans over the same time period, we are connecting the spreads on these loans to the recent idiosyncratic conditions in their respective officers' local areas within the state. An important motivation for our approach is the idea that these idiosyncratic conditions closer to officers are unlikely to be informative about their borrowers' fundamentals.

4.2. Empirical Specification

We implement the identification strategy discussed above. Intuitively, we track local experiences of officers prior to each loan's origination date and contrast loans originated by officers in the same state and time period. More precisely, we estimate the following specification:

$$log(Spread)_{lift} = \eta_i + \lambda_{s(l,i),y(l)} + \beta Past\ Local\ HPGrowth_{lit} + \delta' \boldsymbol{X}_{lift} + \epsilon_{lift}, \qquad (1)$$

where $log(Spread)_{lift}$ is the log of the loan spread for facility l issued to firm f by officer i in month t. $s(l,i)$ represents the state where officer i's properties are located at the time of loan origination and $y(l)$ is the year of loan origination. We use the log of spread as the outcome variable to limit the influence of large changes in the level of spreads for a small subset of risky loans. In the Internet Appendix (Section 7), we show that our results remain similar when estimated using the level of spreads.[20]

[20] Our mechanism predicts stronger effects for riskier loans and loan spreads have a right-skewed distribution, with a few borrowers that have significantly higher spreads and could drive average changes in the level of spreads. As we analyze the log of spreads, our effects can be interpreted as capturing percentage changes in the spreads paid by borrowers. Other studies following this approach include Graham, Li, Qiu (2008), Bae and Goyal (2009), Valta (2012), and Dougal et al. (2015).

Our variable of interest is $Past\ Local\ HPGrowth_{lit}$, the local housing price growth around officer i's properties during a recent period (quarter, semester, or year) prior to the month of loan origination (defined in Section 2.2). Its coefficient β captures the relation between loan spreads and officers' local economic experiences in the period leading up to loan origination. Two features of this specification capture the central points of our identification strategy. First, we include officer state-year fixed effects (λ_{st}) to contrast only loans originated by officers within the same state and year. This contrast isolates the sensitivity of credit spreads to recent idiosyncratic conditions within the state faced by the officer originating the loan. Second, we include loan officer fixed effects (η_i) and exploit the variation in loan pricing by the same officer over time.

Our estimation includes a range of controls (***X***), including borrower characteristics measured in the year prior to loan origination, loan characteristics (e.g., amount and maturity), and characteristics of officer's local areas (demographics and home values). More importantly, we include controls to address the concern that we could capture the effect of aggregate fluctuations (at the state or national level) in real estate price growth within a year, due to factors such as housing market seasonality. These controls include *Adjacent Area HPGrowth* and *Matched Officer Growth – State* (both defined in Section 2.2). Both variables are measured during the same period (year, semester, and quarter) as *Past Local HP Growth* and capture housing growth in the MSA or state of officers' local areas. We also show results with finer fixed effects (state × quarter) to control for these aggregate shocks (see Internet Appendix Section 18). Since there are few loans within the same state and quarter, our results become less precisely estimated with such finer fixed effects and we use this analysis as an additional robustness check on this issue. In some specifications, when analyzing the effect of loan officers' local growth in the past quarter (semester), we also control for officers' local growth rates in earlier quarters (semester) within a one-year range, such as quarters -4, -3, and -2 (semester -2) prior to the month of origination. However, given the serial correlation in house price changes, including these controls can limit the variation that we can use to estimate the effect of *Past Local HP Growth*. Therefore, we do not include these controls in our main specifications.

4.3. 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 3 reports the central results of our paper, based on the estimation of Equation (1) using the lender-loan sample during the period of 2000-2018. In Panel A, we analyze loan officers' local housing price growth during the year (12 months) leading up to loan origination. Panel B reports results for this local growth during the semester (6 months) prior to loan origination, and Panel C measures this growth in the quarter (3 months) prior to loan origination. As explained in Section 2.2, officer local areas are defined using the 20-mile radius centered in the properties' zip codes. We present results by adding controls in stages. In column (1), we only include loan officer fixed effects, loan term controls, loan type fixed effects, borrower characteristics, and local characteristics. In column (2), we include officer state × year fixed effects, which is a central point of our identification strategy. In column (3), we additionally control for adjacent area growth over the same horizon. We next add matched officer growth control in the same state (column (4)), and lastly, we remove loan term controls to test the sensitivity of our results to these controls (column (5) of Panel A). In column (5) of Panels B and C, we layer on the control for local housing price growth rates in previous periods (*Semester -2* and *Quarter -4, -3, and -2* prior to the origination date). We then add column (6) where loan term controls are removed. Across all horizons of past local growth, we find that more positive officer local experiences in the period before loan origination are associated with significant reductions in loan spreads. In each panel, our results remain significant across all control specifications and generate similar coefficients. Our results remain robust and become stronger as we include state × year fixed effects and controls for aggregate shocks to housing price growth, i.e., as we move from column (1) to (4) in each panel. This suggests that these aggregate shocks (at the national or state level) do not drive our results.

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 empirical approach is designed to isolate the influence of this component of experiences. As reported by Table 1, the idiosyncratic component of the last-quarter (last-year) experience has a standard deviation of 0.6 (1.8) percentage points. A one-standard-deviation shock to the last-quarter and last-year experiences leads to changes in loan spreads between 7 and 13 bps.[21] Given the face value of syndicated loans in our sample, a 13 bps change in spreads translates to around a $1 million difference in interest payment for a borrower per year. While these numbers may seem small, note

[21] For example, when analyzing the magnitude of the effect in column (5) of Panel A (-3.457), we predict a reduction in loan spreads equal to 3.457 × 0.018 × 211 bps = 13.12 bps, where 212bps is the average loan spread in our sample.

that we are capturing the effect of individual loan officers, which are subject to various constraints such as market conditions and bank policies. In Section 4.5, we show that these magnitudes are stronger during the boom-and-bust period in real estate prices (2000-2012) and comparable to the ones in related studies.

Taken together, our baseline results are consistent with the argument that lender optimism, induced by their recent local economic experiences, significantly influences credit spreads. In Internet Appendix Section 5, we show that these results are also similar when we define local areas using a 10-mile or 30-miles radius centered in officers' properties.

4.4. Loan-level Results

We repeat our baseline analysis (Equation (1)) using a loan-level sample ranging from 2000 to 2018. This helps address the concern that the outcome variable, loan spreads, has repeated values across officer observations associated with the same loan.[22] As discussed in Section 2.2, when there are multiple lead officers, we choose the officer that appears most frequently in our main sample. This choice is motivated by the fact that we include officer fixed effects in our analysis, which tracks how individual officers price loans over time in response to shocks to their recent experiences. Table 4 reports the results in a way that is parallel to our results in Table 3. Across all horizons and control specifications, our results remain economically and statistically significant with coefficients that are economically similar to the ones in Table 3. A one-standard-deviation shock to the last-quarter and last-year experiences now leads to changes in loan spreads between 7 and 17 bps. Overall, these findings show that our main results are not influenced by the issue above of repeated loan observations.

4.5. Results During and Outside the Boom-and-Bust Period

Our sample period covers a significant boom-and-bust cycle in real estate prices between the early 2000s and the aftermath of the subsequent financial crisis (see Figure 3). Motivated by historical narratives of lender optimism (Kindleberger (1978), Minsky (1986)), we examine if officers' local economic experiences have a stronger effect on loan spreads during this boom-and-bust period. According to these narratives, excessive fluctuations in lender optimism are mostly relevant during periods with unusual movements in asset prices such as real estate prices. Since

[22] One potential issue is that we might overstate the number of independent observations. In our main results, we take this issue into account by clustering our standard errors at both the borrower and officer levels.

these episodes are rare, professionals are more limited in their ability to rely on historical data or existing methods to make decisions during these periods (Greenwood and Nagel (2009)). This suggests that officers' personal experiences might affect more their decisions during these episodes. Additionally, professionals might pay more attention to their local real estate prices during this period (see below).[23]

We follow Favilukis, Ludvigson, and Van Nieuwerburgh (2017) and define this boom-and-bust cycle as the period from 2000 to 2012. Recall that our sample starts in 2000 and Panel A of Figure 3 suggests that this cycle ended around 2012, as prices start recovering in 2013. Panel B of Figure 3 depicts the google search volume for "real estate prices", which shows that the public paid significantly more attention to housing prices in the period until 2012. We separately estimate our main results (Tables 3 and 4) during this cycle (2000-2012) and the subsequent period (2013-2018). In the Internet Appendix Section 8, we report results using alternative years for the end of this cycle (2011 or 2013).

Table 5 shows the results. Panel A (B) reports results from the lender-loan (loan-level) sample. Within each panel, columns (1) and (2) present effects of past-year and past-quarter growth during the 2000-2012 period, and columns (3) and (4) present effects during the 2013-2018 period. Across both samples, we find that effects of local economic experiences are concentrated during the boom-and-bust period and are economically small and statistically insignificant outside that period. We evaluate the economic magnitudes of the effects during 2000-2012 using the same approach as in Section 4.3 (with values for this subsample). In Panel A (B), a one-standard-deviation shock to the last-quarter and last-year experiences leads to changes in loan spreads between 12 and 17 bps (17 and 25 bps) during the 2000-2012 period. This magnitude is reasonable given that the effect of individual lead banks on loan pricing is constrained by market conditions and the loan officers in our study only have some influence over decisions within their lead banks. In the extreme context of the 2007-09 financial crisis, Chodorow-Reich (2014) estimates that loan spreads for lead banks at the 10th percentile of bank health increased by 30-60 bps more than loan spreads for lead banks at the 90th percentile. Our magnitude is also similar to the one found in Dougal et al. (2015) when studying anchoring effects in the syndicated loan market. While this magnitude is limited, we

[23] Also motivated by these ideas, Greenwood and Nagel (2009) and Chernenko, Hanson, and Sunderam (2016) focus on understanding the decisions of finance professionals around unusual boom-and-bust patterns in the price of important assets (stocks and real estate).

should expect stronger effects from aggregate shocks to the personal economic experiences of many individuals in the market (when market constraints do not limit effects).

In the Internet Appendix (Section 8), we show that the insignificant effect from the 2013-2018 period is not driven by less variation in officer local experiences during this period. In Panels C and D of Table 5, we separately analyze our results during the real estate price boom (2000 to June of 2007) and crash (July of 2007 to 2012) periods. The starting point for the crash (2007Q3) is the beginning of the 2007-2009 financial crisis (Kahle and Stulz (2013)). We find that, while our results are economically stronger during the boom, they are also significant during the bust. Overall, our evidence is consistent with the view above from historical narratives of lender optimism, suggesting that lenders' economic experiences are most relevant during periods with unusual fluctuations in asset prices.

4.6. Timing of the Effects

We now focus on the 2000-2012 period and analyze the timing of the effects from officer local experiences in greater detail. Specifically, we estimate the effects from officers' local housing price growth in different time periods (years, semesters, or quarters) before and after the month of loan origination. If our results capture the effect of officers' local experiences, we should only observe a link between loan spreads and officers' local housing growth *before* loan origination, but not a link between loan spreads and these experiences *after* loan origination. We examine this prediction and also analyze the timing of the effect from experiences prior to loan origination in greater detail.

To implement this analysis, we estimate the following specification:

$$log(Spread)_{lift} = \eta_i + \lambda_{s(l,i)y(l)} + \sum_{k=t_1}^{t_2} \beta_k \times Local\ HPGrowth\ (Period\ k)_{lit} + \delta' \boldsymbol{X}_{lift} + \epsilon_{lift}, \quad (2)$$

where *Local HPGrowth* $(Period\ k)$ is officer i's local housing price growth during period k around loan origination. This variable is constructed in an analogous way to *Past Local HPGrowth* using different time periods. The time interval $[t_1, t_2]$ captures the overall period around loan origination where we measure local experiences. The remaining terms are defined in the same way as in Equation (1) (recall that t denotes the month of loan origination). When we examine annual growth rates, period k is a year and $[t_1, t_2]$ covers the time window from two years prior to loan origination until one year after this event. We measure officers' local growth in these three years separately, including *Year -1* (period from month *t*-13 to *t*-1), *Year -2* (month *t*-25 to *t*-13), and *Year +1* (month *t+1* to *t*+13). When we consider semester effects, k represents a semester (6 months), and $[t_1, t_2]$ is the time window from two semesters prior to loan origination until two semesters after loan origination. We thus measure officers' local growth in the following semesters: *Semester -2* (month *t*-13 to *t*-7), *Semester -1* (month *t*-7 to *t*-1), *Semester +1* (month *t+1* to *t+7*), and *Semester +2* (month *t+7* to *t+13*). Finally, we also analyze quarterly effects, where period k is a quarter (3 months) and $[t_1, t_2]$ covers the time window from four quarters prior to loan origination until four quarters after this event. We measure officers' local growth in *Quarter k* for *k = -4, -3, -2, -1, +1, +2, +3, +4*. For example, *Quarter -1* captures this growth from month *t*-4 to *t*-1, *Quarter -2* from month *t*-7 to *t*-4, *Quarter +1* from month *t+1* to *t+4*, and so on.

