Anchoring and the Cost of Capital *

Casey Dougal
Joseph Engelberg
Christopher A. Parsons
Edward D. Van Wesep†

December 7, 2010

Abstract

This paper documents that a firm's *current* cost of borrowing is strongly influenced by the nominal value of its *historical* borrowing costs, after accounting for current fundamentals and market conditions. The effect is strongest when the historical reference deal is more recent, when the firm borrows again from the same bank, and when the firm's CEO or CFO has not changed. Overall, the evidence suggests that borrowers and lenders use past terms as anchors (Tversky and Kahneman [1974]), and that these seemingly irrelevant reference points influence future transactions.

Keywords: Capital structure, Cost of Debt, Anchoring, Reference Points
JEL: G30, G32

*We thank Aydogan Alti, Jay Hartzell, Anil Shivdasani, Sheridan Titman, and seminar participants at UNC for helpful feedback. The usual disclaimer applies.

†All authors are from the Kenan-Flagler Business School, University of North Carolina at Chapel Hill. Contact: Casey Dougal, (Email) casey_dougal@kenan-flagler.unc.edu, (Tel) 208-284-3117; Joseph Engelberg, (Email) joseph_engelberg@kenan-flagler.unc.edu, (Tel) 919-962-6889; Christopher Parsons, (Email) chris_parsons@kenan-flagler.unc.edu, (Tel) 919-962-4132; Edward Van Wesep, (Email) vanwesee@kenan-flagler.unc.edu, (Tel) 919-962-8466.

1 Introduction

There is overwhelming experimental and laboratory evidence that people violate the standard economic assumptions of full rationality (Camerer, Lowenstein and Rabin [2003]). However, critics of behavioral economics remain skeptical about the extent to which observations in the lab translate into the "real world" (Becker [2010], Cochrane [2010] and Rubinstein [2006]).[1] Do behavioral biases affect competitive markets run by experienced professionals engaging in high-stakes transactions?

In this paper, we provide evidence that a well-known behavioral bias called "anchoring" (Tversky and Kahneman [1974]) manifests itself in a highly competitive, trillion-dollar market run by experienced financiers: the market for syndicated loans. We find that when banks lend to firms, they tend to use past deal terms as reference points, even when these would appear irrelevant for subsequent transactions.

This is difficult to reconcile with textbook theories of capital structure that specify borrowing costs as forward looking. Current or past variables should be relevant only to the extent that they provide information about the future, be it about the firm's risk, the risk premium, or frictions in the supply of capital (e.g., information asymmetries). Indeed, we often explain borrowing costs with characteristics like size or profitability because they are thought to correlate with future events, affecting default probabilities or expected default losses. There should be no role for purely historical information.

Yet in this paper, we find strong evidence that a firm's borrowing *history* matters for its *current* cost of debt. Figure 1 illustrates what we mean. In the first panel, we plot the average spread above LIBOR for long-term lines of credit (maturity greater than or equal to one year) during the years 2005-2008. For every credit rating category, spreads were relatively stable between 2005 and 2007, but then spiked during the Financial Crisis of 2008. The overall average increase was about 35 percent, so that a BBB firm borrowing at a 200 basis point spread in 2006 could be expected to pay roughly 270 basis points in 2008.

In the second panel, we identify every firm in our sample that took out a line of credit exactly once during the interval 2005-2007, and then once again in 2008. To keep the comparison as clean as possible, we consider only firms that maintained the same credit

[1] Referring to experiments in behavioral economics, Cochrane (2010) says, "These experiments are very interesting, and I find them interesting too. The next question is, to what extent does what we find in the lab translate into understanding how people behave in the real world?" Becker (2010) questions the applicability of lab evidence from behavioral economists: "They're dealing with people in the lab. Economists are dealing with people in the real world. And there's a difference between the lab and the real world." Rubinstein (2006) questions the applicability of many experiments, especially those involving animals. For example, he questions the applicability of Chen et al.'s (2006) capuchin monkey experiments: "It is truly amazing to watch a monkey pay for food. It would only be more amazing to watch a monkey following a Wall Street ticker tape and trading options." (Rubinstein [2006])

rating between borrowing events—e.g., the firm's credit rating was BBB in both 2006 and 2008. The histogram shows the percentage change in spreads between the first, pre-crisis loan and the second, post-crisis loan. Quite clearly, the borrowing costs for this sample reflect the increase in aggregate spreads. However, what is most striking is the nearly 30% of firms that are extended credit at *exactly* the same rate they were afforded in the pre-crisis period. By a factor of two, this is the most common outcome in the repeat round of financing for this sample.

