Anchoring on Credit Spreads

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ABSTRACT

This paper documents that the *path* of credit spreads since a firm's last loan influences the *level* at which it can currently borrow. If spreads have moved in the firm's favor (i.e., declined), it is charged a higher interest rate than justified by current fundamentals, and if spreads have moved to its detriment, it is charged a lower rate. We evaluate several possible explanations for this finding, and conclude that anchoring (Tversky and Kahneman [1974]) to past deal terms is most plausible.

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Virtually all borrowers pay a spread above the risk free rate, a premium that compensates lenders for expected losses in default, illiquidity, and other considerations. Calculating the appropriate spread requires answering a number of questions. How likely is the firm to default, and over what horizon? If it does default, how big are the losses? Is default more likely to occur in bad economic times, or is it largely dependent on firm-specific factors? How easy will it be to sell the firm's bonds or bank notes, and will this be more difficult during certain economic states?

The common element in these questions is their perspective: they are all forecasts. Retrospective information is only relevant to the extent that it improves the lender's estimate of forward looking variables. Data that are purely historical – in the sense that they do not provide information about the firm's creditworthiness – should not affect spreads. This paper provides evidence that non-informative historical signals do, in fact, influence borrowing costs, suggesting that observed credit spreads likely depart from a fully rational benchmark spread.

What do we mean by non-informative, historical signals? Suppose that two neighbors living across the street from one another both want to refinance their home loans, and that prevailing rates on 30-year mortgages are currently 6% on average. Neighbor 1 originated his mortgage five years ago, when average rates were 8%, and neighbor 2 originated her mortgage ten years ago, when average rates were 4%. It would be surprising if this differential timing in their prior loan originations translated to a difference in borrowing costs on new loans today: we would not expect for neighbor 1 to pay a higher rate today than neighbor 2, given that the only (observable) distinction is when each happened to have last borrowed.

And yet, this is precisely what we find in the syndicated loan market. On average, when a firm borrows money from a bank, the level of aggregate credit spreads when the firm last borrowed correlates with the spread it receives on a new loan. The pattern is consistently one of stickiness, whereby firms that last borrowed when spreads were high pay a premium, and firms that last borrowed when spreads were low receive a discount.

To give a specific example, suppose that a BBB-rated firm took out a 5-year term loan in the year 2000, and then another in 2003. Suppose also that the average BBB credit spread rose by 50 basis points over these three years. We find that, relative to other BBB borrowers in 2003, the firm will receive a discount of approximately 6 basis points, negating 12% of the change in aggregate spreads.

In order to highlight how bizarre this finding is, in the preceding example we excluded all firm-specific information. However, prior aggregate spreads must ultimately translate to current borrowing costs through some firm-specific variable, and the most likely candidate is the firm's *actual spread* at the time it last borrowed. When we extend the analysis to also consider the specific spread at which the firm last borrowed, the results strengthen. Returning to the example, instead of taking the difference between average BBB term loan indices in 2000 and 2003, we take the difference between average BBB term loan spreads in 2003, and the actual spread the firm paid in 2000. Here, a firm's current borrowing spread is dragged about 20% of the way toward the spread at which it last borrowed.

Our preferred explanation for this finding is anchoring, whereby borrowers and/or lenders are influenced by past deal terms and, perhaps unintentionally, allow seemingly stale signals to enter negotiations. The spread that borrowers pay on bank debt is typically similar to prevailing spreads paid by similar borrowers in the recent past, but there is always uncertainty as to the precise risks that the lender faces for any given borrower. Further, information asymmetry can yield surplus from a particular lender-borrower match. Both facts suggest a range of feasible interest rates for any given borrower, implying that its ultimate borrowing cost may be subject to a variety of psychological influences, of which anchoring is one.

Four types of evidence support an anchoring interpretation, two positive and two negative. First, there appears to be an overt fixation on the "headline terms" of the deal, namely the past spread, in current spread negotiations. Consequently, when we compare past and current spreads, we observe a disproportionate number of loans made at exactly the same rate, even when market conditions have drastically changed. This is visually apparent in Panel B of Figure 1, which plots the percentage change in spread for firms borrowing both immediately prior to the crisis of 2008 (i.e., in 2006 or 2007), and then again during the crisis (2008). Despite aggregate spreads rising roughly 35% (Panel A), the modal outcome is a spread change of exactly zero, fairly direct evidence that borrowing histories were explicitly considered.

Second, the impact of prior spreads on the current spread is largest in situations for which the prior spread would be most salient. The effect of the prior spread is largest when the prior loan is more recent, when the lead arranger has not changed, and when firms have no debt rating. The importance of salience on the magnitude of anchoring is well established, and these results are in line with the theory.

Third, borrowing histories don't appear to tell us much about a borrower's current creditworthiness, suggesting that they shouldn't be part of an optimal forecast. We show that there is no difference in profitability, leverage, valuation ratios, or past stock price performance for firms that borrow when spreads are high versus low. We also show that future stock returns are no different, so there are neither observable nor unobservable differences between firms with differently timed borrowing.

Fourth, the pattern persists in sub-samples that are not subject to a variety of alternative explanations. For example, borrowing histories still affect spreads when a firm switches lenders between consecutive loans. This fact rejects two alternative explanations for our findings, namely (i) that banks and firms agree to smooth variation in spreads over time, and (ii) that many loans in the sample are not new loans, but merely renegotiations of old loans for which changing the spread is not “on the table” (see Roberts 2010). Additionally, if we consider only repeat loans whose origination date falls after the expiration date of the previous loan (which serves to establish the historical anchor), the results remain. Here too, any concerns over our results reflecting simple tweaks or reclassifications of existing loans are eliminated.

We have used the term “anchoring” to denote the connection between past spreads and current spreads, but there are a variety of behavioral biases that could generate this connection. For example, if a given firm-lead arranger pairing generates surplus relative to alternative pairings, a negotiation must take place to determine the loan spread, and hence the split of the surplus. If the side that negotiates more effectively is the side with more to lose, then our results suggest that the side that aggregate spreads have moved against is the side with more to lose.

This is consistent with loss aversion, in which the dis-utility from losses, relative to some reference point, is greater than the utility from gains. If the spread in the previous deal is the reference point, then loss aversion suggests that a firm’s past spread will influence its current spread as we observe. In fact, the joint-utility-maximizing choice of spread will often be precisely at the prior spread, where both negotiators have kinks in their utility functions. This is what we observe in a variety of samples, including the aforementioned Figure 1 (see Shefrin 2008 for theoretical examples illustrating this point).

It is not possible to reject loss-aversion as the explanation with the available data, nor is it possible to reject several other theories based upon behavioral biases. The primary difference between various behavioral explanations relates to whether the explanation is preference- or bias-based. If loss averse negotiators are driving our results, then the spreads we observe result from preferences, not mental errors. Regardless of whether the effects we see are preference-based or bias-based, the effect on the division of surplus and the size of the effect on spreads is identical. Of the behavioral biases that are consistent with our data, we choose to focus upon anchoring because it seems natural in this setting, and because it is the baseline bias in similar studies outside finance. Throughout the manuscript, the term “anchoring” should be understood to represent a variety of potential behavioral explanations.

We next turn to the question of whether anchoring implies mis-pricing. One might imagine that a rate discount (premium) could be accompanied by stricter (looser) covenants, or other non-priced deal terms, so there is no net mis-pricing. The data suggest that this is not the case. We find that, whether the borrower or lender is to benefit from anchoring, the most common change in the number of covenants or fees, from the prior to current loan, is zero. Inaction on other deal terms is far more likely than compensation. If the borrower is to benefit from anchoring, it is equally likely that covenants or fees decrease rather than increase, providing further evidence against compensating behavior. The only evidence we find in favor of compensation is that fees are more likely to fall than rise (though still most likely to stay the same) when the bank benefits from anchoring. Together, it does not appear that negotiators compensate for anchoring with other deal terms – anchoring probably causes mis-pricing.

We finally turn to the question of how anchoring can manifest: the market for syndicated loans is both large and competitive, factors that should mitigate the expression of suboptimal behavior. Here, the relatively modest size of the result is comforting, especially compared to enormous magnitudes documented in Tversky and Kahneman's (1974) original laboratory experiments. The average loan in our sample is roughly $250 million, and may be mis-priced by perhaps 15 basis points, distorting interest costs approximately $375,000 per year.

Even so, to the extent that this does represent an inefficiency, it demands some explanation. We begin by asking who is most subject to anchoring, and how competitive forces may mitigate its expression. If the borrower is subject to anchoring, then we would expect anchoring to manifest when it benefits the banks – when aggregate spreads have been falling. If lenders are subject to anchoring, then it should manifest when aggregate spreads have been rising. We find that it is present both when spreads have risen and fallen, so both bankers and borrowers appear to be subject to the bias. We also present limited evidence that the effect is larger when spreads have been falling, suggesting that borrowers are more subject to anchoring bias than lenders.

Competition may be a mitigating factor. If the spread to which the borrower agrees is too high, then competing lenders can offer a lower spread. This limits the size of anchoring when the lender benefits, and also suggests that the amount of anchoring may be larger when there is less competition among lenders. If the agreed-upon spread is too low, however, there is no direct competitive force that mitigates the influence of anchoring. The lender's profits will be lower due to the anchoring, but there is no incentive for other lenders not subject to the bias to intervene in the transaction.

We present two tests of whether the size of anchoring is mitigated by competition. First, we stratify the sample according to a firm's number of historical lead arrangers. We find that competition does appear to reduce the expression of anchoring: when anchoring benefits the lender, the effect of anchoring is reduced when there are more historical lead arrangers. Second, we hypothesize that there is a greater opportunity for a lender to discover private information about a borrower when credit rating agencies have not already revealed such information. This generates a surplus to a lender-borrower match, and hence a greater scope for anchoring. We find that the effect of anchoring is stronger among unrated versus rated firms. These results suggest that anchoring is partly mitigated by competition.

Our results generally indicate that even in highly competitive settings, behavioral biases may be difficult to completely eradicate. The specific bias of interest here, anchoring, has been shown to have large effects in a number of experiments and field studies, but its relevance in actual markets remains largely unexplored. Beggs and Graddy (2009) find that sellers of collectible art ask more (less) for paintings originally acquired in hot (cold) markets, virtually identical to what we find for lenders last lending in tight credit markets. In finance, reference-dependence has been studied in a variety of settings. Baker, Pan, and Wurgler (2012) find that a target stock's 52-week high price is the modal takeover offer, suggesting that shareholders may anchor to that high as an upper-bound for the stock's value. Loughran and Ritter (2002) find that IPO are most underpriced when the offering is higher relative to the pre-offer file price range. They argue in favor of prospect theory, in which the midpoint of the file price range is viewed by issuers as the *status quo*. Lambson *et al* (2004) find that out-of-state buyers of real estate in Phoenix tend to pay more, and provide some evidence that this is because their price perceptions are anchored to price levels in their home regions. Baker and Xuan (2009) find that follow-on equity offerings are discontinuously more likely when the stock price exceeds the price when the CEO first took the helm of the firm, suggesting a fixation on that price.

The paper is organized as follows. In Section I, we begin by showing that histories matter, using only fluctuations in aggregate (i.e., market level) spread indices. We find that having last borrowed when aggregate spreads were low (high) confers a discount (premium) in current transactions. Section (II) refines our concept of a borrowing history by incorporating firm-specific past borrowing spreads into the analysis. This not only strengthens the results, but also facilitates non-parametric comparisons (i.e., simple histograms) that indicate the special significance of past borrowing spreads for current negotiations. In this section we revisit alternative explanations that apply to firm-specific borrowing histories but not to aggregate histories. Section III provides further discussion, establishing the impact of salience on the level of anchoring, characterizing mis-pricing implied by anchoring, and speculating on why market forces have apparently not eliminated the perceived inefficiency. We conclude in Section IV.

I. Aggregate spread histories and borrowing costs

Consider a sample of firms borrowing in any given year t. Some of these firms will have last borrowed in a comparatively tight market characterized by high aggregate credit spreads, and some will have last borrowed when spreads were relatively low. In this section, we are interested in whether, and why, a firm's current cost of borrowing may be related to aggregate market conditions prevailing the last time it last accessed debt markets.

After a brief description of the data, we begin by characterizing these empirical patterns, first aggregating across all credit ratings groups in subsection I.B, and then conducting the analysis within credit categories in subsection I.C. In both cases, a firm's borrowing history is related to the rate it pays for bank debt today. Then, in subsection I.D, we explore the extent to which a firm's historical borrowing activity tells us something about its current creditworthiness. We find no empirical evidence to support this view, suggesting that, whatever the reason histories appear to enter negotiations, their use to improve risk forecasts is unlikely to be the motivation.

A. Data

Our sample starts with the universe of term loans and long-term lines of credit (Revolvers $\geq$ 1 year) listed in the Reuters Dealscan database from 1987-2008.[1] Because we are interested in the impact of firms' borrowing histories on their future transactions, we limit the sample to firms borrowing at least twice during the sample period. Further, we require at least one year to have passed between consecutive loans. This condition helps minimize the possibility that what might appear as a new loan in the Dealscan database is simply a reclassification or renegotiation of an existing loan (see Roberts, 2010). We explore this possibility in more detail in Section II.C.2. This procedure results in 15,536 loan-level observations, about one quarter of which correspond to term loans, and the balance to long-term revolving lines of credit.

Two main samples of repeat loans are used throughout the paper: (1) the sample of all repeat term loans and long-term lines of credit found in Dealscan (15,536 observations), and the sample of these loans matched to Compustat data (8,525 observations). The smaller Compustat-matched sample is necessary to perform our two-stage regression analysis discussed more in section II.B. This sample is smaller because we require each loan to have a number of corresponding Compustat variables be non-missing in order to be included in the sample[2]. Table I provides some summary statistics regarding the firms and loans in both of these samples. Focusing on the Compustat-matched sample, we find that sales ($2.5 billion) and assets ($3.2 billion) are representative of the typical large public firm. The typical long-term revolver has a maturity of 45 months, is about $253 million, and is accompanied by a spread of roughly 177 basis points above LIBOR. The comparable amounts for term loans are 59 months, $205 million, and 263 basis points.