Figure 4 shows these results. We first analyze the annual effects. Panel A (B) reports the results from the lender-loan (loan-level) sample. Consistent with the prediction above, there is no significant link between loan spreads and officers' experiences in the year *after* issuance (year +1). Additionally, we find a negative link between officers' local housing price growth in the year immediately before loan issuance and loan spreads, but not the second year before issuance (i.e., year -2). This timing supports the idea that the most recent experiences have the strongest effect on beliefs and loans spreads, and is consistent with previous research (e.g., Fuster, Laibson, and Mendel (2010), and Murfin (2012)).[24]

In Panels C through F, we estimate these results for each semester or quarter around loan origination. In both cases, we find no significant link between loan spreads and officer local experiences right after loan origination. This provides additional support to the prediction above. In terms of the timing prior to loan origination, we find the most statistically significant and robust effects in the last period (quarter or semester). There is no clear trend in the quarterly coefficients as we get closer in time to loan origination (especially in the lender-loan sample), which could reflect the serial correlation in house price changes. Because of this serial correlation, there can be limited variation in the data to precisely estimate each of these effects over subsequent short

[24] In the same setting that we analyze, Murfin (2012) finds that bank-wide experiences with loan losses in the quarter before loan origination significantly affect lending terms. 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*…" (their emphasis).

periods. Therefore, it can be challenging to empirically isolate the timing patterns for these effects as we move into shorter time periods.

5. Economic Mechanism

We analyze the economic mechanism linking officers' local economic experiences to their beliefs and the pricing of loans. To do so, we examine the importance of our effects across different types of borrowers, lenders, and officer local areas. Specifically, we estimate the following specification:

$$log(Spread)_{lift} = \eta_i + \lambda_{s(l,i),y(l)} + \beta Past\ Local\ HPGrowth_{lit} \times Z_{lift}$$
$$+ \gamma Past\ Local\ HPGrowth_{lit} + \phi Z_{lift} + \delta' \boldsymbol{X}_{lift} + \epsilon_{lift}, \tag{3}$$

where Z_{lift} is a borrower, lender, or officer local area characteristic, $\boldsymbol{X}_{lift}$ is a vector of controls, and all other terms are defined in the same way as in Equation (1). The coefficient of interest β captures the differential importance of our effects for loans with characteristic Z. For expositional simplicity, we present the coefficients for *Past Local HP Growth* and its interaction with *Z* (β and γ), but not the coefficient on *Z* (ϕ). In all regressions, $\boldsymbol{X}_{lift}$ includes the same set of controls used in the estimation in Equation (1). Additionally, we control for the interaction between *Z* and *Adjacent Areas HP Growth* to address the concern that real estate growth in broader areas may have a link with loans spreads that depends on characteristic *Z*. To provide a better sense of economic magnitudes, we also report *Scaled Effect* when *Z* is a continuous variable: the product of β and the gap between the mean values of *Z* in the top and bottom 50% of its distribution. Motivated by the evidence from Section 4.5, we implement these additional analyses using the period 2000-2012, where our effects are concentrated.

5.1. Are the Results Driven by Beliefs About Real Estate Values?

We provide evidence on the type of beliefs driving the effect of officer local experiences on loan spreads. Our analysis builds on the idea that individuals overweight their recent local economic experiences when forming beliefs about national outcomes such as real estate prices (Kuchler and Zafar (2019), hereafter KZ). Officers' views about such national outcomes should shape their beliefs about the credit risk of large non-local borrowers. In principle, this link between local economic experiences, officer beliefs, and spreads could be driven by two different mechanisms.

In the first mechanism, the personal experience effects we capture are *domain specific* to real estate. Specifically, local experiences with real estate prices primarily shape lenders' beliefs about national real estate prices and the value of real estate assets on the balance sheet of large, non-local borrowers.[25] In contrast, these local real estate experiences do not affect other types of beliefs by loan officers, such as beliefs about national employment conditions. This mechanism is suggested by the evidence from KZ. Using expectations data, they show that individuals overweight their recent local housing price growth experiences when forming beliefs about national real estate prices, but not when forming beliefs about other national outcomes such as employment. As lenders become more optimistic about the value of these assets, their perceived exposure to credit risk is reduced, i.e., they expect smaller losses-given-default. The role of borrower real estate values in affecting creditors' recovery in default is consistent with previous research and institutional arrangements in the syndicated loan market. Acharya, Bharath, and Srinivasan (2007) show that higher liquidation values for firms' tangible assets significantly improve creditor recovery value. S&P (2011) mentions the importance of collateral for evaluating loss-given-default risk in this market and explains that syndicated corporate loans are typically secured by a broad range of assets, including tangible assets such as real estate.[26]

In the second mechanism, local economic experiences affect lenders' beliefs about a broader range of economic conditions faced by borrowers, including unemployment conditions. Our results might capture an effect of general local economic conditions (of which house prices is one aspect) on the assessment about the economy in general. Relatedly, we might capture an effect of local experiences with real estate prices, but these real estate experiences could influence officers' beliefs about a broader range of economic conditions. These beliefs could then influence how lenders perceive other determinants of credit risk, in addition to borrower real estate values, such as borrower cash flows.

[25] 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).

[26] While the current value of real estate assets can be asserted by appraisals, there is still significant uncertainty about the *future* value of real estate assets that will matter for future loan repayment (Littlejohns and McGairl (1998), Benmelech and Bergman (2009)).

To evaluate the importance of these two mechanisms in explaining our findings, we build on the following predictions. If our results are domain specific to real estate, officers' local experiences should have a stronger effect on loan spreads when borrower real estate assets matter more for loan performance. Moreover, under this scenario, officers' local experiences should not significantly affect loans when real estate assets have limited relevance for loan performance. This provides a key falsification test. On the other hand, if our results capture beliefs about the economy in general, officers' local experiences should affect loan pricing regardless of the relevance of borrower real estate assets for loan performance.

We examine these predictions by estimating Equation (3) with variables that capture the importance of borrower real estate values for loan performance (as the interacted variable *Z*). Panel A of Table 6 reports the summary statistics for the interacted variables (*Z*) used in this analysis. Panel B shows these results using officer experiences in the quarter before origination and the lender-loan sample. In the Internet Appendix (Section 11), we show similar results when we analyze experiences in the year before loan origination or use the loan-level sample.

First, we contrast loans backed by collateral that includes real estate assets with other loans, which can be unsecured or backed only by other types of collateral such as marketable securities or working capital. Here, *Z* is an indicator for loans backed by real estate (*Secured by Real Estate*). Borrowers' real estate assets should matter more for loan performance when real estate assets are included as part of the collateral in the loan. In the Internet Appendix (Section 9), we show that two thirds of loans in our sample 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.[27]

These results are reported in column (1), Panel B of Table 6. We find that officer local experience effects are differentially important for loans backed by real estate and are not significant for other loans. This supports the view that the effects we document are domain specific to real estate. However, one limitation of this evidence is that the choice of secured financing and 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.

[27] This pattern is similar for the universe of Dealscan-Compustat loans. Loans without information on the existence of collateral or collateral type are excluded from this analysis.

We overcome this limitation by exploiting differences in the composition of firms' tangible assets and focusing on the share of real estate assets on borrowers' balance sheets. In this analysis, *Z* is based on firms' real estate share, measured as a percentage of PPE. As we sort firms using their real estate share, we address the concern that higher values for *Z* might be associated with riskier borrowers. Prior literature documents that real estate share is higher for larger, older, and more profitable firms (Chaney, Sraer, and Thesmar (2012)). Indeed, we verify that a higher real estate share is marginally associated with lower credit risk (Internet Appendix Section 10). Our evidence from Section 5.3 then implies that this selection effect (link between *Z* and credit risk) should lead to slightly weaker results for firms with a higher real estate share. As discussed above, increases in the value of borrowers' assets provide a stronger protection for lenders. Consequently, higher real estate prices should matter more for loan performance when firms own more real estate, i.e., for real estate-intensive firms. Indeed, consistent with this idea, previous research has found that shocks to real estate prices have a stronger effect on the borrowing capacity of real estate-intensive firms (Chaney, Sraer, and Thesmar (2012), and Carvalho (2018)).

We use borrowers' real estate share to construct three measures for their real estate intensity (the interacted variable *Z*). We use a continuous ratio of real estate assets over net PPE (*Real Estate Ratio*), but also compare groups of firms with high and low *Real Estate Ratio*. This partition limits potential measurement error in real estate share and checks if our results are driven by firms with a high share of real estate.[28] Specifically, we use the following partitions. *High RERatio (>Median)* is an indicator that equals one if *Real Estate Ratio* is above its median value in the sample. *High RERatio (Top Tercile)* is an indicator that equals one if *Real Estate Ratio* ranks in the top tercile in the sample. On average, real estate assets represent 23% of the fixed assets from firms in our sample. The average value of *Real Estate Ratio* in groups with high real estate intensity is between 34 and 42 percent. This mean value is between 4 and 11 percent for firms with low real estate intensity. Firms without information on values for real estate assets are excluded from the sample.

[28] 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 *Real Estate Ratio* 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.

Panel B of Table 6 (columns (2) to (4)) reports the effects of officers' local experiences for firms with high and low real estate shares. Across the three different measures described above, we find that our results are significantly stronger for real estate-intensive firms. This differential effect for real estate-intensive firms has an economic magnitude comparable to the one from our average effect. Moreover, we find that the lender experience effects are never statistically or economically significant for firms with low 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 on their balance sheet.

This evidence suggests that our results are driven by beliefs about real estate. In principle, one might still be concerned that other types of beliefs by officers could also differentially affect borrowing terms for real estate-intensive firms. We provide arguments and evidence that mitigate this concern. We note that, 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. We then directly examine the differential effect of multiple types of economic shocks on the subsequent borrowing of real estate-intensive firms. Specifically, we follow the analysis of Carvalho (2018), who examines the effect of predicted shocks to regional real estate price growth on the borrowing of real estate-intensive firms. The Internet Appendix (Section 13) shows these results and provides more details. We first confirm that the borrowing (net debt issuance) of real estate-intensive firms differentially increases in response to higher regional real estate price growth. Next, we show that a range of alternative positive shocks to economic conditions, such as state employment growth, do not lead to this differential pattern. This suggests that, even if present, a link between local economic experiences and lender beliefs about alternative economic conditions would not have a stronger effect on real estate-intensive firms.

To further separate the two mechanisms, we examine additional predictions. If the previous results capture the effect of officers' local experiences with real estate, as opposed to general local economic experiences, these results should not significantly change when we control for officer local employment experiences. Additionally, if personal experience effects are domain specific, officers' local employment experiences should not affect their beliefs about real estate values. Therefore, under this scenario, officers' local employment experiences should not asymmetrically affect loan spreads for real estate-intensive firms.

We examine these predictions in multiple ways. We start by showing that our main results (Tables 3 and 4) remain virtually unchanged after we control for officer local employment experiences (Internet Appendix, Section 12). We then follow Equation (3) and include the interactions between real estate intensity with both local housing price growth experiences (*Past Local HPGrowth*) and local employment growth experiences (*Past County EmpGrowth*). We focus on the specification using *High RERatio (>Median)* as the measure of real estate intensity (column (3) of Panel B). *Past County EmpGrowth* is the employment growth in the county of officers' properties during the quarter prior to loan origination. This variable is constructed in an analogous way to *Past Local HPGrowth*. Panel C of Table 6 reports the results. We do not find an economically or statistically significant effect of local employment experiences on the loan spreads of real-estate intensive firms. Moreover, the effect of local housing growth experiences on loan spreads for real-estate intensive firms remains similar after we include these employment controls (column (1) of Panel C). These analyses provide additional evidence that the results we document are domain specific and capture the effect of local experiences with real estate prices.

Our collective evidence suggests that our results are driven by changes in lender beliefs about real estate values and capture personal experience effects that are domain specific. This interpretation is supported by direct evidence on the effect of local housing price experiences on individuals' beliefs and the fact that our results match the detailed predictions from this mechanism.

5.2. The Role of Lender Discretion

The beliefs of individual loan officers should only affect the pricing of loans to the extent that officers face discretion when determining loan spreads. We study whether our results become stronger when lead officers are likely to matter more for setting loan spreads. We examine this prediction by estimating Equation (3) with variables that capture the importance of officer discretion (as the interacted variable *Z*). In the analyses of interacted effects that follow, we use the lender-loan sample to ensure that we have more variation on the interacted variables *Z* of interest. Table 7 reports these results using officers' experiences in the quarter before loan origination. In the Internet Appendix (Section 16), we show all results from this table using officer experiences in the year prior to origination.[29] As discussed in Section 1, lead officers should have more discretion 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.[30] Additionally, when this information asymmetry is more pronounced, lending relationships between lead banks and borrowers are stronger (Bharath et al. (2011)), which also makes 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. In these tests, *Z* is *Size* (log of total assets) or *Analyst Coverage* (number of analysts following the borrower). Consistent with the view that lead arrangers are more important in such loans and need to have more "skin in the game," we show in the Internet Appendix (Section 14) that lead banks hold a larger share of the loan when borrowers are smaller or have less analyst coverage. Columns (1) and (2) then show that lenders' personal experiences generate stronger effects on loan pricing for borrowers with these characteristics.