The illustration suggests that firms and their lenders use past deal terms as reference points, even when faced with more recent information about firm fundamentals or the price of risk. In this paper, we generalize this anecdote using over 19,000 syndicated loan transactions between 1987 and 2008. We adopt the regression framework of Genesove and Mayer (2001) and Beggs and Graddy (2009) to identify anchoring within these loan transactions. In particular, for each year t we run a hedonic regression to obtain a predicted log spread, p, for firm i's loan:

$$p_{i,t} = X_{i,t}\beta_t, \tag{1}$$

as a function of time t observable characteristics, X, like debt ratings, size, profitability, purpose of the loan, etc. We then use these predicted spreads as inputs into our main estimating equation:

$$s_{i,t} = \mu \cdot \underbrace{p_{i,t}}_{Predicted} + \delta \cdot \underbrace{(s_{i,t-} - p_{i,t-})}_{Quality} + \lambda \cdot \underbrace{(s_{i,t-} - p_{i,t})}_{Anchor} + \varepsilon_{i,t} \tag{2}$$

where s is the realized log spread, and time subscripts refer to either the current year, t, or time of a previous loan, $t-$. The first term captures the effect of current observable firm or market characteristics on spreads; unsurprisingly, we find estimates of μ very close to one.

The second term is the residual from firm i's previous loan, intended to measure the effect of persistent, but unobservable characteristics (i.e., not captured in X) on borrowing costs. For instance, the firm may possess unusually capable management or other competitive advantages not reflected in its observable characteristics, but these attributes may be privately conveyed to banks in the lending process. Our full-sample estimate of δ suggests that roughly 16 percent of the residual value from a past deal carries through to future transactions.[2]

[2]Genesove and Mayer (2001) employ similar reasoning in their analysis of anchoring on past transaction prices of residential houses. They note that house prices may deviate from their predicted values due to unique attributes (charm, quiet neighbors, etc.), but that these features may not be captured in a hedonic regression. Thus, the authors allow a residual from a past sales price to, proportionally, affect the asking price in a repeat transaction.

The final term measures what is commonly referred to as the anchoring effect.[3] As indicated by Equation (2), it measures whether, *after controlling for the effect of observable (first term) and unobservable (second term) fundamentals on predicted rates*, the nominal value of the firm's historical borrowing cost, s_{t-}, nevertheless influences the current spread it is charged. A positive estimate on λ would imply that an increase in predicted spreads from $t-$ to t ($s_{t-} - p_t < 0$) would pull current realized spreads downward, and that a decrease ($s_{t-} - p_t > 0$) would push them upward. In either case, s_{t-} acts as an anchor that pulls the current spread toward its own historical value, similar to what we saw in Figure 1.

Averaged across all observations in our sample, we obtain an estimated value of λ close to 0.16 (bootstrapped t-statistic = 10.66).[4] The effect is approximately symmetric, applying equally well to periods of spread increases and decreases. To put this estimate in perspective, the standard deviation of predicted log spreads, p, is about 0.6. However, as Equation (2) makes clear, the presence of anchoring ($\lambda > 0$) means that s will not move one-for-one with p. Specifically, the net effect is only $\mu - \lambda \approx 1 - .16 = .84$, so that only about 0.5 of the 0.6 innovation in p is translated to realized rates, s. This implies that the typical firm's debt is mispriced by approximately 13.5 basis points, similar to the effect of moving between an AA credit rating and an A credit rating.

Depending on the cross-sectional unit of observation, such anchoring-induced loan mispricing can be much larger. Specifically, the anchoring effect is two to three times as large for short-term (< 1 year) lines of credit ($\lambda = 0.43$) versus long-term lines of credit ($\lambda = 0.14$) or term loans ($\lambda = 0.17$), although it is significant at the 1 percent level for all three types. Likewise, past spreads are more predictive for firms lacking public debt ratings, which, given their importance in predicting borrowing costs, might explain banks substituting historical heuristics in their place.[5]

The effects of anchoring decline with time, i.e., as the distance between t and $t-$ in Equation (2) increases. Consistent with the idea that more recent transactions are viewed as more salient by the respective parties (as Beggs and Graddy [2009] find for resales of collectible art), estimates of λ decline steadily with the time between transactions: 0.29, 0.18, 0.14, 0.09, 0.07 for < 1, 1–2, 2–3, 3–4, and 4–5 years between transactions respectively.