B. *Borrowing histories matter: evidence across rating groups*

Our first exercise links a firm's current spread to the change in aggregate credit spreads, averaged across all ratings groups, since its most recent loan. Accordingly, we calculate yearly averages for each loan type, resulting in a 22-observation time series index of average credit spreads for term loans, and another 22-year index for long-term revolvers. [3] As shown in Figure 2, these series are highly correlated, being low in the mid 1990s, rising during the tech bubble, falling after its burst, and rising again during the Financial Crisis of 2008. The fluctuations in these aggregate indices represent the source of variation for the comparisons we wish to make.

During each year of our sample, we assign every loan into one of three groups: 1) those for which aggregate spreads have declined by more than 25% since the firm's last loan, 2) those for which aggregate spreads increased by more than 25% since the firm's last loan, and 3) those for which aggregate spreads have been relatively stable (+/- 25%) since the firm's last loan.

To illustrate our methodology, and referring back to Figure 2, suppose that the year is 2005, when the average spread on term loans is about 282 bp. Take three sample term loan borrowers in 2005, firms A, B, and C. The important distinction between these firms is the timing of their most recent term loan, and whether aggregate spreads have risen or fallen since. In this specific case, firm A's most recent term loan was in 1991, when spreads were very close to those in 2005; firm B's most recent term loan was in 1996, when spreads were considerably lower than in 2005; and firm C's most recent term loan was in 2002, when spreads were much higher than in 2005. Our interest is whether the borrowing rates for these three firms differ, *ceterus paribus*, in 2005.

Figure 3 presents the results of these comparisons. Panel A corresponds to the case described above, when yearly averages are taken across all credit rating categories. Starting with long-term revolvers on the left side, the dark grey bar reaches a height of 4%, indicating that for long-term revolving lines of credit, firms in group 1 borrow at a roughly 0.04*177 = 7 bp premium compared to the typical borrower. Likewise, the lighter bar suggests that the typical group 2 firm receives a discount in the range of 0.1*177 = 17.7 bp on repeat long-term lines of credit. The results for term loans, shown on the right side of the figure, indicate a similar magnitude for firms in group 1 (about 4%), but a much smaller magnitude for firms that last borrowed when spreads were lower.

Panel A of Table II considers the same comparisons in a regression framework. The unit of observation is an individual loan, and the dependent variable, $log(spread)_{i,j,t}$, is firm i's time t natural logarithm of the all-in drawn spread for loan type j, which is either a term loan or long-term revolving line of credit. As we did in Panel A of Figure 3, we relate $log(spread)$ to variables that capture the extent to which credit market conditions have tightened (spreads have risen) or loosened (spreads have fallen) since firm i's last transaction. Denoting as t^* the most recent year the firm borrowed, we will consider as covariates dummy variables $Spreads\ Fell_{i,j,t^* \to t}$ and $Spreads\ Rose_{i,j,t^* \to t}$, which take values of 0 or 1 depending on the aggregate evolution of spreads for loans of type j between t^* and t. If spreads have risen by 25% or more, $Spreads\ Rose$ will take a value of 1, and if spreads have fallen by 25% or more, $Spreads\ Fell$ equals 1. Alternatively, some specifications will include the numerical value for the evolution of spreads, $\Delta\ Agg.\ log(spread)_{i,j,t^* \to t}$. However changes in market conditions are parameterized, firm i's contribution to the average spread in both years t^* and t is always removed.

The estimated coefficient on $Spreads\ Rose$ in the first column indicates that, on average, a firm that last borrowed when spreads were at least 25% lower than their current levels enjoys a 13 percent discount relative to the typical firm ($s.e. = 0.02$). Likewise, the second column (and row) considers separately firms for which aggregate spreads have risen, corresponding to the group 2 firms in the classification scheme described above. The magnitude here is virtually the same (0.15, $s.e. = 0.02$), although in the opposite direction. Column 3 compares the spreads of groups 1 and 2 firms in the same regression, which leads to virtually identical results.

Taking the difference between these estimated effects, the results suggest a spread difference of $13 + 15 = 28$ percent between firms differing in the timing of their past borrowing activities. Recalling the average spreads for each loan type from the summary statistics in Table I, the effect on borrowing costs is in the range of 50 bp for revolvers (177*0.28), and 78 basis points (277*0.28) for term loans. However, when interpreting these results, note that we are already conditioning on relatively large changes ($< -25\%$ or $> +25\%$) in aggregate spreads when making these comparisons.

Column 4 allows for the evolution of aggregate spreads to enter in a continuous rather than discrete fashion. The estimated coefficient on $\Delta\ Agg.\ log(spread)$ is -0.49 ($s.e. = 0.05$), indicating that roughly half of any change in aggregate spreads since a firm's last loan is reflected in its current cost of borrowing. How big is the effect on average? The sample standard deviation in $\Delta\ Agg.\ log(spread)$ is 0.23, so that the average impact of past fluctuations in aggregate spreads is approximately 17 bp.[4] For comparison, the estimates in Table I indicate that this difference is slightly larger than the transition from A to BBB loans, a modest but nevertheless economically meaningful magnitude.

C. Borrowing histories matter: evidence within rating groups

The evidence in subsection I.B uses all loans in every year to construct the time series of aggregate spreads. Here, we make the same comparisons as before, but now calculate yearly averages within a credit rating-loan type pair. For example, rather than a single time series index of average spreads for term loans, we have seven – one for AA/AAA, another for A, BBB, BB, B, CCC (or worse), and unrated. In every year a firm takes out a loan, we assign it to the relevant index based upon the loan type, and the firm's credit rating at that time. Then, as before, we ask whether historical fluctuations in the levels of these indices are associated with the levels of future borrowing costs.

Returning to Figure 3, the relevant results are shown in Panel B. Similar to Panel A, which aggregated across all credit categories, histories remain relevant within each rating-specific index. Six of seven group 1 comparisons, shown in the dark grey bars, indicate a spread premium for firms that last borrowed when aggregate spreads were much higher than current aggregate spreads. In percentage terms, this premium is fairly stable across groups, in the range of 5-6% above each year's sample average. Converted to actual spreads, the magnitudes are a bit larger for firms with lower credit quality. For example, firms rated BB or lower pay a premium of about 5 bps, versus a premium of approximately 3 bps for firms above the investment grade threshold.

The pattern is even more pronounced for firms in group 2, for which spreads have risen since the firm's last borrowing activity. In percentage terms, the magnitudes are larger for firms with higher credit quality, with an average discount exceeding 10% relative to the credit rating-benchmarked yearly average spread. On the other hand, for firms below investment grade (BB, B, or CCC/worse), the overall point estimate is still negative, but very small.

In Panel B of Table II, we present these results in a regression framework. The first, second, and third columns allow the evolution of spreads to enter as discrete variables, with the same +/- 25% cutoff that we used previously. The magnitudes we observe are very similar, with approximately a 22% percent difference in borrowing costs between group 1 (0.11, *s.e.* = 0.01) and group 2 (-0.11, *s.e.* = 0.01) firms. Recalling that our dependent variable is in logarithms, 22 percent of the average AA/AAA spread for term loans is only 3-4 basis points, but for borrowers with BB credit ratings is roughly 40 basis points. When changes in rating-specific aggregate spreads enter continuously in column 4, we estimate a magnitude of -0.21 (*s.e.* = 0.02), indicating that about one-fifth of any change in aggregate spreads is reflected in current borrowing costs.

Two distinctions between Panels A and B of Table II are worth noting. First, the standard errors of the estimated coefficients are generally much lower in Panel B. Almost entirely, this is because controlling for firm risk through credit ratings drastically improves the fit of the model (more than tripling the R^2) and increases the precision of the estimated effects.

Second, the estimated coefficients on Δ *Agg. log*(*spread*) are twice as large in Panel A (-.49 versus -0.21). Although this looks economically important, it is not. The reason is that aggregate spreads are less volatile when we aggregate across rating categories (Panel A), versus when we calculate them within rating categories (Panel B). Because these series are highly correlated, variations in how we calculate Δ *Agg. log*(*spread*) amount to little more than scaling.[5] To see this, note that in Panel A, a one standard deviation change in Δ *Agg. log*(*spread*) alters realized log spreads about $-0.49 \times 0.23 \approx -0.11$, while in Panel B, the effect is $-0.21 \times 0.41 \approx -0.09$.

In the final two columns, we restrict our attention to firms that do not switch rating classes between the time of their last loan, t^*, and the current time, t. Although this restriction would be irrelevant in Panel A, where credit ratings were ignored entirely, this is not true in Panel B, where firms are assigned to groups 1, 2, and 3 based on a rating-specific index. In the first four columns of Panel B, variables related to changes in aggregate spreads (*Spreads Rose*, *Spreads Fell*, and Δ *Agg. log*(*spread*)) were calculated based on a firm's credit rating at the time the loan was taken out. To illustrate, if a term loan borrower had a credit rating of BBB during 2006, and a credit rating of AA when it next borrowed in 2008, Δ *Agg. log*(*spread*) would correspond to the difference between BBB-rated term loan borrowers in 2006 and AA-rated term loan borrowers in 2008. Clearly, this method conflates time-series variation in a rating-specific index (e.g., Δ *Agg. log*(*spread*) only for BBB-rated term loan borrowers) with time series variation in firm-specific creditworthiness.

The last two panels remove this ambiguity by sample selection, where we have simply removed any firm whose credit rating changes between loans. The results are very similar. Although the estimated coefficient on $Spreads\ fell$ drops to 0.04 ($s.e. = 0.02$), the coefficients on $Spreads\ rose$ and Δ *Agg. log*(*spread*) remain very similar to those estimated in in columns (3) and (4), and remain highly significant. To summarize, the results in Table II suggest that differences between firms' borrowing histories correlate with current borrowing costs.

D. Do borrowing histories indicate current creditworthiness?

We have established that firms that last borrowed when spreads were high (low) pay more (less) on new loans today. This may be because firms borrowing in different aggregate spread regimes have different average risk levels, or because of behavioral bias influencing the pricing of loans. We investigate the former possibility in this section.

Returning to Figure 3, note that the dark bars indicate a premium for firms last borrowing when aggregate spreads were high, while the lighter bars indicate a discount for firms last borrowing when aggregate spreads were low. For this to be consistent with a risk-based story, group 1 firms (those shown with darker bars) should be riskier than group 2 firms (lighter bars). Note that this relationship must hold both across (Panel A) and within (Panel B) credit groups.

To see how this might occur, consider again Figure 2, which illustrates the borrowing histories of three sample firms A, B, and C. Standing in the year 2005, the concern is that even when conditioned on credit ratings, firm C might be riskier than firm A, which might in turn be riskier than firm B. If this is the case, then the patterns in Figure 3 and Table II might simply reflect compensation for risk not captured adequately by credit ratings.

In Table III, we investigate this possibility directly. Here, rather than the dependent variable being the firm's credit spread, we consider a number of empirical proxies for the firm's creditworthiness, which may provide additional information about default risk, even after controlling for credit ratings. The specific equations we estimate are:

$$creditworthiness_{i,t} = \beta \Delta\ Agg.\ log(spread)_{i,j,t^* \to t} + \alpha \cdot year \cdot loan\ type \cdot credit\ rating_{i,j,t} + \epsilon_{i,j,t},$$

where $creditworthiness_{i,t}$ is measured various ways. The firm's sales growth, earnings growth, and investment rate each gives some indication of its business fundamentals using accounting information. Stock based variables include market-to-book ratios, trailing stock returns (i.e., leading up to the borrowing event), future stock returns (i.e., after the borrowing event), and trailing return volatility. We also include three measures of borrower quality using the firm's existing indebtedness: current ratio, debt-to-assets, and future change in credit ratings. Variable definitions are given in the header to Table III.

Importantly, the inclusion of fixed effects ensures that all comparisons are made within $year * loan\ type * credit\ rating$ groups (e.g., 2004 term loans initiated by firms with BB ratings). Moreover, note that the sample is restricted to firms whose credit ratings have not changed since last borrowing, as in the last two columns of Panel B of Table II.

The covariate of interest is $\Delta\ Agg.\ log(spread)$, calculated as in Table II. Recalling that the relation between $\Delta\ Agg.\ log(spread)$ and firm-specific spreads is negative (Table II), under a risk-based explanation, a *positive* relation between *creditworthiness* and $\Delta\ Agg.\ log(spread)$ should obtain. In other words, at time t, firms for which aggregate spreads have risen since their last borrowing event ($\Delta\ Agg.\ log(spread) > 0$), should be better credit risks that firms for which spreads have declined.

The evidence in Table III fails to provide support for the risk-based story. Of the nine measures of creditworthiness, five of them are estimated to have a negative relation to aggregate spreads, with two being statistically significant. Regarding these, column 7 indicates that firms having last borrowed when aggregate spreads were lower than current levels, or $\Delta\ Agg.\ log(spread) > 0$, have lower ratios of current assets to current liabilities; likewise, column 8 indicates that such firms have higher leverage ratios. On the other hand, four of the nine measures of creditworthiness are positively associated with changes in aggregate spreads, though as before, the majority are insignificant. Only trailing stock volatility is consistent with a risk-based story (column 6), though the magnitude is very small.

It is worth emphasizing that Table III includes two measures of creditworthiness measured *after* the loan date. Unlike most of the measures of creditworthiness analyzed in Table III, examining performance subsequent to the loan date helps us understand the role played by factors (at time t) observable to banks, but not the econometrician. Think about a drug company that has private knowledge about upcoming FDA approval, but which is not yet incorporated into stock prices. In such cases, bankers may make credit decisions based on information not yet public, but is expected to become so with the passage of time. Examining the firm's subsequent performance is one way to assess the importance of such factors.

Yet, as with most of the other measures of creditworthiness, a firm's future performance does not appear to be reliably related to changes in aggregate spreads. Stock returns over the next year are estimated to be negatively associated with past changes in aggregate spreads, though the relation is not statistically significant. Likewise, changes in future credit ratings, estimated with an ordered logit (0 for a downgrade, +1 for no change, and +2 for an upgrade), also gives a non-result, though the point estimate is, again, inconsistent with the risk-based story.

In summary, the results in this section indicate path dependence in borrowing costs. If a firm last borrowed when aggregate credit market conditions were tight, it pays a premium relative to otherwise similar firms. The opposite is true for firms having last borrowed when aggregate spreads were relatively low. Moreover, we find virtually no evidence that the timing of a firm's borrowing history tells us anything about its current or future fundamentals.