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 loan's syndicate structure, i.e., the number of banks and participant lenders. *BankLoanShare* is the average share for lead banks on other loans with the same structure (*Z* variable in this analysis).[31] Column (3) confirms that our results are stronger when the predicted lead share is larger and shows that this effect is economically important. In addition, we consider the role of officer age. Previous research suggests that older and more experienced individuals are less likely to overweight recent experiences or data when forming beliefs (Greenwood and Nagel (2009), Malmendier and Nagel (2016), Chernenko, Hanson, and Sunderam (2016)). Motivated by this evidence, we also examine the interaction between officer local experiences and *Officer Age*, the age of the loan officer (in

[29] This analysis leads to the same qualitative patterns with comparable magnitudes to the ones in the quarterly effects. However, these annual effects are less precisely estimated, what limits our ability to detect statistically significant patterns.

[30] 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.

[31] Using predicted shares, as opposed to actual shares, addresses the issue that these shares and spreads are jointly determined by the lead arranger. Additionally, the data on these shares is missing for many loans.

years). Consistent with this literature, column (4) shows that our results are mostly relevant for younger loan officers.

In the Internet Appendix (Section 15), we perform a placebo test using officers working for participant banks, who have limited ability to influence loan spreads. We collect data on participant officers and do not find a statistically significant relationship between participant officers' housing price experience and loan spreads. This confirms that the link between local housing price growth experiences and credit spreads only exists for lead lenders who have influence over loan spreads.

5.3. The Role of Borrower 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, shifts in lender optimism about borrower asset values should differentially matter for riskier loans. When lenders are more optimistic about the value of borrowers' assets, they should expect smaller losses given default and be less concerned about increases in the risk of default. Therefore, if our results capture shifts in lender beliefs about real estate values, it is plausible to expect positive local experiences by lenders to disproportionately reduce spreads for riskier loans.[32]

We examine this idea by analyzing the link between our results and measures of borrower credit risk using Equation (3) (Z captures differences in this credit risk). Table 7 reports these results using officers' experiences in the quarter before loan origination.[33] We measure borrower credit risk using the Merton (1974) distance-to-default (*Distance-to-Default*), 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 large 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 that capture cross-sectional differences in borrower credit risk. *Distance-to-Default (Rank)* is the quintile ranking (1 to 5) of a

[32] We might also expect this result for other types of lender beliefs. For example, see Bordalo, Gennaioli, and Shleifer (2018) for a framework where lender optimism about borrower cash flows also leads to this prediction. This prediction is not unique to lender beliefs about real estate values but represents a consistency check on our mechanism.
[33] Recall that, in the Internet Appendix (Section 11), we show the results from Table 7 using officer experiences in the year prior to origination.

firm's distance-to-default in the universe of loans in Dealscan-Compustat originated in the same quarter. A higher value means a higher distance-to-default and lower credit risk. This measure is equivalent to the main measure of borrower credit risk used in Greenwood and Hanson (2013). *Distance-to-Default (Demeaned)* is the difference between the average distance-to-default across all borrowers in the same risk quintile (defined above) and the average distance-to-default across all loans, both defined using all Dealscan-Compustat loans and measured in the origination quarter of the loan of interest.

Columns (5) and (6) report the results, which show that lenders' personal experiences disproportionally affect riskier borrowers. The scaled effects suggest that the differential coefficients for firms with high versus low credit risk (above and below median) are around 9 and are similar to our average coefficient (see column (3) of Panel A in Table 5).

5.4. The Role of Local Real Estate Price Informativeness

We interpret our evidence as capturing a mechanism where officers overweight their recent local economic experiences when forming beliefs about national outcomes. This mechanism is motivated by the idea that sophisticated lenders often need to rely on their intuition when making loan pricing decisions, and this reliance on intuition exposes them to personal experience effects. An alternative possibility is that officers have fully rational expectations but, because information acquisition is costly, they rely on their local real estate prices when forming beliefs about the value of borrower real estate assets. An important challenge for this alternative mechanism is the fact that it implies significant costs for the acquisition of information about other neighborhoods in the same state.[34] We provide evidence against this costly information acquisition mechanism by analyzing the link between our results and the informativeness of local real estate prices (or local conditions more broadly) for borrower conditions. Under this mechanism, officers should rely more on local conditions when these conditions provide more informative signals about borrowers. In contrast, previous research suggests that the effect of local experiences on beliefs about national outcomes is unrelated to the informativeness of these local experiences (Kuchler and Zafar (2019)).

[34] As discussed in Section 4.3, our effect translates to a dollar amount of approximately $1 million (per year). In this narrative, costs of acquiring local information need 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.

We consider the following measures for the informativeness of local real estate prices (Z interacted variables). First, officer local conditions should be more informative about a borrower when the borrower's industry is well represented in the local area, i.e., high *Ind. Representation*. *Ind. Representation* is defined as the share of the officer's county employment by the borrower's industry (defined at the 3-digit NAICS level) divided by the employment share of this industry at the national level. Local real estate prices that are highly correlated with national prices can contain more information regarding the value of borrowers' real estate assets, which are large, remotely located corporations. Additionally, local prices are more likely to be informative signals about borrowers' assets when these prices are less volatile. We capture these points by measuring the correlation between local and national housing price growth (*HP Correlation*) and the volatility of local housing price growth (*HP Volatility*) in the five years prior to loan origination (using quarterly growth rates). Table 8 reports the interactions between our results and these variables. None of the interaction terms generate statistically significant coefficients. The scaled effects from these interactive coefficients are also economically small. These additional analyses reinforce the view that our results are unlikely to be explained by a mechanism where officers have rational expectations but face costs in acquiring information.

6. Alternative Explanations and Robustness Checks

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 as well as implement additional robustness checks on our findings.

One concern related to our results is that local experiences could capture borrowers' fundamentals. For example, in principle, local conditions in officers' neighborhoods could capture valuable information for predicting their borrowers' credit risk. Given our identification strategy (see Section 4.1), this concern is only relevant if officers' idiosyncratic conditions within their state predict the fundamentals of non-local borrowers. In Appendix C, we address this concern by showing that our results continue to hold in subsamples where this is unlikely to be the case, e.g., when the geographic or economic distance between the areas where borrowers and officers are located is larger. A related concern is that recent local conditions predict differences in spreads because they affect lenders' choice of borrowers. However, as we also show in Appendix C, our findings remain largely unchanged when we add or drop key controls for borrower credit risk, suggesting that this selection effect is unlikely to explain our results.

We next consider the possibility that our results might be 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. In Appendix C, we show that our findings are robust to more refined controls of bank-level conditions, including bank-level lending outcomes, bank-year fixed effects and matched experiences from officers working in the same bank and census division. This analysis suggests that bank fundamentals are unlikely to explain our results.

We then discuss and address the concern that the effect of housing price shocks on credit spreads that we capture may be explained by fluctuations in officers' wealth. Specifically, the concern is that officers' wealth may 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 simple wealth effect with some of our results. First, our results are only important for firms that own significant amounts of real estate assets (Panel B of Table 6). Second, these effects are not significantly more pronounced when local housing prices are better predictors of future wealth levels, i.e., when housing prices are less volatile (Table 8).

More importantly, recall that officers' local housing price growth in the last year before loan origination (year -1) matters more for credit spreads than this same local growth in the previous year (year -2) (Figure 4). 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). In contrast, as discussed in Section 4.6, it is plausible for officer personal experience effects to be the strongest in the data in the most recent period. Motivated by this point, in column (1) of Table 9, we explicitly contrast local experience effects across these two years. We estimate the same specification as in Figure 4 but now only include *Past Local HPGrowth (Year -1)* and *Past Local HPGrowth (Year -2)*. We find that officers' housing price experience in the year immediately before loan origination (*Year -1*) generates a significant, negative effect on loan spreads, while the housing price experience in the previous year (*Year -2*) does not. We analyze the difference between the two coefficients and find that it is economically and statistically significant.

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 predicting future housing prices in officers' local areas using their local growth in the two years before loan origination. To do so, we estimate the previous specification using an alternative outcome variable: *Local HP Growth (Quarter -8 to +4)*. This measures the cumulative local growth in officers' local areas from two years prior to loan origination until one year after loan origination (month *t*-25 to *t*+13, where *t* is the month of loan origination). Column (2) of Table 9 shows that local housing growth in the last year (year -1) is not a stronger predictor of future local prices than local housing growth in the previous year (year -2). In column (3), we find the same result when we include the same set of controls as in column (1).[35] This lends support to the argument that an officer influenced by simple wealth effects that has rational expectations should not react more strongly to housing price experiences in the last year relative to experiences in the previous year. This analysis suggests that simple wealth effects are unlikely to drive our results.

Could our results be explained by alternative officer characteristics? As we include officer fixed effects in our results, any confounding officer characteristics would need to change over time in a way that is systematically correlated with officers' recent local experiences. Our evidence in Section 4.6 further addresses this concern by showing that there is no link between spreads and officer local growth immediately after loan origination. If officer characteristics that predict loan spreads have some persistence over time, we should expect this selection effect to matter right after loan origination.

Lastly, we implement a few additional robustness checks. First, we test the robustness and importance of our results in an extended sample that also includes officers that rent properties (renters). We describe the construction of this sample in the Internet Appendix Section 1.4. In the Internet Appendix (Section 17), we report results using these owner-renter-combined samples during 2000-2018. We continue to find a statistically significant, negative relation between officers' local housing price growth experience and loan spreads, with coefficients that are comparable to the ones in Tables 3 and 4. We also separately estimate the effects for renters. While renters' real estate price experiences continue to generate negative coefficients for loan spreads, they are not statistically significant. This could be due to measurement error and the noise associated with the data collection process for renters and the fact that this sample is significantly smaller than our main sample.

[35] To implement a simple predictability regression of future real estate price growth, and avoid a look ahead bias, column (2) also shows this regression without officer fixed effects and borrower, officer, and loan controls.

We also show our main results using finer fixed effects (state × quarter fixed effects), building on our discussion in Section 4.2. While this approach allows us to better control for aggregate shocks (state or national) to officers' recent experiences within a year, it should lead to less precise estimates. Recall that our baseline results use state × year fixed effects but include important controls for these aggregate shocks within a year. The Internet Appendix (Section 18) shows that our results are robust to the inclusion of these finer fixed effects and remain with comparable magnitudes. Moreover, consistent with our discussion in Section 4.2, these effects are less precisely estimated.

In Internet Appendix Section 19, we examine the effect of officer local experiences on additional loan terms. Before discussing this evidence, 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 in lender optimism across the credit cycle (e.g., López-Salido, Stein, and Zakrajšek (2017), Mian, Sufi, and Verner (2017)). Our findings on loan spreads are directly connected to these important ideas and empirical patterns. We provide evidence that higher officer housing price growth is also associated with larger loan shares for officers' lead banks, but these effects are not statistically significant, likely due to data limitations or measurement errors. These patterns are consistent with the view that, given the data and mechanism we analyze, effects of officer personal experiences should be particularly strong for loan spreads.

7. Conclusion and Discussion

Do excessive fluctuations in lender optimism help amplify changes in credit spreads across the credit cycle? We find that higher recent growth in officers' local areas leads to significant reductions in loan spreads that are concentrated on borrowers with substantial real estate ownership and riskier loans. Our results suggest that these effects are driven by lenders' beliefs about real estate values and are domain-specific to real estate. Moreover, we find that these effects are concentrated during a period with a significant boom-and-bust cycle in real estate prices, when there is greater uncertainty about real estate values. Our analysis provides evidence that lender beliefs can induce excessive fluctuations in credit spreads and document the importance of a specific mechanism driving these effects: sophisticated lenders overweight their recent personal experiences when forming beliefs about credit risk. This mechanism contrasts with a market timing view where sophisticated agents primarily respond to distortions in beliefs by naive market participants, e.g., originating loans and then selling them to overly optimistic investors.

We note that our identification strategy focuses on the idiosyncratic experiences of loan officers, which could limit the economic magnitudes implied from our results. We should expect stronger effects from aggregate shocks to the personal economic experiences of many individuals in the market. For example, during an aggregate boom, all agents in this market become more optimistic and the beliefs of individual officers should be less constrained by opinions of bank credit committees and competitive pricing by other lenders.

These personal experience effects might also be relevant outside the corporate loans market that we analyze. Our identification approach exploits a useful feature of this market, i.e., corporate borrowers are large and located remotely from loan officers. However, personal experience effects may extend beyond this market. Mian, Sufi, and Verner (2017) analyze household debt booms around the world between 1960 and 2012, which are matched with low mortgage credit spreads and housing price booms. These credit booms are followed by subsequent declines in economic conditions and matched with overly optimistic forecasts about future economic activity. In the context of this broad range of credit booms, the distortions in lender beliefs about real estate values that we analyze could be relevant in shaping mortgage lending. While previous research has suggested that household optimism about real estate prices do not lead to increases in household leverage (Bailey et al., 2019), these lender optimism effects can potentially rationalize increases in household leverage during real estate price booms.