II. Firm-specific histories and borrowing costs

The results in the previous section establish that when a firm takes out a loan, the interest rate it pays is a function of how aggregate credit spreads have evolved since it last borrowed. We have excluded any firm-level covariates from the right hand side, forcing macro fluctuations alone to generate the relevant variation. This severely limits the number of stories that can explain the patterns we document – indeed, only two explanations for our results are possible. First, firms that last borrowed when aggregate spreads were high are fundamentally riskier today than those that last borrowed when spreads were low. We provided evidence that this is not the case. Second, borrowers and lenders incorrectly allow some variable that correlates with aggregate spreads to affect spreads on new loans. We investigate this possibility below, focusing upon the most obvious candidate: the firm's prior loan spread. A firm's loan spread clearly correlates with aggregate spreads, and seems a likely starting point for future spread negotiations.

We provide evidence in this section that the firm's prior spread is, indeed, the source of anchoring identified above. In subsection II.A, we compare a firm's current spread to the value of its most recent historical spread. The pattern that emerges is unmistakable, with the prior spread being the modal current spread, despite market conditions often having fluctuated significantly over time. We supplement these univariate comparisons with regressions similar to those in the previous section, but instead of measuring historical fluctuations relative to aggregate indices, we allow the firm's historical borrowing cost to enter directly into the analysis. We confirm the patterns observed in the previous section and, because we focus upon the actual variable of interest rather than a noisy proxy, we are able to characterize a number of additional cross-sectional patterns.

A. *Fixation on past deal terms: distributional evidence*

In this section we look for evidence of anchoring by examining distributional evidence in the spirit of Genesove and Mayer (2001) and Baker, Pan and Wurgler (2012). If past spreads are correlated with current spreads because of unobserved risk factors, then the precise past spread should not be especially important – the approximate last spread would be approximately as predictive. However, if the past spread acts as an anchor for current spread, then the precise value of the past spread may be very important.

To illustrate, suppose a firm last borrowed at 90 basis points. Under the rational view, it makes little difference whether the firm last borrowed at 85, 90 or 95 basis points: what matters is that the firm borrowed in a low-spread regime, and this indicates that the firm is of high-quality. Under the behavioral view, 90 – not 85 or 95 – is a salient number in the minds of firms and banks in negotiation. Thus, future deal terms will be tethered to this special number, and distributional evidence will reveal "obsessions" with the precise value in prior deal terms. For example, Baker, Pan and Wurgler (2012) examine M&A offer prices and find sharp discontinuities around precisely the 52-week high of the target firm. If offer prices rationally reflect stand-alone value plus synergies, it is unlikely that the sum of these two parts equal precisely the 52-week high as often as they find.

For each repeat loan, we calculate the change in $log(spread)$, which is approximately the percentage change in loan spread from the prior loan to the current loan. Figure 4 plots the distribution of these spread changes for three different groups of repeat loans: those for which aggregate spreads have risen by more than 25% (Panel A), those for which the change in aggregate spreads in less than 25% (Panel B) and those for which aggregate spreads have fallen by more than 25% (Panel C).[6] Aggregate spread changes are calculated within credit rating category.

The notable feature of each histogram plot is the sharp spike at 0%. Without exception, the mode of each plot is at 0%, even in the cases in which aggregate spreads have changed by more than 25% in absolute value. For example, in Panel A, where aggregate spreads have fallen by at least 25%, 11% of the repeat loans are in the 0% change histogram bin.[7] The next tallest histogram bar occurs at -20% where 7% of the observations fall. Similarly, in Panel C, where aggregate spreads have risen by at least 25%, still 9% of the repeat loans are in the 0% change histogram bin.

To formally test for a discontinuity at zero, we follow the methodology of Bollen and Pool (2009). This requires two steps. First, we fit nonparametric kernel densities using a Epanechnikov kernel to estimate a smooth distribution for our sample. The density estimate at a point t is defined as

$$\hat{f}(t;h) = \frac{1}{Nh} \sum_{j=1}^{N} \phi \left(\frac{\Delta \log(spread_j) - t}{h} \right)$$

where N is the number of repeat loans, ϕ is the Epanechnikov kernel function, and h is the kernel bandwidth chosen following Silverman (1986). Under the null hypothesis of no discontinuity, this distribution serves as a reference to determine the expected number of observations per histogram bin.

Second, we test to see if the actual number of observations in a given bin is significantly different from what would be expected under the smooth distribution estimated in the first step. In particular, the DeMoivre-Laplace theorem states that the actual number of observations in a given bin will be asymptotically normally distributed with mean Np and standard deviation $\sqrt{Np(1-p)}$, where N is the total number of observations, and p is the probability that an observation resides in the given bin, i.e., the integral of the kernel density between the boundaries of the bin. Our test results reject the "no discontinuity at zero" hypothesis at less than the 1% level for the sample where aggregate spreads rose (t-stat = 2.97) and the sample for where aggregate spreads were similar (t-stat = 3.99). The test also rejects the null at the 5% level for the sample where aggregate spreads have fallen (t-stat = 2.27). Various alternatives for estimating the kernel density and bandwidth do not alter this conclusion.

These sharp discontinuities at zero are hard to understand under the rational view. If changes in spreads reflect changes in the riskiness of firms, or in the market price of that risk, then the distributions in Figure 4 look peculiar. For example, consider again the case of Panel A where macro conditions are such that spreads fell by 25%. If there were a macro shock that decreased spreads for all firms by 25% then a firm's risk profile would have to have deteriorated to increase its idiosyncratic spread by *exactly* 25% in order to have an identical spread for both loans. Such a coincidence would allow the firm to borrow at exactly the same spread as before, but Figure 4 suggests far too many such coincidences. In Panel A, when compared to the histogram buckets immediately above (+10%) and immediately below (-10%), the empirical frequency of no change (0%) is nearly three times greater. In Panel B (C), no change is twice (1.33 times) as likely as a change of 10% more or less.

B. Regression evidence

The bunching of mass around zero shown in the histograms is one way to appreciate the relevance of past spreads in current spread negotiations. However, while visually compelling, they highlight only the extreme situations – "full" anchoring – and therefore may not provide a complete characterization of the relevance of histories in current spread negotiations. For this purpose, a regression framework that measures the average effect may be more suitable.

In this section, we adopt the econometric model developed by Beggs and Graddy (2009) in their study of collectible art (re)sales. Their focus, like ours, is on histories, e.g., whether two similar Picassos auctioned in 2005 sell for different prices, given that one painting was originally acquired in a hot market (e.g., 2001), while the other one last sold in a cold market (e.g., 1997).

Beggs and Graddy's procedure entails two steps. First, for each auctioned painting, they run a hedonic regression based on its observable characteristics like its size, year, and artist to obtain a predicted sales price. Then, in the second step, they compare whether the actual sales price is higher or lower than this benchmark spread prediction and, specifically, whether any deviation is related to the evolution of art prices since the painting was last sold.

Our setting is appropriate for applying their methodology, perhaps more so given the dozens of observable firm and loan characteristics available to form the benchmark spread predictions. During each year, we run a standard cross-sectional model predicting borrowing spreads,

$$\widehat{s}_{i,t} = X_{i,t}\beta_t, \tag{1}$$

where $\widehat{s}_{i,t}$ is the log spread of firm i in year t, and X includes things like credit ratings, size, profitability, loan purpose, etc. The set of control variables is taken directly from Ivashina (2009). As Table IV indicates, the typical R^2 from such cross-sectional regressions is high – in the neighborhood of 70% – which generates a series of precise benchmark spread predictions, $\widehat{s}_{i,t}$, for input into the second step.

We then estimate:

$$s_{i,t} = \beta\widehat{s}_{i,t} + \delta(s_{i,r} - \widehat{s}_{i,t}) + \gamma(s_{i,r} - \widehat{s}_{i,r}) + \epsilon_{i,t}, \tag{2}$$

where s refers to a log realized spread, and $\widehat{s}$ to a log predicted spread from the hedonic regression in Equation (1). The current time is denoted t, and the date of the firm's most recent borrowing activity r.

The first term, β, is the coefficient on the *predicted spread*, and captures the effect of time t observables. The coefficient on β should be close to one if our first stage model provides an accurate estimate of the appropriate spread: if the first stage were perfect, then we would observe a coefficient on β of precisely one. As the amount of measurement error from the first stage increases, β will become noisy and biased toward zero. Our coefficient for long-term lines of credit is 0.98, very close to unity, suggesting a high quality first-stage model. Our estimate for term loans, 0.79 is somewhat attenuated, 0.79, suggesting a more noisy first-stage.[8] Standard errors are bootstrapped using 1,000 iterations.

The second term, δ, is the coefficient on the *spread evolution*, and is our main focus. Importantly, it is the only term in Eq. (2) that combines information from two dates, both today (t) and the time the firm last borrowed (r). The hypothesis that $\delta = 0$ represents the baseline view that borrowing histories are not relevant: only current observables and past deviations from expectations are useful in predicting spread today. We strongly reject this hypothesis for both revolvers and term loans. Table V indicates an estimate for δ of 0.22 (bootstrapped $s.e. = 0.01$) on revolvers and 0.16 (bootstrapped $s.e. = 0.04$) on term loans, indicating that approximately one-sixth of would-be evolutions are not incorporated into realized spreads.[9][10] Roughly speaking, if the combination of a firm's fundamentals and credit market conditions predict that its spread should have increased by 60%, we will observe an increase of only 40%.[11]

The third term, γ, is the coefficient on the *previous residual*, and is our proxy for time t unobservables. If the firm was able to borrow for, say, 50 bps below what we would expect when it last borrowed, then it may well borrow for less than expected today. We addressed this concern in our aggregate spread regressions by noting a lack of differences in observables and future returns between firms borrowing in low spread versus high spread regimes, but now that we allow firm-specific information into our regressions, we can do better. To account for persistent firm attributes not observable to the econometrician, we include as a regressor at time t the deviation from the hedonic model at time r, $s_{i,r} - \widehat{s}_{i,r}$.

Intuitively, this is similar to including a firm fixed effect, but instead of washing the unobserved heterogeneity out through a static, firm-specific intercept, a firm's unobserved quality is allowed to vary through time. We expect for γ to be positive but, because some fraction of time r unobservables are expected to become observable by time t, less than one. Indeed, as Table V shows, γ is estimated to have a magnitude of roughly 0.06 (bootstrapped $s.e. = 0.02$) for revolvers and 0.1 (bootstrapped $s.e. = 0.04$) for term loans. This indicates that if past deviations from predicted values capture unobserved quality, little of this is persistent.

C. Potential alternatives to anchoring

As with the evidence using only aggregate spreads (Table II), the patterns in Figure 4 and Table V are consistent with anchoring to historical spread values. However, they are also consistent with a number of non-behavioral alternatives capable of producing path dependency in credit spreads. In this section, we explore a number of these possibilities, some of which we have already considered and rejected in Section I.D, and some of which are new.

C.1. Relationship lending

It is possible that a bank and a firm could have an implicit agreement to share the risk from fluctuations in aggregate spreads over time: the lender offers lower spreads when market spreads are high and, in return, the firm pays higher spreads when market spreads are low. The literature has historically focused on the benefit of inter-temporal interest rate smoothing (e.g., Fried and Howitt 1980, Berlin and Mester 1998, Boot 2000), but one could make a similar argument about spreads. Given that our study implies that past spreads seem to influence current spreads, one might imagine that this implicit contracting is a potential explanation.[12]

There are two reasons to believe that such a relationship banking story is not driving our results. First, our empirical specification does not yield results consistent with relationship banking. We show that spreads are drawn toward the spread the last time the firm borrowed. The relationship banking story, however, argues that spreads are drawn toward their historical average. The specifications to test these two stories are fundamentally different. We show that the marginal impact of higher spreads last time is positive: higher spreads last time imply higher spreads today. If relationship banking were driving the results, higher spreads last time would imply either (i) higher spreads this time, if spreads were either abnormally high or abnormally low in both instances, or (ii) lower spreads this time, if spreads were abnormally high last time and abnormally low this time, or vice versa. The sign on the marginal effect of higher spreads last time is positive in the former case, and negative in the latter. Our specification will yield the weighted average of these, which is probably close to zero.

Second, additional results are inconsistent with that story. In Table VI, regressions are run separately among observations for which there is the same lead arranger in the prior and current deal (column 1), as well as those for which the lead arranger has changed (column 2). The coefficient on spread evolution is lower when the lead arranger has changed, but remains statistically significant at the 1% level. When the lead arranger in a syndicate changes, there is no reason for the new lead arranger to be bound by an implicit or explicit agreement between the previous lead arranger and the CEO or CFO of the firm.

Figure 5 presents the same data in histogram form. While the frequency of "full" anchoring is clearly higher when the lead arranger does not change from the prior deal to the current deal, there remains a discontinuity at "no change" when the lead arranger has changed.

C.2. Renegotiations or reclassifications of existing loans

Figure 4 shows that banks and firms often maintain spreads from previous loans even when there has been a significant change in market credit conditions. A natural explanation for this would be that renegotiation is costly and, when it appears that renegotiation will lead to a similar spread as agreed for the previous loan, both parties find it better to simply maintain the previous spread. It is not clear why the previous loan spread should be the default, but assuming that it is, renegotiation costs could potentially explain our results.

In an examination of dynamic contracting, Roberts (2010) estimates that nearly 50 percent of all observations in Dealscan are renegotiations of existing loans. For example, if a firm or bank renegotiate the terms of a particular covenant in the original loan and this is filed as an amendment to the original agreement, Dealscan may create a new observation for this renegotiated loan. In this example, all of the other deal terms (which did not change) including the spread, will appear sticky.[13] The regression results in Table VI would then be the average of two effects. First, most loan terms would be renegotiated and would have no anchoring. Second, some loan terms would not be renegotiated, and would have complete anchoring.

We address this concern in two ways. First, in column (3) of Table VI, we remove all observations for which there is no change in spread between loans. That is, we include only observations in which the spread has been renegotiated. The anchoring effect is still present, with a coefficient of 0.17 (bootstrapped *s.e.* = 0.01) on the spread evolution term. While excluding all cases of full anchoring reduces the magnitude of the anchoring coefficient, it is still economically and statistically significant.

Second, column (4) in Table VI performs the standard regression given in Equation (2) for observations that are certainly not renegotiations: term loans whose origination date comes *after* the maturity of the prior term loan (See Panel A of Figure 6 for a timeline of the origination and maturity of loans in this sub-sample). The coefficient on spread evolution is 0.2 (bootstrapped *s.e.* = 0.03), and is statistically significant in spite of the reduction in observations. While costly renegotiation may be at play, it is not the primary driver of our results.