Finally, the notion that personal experiences from sophisticated lenders can shape their beliefs and lending terms is not limited to lenders' beliefs about real estate values. Our sample period covers a historically important credit cycle, where movements in real estate prices played an important role and received significant attention. This might explain the importance of real estate experiences and beliefs in our analysis. During other credit boom-and-bust episodes, other types of personal experiences and belief distortions could be relevant. The finding in Greenwood and Hanson (2013) that U.S. corporate credit booms predict low excess returns on corporate bonds is consistent with the broader relevance of these effects.

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Figure 1
Credit Spreads and Lender Economic Experiences: Aggregate Patterns

This figure shows aggregate patterns for corporate credit spreads on bank loans and measures of the recent economic experiences of lenders between 2000 and 2018. The solid line represents aggregate loan spreads, the average value of loan spreads across the universe of Dealscan-Compustat loans issued in the quarter. Loan spreads are in basis point over the LIBOR. The dashed line represents year-on-year national housing price growth (Past National HP Growth) for each quarter. National housing price growth is measured by the log difference of the national housing price index (Zillow) between month *t*-1 and *t*-13 (we calculate an equally weighted average of this growth across every month *t* in the quarter).

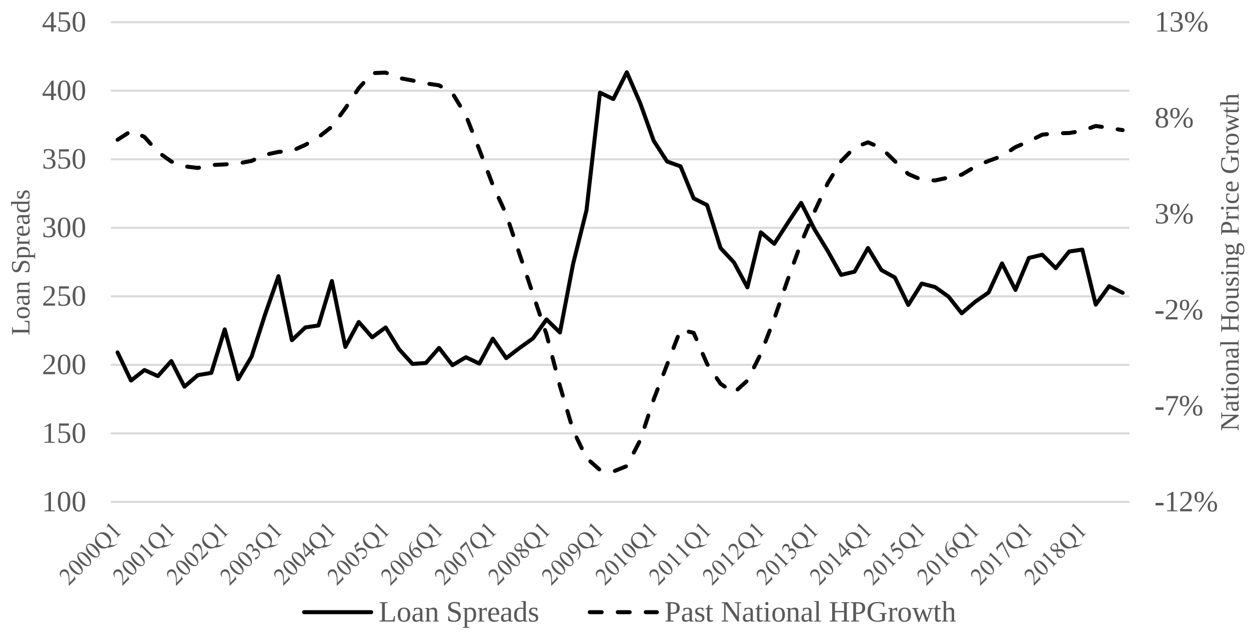


Figure 2
Identification Strategy: Example

This figure helps illustrate the identification strategy used in our empirical analysis. We provide an example of two officer local areas (20-mile radius) in our sample, centered around Huntington (Suffolk County) and Cold Spring (Putnam County), both in New York State. The figure also shows the location of the borrowers' headquarters in the loans associated with these two officer areas.

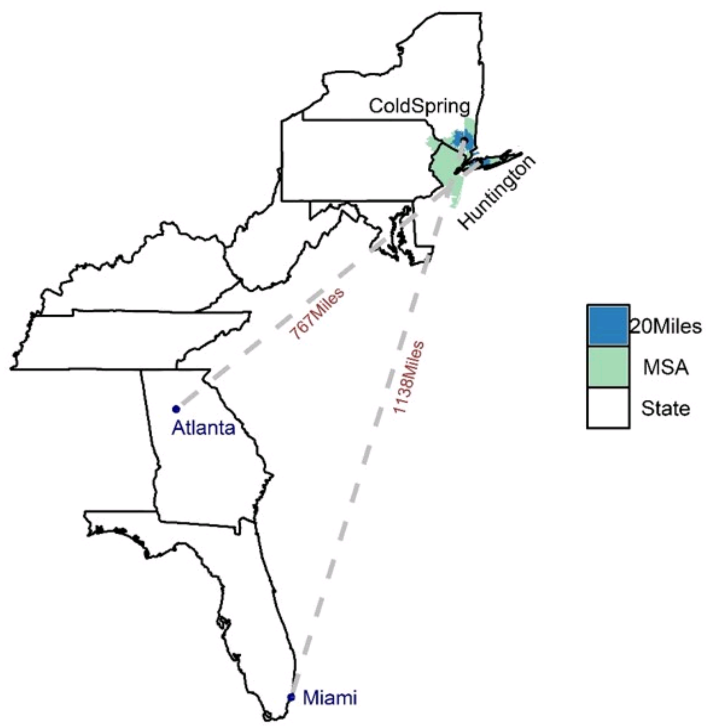
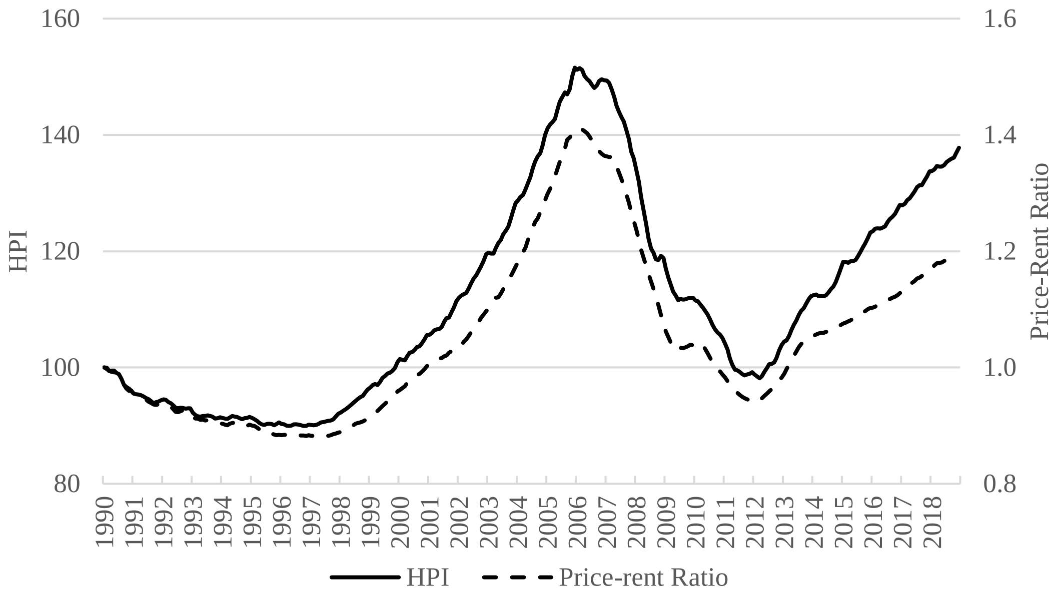


Figure 3
Real Estate Prices and Attention to Real Estate Prices

This figure shows real estate prices and attention to real estate prices during the period 1990-2018. Panel A shows housing prices using the Freddie Mac House Price Index (HPI) and price-rent ratios. The HPI is inflation adjusted. Price-rent ratios are calculated based on this HPI and the index of shelter from the Bureau of Labor Statistics (BLS). The HPI and price-rent ratios are normalized to a value of 100 and 1.0 in January of 1990, respectively. Favilukis, Ludvigson, and Van Nieuwerburgh (2017) show that these patterns are similar across data sources. Panel B shows the volume of Google searches for "real estate price" in each year between 2004 and 2018. This series is normalized to a value of 100 in 2004.

Panel A: National Housing Prices and Price-Rent Ratios Over Time



Panel B: Google Search Volume for "Real Estate Prices"

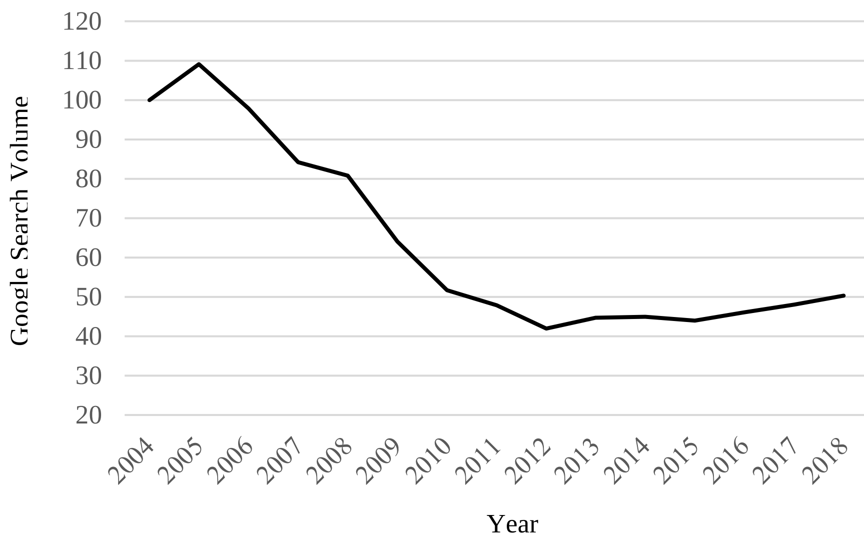
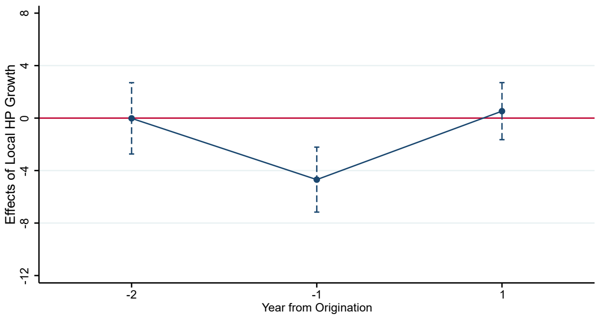


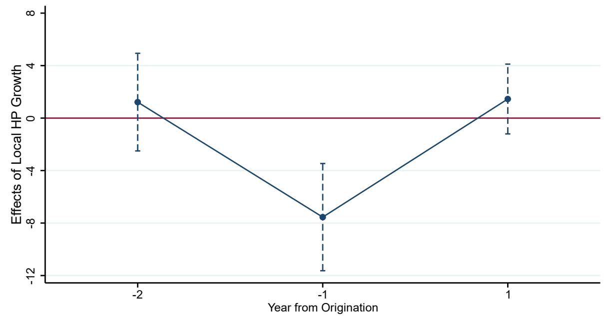
Figure 4
Timeline of Effects

This figure shows the effects from lenders' local economic experiences during different periods around loan origination. The results are based on the estimation of Equation (2). Panels A and B report annual effects, Panels C and D report effects by semester, and Panels E and F report quarterly effects (see Section 4.6 for more details). The outcome variable is *log(Spread)*. The left column reports results from the lender-loan sample, and the right column reports results from the loan-level sample. Within each panel, the horizontal axis represents the time past origination (i.e., -1 represents the year or the quarter before the month of origination). Note that there is no period zero because months before origination are classified as pre-periods and months after origination are classified as post-periods. The solid dots represent coefficient estimates, with 95th percent confidence intervals reported. Standard errors are heteroskedasticity robust and double clustered at borrower and loan officer level.