C.3. Rounding

It may be the case that many banks and firms settle on spreads in natural increments, like 1/4 or 1/8 of a percentage point, or 10 or 20 basis points. If so, then small changes in the "unrounded" spread for the firm could lead to no change in the actual loan terms.[14] This is, mathematically at least, anchoring, but it is anchoring to the nearest round number and is not particularly interesting. We therefore must make sure that our specification would not pick up this sort of anchoring by mistake.

The *direction* of rounding (up or down) can be the same or different between periods r and t. If the direction is the same at each time, then the *amount* of rounding—the correct spread minus the rounded/actual spread—can be larger or smaller in period r versus t. If the direction is different, then the change can have been from "down" to "up" or vice versa. There are therefore four cases in which rounding can affect the estimate of the anchoring coefficient.[15] Two will cause the anchoring term to be overestimated (too large) and two will cause the anchoring term to be underestimated (too small or negative). The likelihood of each pair, under most reasonable assumptions, is the same, and the magnitude of the bias resulting from each is the same as well. Our estimate of a positive coefficient on spread evolution is therefore not consistent with a rounding explanation.

Figure 7 illustrates these four cases in panels A-D. In each case, we assume that the correct spread is rounded to the nearest eighth of a percentage point. The dotted lines represent thresholds: if the correct spread crosses a threshold, the direction of rounding switches.

To be sure that rounding does not affect our results, we again refer to column (3) of Table VI. By removing outcomes in which the spread is unchanged from period r to period t, we remove cases where the rounding would bias results by increasing the anchoring coefficient, leaving only those where the bias is against finding a positive anchoring effect. Results are largely unchanged, suggesting that rounding is not an issue.

C.4. Risk

Throughout our analysis, we have implicitly assumed that after controlling for $\widehat{s}_{i,t}$ and $(s_{i,r} - \widehat{s}_{i,r})$, changes in a firm's borrowing cost, relative to its historical costs, do not provide additional information about its current risk. In other words, the fact that credit market conditions, for example, happen to have been tighter the last time a firm borrowed should have no bearing on its current borrowing cost, after controlling for current market conditions. But here, the "happen to have been" phrase is important: if historical credit market conditions cannot be taken as exogenous—i.e., they tell us something about unobservable risk characteristics going forward—then we can no longer strictly interpret the coefficient on $(s_{i,r} - \widehat{s}_{i,r})$ as anchoring.

We addressed this concern in Section I.D in our aggregate spread analysis, and showed that there do not appear to be differences in firms borrowing in high and low aggregate spread regimes. Now that we allow firm-specific information into our regressions, we have an additional tool with which to address this concern.

Column (5) in Table VI shows the results of our main regression, aggregated across all deal types, but only for the sample where the *immediate predecessor loan was the result of refinancing.* To illustrate, suppose that we wish to explain the spread on a 5-year term loan that IBM initiates in the year 2006. Because it also borrowed from the same lending syndicate in 2002, anchoring would suggest that the rate IBM was awarded by this syndicate would influence the rate it is charged in 2006. The specific concern is that if spreads were unusually high (or low) in 2002, then the fact that IBM borrowed during that time might contain relevant information about its risk profile in 2006.

To address such a concern, column (5) of Table VI includes only observations where the predecessor loan (e.g., IBM's loan in 2002) was itself a rollover of a previous loan. Continuing with the example, IBM may have borrowed in 2002 because a 5-year term loan initiated in 1997 was maturing in 2002, requiring it to refinance. We assume that when IBM originally borrowed in 1997, it could not foresee credit market conditions in 2002, and thus, the borrowing environment in 2002 is exogenous. Now, in 2006, we can reasonably assume that the firm's borrowing activity in 2002, and therefore credit market conditions in 2002, are unrelated to its risk. (For further understanding, see Panel B of Figure 6 for a timeline of the origination and maturity of loans in this sub-sample).[16]

The anchoring coefficient in this sub-sample is 0.23, compared to 0.22 for the full sample. As with our aggregate analysis, a correlation between risk and the timing of borrowing does not appear to be a driver of our results.

III. Discussion

A. Salience

We have presented evidence against several explanations for the association between the spread at the time a firm last borrowed and its spread today. In this section, we present positive evidence for anchoring. The simplest way to establish the existence of anchoring, or a similar psychological bias, is to show that (i) the prior spread should be irrelevant, but it appears to matter, and (ii) it matters more when it is more salient in the minds/memories of negotiators. We posit that salience will be greater when the loan is more recent, when the firm is unrated, and when the individuals involved in the negotiation are the same.

A.1. Recent vs. Distant Reference Points

Because anchoring is a manifestation of a psychological bias, it is reasonable to suppose that the passage of time might "clear the deck" by rendering past deal terms less salient. If so, then we would expect that more recent deals would have a more pronounced influence on current transactions. Figure 8 shows visual evidence of this: the percentage of repeat loans with the same spread decreases as the length of time between transactions grows. More formally, Table VII tests for this directly: columns (1) to (6) break up the sample of repeat deals into those where the most recent deal was less than one year prior (column 1), between one and two years prior (column 2), and so forth. Column (6) includes deals whose most recent predecessor is more than five years in the past.[17]

What is immediately clear is that the effects of anchoring degrade with time. For deals completed within the most recent year, past spreads have an anchoring effect of 0.33 (bootstrapped *s.e.* = 0.02). Advancing forward a year (column 2), we see the effect cut to 0.27 (bootstrapped *s.e.* = 0.02). The effect decreases monotonically as the years between loans increases.[18]

It is also worth noting that the explanatory power of the residual from past deals (*Quality*) does not drop over time, remaining in the neighborhood of 5%-10%. The coefficients become statistically insignificant and volatile as the sample size drops, but they don't clearly drop off. One cannot statistically reject that the size of the coefficients is constant over time, suggesting that while most of any deviation in actual spreads from predicted spreads is one-off, the 5%-10% that remains may be permanent. This also suggests that the spread evolution term is not picking up unobserved quality.

A.2. Same vs. Different Lead Arranger

Most of the literature on reference points focuses on the behavior of a *single* agent, be it a home owner (Genesove and Mayer 2001), art seller (Beggs and Graddy 2009), or stock trader (Barberis, Huang, and Santos 2001). In addition to extending these results to a different market—arguably one where the impact of behavioral biases should be mitigated—our setting allows us assess the relevance of reference points when *both* sides of the initial negotiation transact again. That is, we can compare anchoring when: (i) the borrower transacts with the same lead bank, and (ii) the borrower transacts with a different lead bank.[19]

Figure 5 and Table VII, which we have already discussed above, show the results for this segregation. Figure 5 shows that 20% of repeat loans have the same spread when the same lead arranger is used, but only 11% of loans have the same spread when the lead arranger is changed. Columns (1) and (2) of Table VI reveal that the anchoring term has a magnitude of 0.31 when the lead arranger is the same as used in the previous loan, but only 0.19 when the lead arranger is different. Although the effect of anchoring still appears relevant when the lead bank has changed, it is smaller. Because it's possible that changes in the lead arranger may be proxying for longer time passing between repeat loans, only repeat deals completed within four years of the reference transaction are included in the sample used to estimate these results. This breakpoint is arbitrary, but similar results are found if we move the date forward or backward one or two years.

Taken together, these results suggest that the anchor is more powerful when it is more salient.[20]

B. Does anchoring imply mis-pricing?

In this section, we evaluate whether anchoring of spreads implies that loans are mis-priced. That is, do borrowers and lenders compensate for "incorrect" spreads by changing other deal terms like up-front fees, annual fees, or covenants? If there is, indeed, mis-pricing, we ask how this is possible. While psychological biases appear to be present in many settings, the presence of anchoring in a high stakes, competitive market, populated by highly trained individuals, requires some explanation.

B.1. Do borrowers and lenders compensate with other deal terms?

It is possible that, for some reason, borrowers and lenders prefer that a borrower have less change in spreads from one deal to the next than would be driven by the overall market, but that they compensate by adjusting other deal terms. In this case, there would be no mis-pricing.

We investigate this possibility in Table VIII. Panel A presents a 2×3 sort, separating into rows the cases where spreads have risen since the most recent loan, in which case the borrower is benefitting from anchoring, from the cases where spreads have fallen since the most recent loan, and the bank is benefitting. Columns are formed by separating cases where the number of covenants associated with the loan have decreased, stayed the same, and increased, since the most recent loan.

If negotiators are compensating for anchoring on spreads, then we would expect the number of covenants to increase when the borrower is benefitting from anchoring. Instead, we find that (i) covenants are as likely to decrease as increase, and (ii) it is even more likely that the number of covenants is unchanged. When the bank benefits, we expect the reverse – the number of covenants should decrease – but, instead, we find the same pattern as when the borrower benefits.

Panels B and C present similar sorts on the up-front fee and annual fee. In this case, it is even more common for no change in fees from one loan to the next, occurring in nearly 60% of observations. When the borrower benefits from anchoring, it is as likely to see an increase in up-front and annual fees as a decrease. When the bank benefits, we do observe some compensating behavior: fees are significantly more likely to decrease when the bank benefits from anchoring.

Taken together, there are effectively twelve tests in these three panels of the claim that negotiators compensate for anchoring with other deal terms. The modal change in non-spread deal terms is zero in all six cases (whether the bank or borrower benefits, for three non-spread deal terms). *Inaction is far more likely than compensation.* In the three cases in which the borrower benefits from anchoring, compensating behavior is no more common than anti-compensating behavior. In the three cases in which the bank benefits from anchoring, two suggest compensating behavior and one does not. Together, two out of twelve cases suggest compensating behavior and ten out of twelve do not. Overall, the evidence is against compensation and in favor of mis-pricing.

B.2. How can anchoring manifest?

The question remains as to how anchoring is possible in a market as large, competitive and sophisticated as syndicated lending. We separate this question into three sub-questions. First, which market participants appear to be subject to anchoring? Is it only the borrower, only the lender, or both? Second, if the lead arranger or borrower are affected, then can competitive forces limit the effect? Third, if all market participants are subject to anchoring bias, then how can this be so? It appears highly unlikely unless the typical mis-pricing due to anchoring is small relative to the noise in risk assessments that generate spreads.

B.2.1 Which market participants are subject to anchoring?

If the borrower's predicted spread is higher than the spread on its most recent loan, then anchoring would yield a spread below market rates, harming the lender. This is therefore possible only if the lender is subject to anchoring bias. In Table IX, column (1) we include only observations where the spread evolution term is negative, so the borrower would benefit from anchoring. The coefficient on the spread evolution term is 0.04, much smaller than our baseline estimates, but still statistically significant at the 10% level. On the other hand, if the borrower's predicted spread is *lower* than the spread on its most recent loan, then anchoring would yield a spread above market rates, harming the borrower. This is therefore possible only if the borrower is subject to anchoring bias. In column (2) of Table IX, we include only observations where the spread evolution term is positive, so the lender would benefit from anchoring. The coefficient on the spread evolution term is 0.23 (bootstrapped $s.e. = 0.03$). Together, these coefficients suggest that professional lenders are less subject to anchoring bias, as perhaps might be expected, but neither side of a deal is completely immune.

B.2.2 The effect of competition

Given that both lender and borrower are affected by anchoring bias – although to differing degrees – we can see whether competition seems to solve the problem. If the borrower benefits from anchoring, then its spread is too low. Competition will not solve this problem: no other lender or borrower can take advantage of the mis-pricing. If the bank benefits, however, the spread is too high and competition can limit the mis-pricing. A competitor that undercuts the spread but still overcharges the borrower will earn a profit. We therefore look for proxies for competitiveness for a loan in the sample where the bank benefits from anchoring. Finding such proxies is difficult – we only see the ultimate syndicate and borrower, not the negotiations with potential competitors prior to the loan's origination. Thus, we proxy for competition for the loan by the firm's number of historical lenders.

Columns (3) to (5) present regressions in sub-samples defined by the borrower's number of historical lenders, ranging from one in column (3) to greater than or equal to five in column (5). The coefficient on the spread evolution term decreases from 0.25 in the first case to 0.14 in the last: competition reduces, but does not eliminate, the influence of anchoring. We note that it is possible that a better proxy for competition would yield a full elimination of anchoring, but we do not have evidence for this.

B.2.3 The size of the effect on spreads

We now ask how competition could fail to fully eliminate anchoring. That is, *is the typical mis-pricing due to anchoring small relative to the noise in risk assessments that generate spreads?* We have two pieces of evidence that it is. First, we have found that a typical loan is mis-priced by about 15 basis points. This is large enough to have some impact on the borrower's and lender's profitability, but is small relative to the variation in spreads we see within credit rating categories in practice. For example, spreads on BBB loans made in 2003 averaged 117 bps., with a standard deviation of 55 bps. The high value that year was 450 basis points. Moreover, 2003 was not special: the average standard deviation in spreads for BBB loans over the sample period is 54 bps. While some of this variation is due to credit ratings being a poor proxy for risk, some is likely to be noise. We suspect that 15 basis points due to anchoring is likely to be very difficult to disentangle from 15 basis points due to risk, though that is simply our conjecture.

Second, we hypothesize that there is a greater opportunity for a lender to discover private information about a borrower when credit rating agencies have not already revealed such information. This generates a surplus to a lender-borrower match, and hence a greater scope for anchoring. We find that the effect of anchoring is stronger among unrated versus rated firms. Table VI, column (6) shows that the coefficient on spread evolution among unrated firms is 0.26, whereas it is 0.19 among rated firms. The difference is significant at the 1% level, providing some evidence that competition partly mitigates the expression of anchoring, although other explanations are also consistent with the finding.

Together, these results suggest that (i) anchoring is present on both sides of the lending transaction, (ii) is stronger for the CEO/CFO than for the bankers, and (iii) is partly, though not fully, mitigated by competition when the bank benefits.

IV. Conclusion

We provide evidence that the spread that a firm received on its most recent loan affects the spread it receives on a new loan, acting as an anchor. We reject risk-based explanations in favor of the behavioral bias of anchoring. The difference in spreads is not compensated for in other deal terms like fees or covenants, which implies mis-pricing. Although we have not explored the strategic implications of credit spread anchoring, it is apparent that any ability to time fluctuations in debt markets (e.g., Baker and Wurgler 2002, for equity markets) will be enhanced. Without anchoring, a firm with market timing ability benefits exactly once (when it borrows), but with anchoring, the benefits of raising "cheap" money are realized in subsequent transactions. This appears to be exactly the sentiment of Morgan Stanley's head of fixed income who, commenting on Googles recent bond offering in 2011 said[21]:

> Often times the only way to create meaningful growth is by making a large strategic acquisition. If you want to fund that with debt, then now is an opportune time to establish a yield curve at historically low rates.