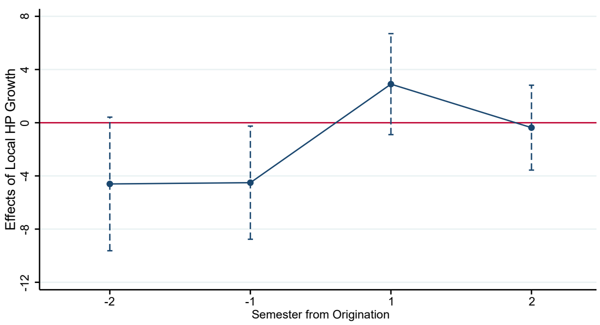
Panel A: Annual Effect, Lender-Loan Sample



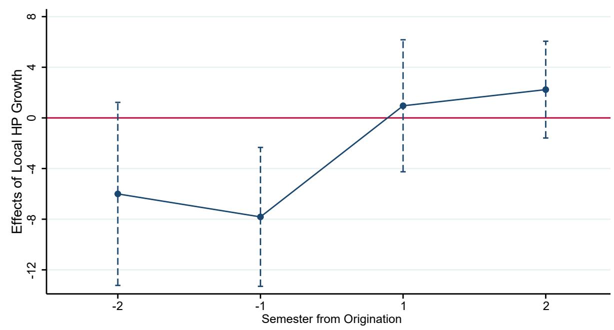
Panel B: Annual Effect, Loan-Level Sample



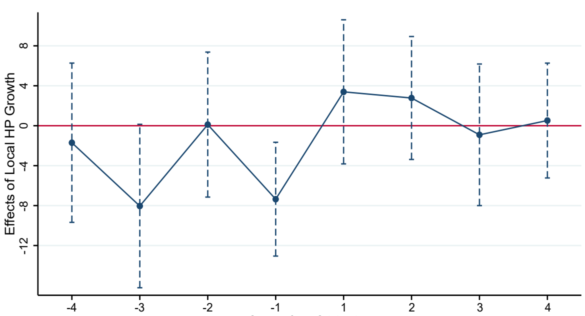
Panel C: Semester Effect, Lender-Loan Sample



Panel D: Semester Effect, Loan-Level Sample



Panel E: Quarterly Effect, Lender-Loan Sample



Panel F: Quarterly Effect, Loan-Level Sample

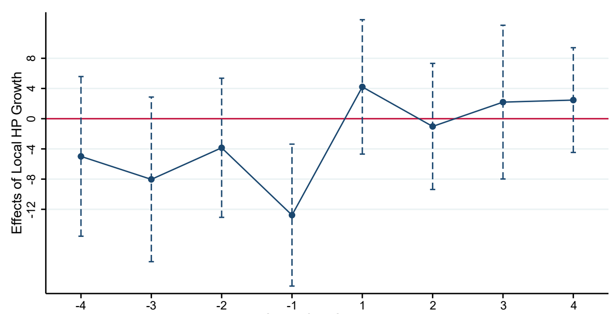


Table 1
Summary Statistics

This table presents the summary statistics for the main variables used in our study. We present the statistics for two samples: the lender-loan sample where the unit of observation is a loan contract-lead officer, and the loan-level sample, where the unit of observation is a loan. The construction of these samples is described in Section 2. Panel A shows the summary statistics for loan contract terms. Panel B shows the summary statistics for local housing price growth variables. Adjusted variables equal the original variable minus its mean in the officer state-loan year. Panel C shows the summary statistics for borrower characteristics. Panel D shows summary statistics for the distance between officers' properties and borrowers' headquarters. Observations with missing borrower headquarter locations are dropped. See Appendix B for variable definitions.

Panel A: Loan Terms

Sample:	Lender-Loan (2,590 obs)			Loan-Level (1,737 obs)		
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
Spread (in bps)	210.797	175	124.815	212.659	175	126.031
log(Spread)	5.184	5.165	0.610	5.186	5.165	0.630
Maturity (in Months)	54.490	60	17.990	54.180	60	18.674
Loan Amount (in $Millions)	906.946	450	1,811.402	796.411	400	1,864.021

Panel B: Local Housing Price Growth Variables

Sample:	Lender-Loan (2,590 obs)			Loan-Level (1,737 obs)		
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
Past Local HPGrowth (Quarter)	0.007	0.008	0.018	0.008	0.010	0.018
Past Local HPGrowth (Quarter) - Adjusted	0	0	0.006	0	0	0.006
Past Local HPGrowth (Semester)	0.013	0.019	0.036	0.016	0.022	0.036
Past Local HPGrowth (Semester) - Adjusted	0	0	0.011	0	0	0.010
Past Local HPGrowth (Year)	0.028	0.040	0.070	0.033	0.045	0.071
Past Local HPGrowth (Year)- Adjusted	0	0	0.018	0	0	0.018

Panel C: Borrower Charateristics

Sample:	Lender-Loan (2,590 obs)			Loan-Level (1,737 obs)		
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
Size	8.045	7.997	1.523	7.867	7.789	1.483
Age	23.169	18	16.987	23.022	17	16.795
Equity Volatility (Annualized)	0.377	0.328	0.186	0.385	0.336	0.189
Tangibility (Net PPE/Assets)	0.317	0.213	0.278	0.303	0.204	0.268
Leverage	0.328	0.317	0.211	0.323	0.305	0.218
Profitability	0.128	0.119	0.088	0.131	0.121	0.084
M/B	1.855	1.558	1.120	1.862	1.560	1.105
Rated	0.632	1	0.482	0.617	1	0.486

Panel D: Distance from Borrower HQ to Officer Properties

Sample:	Lender-Loan (2,526 obs)			Loan-Level (1,685 obs)		
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.
Distance	728.118	534.513	725.486	737.147	538.74	726.822

Table 2
Credit Spreads and Officer Economic Experiences: Initial Evidence

This table examines the link between the recent housing price growth in the tri-state area (states of NY, CT, and NJ) and corporate loan spreads. The sample period is 2000-2018. We estimate the following regression model:

$$log(Spread)_{lft} = \beta \times TriState\ Past\ HPGrowth_t + \delta' \boldsymbol{X}_{lft} + \epsilon_{lft},$$

where l represents a loan facility, f indicates the borrower firm, and t is the month of loan issuance. The unit of observation is a loan facility. The outcome variable *log(Spread)* is the log of the all-in-drawn interest rate (in basis points) over the LIBOR. *TriState Past HPGrowth* is the equally weighted average growth rate (log difference) in the state HPI index across the three states prior to loan origination. This growth is measured over the past year (12 months), semester (6 months), and quarter (3 months) in Panels A, B, and C, respectively. In each panel, columns (1) and (2) use all Dealscan loans to publicly listed firms outside of financial (SIC in 6000-6999) and utility (SIC in 4900-4999) industries with available data on firm characteristics. In columns (3) and (4), we exclude loans with borrowers headquartered in the tri-state area. *Borrower State Past HPGrowth* is the borrower headquarter state's HPI index growth over the same period (past year, semester, or quarter) as the tri-state growth. *Past Macro Conditions* include *S&P Returns*, *GDP Growth*, *Banking Sector Equity Growth*, and *Banking Sector Loan Losses*, measured over the same period as the tri-state growth. *Controls and Industry FE* include: *Borrower Characteristics* (*Equity Volatility*, *Size*, *Firm Age*, *Leverage*, *Profitability*, *Tangibility*, *M/B*, and *Rated*) measured during the year before loan origination, *Loan Term Controls* (*Loan Size* and *Loan Maturity*), 2-digit SIC industry fixed effects, and loan type fixed effects. See Appendix B for variable definitions. Standard errors are heteroskedasticity robust and clustered at the borrower level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Effect of Tri-state HP Growth, Last Year				
	Outcome: *log(Spread)*			
Sample:	All Dealscan-Compustat Loans		Loans with Borrowers Outside Tri-State	
	(1)	(2)	(3)	(4)
TriState Past HPGrowth (Year)	-1.793***	-2.006***	-1.782***	-2.013***
	(0.550)	(0.505)	(0.534)	(0.489)
Borrower State Past HPGrowth (Year)		Yes		Yes
Past Macro Conditions (Year)	Yes	Yes	Yes	Yes
Controls and Industry FE	Yes	Yes	Yes	Yes
Observations	24,459	24,459	21,432	21,432
R-squared	0.486	0.487	0.478	0.479
Panel B: Effect of Tri-state HP Growth, Last Semester				
	(1)	(2)	(3)	(4)
TriState Past HPGrowth (Semester)	-3.934***	-4.435***	-3.928***	-4.449***
	(1.082)	(0.953)	(1.063)	(0.941)
Borrower State Past HPGrowth (Semester)		Yes		Yes
Past Macro Conditions (Semester)	Yes	Yes	Yes	Yes
Controls and Industry FE	Yes	Yes	Yes	Yes
Observations	24,459	24,459	21,432	21,432
R-squared	0.475	0.476	0.467	0.468
Panel C: Effect of Tri-state HP Growth, Last Quarter				
	(1)	(2)	(3)	(4)
TriState Past HPGrowth (Quarter)	-8.113***	-9.211***	-8.005***	-9.138***
	(2.189)	(1.898)	(2.137)	(1.855)
Borrower State Past HPGrowth (Quarter)		Yes		Yes
Past Macro Conditions (Quarter)	Yes	Yes	Yes	Yes
Controls and Industry FE	Yes	Yes	Yes	Yes
Observations	24,459	24,459	21,432	21,432
R-squared	0.471	0.472	0.463	0.464

Table 3

Credit Spreads and Officers' Local Economic Experiences: Lender-Loan Sample

This table reports results connecting corporate loan spreads to the recent housing price growth in loan officers' local areas. We estimate Equation (1) using the lender-loan sample described in Section 2.2. The unit of observation is a lead officer-loan pair. The sample period ranges from 2000 to 2018. The outcome variable is *log(Spread)*, the log of the all-in-drawn interest rate (in basis points) over the LIBOR. The independent variable of interest is *Past Local HPGrowth*, the housing price growth rate (log difference in prices) in officers' local areas during the period prior to loan origination (see Section 2.2). Local housing price growth is measured over the past year (12 months), semester (6 months), and quarter (3 months) in Panels A, B, and C, respectively. Officer state is the state where officers' properties are located before loan origination. *Adjacent Areas HP Growth* is the average housing price growth in zip codes within the same MSA but outside officers' local areas. *Matched Officer Growth – State* captures the average housing price growth across local areas from other officers in the same state. Both variables are measured over the same time period (last year, semester, or quarter) as *Past Local HPGrowth*. *Loan Term Controls* include *Loan Size* and *Loan Maturity*. *Other Controls and Industry FE* include: *Borrower Characteristics* (*Equity Volatility*, *Size*, *Firm Age*, *Leverage*, *Profitability*, *Tangibility*, *M/B*, and *Rated*), *Local Area Characteristics* (*Population*, *Average Home Value*, *Income per Household*, *Black Share*, and *Hispanic Share*), 2-digit SIC industry fixed effects, and loan type fixed effects (term loan, revolver, or other). We control for officers' local housing price growth during earlier periods prior to loan origination in Panels B (semester -2) and C (quarter -2, -3, -4). See Appendix B for variable definitions. Standard errors are heteroskedasticity robust, double clustered at the officer and borrower levels, and reported inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Past Year (12-Month) Housing Price Growth						
	Outcome: *log(Spread)*					
Sample: Lender-Loan (2000-2018)	(1)	(2)	(3)	(4)	(5)	
Past Local HPGrowth (Year)	-1.909***	-3.140***	-3.448***	-3.391***	-3.457***	
	(0.394)	(0.701)	(0.776)	(0.800)	(0.802)	
Loan Terms Controls	Yes	Yes	Yes	Yes		
Observations	2,590	2,590	2,590	2,590	2,590	
R-squared	0.611	0.720	0.720	0.720	0.718	
Panel B: Past Semester (6-Month) Housing Price Growth						
	Outcome: *log(Spread)*					
Sample: Lender-Loan (2000-2018)	(1)	(2)	(3)	(4)	(5)	(6)
Past Local HPGrowth (Semester)	-2.926***	-3.889***	-3.724***	-3.637***	-3.000**	-2.987**
	(0.680)	(1.105)	(1.250)	(1.277)	(1.303)	(1.321)
Loan Terms Controls	Yes	Yes	Yes	Yes	Yes	
Observations	2,590	2,590	2,590	2,590	2,590	2,590
R-squared	0.604	0.718	0.718	0.718	0.719	0.718
Panel C: Past Quarter (3-Month) Housing Price Growth						
	Outcome: *log(Spread)*					
Sample: Lender-Loan (2000-2018)	(1)	(2)	(3)	(4)	(5)	(6)
Past Local HPGrowth (Quarter)	-5.388***	-6.974***	-6.300***	-6.124***	-5.301***	-5.375***
	(1.221)	(1.783)	(1.842)	(1.829)	(1.819)	(1.830)
Loan Terms Controls	Yes	Yes	Yes	Yes	Yes	
Observations	2,590	2,590	2,590	2,590	2,590	2,590
R-squared	0.602	0.719	0.719	0.719	0.720	0.719
Additional Controls and Fixed Effects Used in Each Column:						
Other Controls and Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes	Yes	Yes
Officer State × Year FE		Yes	Yes	Yes	Yes	Yes
Adjacent Areas HP Growth			Yes	Yes	Yes	Yes
Matched Officer Growth - State				Yes	Yes	Yes
Earlier Local HPGrowth (Panels B and C only)					Yes	Yes

Table 4

Credit Spreads and Officers' Local Economic Experiences: Loan-Level Sample

This table replicates our baseline results (Table 3) using the loan-level sample, described in Section 2.2, where the unit of observation is a loan. The sample period is from 2000 to 2018. For each loan contract with more than one lead officer in our main sample, we select the officer that issues the largest number of loans. The outcome variable is *log(Spread)*, the log of the all-in-drawn interest rate (in basis points) over the LIBOR. *Past Local HPGrowth* and all control variables are defined in the same way as in Table 3. See Appendix B for variable definitions. 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% level, respectively.