Whether firms think strategically in this way we cannot say generally, but the interaction between market timing and the path dependence of borrowing costs is, we think, an interesting area for future study.

Appendix A. Appendix

Our tests are properly specified to identify δ and γ, but if we wish to measure how far s_t is from the "correct" value of s_t, we must note that the prior value, s_r, may have itself been "incorrect." We can derive how the loan spread evolves iteratively under the null.

Because we must refer to a reference loan's reference loan, iteratively, it will be convenient to define the loan prior to a loan at time t as loan $t-1$. The loan prior to that is loan $t-2$, etc. We now calculate the value of $s_{i,t}$ under the hypothesis that it satisfies

$$s_{i,t} = \beta \widehat{s}_{i,t} + \gamma(s_{i,t-1} - \widehat{s}_{i,t-1}) + \delta(s_{i,t-1} - \widehat{s}_{i,t}) + \varepsilon_{i,t}.$$

We define

$$\widehat{s}_{i,t} = \widehat{s}_{i,t-1} + \nu_{i,t}.$$

Plug the latter into the former to get

$$s_{i,t} = \beta \widehat{s}_{i,t} + (\gamma + \delta)(s_{i,t-1} - \widehat{s}_{i,t-1}) + (\varepsilon_{i,t} - \gamma \nu_{i,t})$$

Note that the effect of changes in $s_{i,t-1}$ and $\widehat{s}_{i,t-1}$ on current spreads is identical. For simplicity, let $\beta = 1$, which it should be (we may observe $\widehat{\beta} \neq 1$ in practice, but this should be statistical noise). We can get rid of $s_{i,t-1}$ altogether by iteratively substituting it out:

$$\begin{aligned}
s_t &= \widehat{s}_t + (\gamma + \delta)(\widehat{s}_{i,t-1} + (\gamma + \delta)(s_{i,t-2} - \widehat{s}_{i,t-2}) + (\varepsilon_{i,t-1} - \gamma \nu_{i,t-1}) - \widehat{s}_{i,t-1}) + (\varepsilon_{i,t} - \gamma \nu_{i,t}) \\
&= \widehat{s}_t + (\gamma + \delta)^2 (s_{i,t-2} - \widehat{s}_{i,t-2}) + (\gamma + \delta)(\varepsilon_{i,t-1} - \gamma \nu_{i,t-1}) + (\gamma + \delta)^0 (\varepsilon_{i,t} - \gamma \nu_{i,t}) \\
&= \widehat{s}_t + \sum_{i=0}^{\infty} (\gamma + \delta)^i (\varepsilon_{t-i} - \gamma \nu_{t-i})
\end{aligned}$$

The ε terms are period-by-period idiosyncratic shocks to the firm's spread that reflect random or unobservable reasons for the spread to differ from the predicted spread. The ν terms are changes each period in the firm's predicted spread. Either way, the deviation in the firm's current spread from its predicted spread is due in part to its current unobserved quality (ε_t), changes in its quality over time (all past values of ν_t), and its past unobserved quality terms (past ε_t terms). The latter two are due to path dependence.

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Notes

[1]The majority (roughly 70%) of the Dealscan database are term loans and long-term lines of credit (more than 1 year maturity). Our results are uniformly stronger if we also include short-term revolving lines of credit; however, to guard against the possibility of loan spreads being mechanically smoothed, we exclude all short-term loans. Thus, we view this sample selection criterion to be conservative about the true magnitude of the effect.

[2]See Table IV for the exact list of non-missing Compustat variables required for sample inclusion.

[3]Our data span 1987 to 2008, a total of 22 years.

[4]The estimated regression in column 4 of Panel A, Table II is $Log(spread) = 5.04 - .49 \times \Delta Agg.log(spread)$, which can be written $spread = \exp(5.04 - .49 \times \Delta Agg.log(spread))$. For this specific sample Δ *Agg.* log(*spread*) has a mean of -0.02 and a standard deviation of 0.23. Hence, a one-standard deviation increase in Δ *Agg.* log(*spread*) (i.e., aggregate spreads falling 23%) will lead to an increase in loan spreads on average of roughly $\exp(5.04 + 0.49 \times 0.02) - \exp(5.04 - 0.49 \times 0.21) = 16.6$ bps.

[5]A good analogy would be coefficients resulting from regressions of a given firm's stock returns on the S&P 500 Index, and also on an index of tech stocks. Because the S&P 500 and tech indices are highly correlated, the ratio of estimated betas will reflect little more than the respective variances of these indices.

[6]Roberts (2010) estimates that nearly 50 percent of all observations in Dealscan are renegotiations of existing loans. For example, if a firm and bank renegotiate only the terms of a particular covenant in the original loan and this is filed as an amendment to the original agreement, Dealscan may create a new observation for this renegotiated loan. In this example, all of the other deal terms, including the spread, will appear sticky. To avoid the possibility that features of the Dealscan database mechanically generate our results, we restrict the sample used to construct our histogram evidence to only "new deals", i.e., loans where the origination date of the second loan is after the maturity date of the first loan.

This ensures that any "stickiness" in loan spreads we observe is not due to Roberts' findings. Including these additional observations, however, further strengthens our results.

[7]Of loans in the 0% change bin 78% experience exactly no change in spread.

[8]It is unclear why this value of β is so far from unity. In every subsequent regression of this form that we present, β is between 0.9 and 1.02.

[9]It is possible that spreads for the set of repeat borrowers relate to our first-stage variables differently from spreads for other borrowers. We therefore also re-run the regression in equation (1) using the sample of repeat borrowers. Our coefficient on spread evolution is $\delta = 0.24$.

[10]In untabulated results, we repeat the regressions of column (1) for pre- and post-2000 samples. We find coefficients on spread evolution of 0.23 and 0.21, respectively.

[11]This interpretation is only approximately correct. Because the spread on the reference loan, s_r, is itself subject to anchoring, the total error in pricing may be greater than δ. We derive the precise expression in the Appendix, and note that our approximation is fairly good for small δ.

[12]It is unclear why other banks in a syndicate would agree to abnormally low spreads for a client firm. Even if a lead bank has an implicit rate spread smoothing agreement with a firm, other banks in the syndicate would have to believe that they will also benefit—i.e., that they will be (i) asked to join future syndicates, and (ii) allowed to share in the rewards in future deals—in order to agree to below-market spreads today. This seems unlikely.

[13]For such "intradeal" transactions, it is unclear why renegotiation would not extend to spreads. For example, consider again the Financial Crisis anecdote in Figure 1 and suppose that the unusual number of unchanged credit spreads were associated with renegotiations. When firms and banks sit down to renegotiate other deal terms, why not renegotiate spreads which have increased by 30 percent during the crisis at the same time? In other words, stickiness among intradeal transactions is simply another form of anchoring to historical terms, though perhaps less surprising than the anchoring that we uncover.

[14]In this section we use the term "correct" to mean un-rounded. We do not mean to suggest that it is incorrect to round. We simply need a term for the pre-rounding number.

[15]There are actually six cases, but two are redundant. If rounding is up in both t and r, the results are the same as if rounding is down in both periods, so we exclude discussion of those cases.

[16]Even though firms are effectively forced to refinance when their term loans mature, they could have refinanced prior to maturity. Reaching maturity is, therefore, not exogenous. Firms that do not refinance early may be fundamentally different from those that do. This analysis, however, uses as its sample only firms that do not refinance, so this source of endogeneity does not affect results within the sample. It may suggest a lack of external validity of this specific analysis, but does not bias any of our coefficients.

[17]All results throughout the paper restrict repeat loans to occur more than 1 year apart with the exception of column 1 of Table VII.

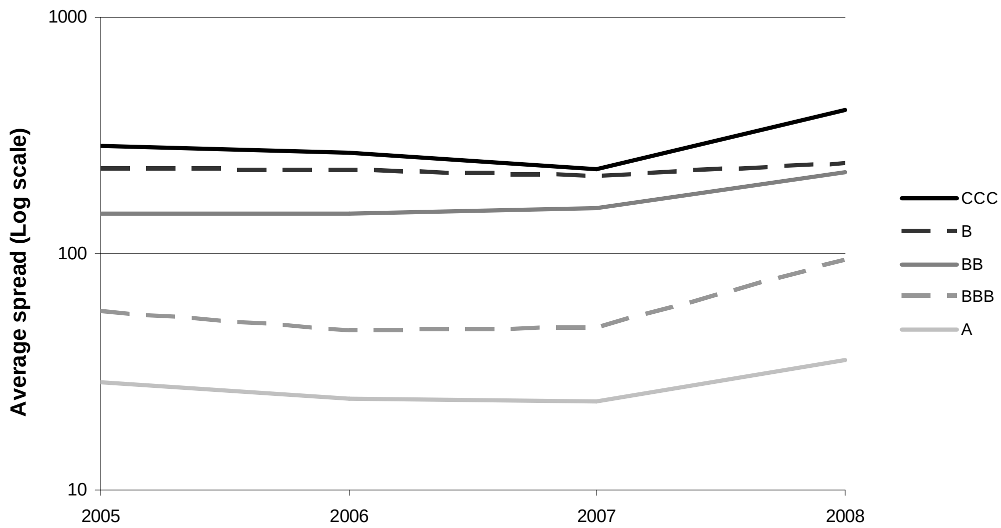
[18]This result parallels the findings of Beggs and Graddy (2009), who document that past sales prices in art auctions predict current asking and sales prices, particularly when they are recent.

[19]In the case of multiple lead banks, we consider the case where one bank was not present in the first transaction but appears in the second transaction as a different lead bank.

[20]Some of our salience measures are correlated. In a (untabulated) joint specification with all three salience measures included, we find that time between loans and same/different lead bank have the strongest relationship with anchoring in the cross-section.

[21]See Robinson (2011).

Panel A: Average spreads over time by debt rating



Panel B: Change in spreads before/during the crisis

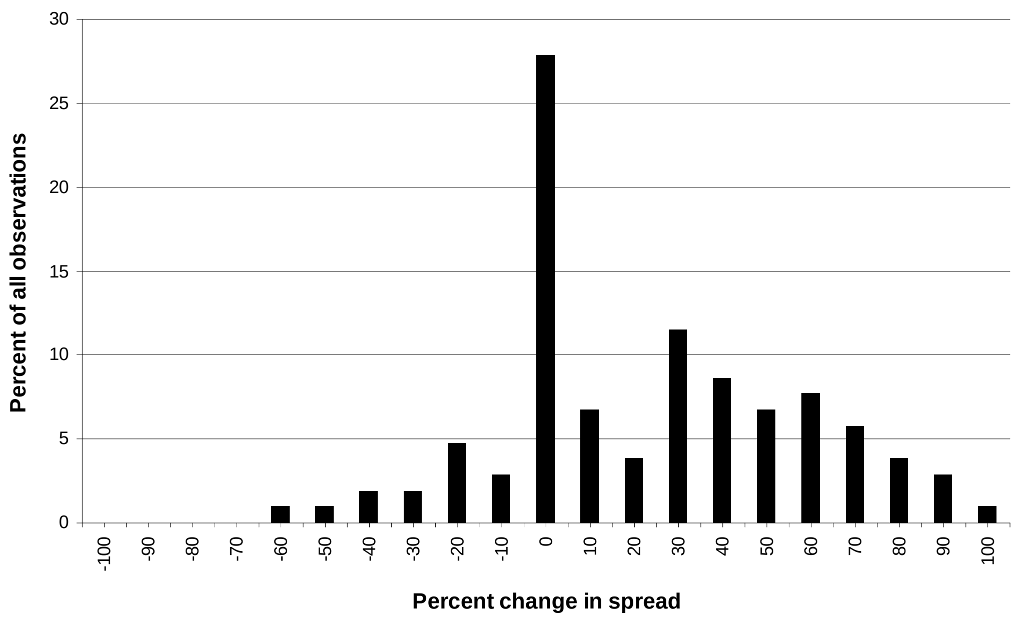


Figure 1. Spreads before and during the financial crisis of 2008. Panel A plots the average spread above LIBOR for long-term lines of credit during the years 2005-2008. Panel B plots the histogram of spread changes for every firm in our sample that took out a line of credit from a banking syndicate exactly once during the interval 2005-2007, and then once again in 2008. We include only firms that maintained the same credit rating between borrowing events. The histogram shows the percentage change in spreads between the first, pre-crisis loan and the second, post-crisis loan.