Panel A: Past Year (12-Month) Housing Price Growth

	Outcome: *log(Spread)*				
Sample: Loan-Level (2000-2018)	(1)	(2)	(3)	(4)	(5)
Past Local HPGrowth (Year)	-2.180***	-4.446***	-4.264***	-4.077***	-4.085***
	(0.514)	(0.950)	(1.122)	(1.135)	(1.148)
Loan Terms Controls	Yes	Yes	Yes	Yes	
Observations	1,737	1,737	1,737	1,737	1,737
R-squared	0.604	0.741	0.740	0.741	0.737

Panel B: Past Semester (6-Month) Housing Price Growth

	Outcome: *log(Spread)*					
Sample: Loan-Level (2000-2018)	(1)	(2)	(3)	(4)	(5)	(6)
Past Local HPGrowth (Semester)	-3.235***	-5.612***	-4.932***	-4.674***	-4.195**	-4.053**
	(0.896)	(1.720)	(1.707)	(1.730)	(1.724)	(1.759)
Loan Terms Controls	Yes	Yes	Yes	Yes	Yes	
Observations	1,737	1,737	1,737	1,737	1,737	1,737
R-squared	0.594	0.737	0.737	0.737	0.740	0.737

Panel C: Past Quarter (3-Month) Housing Price Growth

	Outcome: *log(Spread)*					
Sample: Loan-Level (2000-2018)	(1)	(2)	(3)	(4)	(5)	(6)
Past Local HPGrowth (Quarter)	-5.379***	-7.882***	-6.826***	-6.338**	-5.233**	-5.298**
	(1.590)	(2.806)	(2.636)	(2.682)	(2.536)	(2.545)
Loan Terms Controls	Yes	Yes	Yes	Yes	Yes	
Observations	1,737	1,737	1,737	1,737	1,737	1,737
R-squared	0.591	0.736	0.736	0.736	0.740	0.737

Additional Controls and Fixed Effects Used in Each Column:						
Other Controls and Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes	Yes	Yes
Officer State × Year FE		Yes	Yes	Yes	Yes	Yes
Adjacent Areas HP Growth			Yes	Yes	Yes	Yes
Matched Officer Growth - State				Yes	Yes	Yes
Earlier Local HPGrowth (Panels B and C only)					Yes	Yes

Table 5

Effects During and After the Boom and Bust in Real Estate Prices

This table separately examines the effect of officer local experiences during 2000-2012 (subperiod with boom-and-bust pattern in real estate prices) and 2013-2018 (subsequent period). In Panel A (B), we use the lender-loan (loan-level) sample to estimate the results from Table 3 (Table 4) in each of these subperiods. We further divide the 2000-2012 period into *Boom* (2000-Jun 2007) and *Bust* (July 2007-2012) and estimate our effects in *Boom* and *Bust* separately. Panel C (D) reports the subperiod results for *Boom* and *Bust* separately using the lender-loan (loan-level) sample. In all panels, the outcome variable is *log(Spread)*, the log of the all-in-drawn interest rate (in basis points) over the LIBOR. The independent variable of interest is *Past Local HPGrowth*, the housing price growth rate (log difference in prices) in officers' local areas during the year or quarter prior to loan origination (see Section 2.2). To estimate the difference between the coefficients of two subperiods (effect in period A minus the one in period B) and its standard error, we use the combined period (A and B). We start with the original specification used to estimate the results (Equation (1)) and add an indicator for period A, the interaction between this indicator and *Past Local HP Growth*, and the interactions of this indicator with all control variables and fixed effects in the original specification. *Controls and Industry FE* include: *Adjacent Areas HP Growth*, *Borrower Characteristics*, *Local Area Characteristics*, *Loan Term Controls*, industry fixed effects, and loan type fixed effects, all defined in the same way as in Table 3. See Appendix B for variable definitions. Standard errors are heteroskedasticity robust and double clustered at borrower and loan officer level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Effects During and After the Boom-and-Bust Cycle, Lender-Loan Sample

	Outcome: *log(Spread)*			
Sample Period:	Boom and Bust (2000-2012)	Post Period (2013-2018)	Boom and Bust (2000-2012)	Post Period (2013-2018)
Sample: Lender-Loan	(1)	(2)	(3)	(4)
Past Local HPGrowth (Year)	-4.494***	0.412		
	(1.199)	(1.034)		
Past Local HPGrowth (Quarter)			-9.519***	-3.144
			(2.767)	(2.222)
Controls and Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
Officer State × Year FE	Yes	Yes	Yes	Yes
Difference (2000 to 2012 - 2013 to 2018)	-4.906***		-6.375*	
	(1.614)		(3.602)	
Observations	1,463	1,127	1,463	1,127
R-squared	0.720	0.816	0.721	0.816

Panel B: Effects During and After the Boom-and-Bust Cycle, Loan-Level Sample

	Outcome: *log(Spread)*			
Sample Period:	Boom and Bust (2000-2012)	Post Period (2013-2018)	Boom and Bust (2000-2012)	Post Period (2013-2018)
Sample: Loan-Level	(1)	(2)	(3)	(4)
Past Local HPGrowth (Year)	-6.963***	-0.136		
	(1.872)	(1.382)		
Past Local HPGrowth (Quarter)			-14.602***	0.816
			(4.160)	(2.216)
Controls and Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
Officer State × Year FE	Yes	Yes	Yes	Yes
Difference (2000 to 2012 - 2013 to 2018)	-6.827***		-15.418***	
	(2.412)		(4.833)	
Observations	980	757	980	757
R-squared	0.760	0.793	0.757	0.793

Panel C: Effects During the Boom and the Bust, Subsample Analysis, Lender-Loan Sample				
	Outcome: *log(Spread)*			
Sample Period:	Boom (2000-Jun 2007)	Bust (Jul 2007-2012)	Boom (2000-Jun 2007)	Bust (Jul 2007-2012)
Sample: Lender-Loan	(1)	(2)	(3)	(4)
Past Local HPGrowth (Year)	-10.142***	-2.326		
	(3.324)	(1.580)		
Past Local HPGrowth (Quarter)			-20.568**	-5.809**
			(9.311)	(2.641)
Controls and Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
Officer State × Year FE	Yes	Yes	Yes	Yes
Difference (Boom - Bust)	-7.816**		-14.759	
	(3.646)		(9.668)	
Observations	472	991	472	991
R-squared	0.846	0.738	0.840	0.744

Panel D: Effects During the Boom and the Bust, Subsample Analysis, Loan-Level Sample				
	Outcome: *log(Spread)*			
Sample Period:	Boom (2000-Jun 2007)	Bust (Jul 2007-2012)	Boom (2000-Jun 2007)	Bust (Jul 2007-2012)
Sample: Loan-Level	(1)	(2)	(3)	(4)
Past Local HPGrowth (Year)	-17.036***	-3.790		
	(5.465)	(2.533)		
Past Local HPGrowth (Quarter)			-27.286**	-8.907*
			(10.535)	(4.583)
Controls and Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
Officer State × Year FE	Yes	Yes	Yes	Yes
Difference (Boom - Bust)	-13.246**		-18.379	
	(5.860)		(11.798)	
Observations	362	618	362	618
R-squared	0.868	0.768	0.855	0.771

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. We estimate Equation (2), where Z represents the importance of real estate values for the borrower or loan. The sample is the lender-loan sample during the period from 2000 to 2012. The outcome variable is log(Spread), the log of the all-in-drawn interest rate (in basis points) over the LIBOR. *Past Local HPGrowth (Quarter)* is the housing price growth rate (log difference in prices) in officers' local areas during the quarter prior to loan origination (see Section 2.2). Panel A reports summary statistics for the real estate variables used in the analysis. Panel B reports the interactive effects between our main results and the real estate variables. We use the following variables as Z. *Secured by Real Estate* is an indicator that equals one if the loan is secured by an asset class that includes real estate. *Real Estate Ratio* is the ratio of borrowers' real estate assets over PPE in the year prior to the loan. *High RERatio (>Median)* and *High RERatio (Top Tercile)* indicate whether *Real Estate Ratio* is above the sample median and in the top sample tercile, respectively. In Panel C, we extend our specification by adding *Past County EmpGrowth*, the employment growth in the county of officers' properties in the quarter before loan origination, and its interaction with *High RERatio (>Median)*. *Past County HPGrowth* is defined using counties as officers' local areas. In column (1) of Panel B, we exclude observations without information on the presence of collateral and its type. Interacting variables Z are included in the regressions but their coefficients are not reported. *Controls and Industry FE* include: *Adjacent Areas HP Growth*, *Borrower Characteristics*, *Local Area Characteristics*, *Loan Term Controls*, industry fixed effects, and loan type fixed effects, all defined in the same way as in Table 3. We also control the interactions between *Adjacent Areas HP Growth* and Z. For each interacting variable Z that is not an indicator, we report *Scaled Effect*, the product of the coefficient for the interaction and the gap between Z's mean in the top 50% and bottom 50% of its distribution. See Appendix B for variable definitions. Standard errors are heteroskedasticity robust and double clustered at borrower and loan officer level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Summary Statistics for Real Estate Variables

Sample: Lender-Loan (2000-2012)	Mean	Median	Std. Dev.	N
Secured by Real Estate	0.412	0	0.492	939
Real Estate Ratio	0.229	0.192	0.210	1,052
High RERatio (>Median)	0.499	0	0.500	1,052
High RERatio (Top Tercile)	0.333	0	0.471	1,052

Panel B: Differential Effects for Real Estate-Intensive Firms

	Outcome: *log(Spread)*			
Sample: Lender-Loan (2000-2012)	(1)	(2)	(3)	(4)
Past Local HPGrowth (Quarter)	5.314	-0.076	-0.797	-2.185
	(5.171)	(5.269)	(4.506)	(4.180)
Past Local HPGrowth (Quarter) × *Secured by Real Estate*	-23.865***			
	(7.091)			
Past Local HPGrowth (Quarter) × *Real Estate Ratio*		-37.070**		
		(15.218)		
Past Local HPGrowth (Quarter) × *High RERatio (>Median)*			-15.348**	
			(6.029)	
Past Local HPGrowth (Quarter) × *High RERatio (Top Tercile)*				-17.420***
				(5.281)
Controls and Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
Officer State × Year FE	Yes	Yes	Yes	Yes
Interacting Variable (Z)	Yes	Yes	Yes	Yes
Adjacent Areas HP Growth × Z	Yes	Yes	Yes	Yes
Scaled Effect		-12.23		
Observations	939	1,052	1,052	1,052
R-squared	0.778	0.785	0.786	0.784

Panel C: Comparing Local HP Growth and Local Employment Growth		
	Outcome: *log(Spread)*	
Sample: Lender-Loan (2000-2012)	(1)	(2)
Past Local HPGrowth (Quarter)	-0.813	
	(4.437)	
Past Local HPGrowth (Quarter) × *High RERatio (>Median)*	-14.290**	
	(6.043)	
Past County HPGrowth (Quarter)		1.395
		(3.937)
Past County HPGrowth (Quarter) × *High RERatio (>Median)*		-8.143*
		(4.406)
Past County EmpGrowth (Quarter)	0.158	0.263
	(2.387)	(2.382)
Past County EmpGrowth (Quarter) × *High RERatio (>Median)*	-2.574	-3.415
	(2.968)	(2.935)
Controls and Industry FE	Yes	Yes
Loan Officer FE	Yes	Yes
Officer State × Year FE	Yes	Yes
Interacting Variable (*High RERatio (>Median)*)	Yes	Yes
Adjacent Areas EMP Growth	Yes	Yes
Adjacent Areas HP Growth × *High RERatio (>Median)*	Yes	Yes
Adjacent Areas EMP Growth × *High RERatio (>Median)*	Yes	Yes
Observations	1,052	1,052
R-squared	0.786	0.783

Table 7
Heterogeneous Effects by Lender Discretion and Borrower Risk

This table analyzes the heterogeneous effects of loan officers' personal economic experiences on loan spreads across borrowers and lenders. We estimate Equation (2). The sample is the lender-loan sample during the period from 2000 to 2012. The outcome variable is *log(Spread)*, the log of the all-in-drawn interest rate loan spread (in basis points) over the LIBOR. *Past Local HPGrowth (Quarter)* is the housing price growth rate (log difference in prices) in officers' local areas during the quarter prior to loan origination (see Section 2.2). We analyze interactions between officers' experiences and the following variables (Z). *Size* is the log of total assets. *Analyst Coverage* is the number of analysts covering the borrower. *BankLoanShare* is the predicted share of the officer's bank (lead bank) in the loan (defined in Section 5.2). *Officer Age* is the age of the loan officer (in years). *Distance-Default (Rank)* is the quintile ranking of a firm's distance-to-default among Dealscan-Compustat loans. *Distance-Default (Demeaned)* is the average distance-to-default across borrowers within the same quintile category, demeaned by the average levels among Dealscan-Compustat loans issued during the same quarter. All interacting variables Z are included in the regressions but their coefficients are not reported. We also control for the interactions between *Adjacent Areas HP Growth (Quarter)* and Z. *Controls and Industry FE* include: *Adjacent Areas HP Growth*, *Borrower Characteristics*, *Local Area Characteristics*, *Loan Term Controls*, industry fixed effects, and loan type fixed effects, all defined in the same way as in Table 3. For each Z, we report *Scaled Effect*, the product of the coefficient for the interaction and the gap between Z's mean in the top 50% and bottom 50% of its distribution. See Appendix B for variable definitions. Standard errors are heteroskedasticity robust and double clustered at borrower and loan officer level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Outcome: *log(Spread)*					
Sample: Lender-Loan (2000-2012)	(1)	(2)	(3)	(4)	(5)	(6)
Past Local HPGrowth (Quarter)	-54.241***	-20.900***	-0.450	-42.138***	-17.659***	-9.038***
	(15.525)	(4.549)	(4.116)	(15.641)	(5.119)	(2.807)
Past Local HPGrowth (Quarter) × *Size*	5.473***					
	(1.882)					
Past Local HPGrowth (Quarter) × *Analyst Coverage*		0.975***				
		(0.335)				
Past Local HPGrowth (Quarter) × *BankLoanShare*			-0.496***			
			(0.182)			
Past Local HPGrowth (Quarter) × *Officer Age*				0.739**		
				(0.334)		
Past Local HPGrowth (Quarter) × *Distance-to-Default (Rank)*					3.969**	
					(1.742)	
Past Local HPGrowth (Quarter) × *Distance-to-Default (Demeaned)*						2.887**
						(1.313)
Controls and Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes	Yes	Yes
Officer State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Interacting Variable (Z)	Yes	Yes	Yes	Yes	Yes	Yes
Adjacent Areas HP Growth × Z	Yes	Yes	Yes	Yes	Yes	Yes
Scaled Effect	13.396	11.766	-10.442	9.414	9.742	9.026
Observations	1,463	1,463	1,463	1,412	1,357	1,357
R-squared	0.728	0.728	0.728	0.726	0.754	0.753