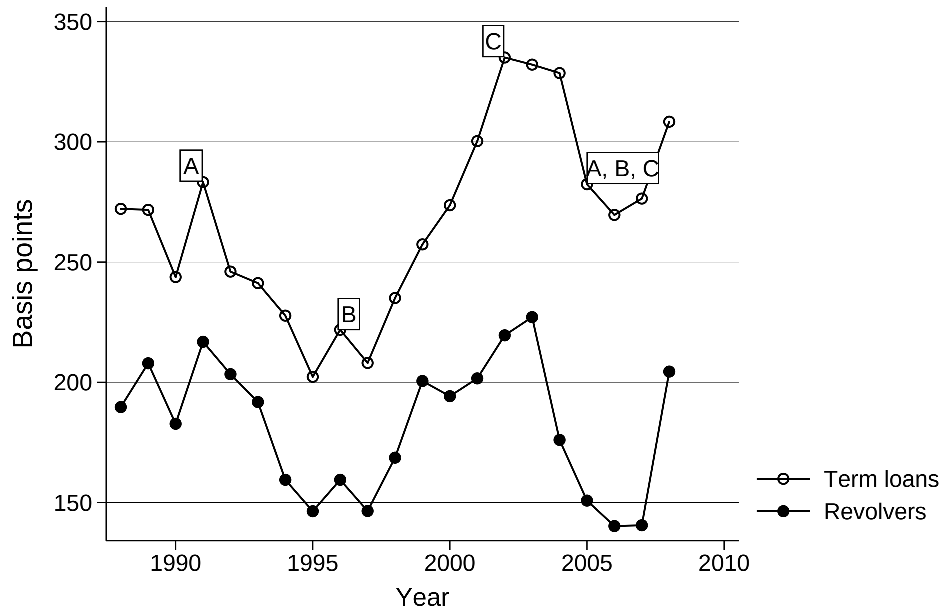
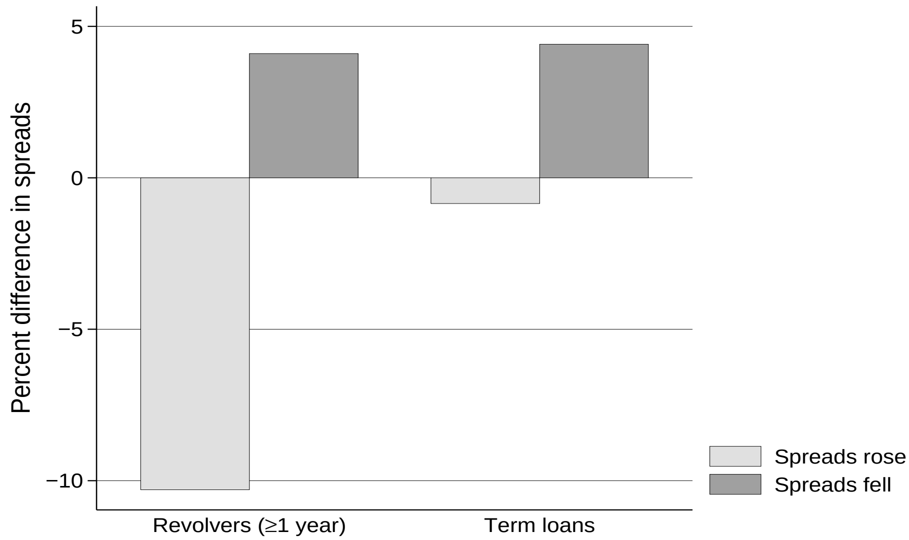


Figure 2. Aggregate spreads and the timing of loans. This figure reports the average spread by year for all repeat loans in our sample by loan type. Both average spread time series are calculated using only repeat deals transacted more than one-year apart. A, B, and C are there for illustrative purposes and represent three BB-rated firms who obtained term loans in the indicated years. The figure indicates that firms A borrowed in 1991 when aggregate spreads were 283 bps; firms B borrowed in 1996 when aggregate spreads were 222 bps; and firms C borrowed in 2002 when aggregate spreads were 335 basis points. Finally, all three firms borrowed once more in 2005 when aggregate spreads were at 282 basis points.

Panel A: Abnormal spreads as defined by year and loan type



Panel B: Abnormal spreads as defined by year, loan type, and debt rating

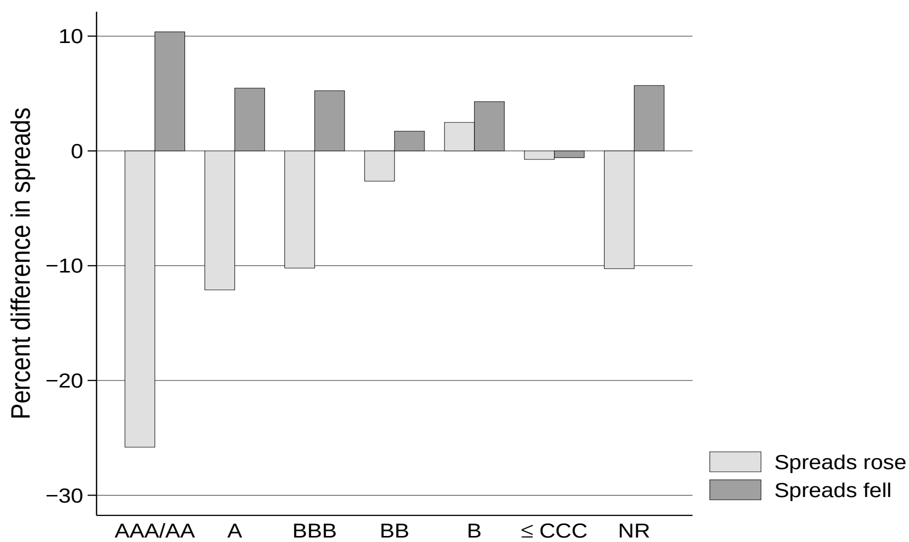
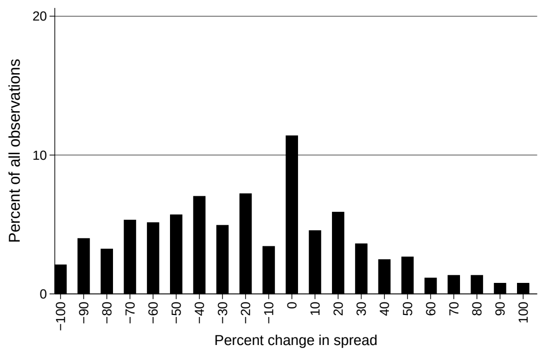


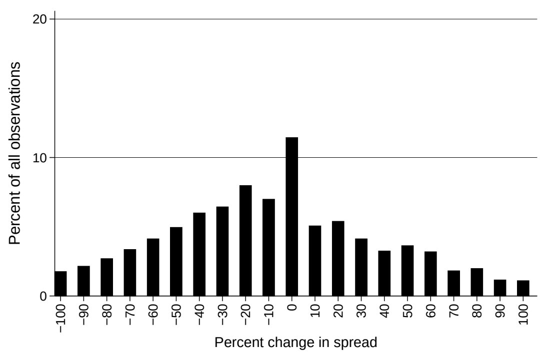
Figure 3. Abnormal spreads and the timing of loans. The abnormal spread of a loan is defined as the difference between the loan's log(spread) and the average log(spread) from a reference group specific to that loan. In panel A the reference group used is all firms with the same loan type, originated in the same year. For example, the abnormal spread for a term loan taken out by a BB-rated firm in 1997 would be the log(spread) of that loan minus the average log(spread) for all term loans originated in 1997. Panel A reports the average abnormal spread for firms that previously borrowed when aggregate spreads were 25% higher (Spreads fell), and for firms that previously borrowed when aggregate spreads were 25% lower (Spreads rose). Results are reported for both long-term lines of credit (Revolvers $\geq$ 1 year) and term loans. (Continued on next page)

Figure 3. Abnormal spreads and the timing of loans (Cont'd). Panel B plots a similar figure, only now the reference group used to calculate abnormal spreads is all firms with the same loan type *and* debt-rating, originated in the same year. For example, the abnormal spread for a term loan taken out by a BB-rated firm in 1997 would be the log(spread) of that loan minus the average log(spread) for all term loans originated in 1997 by BB-rated firms. Additionally, instead of reporting results by loan type, results are reported by debt-rating (where NR stands for "Not-rated", i.e. firms with no recorded debt rating in Dealscan). Both plots use only repeat deals transacted more than one-year apart to calculate results.

Panel A: Aggregate spreads fell by 25% or more



Panel B: Aggregate spreads changed less than 25%



Panel C: Aggregate spreads rose by 25% or more

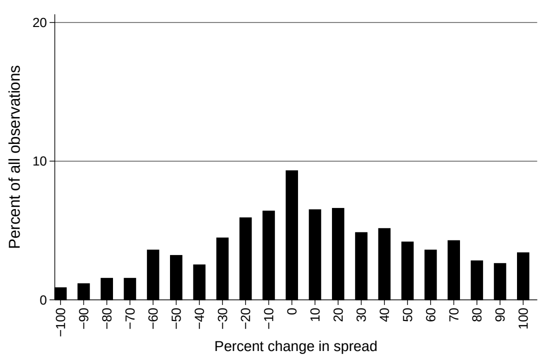
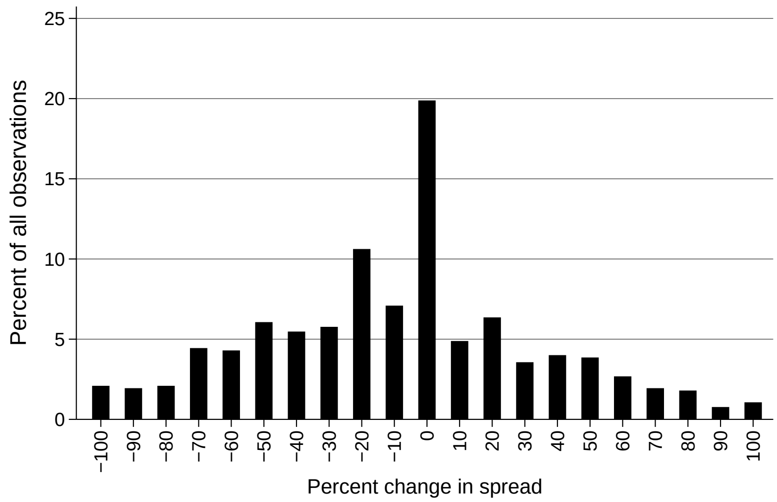


Figure 4. Fixation on prior spreads. This figure plots the empirical distribution of spread changes ($\Delta \log(spread_{i,t}) = \log(spread_{i,t}) - \log(spread_{i,r})$) of repeat loans for three groups: (i) the group of firms that previously borrowed when aggregate spreads were 25% higher (Panel A), (ii) the group of firms that previously borrowed when aggregate spreads changed less than 25% (Panel B), and (iii) the group of firms that previously borrowed when aggregate spreads were 25% lower (Panel C). All plots use only the sample of "new deals", i.e., loans where the origination date of the second loan is after the maturity date of the first loan.

Panel A: Same lead arranger



Panel B: Different lead arranger

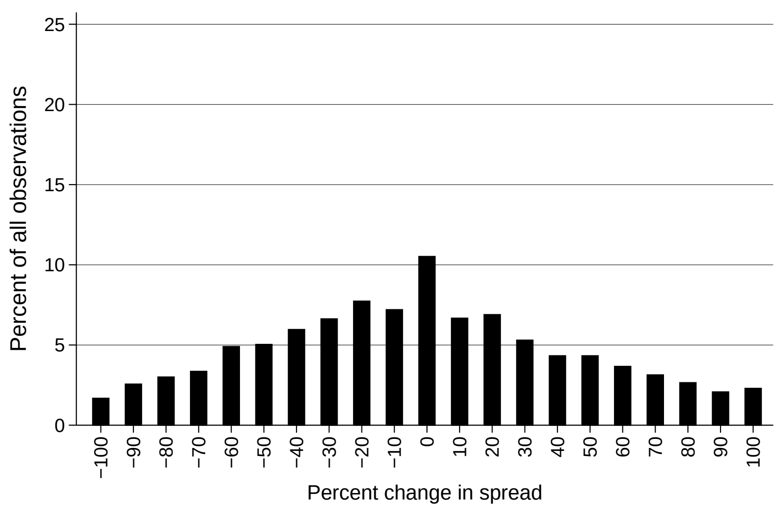
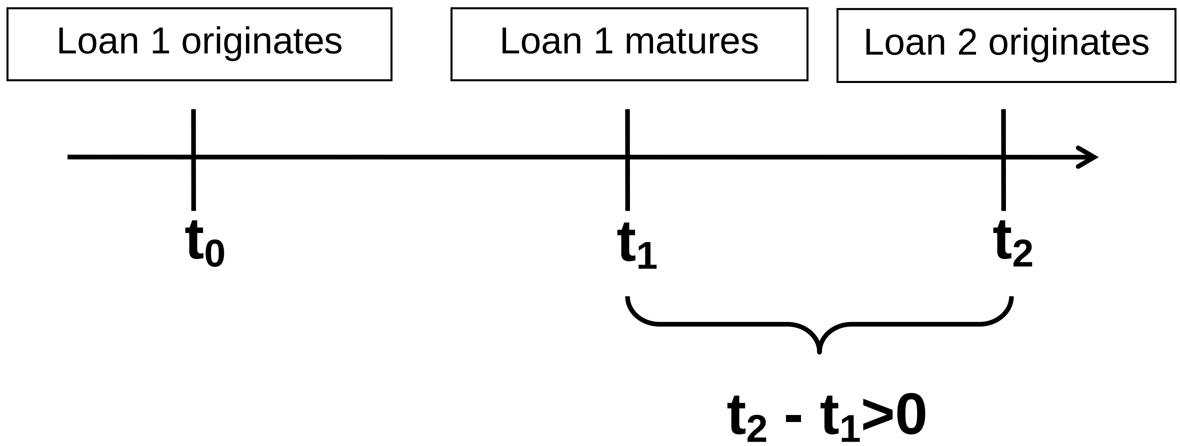


Figure 5. Histogram of spread changes sorted on lead arranger. This figure plots the empirical distribution of $\Delta \log(spread_{i,t}) = \log(spread_{i,t}) - \log(spread_{i,r})$ for the sample of repeat loans in Dealscan where the origination date of the second loan is after the maturity date of the first loan, according to whether the repeat loan has the same lead arranger as the previous loan. Both plots use only the sample of "new deals", i.e., loans where the origination date of the second loan is after the maturity date of the first loan.

Panel A: New deals - Roberts critique sample



Panel B: Forced rollover

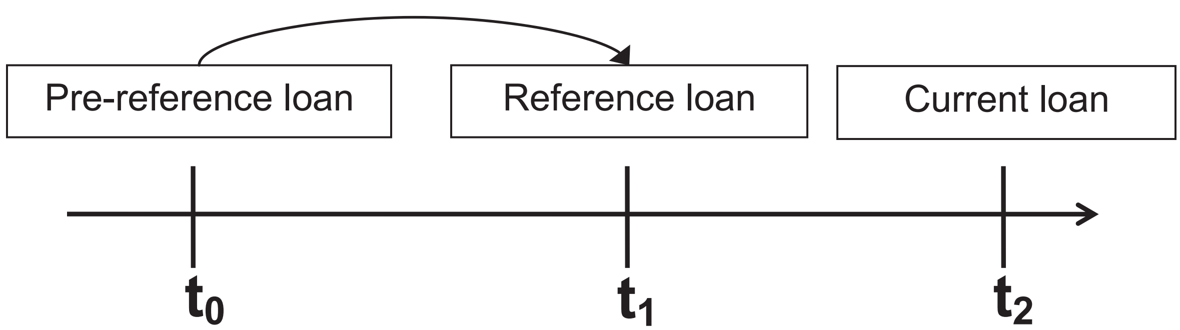
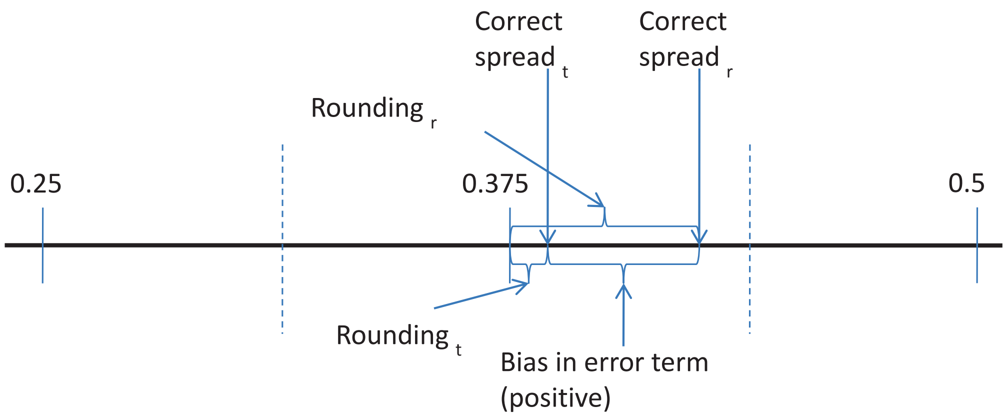


Figure 6. Definition of alternative explanation samples. Panel A illustrates the timeline we use to define "new" loans, i.e., loans where the origination date of the second loan is after the maturity date of the first loan (see Table VI, column 4). Panel B illustrates the timeline we use to define "forced rollovers", i.e., the sample of loans whose previous loan is the result of a forced-rollover of a prior loan (see Table VI, column 5).

Panel A



Panel B

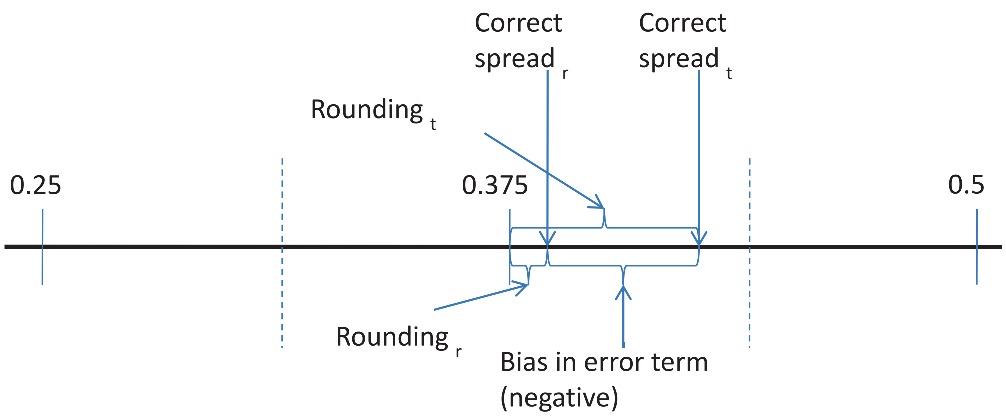
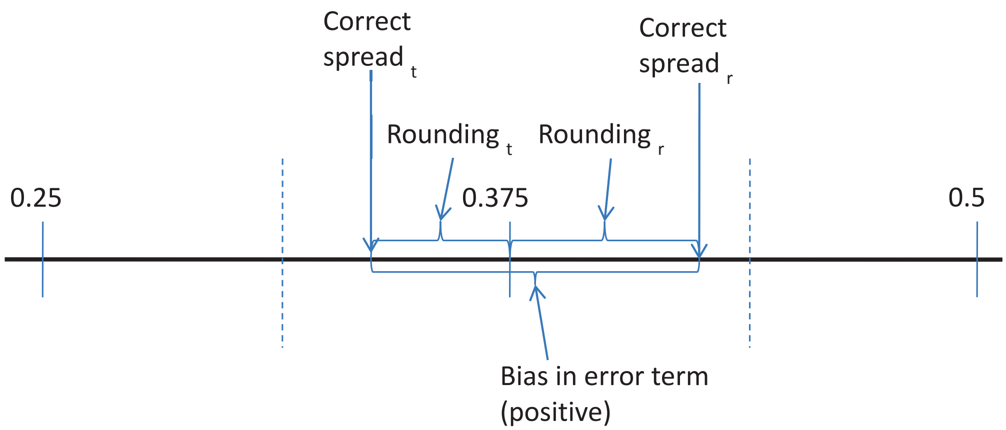


Figure 7. Examples of rounding bias. This figure illustrates the effect of rounding on our parameter estimates in four relevant cases. Panel A shows the case where the direction of rounding is the same in periods t and r, but the rounding was larger in period r. The initial rounding is picked up in our specification as "unobserved quality". Because the rounding is lower for the more recent loan, the error term will appear larger in our specification than in truth. Panel B shows the case where the rounding is larger in period t. In this case, the bias in the error term is reversed.

Panel C



Panel D

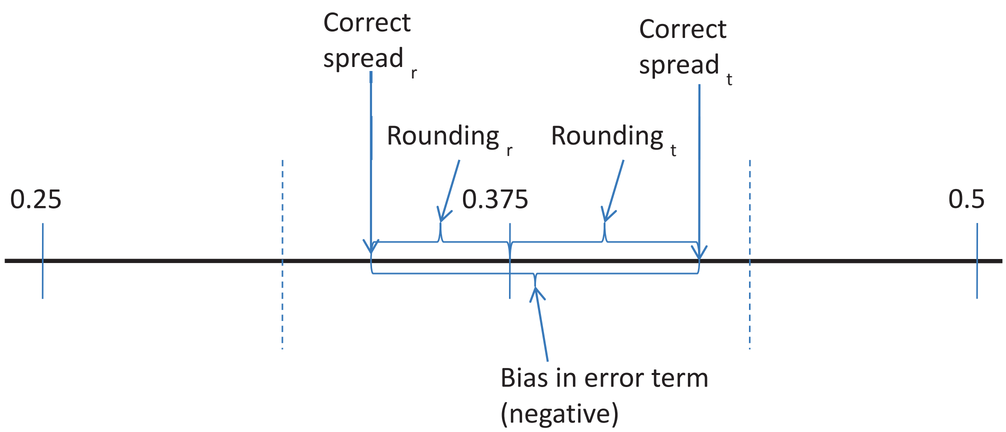


Figure 7. Examples of rounding bias (Cont'd). Panel C shows the case where the direction of rounding changes from "down" to "up" from period r to t. As in panels A and B, the downward rounding is picked up in our specification as unobserved quality. Since the rounding direction switches for the more recent loan, the loan terms appear worse than predicted in period t, rather than better than predicted in period r. The error term will be positively biased. Panel D shows the reverse case, in which case the error term is negatively biased.

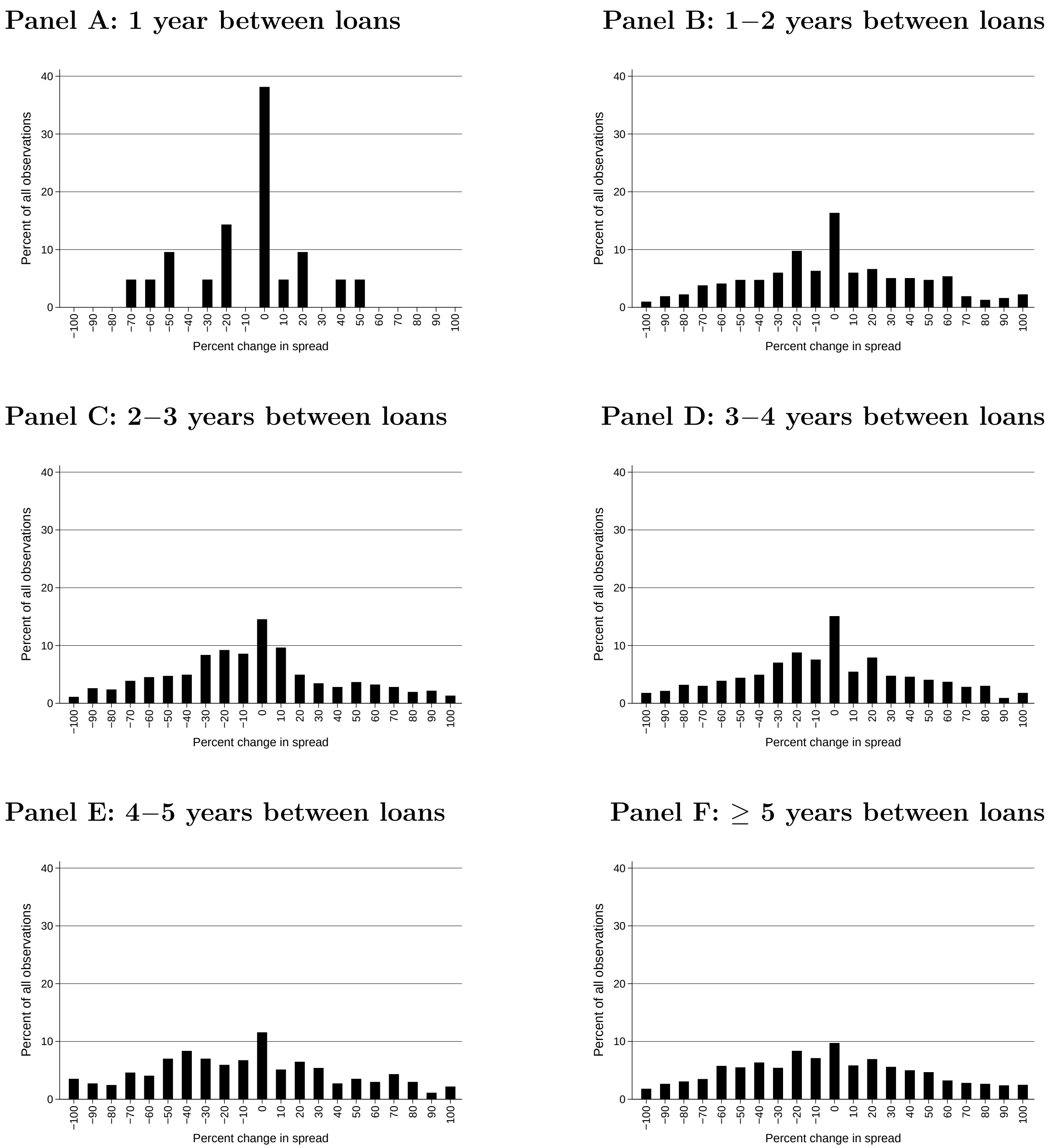


Figure 8. Histogram of spread changes sorted on years between loans. This figure plots the empirical distribution of $\Delta \log(spread_{i,t}) = \log(spread_{i,t}) - \log(spread_{i,r})$ for the sample of repeat loans in Dealscan where the origination date of the second loan is after the maturity date of the first loan, according to the number of years in between transactions.

Table I
Summary statistics.

Two main samples of repeat loans (i.e, loans for which there is a prior loan by the same firm of the same loan type) are used throughout the paper: (1) the sample of repeat loans transacted more than one-year apart (15,536 observations), and (2) the sample of repeat loans transacted more than one-year apart matched to Compustat data (8,525 observations), i.e., repeat loan observations with all the non-missing Compustat variables necessary to estimate (1). Panel A of this table provides summary statics for both of these samples and, for comparison purposes, the sample of all Dealscan long-term revolvers and term loans matched to Compustat firms. The mean and standard deviation of firm assets (Compustat) and sales (Dealscan) are reported for all firms borrowing repeat loans in both the Compustat-matched and unmatched samples (N/A implies that the firm assets are not available for the unmatched sample). Further, this panel reports the average loan maturity, amount, and spread (i.e., the all-in-drawn spread) by loan type (i.e., separately for revolvers and term loans) for all samples. Panel B reports statistics on the loan spread for the sample of repeat loans transacted at least one-year apart. Statistics are reported by firm debt rating, loan type, and for all loans and include the number of observations (N), mean, 10^{th}, 50^{th}, and 90^{th} percentiles.

Panel A: Borrower and loan characteristics of repeat loans

	All loans w/ Compustat firms		**Repeat loans**		**Repeat loans w/ Compustat firms**	
	Mean	SD	Mean	SD	Mean	SD
Borrower						
Assets (Millions)	2845	7473	N/A	N/A	3160	7767
Sales (Millions)	2037	5819	2108	5803	2520	6643
Observations	22562		15536		8525	
Revolvers						
Maturity (Months)	45	19	47	18	47	18
Amount (Millions)	253	530	281	585	294	588
Spread (Bps)	177	119	177	117	163	110
Observations	15713		11854		6935	
Term loans						
Maturity (Months)	59	26	58	25	59	25
Amount (Millions)	205	572	212	494	234	560
Spread (Bps)	263	145	277	159	249	141
Observations	6849		3682		1590	

Table I
Summary statistics (Cont'd.).

Panel B: Average loan spread by debt-rating

	N	Mean	10^{th}	50^{th}	90^{th}
By debt rating					
AAA/AA	144	30	15	20	40
A	780	41	20	30	63
BBB	1706	82	35	65	143
BB	2281	192	88	175	300
B	2184	271	150	250	400
$\leq$ CCC	420	351	200	303	550
NR	8021	221	75	200	387
By loan type					
Revolver	11854	177	40	155	325
Term	3682	277	113	250	450
All loans					
	15536	201	50	175	355

Table II
Loan spreads as a function of borrowing histories.

This table reports results of regressions of log loan spreads on measures of aggregate spread changes. *Spreads rose* is a dummy variable that equals 1 if aggregate spreads have risen 25% or more since the last time a firm has borrowed, and is 0 otherwise. *Spreads fell* is a dummy variable that equals 1 if aggregate spreads have fallen 25% or more since the last time a firm has borrowed, and is 0 otherwise. Δ *Agg. log(spread)* is the log-difference in aggregate spreads between when a firm is currently borrowing and when it last borrowed. Changes in spread for both dummy and continuous variables are calculated by year and loan type in Panel A and by year, loan type, and debt rating in Panel B. Changes in aggregate spreads are calculated for each repeat loan observation excluding the observation of the loan, itself. Additional regressors include year/loan type fixed effects in Panel A, and year/loan type/debt rating fixed effects in Panel B. White standard errors are reported in parenthesis and one, two, and three asterisks denotes statistical significance at the 10, 5, and 1 percent level, respectively.