Table 8

The Role of Local Real Estate Price Informativeness

This table examines if our results are stronger when local real estate prices are more likely to be informative about borrower fundamentals. We estimate Equation (2). The sample is the lender-loan sample during the period from 2000 to 2012. The outcome variable is *log(Spread)*, the log of the all-in-drawn interest rate loan spread (in basis points) over the LIBOR. *Past Local HPGrowth (Quarter)* is the housing price growth rate (log difference in prices) in officers' local areas during the quarter prior to loan origination (see Section 2.2). We analyze interactions between officers' experiences and the following variables (Z). *Ind. Representation* is the ratio of an 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 local areas. *HP Correlation* is the correlation between the quarterly local housing price growth rate in the officer's local areas and the quarterly growth rate of the national housing price index. All interacting variables Z are included in the regressions but their coefficients are not reported. *Controls and Industry FE* include: *Adjacent Areas HP Growth*, *Borrower Characteristics*, *Local Area Characteristics*, *Loan Term Controls*, industry fixed effects, and loan type fixed effects, all defined in the same way as in Table 3. For each Z , we report *Scaled Effect*, the product of the coefficient for the interaction and the gap between Z's mean in the top 50% and bottom 50% of its distribution. See Appendix B for variable definitions. Standard errors are heteroskedasticity robust and double clustered at borrower and loan officer level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Outcome: *log(Spread)*		
Sample: Lender-Loan (2000-2012)	(1)	(2)	(3)
Past Local HPGrowth (Quarter)	-10.156***	-11.309*	-7.516
	(3.263)	(6.842)	(7.826)
Past Local HPGrowth (Quarter) × *Ind. Representation*	0.140		
	(0.919)		
Past Local HPGrowth (Quarter) × *HP Volatility*		242.293	
		(472.598)	
Past Local HPGrowth (Quarter) × *HP Correlation*			-3.070
			(10.042)
Controls and Industry FE	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes
Officer State × Year FE	Yes	Yes	Yes
Interacting Variable (Z)	Yes	Yes	Yes
Adjacent Areas HP Growth × Z	Yes	Yes	Yes
Scaled Effect	0.565	2.291	-1.562
Observations	1,353	1,463	1,463
R-squared	0.741	0.720	0.720

Table 9
The Effect of Recent Experiences on Loan Spreads versus Housing Wealth

This table reports results addressing the concern that changes in officers' housing wealth may explain our main findings. We contrast the effects of officers' experiences in each of the two years prior to loan origination. We use the lender-loan sample during 2000-2018 and estimate the following specification:

$$Y_{lift} = \eta_i + \lambda_{s(l,i),y(l)} + \sum_{k=-2}^{-1} \beta_k \times Past\ Local\ HPGrowth(Year\ k)_{lit} + \delta' \boldsymbol{X}_{lift} + \epsilon_{lift},$$

where i is an officer, l is a loan, f is the borrower firm, s is the officer's state, t is the month of loan origination, and $y(l)$ is the year of loan origination. *Past Local HPGrowth* $(Year\ k)$ is officer i's local housing price growth in one of the two years prior to loan origination (k = -1, -2). We measure officers' local growth in *Year -2* (month *t*-25 to *t*-13) and *Year -1* (month *t*-13 to *t*-1). In column (1), the outcome variable Y_{lift} is *log(Spread)*, the log of the all-in-drawn interest rate loan spread (in basis points) over the LIBOR. In columns (2) and (3), the outcome is the cumulative local growth in officers' local areas from two years prior to loan origination until one year after loan origination (month *t*-25 to *t*+13, or Quarter -8 to +4). In column (2), we predict future price growth using the past price growth in *Year -1* and *Year -2*. In column (3), we repeat the analysis in column (2) using the same set of controls and fixed effects as in column (1). In all columns, we report the difference between the estimated coefficients for the two years $(\widehat{\beta_{-1}} - \widehat{\beta_{-2}})$. We also report estimated standard errors for this difference using $Var(\widehat{\beta_{-1}} - \widehat{\beta_{-2}}) = Var(\widehat{\beta_{-1}}) - 2Cov(\widehat{\beta_{-1}}, \widehat{\beta_{-2}}) + Var(\widehat{\beta_{-2}})$ and estimates for each of these terms come from the estimated covariance matrix for the regression model. *Adjacent Areas HP Growth* controls are constructed in an analogous way to Table 3 but are now defined over the same time periods as *Past Local HPGrowth* $(Year\ k)$. *Controls and Industry FE* include: *Borrower Characteristics*, *Local Area Characteristics*, *Loan Term Controls*, industry fixed effects, and loan type fixed effects, all defined in the same way as in Table 3. See Appendix B for variable definitions. Standard errors are heteroskedasticity robust and double clustered at borrower and loan officer level. We report the standard error for each estimate inside brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Outcome:	*log(Spread)*	*Local HPGrowth* (Quarter -8 to +4)	
Sample: Lender-Loan (2000-2018)	(1)	(2)	(3)
Past Local HPGrowth (Year -1)	-3.419***	1.265***	1.116***
	(0.793)	(0.055)	(0.061)
Past Local HPGrowth (Year -2)	-0.309	1.186***	1.141***
	(0.861)	(0.040)	(0.068)
Controls and Industry FE	Yes		Yes
Loan Officer FE	Yes		Yes
Officer State × Year FE	Yes	Yes	Yes
Adjacent Areas HP Growth	Yes	Yes	Yes
Differential Effect: Year -1 over Year -2	-3.110***	0.079	-0.025
	(1.200)	(0.067)	(0.075)
Observations	2,590	2,590	2,590
R-squared	0.721	0.990	0.995

Appendix A: Sample Steps

The table below reports the sample attrition related to each of our data collection steps.

Sampling Steps	
#Loans in Dealscan-Compustat Universe	24,459
#Loans matched to SEC documents (w. valid lead arranger signatures)	6,332
#Officers extracted from SEC Documents	9,784
#Lead Officers from SEC Documents	3,291
#Lead Officers with At Least 2 Loans	992
#Lead Officers Matched to LexisNexis	560
#Lead Officers Owning Properties Within 12 Months of Loan Origination Date	485
#Loans Originated by the Above Lead Officers	1,737
Total Loan Amount (Billion)	
Loans in Dealscan-Compustat Universe	12,250.34
Loans matched to SEC documents	3,194.48
Loans in our final sample	1,383.37

Appendix B: Variable Definitions

1. Loan Term Variables (*DealScan*)

log(Spread): the log of all-in-drawn loan spread over LIBOR.

Loan Maturity: the log of the loan maturity (in months).

Loan Size: the log of the total loan amount (in U.S. dollars).

Loan Type: an indicator variable that indicates if the loan is a term loan or if the loan is a revolver.

Secured (Unsecured): an indicator variable that equals one if the loan is secured (unsecured).

Secured by Real Estate: an indicator that equals one for loans that are secured by real estate collateral including the following asset classes: all assets, PPE, or real estate. We construct this variable only for loans that either have known collateral or are unsecured.

BankLoanShare: the share of the officer's bank (lead arranger) in the loan. When data on this allocation is missing, we first calculate the average allocation for lead and non-lead banks in syndicates without missing data and the same syndicate structure (number of lead and participant banks). This provides us with the total allocation for lead and participant banks in the syndicate being analyzed. We then equally split the shares among lead and participant banks within each of these two groups (lead and participant banks).

2. Local Housing Price Growth Variables

We denote the month of loan origination as *t* and define the following variables.

Past Local HPGrowth (Quarter): the housing price growth (difference in the log of housing price) in officers' local areas between month *t*-1 and *t*-4 prior to loan origination.

Past Local HPGrowth (Semester): the housing price growth (difference in the log of housing price) in officers' local areas between month *t*-1 and *t*-7 prior to loan origination.

Past Local HPGrowth (Year): the housing price growth (difference in the log of housing price) in officers' local areas between month *t*-1 and *t*-13 prior to loan origination.

For the above variables, local areas around each property are constructed using a 20-mile radius centered around the property. Housing price growth in a local area is the average value across the housing price growth in all zip codes whose centroids belong to the local area. If an office owns more than one property in its state, we take an average across the housing price growth in these local areas.

TriState Past Local HPGrowth: the equally weighted average housing price growth (difference in the log of housing price) across the states of Connecticut, New Jersey, and New York over a period (quarter, semester, year) prior to loan origination. These periods are defined in the same way as in *Past Local HP Growth*.

Borrower State Past HPGrowth: the housing price growth (difference in the log of housing price) in the borrower's headquarter state over a period (quarter, semester, year) prior to loan origination. These periods are defined in the same way as in *Past Local HP Growth*.

Adjacent Areas HP Growth: the average housing price growth in the region outside officers' local areas but within the same MSA as officers' properties over a period (quarter, semester, year) prior to loan origination (defined in the same way as in *Past Local HP Growth*). Housing price growth is computed as the log difference in housing price index in a zip code. We then take the average of this growth rate across all zip codes in the adjacent area.

Matched Officer Growth: the average housing price growth in a set of matched local areas from other loan officers. It is computed in an analogous way as *Past Local HPGrowth*. We define this variable using matched officers in the same state or matched officers in the same bank and Census division. When searching for matched officers, we consider officers in these groups that appear in the sample at any point in time.

HP Correlation: the correlation between the quarterly housing price growth for the officer's local area (20-mile radius area) and the quarterly national housing price growth during the 20 quarters (5 years) prior to loan origination. *HP Volatility* is the standard deviation of the quarterly house price growth for the officer's local area (20-mile radius area) during the 20 quarters (5 years) prior to loan origination.

3. Bank and Loan Officer Characteristics

BankLendingAmount: the total lending amount by the bank in other loans in the same quarter. This lending amount is computed using the shares of each loan retained by the bank. In this calculation, we use the allocation data from *DealScan* to get the shares of the loan allocated to each bank. When data on this allocation is missing, we follow the approach described above in the context of the variable *BankLoanShare.*

BankLoanSpread: the weighted average (using loan size as weight) of loan spreads across all other loans by the bank in the same quarter. This average is calculated only using loans where the bank is a lead arranger.

Bank Lending Controls include the two variables above.

Officer Age is the age of the loan officer (in years).

4. Borrower Characteristics

Equity Volatility: the annualized standard deviation of daily stock returns.

Size: the log of total assets (*at*).

Firm Age: the number of years since the firm first appeared in the Compustat database.

Profitability: the ratio of operating income (*oibdp*) to total assets (*at*).

Tangibility: the ratio of property, plant, and equipment (ppent) to total assets (at).

M/B: (stock price (*prcc*) × shares outstanding (*csho*) + total assets (*at*) – book equity (*ceq*))/total assets (*at*).

Leverage: the ratio of long-term debt (*dltt*) plus current debt (*dlc*) to total assets (*at*).

Rated: an indicator variable that equals one if the firm has a bond rating.

Distance-to-Default: the Merton (1974) distance-to-default measure of credit risk, estimated using the approach in Bharath and Shumway (2008).

Real Estate Ratio: the ratio of real estate assets to Total PPE (*ppegt*), both measured at historical costs. Real estate assets include PPE Buildings (*fatb*), PPE Construction in Progress (*fatc*), and PPE Land and Improvements (*fatp*).

Analyst Coverage: the number of analysts following the borrower (source: I/B/E/S).

Industry Representation: 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 $(\text{Emp}_{c,j,t}/\text{Emp}_{c,t}) \div (\text{Emp}_{US,j,t}\ /\text{Emp}_{US,t})$, 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.

All borrower characteristics listed below are measured in the year prior to loan origination.

5. Local Area Characteristics

Population: the log of the population.

Average House Value: the log of the average house value.

Income per Household: the log of the income per household.