Panel A: Aggregate spread changes defined *across* credit rating groups

	(1) Log(spread)	(2) Log(spread)	(3) Log(spread)	(4) Log(spread)
Spreads rose	-0.13***		-0.13***	
	(0.02)		(0.02)	
Spreads fell		0.15***	0.15***	
		(0.02)	(0.02)	
Δ Agg. log(spread)				-0.49***
				(0.05)
Constant	5.06***	5.02***	5.03***	5.04***
	(0.01)	(0.01)	(0.01)	(0.01)
Year/loan type F.E.	Yes	Yes	Yes	Yes
Observations	15536	15536	15536	14447
R^2	0.145	0.146	0.148	0.150

Panel B: Aggregate spread changes defined *within* credit rating groups

	(1) Log(spread)	(2) Log(spread)	(3) Log(spread)	(4) Log(spread)	(5) Log(spread)	(6) Log(spread)
Spreads rose	-0.12***		-0.11***		-0.08***	
	(0.01)		(0.01)		(0.02)	
Spreads fell		0.12***	0.11***		0.04*	
		(0.01)	(0.01)		(0.02)	
Δ Agg. log(spread)				-0.21***		-0.18***
				(0.02)		(0.05)
Constant	5.06***	5.02***	5.04***	5.05***	5.03***	5.03***
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)
Year/loan type/rating F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Same debt rating	No	No	No	No	Yes	Yes
Observations	15536	15536	15536	14437	11204	10442
R^2	0.537	0.537	0.538	0.539	0.504	0.504

Table III
Timing of borrowing and measures of credit risk.

This table reports results from regressions of firm creditworthiness on changes in aggregate log spreads(Δ Agg. log(spread)), i.e., the difference in aggregate spreads (calculated by debt rating and tranche type) between when a firm is currently borrowing and when it last borrowed. The change in aggregate spreads is calculated for each repeat loan observation, excluding the observation of the loan itself, when averages are calculated. Firm creditworthiness measures used include market-to-book ($M/B_t = \log((\text{CSHTRQ}_t \times \text{PRCCQ}_t)/(\text{ATQ}_t - \text{PSTKQ}_t - \text{LTQ}_t + \text{TXDITCQ}_t)))$, earnings ($Earnings_t = \text{IBQ}_t/\text{ATQ}_{t-4}$), sales growth ($Sales_t = \text{SALEQ}_t - \text{SALEQ}_{t-4}$), returns ($Return_t = \log(\text{PRCCQ}_t) - \log(\text{PRCCQ}_{t-1})$, future returns ($Future\ Return_t = \log(\text{PRCCQ}_{t+1}) - \log(\text{PRCCQ}_t)$), volatility (standard deviation of daily returns taken quarterly), current ratio ($Current_t = \text{ACTQ}_t/\text{LCTQ}_t$), debt-to-assets ($Debt - to - assets_t = \text{LTQ}_t/\text{ATQ}_t$), and future rating change ($Rating\ change_t = \text{Rating}_t - \text{Rating}_{t+1}$), e.g. rating change is positive (negative) when the firm has received an upgrade (downgrade). All creditworthiness measures are calculated in the quarter *prior* to when the loan is originated except for future returns and rating changes, which are calculated in the following quarter. The last result in the table (column 10) is from an ordered logistic regression. White standard errors are reported in parenthesis and one, two, and three asterisks denotes statistical significance at the 10, 5, and 1 percent level, respectively.

	(1) M/B	(2) Earnings	(3) Sales growth	(4) Returns	(5) F. returns	(6) Volatility	(7) Current	(8) Debt-to-assets	(9) F. rating change
Δ Agg. log(spread)	-0.001	0.005	0.044	0.017	-0.018	-0.004***	-0.506***	0.223*	-0.044
	(0.135)	(0.005)	(0.028)	(0.024)	(0.027)	(0.002)	(0.187)	(0.115)	(0.283)
Constant	12.994***	0.008***	0.103***	0.006**	-0.001	0.032***	2.011***	0.598***	
	(0.016)	(0.001)	(0.004)	(0.003)	(0.003)	(0.000)	(0.020)	(0.010)	
Year/type/rating F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Same debt rating	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7266	8016	8015	8286	8126	8306	7302	8305	3295
R^2	0.130	0.034	0.042	0.043	0.055	0.243	0.052	0.024	

Table IV

First-stage regression estimates: Constructing controls for observed and unobserved firm quality.

This table displays results for our first-stage predictive regressions, specified in Equation (1). These regressions are run year-by-year from 1987-2008, excluding observations of the same firm in the same year. This table presents the *Mean* and the standard deviation (*SD*) for the coefficient estimates, standard error estimates, *Observations* and *Adjusted* R^2s from these regressions. Regressors include both borrower and loan specific variables. Borrower-specific regressors include *Commercial Paper Rating*, which is a dummy variable that equals one if the firm has a commercial paper rating, and zero otherwise, *Log(Sales)*, *Log(Assets)*, *Debt-to-assets* ($(AT-SEQ)/AT$), *ROA* (NI/AT), *Current ratio* (ACT/LCT), and *Volatility* (the quarterly standard deviation of daily returns). Loan-specific regressors include the loan's *Lead arranger market share*, *Log(Tranche amount)*, *Maturity* (in months), *# of Lenders* in the loan syndicate. *Collateral* is a dummy that equals one if the loan is secured, and zero otherwise. *Covenants* is a dummy variable that equals one if the loan includes financial covenants, and zero otherwise. *Performance pricing* is a dummy variable that equals one if the loan has a performance pricing stipulation, and zero otherwise. *Prime base rate* is a dummy variable that equals one if the base rate is prime, and zero otherwise. Additional control variables not shown in Table IV include fixed effects for firm S&P long-term debt ratings, loan type, loan purpose, and lead arranger.

	Coefficients		**Std. errors**		**Obs.**		**Adj. R^2**	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Commercial paper rating	-0.06	0.11	0.09	0.04	1167	340	0.70	0.05
Log(Sales)	-0.05	0.02	0.02	0.01				
Log(Assets)	-0.02	0.03	0.02	0.01				
Debt-to-assets	0.30	0.17	0.08	0.03				
ROA	-0.12	0.25	0.11	0.06				
Current ratio	-0.01	0.02	0.01	0.00				
Return volatility	4.11	2.31	1.08	0.41				
Lead arranger mkt. share	0.15	26.59	29.70	22.59				
Log(Tranche amount)	-0.08	0.03	0.02	0.01				
Maturity	-0.002	0.001	0.001	0.000				
# of lenders	0.001	0.004	0.002	0.001				
Collateral	0.34	0.08	0.04	0.01				
Covenants	-0.06	0.16	0.07	0.07				
Performance pricing	-0.07	0.12	0.06	0.09				
Prime base rate	0.10	0.26	0.07	0.04				

Table V
Second-stage regression estimates: The effect of borrowing history on loan spreads after controlling for firm quality.

This table presents results for the second-stage regression, shown in Equation (2). Regressions are done for *All* repeat loans, and for sub-samples of repeat loans sorted by tranche-type, *Revolvers* (tranches labeled "Revolver $<$1 year", "Revolver $\geq$1 year", and "Term/Revolver"); and *Term loans*. The dependent variable is the logarithm of the all-in-drawn spread. *Predicted* represents the coefficient on the predicted value for the spread at time t. *Spread evolution* represents the coefficient on the difference between the spread at time r and the predicted value for the spread at time t. *Previous residual* represents the residual value, or unobserved quality, from the first-stage regression for the loan at time r. Boot-strapped standard errors are reported in parenthesis and one, two, and three asterisks denotes statistical significance at the 10, 5, and 1 percent level, respectively.

	(1) All Log(spread)	(2) Revolver Log(spread)	(3) Term loan Log(spread)
Predicted spread	0.96***	0.98***	0.79***
	(0.01)	(0.01)	(0.03)
Spread evolution	0.22***	0.22***	0.16***
	(0.01)	(0.01)	(0.04)
Previous residual	0.07***	0.06***	0.10***
	(0.01)	(0.02)	(0.04)
Constant	0.17***	0.08**	1.10***
	(0.04)	(0.04)	(0.18)
Observations	8525	6935	1590
R^2	0.688	0.698	0.447

Table VI
Sub-sample analyses to explore alternative explanations.

The first two columns of this table show estimates of our second-stage regression using samples of repeat loans sorted by whether the repeat loan has the same lead arranger (Same lead), or a different lead arranger (Diff lead) than the previous loan. Column (3) (Excluding zeros) presents estimates of our second stage regression for a sample of repeat loans excluding all observations where the repeat loan and the previous loan have the same loan spread. Column (4) presents results for the sample of "new deals", i.e., loans where the origination date of the second loan is after the maturity date of the first loan (see Figure 6, panel A). Column (5) shows estimates for a sample of loans whose previous loan is the result of forced-rollover of a prior loan (see Figure 6, panel B). For example, at time 0 a loan is made with a maturity of X months, and after exactly X months the same loan is made again (time 1). Boot-strapped standard errors are reported in parenthesis and one, two, and three asterisks denotes statistical significance at the 10, 5, and 1 percent level, respectively.

	(1) Same lead arranger Log(Spread)	(2) Diff lead arranger Log(Spread)	(3) Excluding zeros Log(Spread)	(4) New deals Log(Spread)	(5) Forced rollover Log(Spread)
Predicted spread	0.98***	0.95***	0.96***	0.96***	0.99***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Spread evolution	0.31***	0.19***	0.17***	0.20***	0.23***
	(0.02)	(0.02)	(0.01)	(0.03)	(0.04)
Previous residual	0.06**	0.06***	0.06***	0.03	0.10**
	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)
Constant	0.06	0.23***	0.20***	0.16	0.04
	(0.06)	(0.05)	(0.04)	(0.11)	(0.11)
Observations	3269	5239	7493	1887	740
R^2	0.711	0.674	0.667	0.585	0.729

Table VII
Borrowing history salience and loan spreads.

The first 6 columns of this table show estimates for our second-stage regression for samples of repeat loans sorted on the number of years between loans. For example, column (1) shows regression results for repeat loans taking place within the same year, column (2) shows results for repeat loans taking place between one and two years apart, and so on. The sixth column shows deals where the repeat loan occurs more than five years after the previous loan. The last two columns report results for firms with no debt rating (column 7), and rated firms (column 8). Boot-strapped standard errors are reported in parenthesis and one, two, and three asterisks denotes statistical significance at the 10, 5, and 1 percent level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Number of years between loans							
	≤ 1	$1-2$	$2-3$	$3-4$	$4-5$	≥ 5	Unrated	Rated
	Log(Spread)	Log(Spread)	Log(Spread)	Log(Spread)	Log(Spread)	Log(Spread)	Log(Spread)	Log(Spread)
Predicted spread	0.97***	0.98***	0.99***	0.94***	0.93***	0.95***	1.00***	0.95***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.01)
Spread evolution	0.33***	0.27***	0.28***	0.20***	0.13***	0.13***	0.26***	0.19***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)	(0.04)	(0.02)	(0.02)
Previous residual	0.10***	0.08***	0.03	0.07*	0.08	0.02	0.06**	0.06***
	(0.02)	(0.02)	(0.03)	(0.04)	(0.06)	(0.05)	(0.02)	(0.02)
Constant	0.17***	0.10*	0.04	0.28***	0.36***	0.24	-0.02	0.26***
	(0.06)	(0.06)	(0.08)	(0.11)	(0.13)	(0.16)	(0.10)	(0.04)
Observations	3855	3666	2235	1064	677	883	4463	4062
R^2	0.714	0.707	0.706	0.700	0.670	0.583	0.522	0.777

Table VIII
Compensating behavior: Are sticky spreads offset by changing other loan terms?

Panel A of this table presents two-way frequency counts of loans according to whether the number of covenants has decreased, remained the same, or increased from the previous reference loan, and according to which party, the borrower or the lender, is benefiting from the anchoring, i.e, according to whether $s_{i,r} - \widehat{s}_{i,r}$ is less than zero (borrower benefits), or greater than zero (lender benefits). Panels B and C report the same frequency table for changes in the loans upfront fees and annual fees, respectively. Boot-strapped standard errors are reported in parenthesis and one, two, and three asterisks denotes statistical significance at the 10, 5, and 1 percent level, respectively.

Panel A: Number of covenants

	Number of covenants			
	Decrease	Same	Increase	Total
Borrower benefits	862	2,063	982	3,907
Bank benefits	966	2,378	1,274	4,618
Total	1,828	4,441	2,256	8,525

Panel B: Upfront fee

	Upfront fee			
	Decrease	Same	Increase	Total
Borrower benefits	741	2,399	767	3,907
Bank benefits	1,262	2,723	633	4,618
Total	2,003	5,122	1,400	8,525

Panel C: Annual fee

	Annual fee			
	Decrease	Same	Increase	Total
Borrower benefits	674	2,534	699	3,907
Bank benefits	935	3,215	468	4,618
Total	1,609	5,749	1,167	8,525

Table IX
Limits to arbitrage: Anchoring and the number of historical lenders.

The first two columns present second-stage regression results according to which party benefits from anchoring to the previous loan's spread, i.e, according to whether $s_{i,r} - \widehat{s}_{i,r}$ is less than zero (borrower benefits), or greater than zero (lender benefits). The next three columns restrict the sample to only loans where the lender benefits from anchoring ($s_{i,r} - \widehat{s}_{i,r} > 0$) and varies the number of lead arrangers a firm has had in the past. Column (3) presents results for the sample restricted to firms with 1 or fewer previous lead arrangers. Column (4) restricts the sample to firms with 2-4 previous lead arrangers and column (5) restricts the sample to firms with 5 or more previous lead arrangers. Boot-strapped standard errors are reported in parenthesis and one, two, and three asterisks denotes statistical significance at the 10, 5, and 1 percent level, respectively.

	(1)	(2)	(3)	(4)	(5)
			Number of historical lead arrangers		
			1	$2-4$	≥ 5
	Borrower benefits Log(Spread)	Lender benefits Log(Spread)	Lender benefits Log(Spread)	Lender benefits Log(Spread)	Lender benefits Log(Spread)
Predicted spread	1.01***	1.01***	1.03***	1.02***	0.97***
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
Spread evolution	0.04*	0.23***	0.25***	0.19***	0.14**
	(0.02)	(0.03)	(0.05)	(0.04)	(0.07)
Previous residual	0.15***	0.10***	0.11***	0.11***	0.07
	(0.02)	(0.02)	(0.04)	(0.03)	(0.07)
Constant	-0.10*	-0.07*	-0.20***	-0.12**	0.16
	(0.05)	(0.04)	(0.08)	(0.06)	(0.12)
Observations	4516	5435	1442	2840	259
R^2	0.709	0.741	0.717	0.728	0.835