Black Share: the percentage of black population.

Hispanic Share: the percentage of Hispanic population.

All variables are measured at a fixed point in time using information from the 2000 Decennial Census. The variables are calculated using average values for the officer's local area using information from its zip codes. If an office owns more than one property in its state, we take an average across the values for these local areas.

6. Local Employment Experiences (*QCEW*)

Past County EmpGrowth: the change in the log of officers' county employment during a period prior to loan origination. These local employment variables are calculated using averages across all the counties where the officer has properties in the state and using quarterly private-sector employment for counties. For example, if a loan is originated in quarter *q*, we measure *Past County EmpGrowth (Quarter)* using log differences in county employment between quarter *q*-1 and *q*-2.

7. Macroeconomic Indicators

S&P Return: S&P 500 return in a period (quarter, semester, or year) prior to loan origination (Data: CRSP).

GDP Growth: the average growth rate of U.S. GDP in a period (quarter, semester, or year) prior to loan origination (Data: U.S. Bureau of Economic Analysis).

Banking Sector Equity Growth: the average growth rates of equity to asset ratio of the U.S. banking sector in a period (quarter, semester, or year) prior to loan origination (Data: FDIC).

Banking Sector Loan Losses: the average loan losses (scaled by total equity capital) of the U.S. banking sector in a period (quarter, semester, or year) prior to loan origination (Data: FDIC).

Appendix C: Addressing Concerns About Borrower and Bank Fundamentals

We further address the concern that local experiences could capture borrowers' or banks' fundamentals. For example, in principle, local conditions in officers' neighborhoods could capture valuable information for predicting their borrowers' credit risk. Given our identification strategy (see Section 4.1), this concern is only relevant if officers' idiosyncratic conditions within their state predict the fundamentals of non-local borrowers. We address this possibility with the following tests. First, we remove cases where the distance between borrowers' headquarters and their loan officers' properties is at the bottom quartile of our sample.[36] We next exclude cases where borrowers' industries are highly represented in the local areas surrounding loan officers' properties (top quartile in our sample). Industry representation (*Ind. Representation*) is defined as the share of the officer's county employment by the borrower's industry (defined at the 3-digit NAICS level) divided by the employment share of this industry at the national level. 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. While it is possible that local conditions are not directly informative about the borrower's industry but are informative about related industries (e.g., suppliers), we expect direct signals about the borrower's industry to matter most and consider this prediction here. Table C.1 reports results from these tests. Panel A (B) reports the results from the lender-loan (loan-level) sample. In each panel, columns (1) and (3) present coefficients from *Past Local HPGrowth (Year)* and columns (2) and (4) present coefficients from *Past Local HPGrowth (Quarter)*. Our results remain robust and even become slightly stronger in these alternative samples.

Another concern is that recent local conditions predict differences in spreads because they affect lenders' choice of borrowers. If this selection effect drives our results, our findings should change significantly when we add or drop key controls for borrower credit risk. These controls include leverage, equity volatility, credit ratings, and the Merton (1974) distance-to-default, estimated following the approach in Bharath and Shumway (2008). Leverage and equity volatility are already included in the baseline controls, so we examine whether our results are sensitive to removing them. We test the robustness of our results to the addition of three more variables indicating borrower distance-to-default and credit ratings. The first variable is the quintile ranking (1 to 5) of a borrower's *Distance-to-Default* relative to all Compustat-Dealscan loans issued during the same quarter (*Distance-to-Default (Rank)*) and captures cross-sectional differences in borrower credit risk. This measure is equivalent to the main measure of borrower credit risk used in Greenwood and Hanson (2013). The second is a distance-to-default-based market benchmark spread, computed as the average spread across all loans within the same distance-to-default quintile. Finally, we control for a credit rating-based benchmark spread, the average spread on all loans with the same credit rating. These benchmark spreads represent the market pricing of "comparable" firms with similar levels of credit risk. They are computed using all loans in the Dealscan-Compustat universe issued during the same quarter.

Panel C (D) of Table C.1 shows results from this analysis for the lender-loan (loan-level) sample. Note that some observations do not have information to compute distance-to-default (firms with zero debt in the year prior to origination), leading to a small sample attrition. We thus replicate our baseline results using the new sample in columns (1) and (4), add the three credit risk variables

[36] 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.

to the baseline specification in columns (2) and (5), and drop equity volatility and leverage from the list of baseline controls in columns (3) and (6). Our results remain stable across these columns and do not become economically weaker as we include more controls for credit risk.

We next address the concern 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. In Table C.2, we provide two sources of evidence against this possibility. First, we control for bank-level lending policies in several ways, including the total lending amount and the average spread issued by the bank during the same time period (quarter), and the average growth in local areas from other officers in the same bank and Census division (*Matched Officer HP Growth - Bank* x *Census Division*). This matched growth captures all local areas in the same Census division from other officers working in the same bank at any point in our sample. Controlling for this matched growth variable allows us to compare idiosyncratic experiences by a loan officer within the same bank and region. In columns (1), (3), (5), and (6) of Panel A, we find that our results from both the lender-loan sample and the loan-level sample remain significant and economically similar when we add these bank-level controls. Second, we include bank × year fixed effects in the lender-loan sample, which provides a larger set of officers within a bank-year and allows us to more precisely these results. Our effects also remain significant and become slightly stronger with this approach.

In Panel B, we address this issue by restricting our samples (both the lender-loan and loan-level samples) to states with smaller areas. This helps alleviate the concern that in large states such as Texas and California, within-state variation in officer local areas may predict segmented business areas from their own banks. In the restricted sample, our results remain statistically significant and economically similar. Taken together, these findings provide additional evidence against the view that our results are driven by changes in bank fundamentals.

Table C.1

Robustness of Results: Further Controlling for Borrower Fundamentals

This table reports results addressing the concern that officers' local housing price growth may capture differences in borrower credit risk. We estimate Equation (1) using both the lender-loan sample (Panels A and C) and the loan-level sample (Panels B and D). We extend the analyses in Tables 3 and 4 using subsample restrictions and different sets of controls. The sample period is from 2000 to 2018. The outcome variable is *log(Spread)*, the log of the all-in-drawn interest rate (in basis points) over the LIBOR. The independent variable of interest is *Past Local HPGrowth*, the housing price growth rate (log difference in prices) in officers' local areas during the year or quarter prior to loan origination (see Section 2.2). In Panels A and B, we estimate results from Tables 3 and 4 while excluding officer areas 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 headquarter from the borrower is in the bottom quartile of our sample. In columns (3) and (4), we remove cases where the borrower's industry (3-digit NAICS) representation in their officer's county is in the top quartile of the sample. *Ind. Representation* is defined as the ratio of an industry's employment share in the officer's county to its employment share at the national level. In Panels C and D, we examine the sensitivity of our results to varying controls for borrower credit risk. We restrict both the lender-loan and the loan-level samples to observations with data on the additional credit risk controls listed below. In columns (1) and (4), we estimate the baseline results in the restricted samples. In columns (2) and (5), we add *Additional Credit Risk Controls*, which include *DDRank*, *Benchmark Spread (DD)*, and *Benchmark Spread (Rating)*. In columns (3) and (6), we remove leverage and equity volatility from the set of basic controls. Controls and Industry FE include: *Adjacent Areas HP Growth, Borrower Characteristics*, *Local Area Characteristics, Loan Term Controls*, industry fixed effects, and loan type fixed effects, all defined in the same way as in Table 3. 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% level, respectively.

Panel A: Excluding Near and Highly Represented Borrowers, Lender-Loan Sample

	Outcome: *log(Spread)*			
	Excluding Regions with Low Distance		Excluding Regions with High Industry Representation	
Sample: Lender-Loan (2000-2018)	(1)	(2)	(3)	(4)
Past Local HPGrowth (Year)	-2.535**		-4.378***	
	(1.140)		(1.237)	
Past Local HPGrowth (Quarter)		-5.986**		-7.976***
		(2.434)		(2.539)
Controls and Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
Officer State × Year FE	Yes	Yes	Yes	Yes
Observations	1,893	1,893	1,716	1,716
R-squared	0.747	0.747	0.763	0.763

Panel B: Excluding Near and Highly Represented Borrowers, Loan-Level Sample

	Outcome: *log(Spread)*			
	Excluding Regions with Low Distance		Excluding Regions with High Industry Representation	
Sample: Loan-Level (2000-2018)	(1)	(2)	(3)	(4)
Past Local HPGrowth (Year)	-4.669***		-7.521***	
	(1.720)		(2.118)	
Past Local HPGrowth (Quarter)		-8.946**		-11.629***
		(3.593)		(3.826)
Controls and Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
Officer State × Year FE	Yes	Yes	Yes	Yes
Observations	1,263	1,263	1,165	1,165
R-squared	0.766	0.763	0.765	0.762

Panel C: Adding and Removing Credit Risk Controls, Lender-Loan Sample						
	Outcome: *log(Spread)*					
Sample: Lender-Loan (2000-2018)	(1)	(2)	(3)	(4)	(5)	(6)
Past Local HPGrowth (Year)	-3.260***	-3.420***	-2.954***			
	(0.827)	(0.756)	(0.821)			
Past Local HPGrowth (Quarter)				-6.392***	-5.425***	-5.565***
				(1.994)	(1.912)	(1.926)
Additional Credit Risk Controls		Yes			Yes	
Remove Leverage and Equity Vol			Yes			Yes
Controls and Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes	Yes	Yes
Officer State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,431	2,431	2,431	2,431	2,431	2,431
R-squared	0.739	0.796	0.731	0.738	0.794	0.730

Panel D: Adding and Removing Credit Risk Controls, Loan-Level Sample						
	Outcome: *log(Spread)*					
Sample: Loan-Level (2000-2018)	(1)	(2)	(3)	(4)	(5)	(6)
Past Local HPGrowth (Year)	-4.082***	-4.119***	-4.133***			
	(1.171)	(1.092)	(1.175)			
Past Local HPGrowth (Quarter)				-6.931**	-6.791***	-6.252**
				(2.852)	(2.418)	(2.841)
Additional Credit Risk Controls		Yes			Yes	
Remove Leverage and Equity Vol			Yes			Yes
Controls and Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes	Yes	Yes
Officer State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,621	1,621	1,621	1,621	1,621	1,621
R-squared	0.763	0.820	0.758	0.760	0.818	0.754

Table C.2

Robustness of Results: Further Controlling for Bank Fundamentals

This table reports results addressing the concern that officers' local housing price growth may capture differences in bank conditions or policies. We estimate Equation (1) using both the lender-loan sample and the loan-level sample, both ranging from 2000 to 2018. We extend the analyses in Tables 3 and 4 using subsample restrictions and different sets of controls. The outcome variable is *log(Spread)*, the log of the all-in-drawn interest rate (in basis points) over the LIBOR. The independent variable of interest is *Past Local HPGrowth*, the housing price growth rate (log difference in prices) in officers' local areas during the year or quarter prior to loan origination (see Section 2.2). In Panel A, we add controls capturing bank-level conditions, including bank lending controls, bank-region matched growth controls, and bank × year fixed effects. *Bank Lending Controls* include the total lending amount and the average spread by the bank across all loans in Dealscan during the quarter of loan origination. *Matched Officer Growth - Bank × Census Division* is the average past housing price growth across local areas from other officers in the same bank and census division (see Section 2.2). Columns (1) through (4) use the lender-loan sample while columns (5) and (6) use the loan-level sample. In Panel B, we exclude loans where officer states have a total area in the top tercile across all states. *Controls and Industry FE* include: *Adjacent Areas HP Growth, Borrower Characteristics, Local Area Characteristics, Loan Term Controls*, industry fixed effects, and loan type fixed effects, all defined in the same way as in Table 3. See Appendix B for variable definitions. 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% level, respectively.

Panel A: Adding Bank-Side Controls

	Outcome: *log(Spread)*					
Sample:	Lender-Loan				Loan-Level	
	(1)	(2)	(3)	(4)	(5)	(6)
Past Local HPGrowth (Year)	-3.209***	-3.347***			-4.097***	
	(0.811)	(1.144)			(1.115)	
Past Local HPGrowth (Quarter)			-5.823***	-7.887***		-6.546**
			(1.857)	(2.358)		(2.652)
Controls and Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes	Yes	Yes
Officer State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank Lending Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matched Officer Growth - Bank x Census Division	Yes	Yes	Yes	Yes	Yes	Yes
Bank × Year FE		Yes		Yes		
Observations	2,590	2,590	2,590	2,590	1,737	1,737
R-squared	0.726	0.739	0.724	0.738	0.744	0.739

Panel B: Removing Large-Area States

	Outcome: *log(Spread)*			
Sample:	Lender-Loan		Loan-Level	
	(1)	(2)	(3)	(4)
Past Local HPGrowth (Year)	-3.341***		-4.173***	
	(0.950)		(1.342)	
Past Local HPGrowth (Quarter)		-5.478***		-6.933**
		(1.927)		(2.814)
Controls and Industry FE	Yes	Yes	Yes	Yes
Loan Officer FE	Yes	Yes	Yes	Yes
Officer State × Year FE	Yes	Yes	Yes	Yes
Observations	2,059	2,059	1,450	1,450
R-squared	0.731	0.729	0.749	0.743