[3] Although the past borrowing rate, s_{t-}, shows up in both the second and third terms, λ and δ are separately identified because of variation between p_t and p_{t-}.

[4] Because our second-stage regression model contains variables constructed from parameters estimated in the first stage, the covariance matrix of the second-stage estimator includes noise induced by the first-stage estimates. Thus, the covariance matrix generated by OLS estimation of the second-stage is inconsistent. To correct for this we estimate second-stage standard errors using 1,000 bootstrap iterations. See Freedman (1984).

[5] Estimates of λ using only loans from firms lacking a debt rating are 0.21 versus 0.13 for firms with a recorded debt rating.

Perhaps the most compelling evidence that what we are measuring is a behavioral bias can be seen in situations where either a firm switches banking syndicates, or replaces key management team members. Analysis of these instances reveals that, to a large extent, anchoring is mostly linked to *specific individuals*. When a firm borrows from a different lead bank, there is usually some residual anchoring to its most recent loan, but the effect is much smaller than for loans involving the same lead bank. Term loans, for example, have an anchoring coefficient of 0.34 (bootstrapped t statistic = 4.43) when the lead bank is the same, but only 0.12 (bootstrapped t statistic = 2.67) when the lead bank differs. Turnover of the firm's CEO or CFO has a similar effect: in cases where the same individuals are employed for both the reference and current loan, the anchoring effect is stronger than when either has been replaced.

We also explore several alternative explanations for the evidence. One such explanation is that we are simply picking up "rounding," so that if a firm's cost of capital changes by a few basis points, it may be masked if spreads are quoted in increments of say, 25 or 50 basis points. We show that such an argument falls short on both logical and empirical grounds. Even excluding the observations where spreads did not change at all between transactions (those cases of interest in Figure 1), our main results are qualitatively unchanged. In addition to ruling out rounding as a potential alternative, this test also addresses whether costly renegotiation of lending terms is capable of explaining the results. By examining the subset of deals for which terms have certainly been renegotiated, this alternative is rejected as well.

A more fundamental concern is the endogeneity of *when* a firm chooses to access the capital markets. Although we are controlling for a number of observed and unobserved determinants of firm risk, correlation between credit market conditions and unmeasured firm risk is capable of generating a positive coefficient on λ.[6] To address this possibility, we restrict our analysis to only those observations where the historical, reference transactions were themselves rollovers of previous loans. For example, suppose that we are attempting to explain the spread on a line of credit initiated in 2006, as a function of the spread charged on a similar instrument in 2002. Because the concern is that credit market conditions in 2002 may be correlated with unobserved risk characteristics, we consider a subset of observations not subject to this concern: loans initiated at the maturity of a prior loan (forced-rollovers). Under the assumption that market conditions in 2002 are not predictable five years prior, this test allows us to identify a sample of relatively exogenous borrowing events. We find even stronger effects among forced-rollovers, suggesting that sample selection issues are, as

[6]As a specific example, imagine that low quality firms are more likely to borrow when spreads are high, and vice versa. If control variables do not perfectly account for risk characteristics, then a firm's borrowing history would provide information about its risk, and therefore, its borrowing costs.

most, minor.

Taken together, the evidence herein offers potential contributions to two literatures. The first is a large literature which explores capital structure and determinants of the costs of capital. Here, our innovation is to provide evidence that cost of capital may be influenced by borrowing *history* in addition to *expectations* about fundamentals. Baker and Wurgler (2002) provide evidence that a firm's current leverage ratio is related to its historical valuations via past attempts to time the market. The analogy to our findings is immediately apparent—while there is no market timing motive per se in the lending context, we show that histories clearly matter through their impact on the memories of individuals involved in future transactions.[7]

Second, our study adds to a growing literature on the manifestation of psychological biases in economic decisions. The most similar studies are Genesove and Mayer's (2001) analysis of residential house sellers, and Beggs and Graddy's (2009) examination of collectible art resales. Although there is a large body of evidence of behavioral biases in laboratory experiments, an open question remains whether, and to what extent, behavioral biases affect competitive markets with professionals. The evidence here suggests that behavioral forces in fact manifest even when the stakes are large, market forces are strong, and when agents are sophisticated.

The paper is organized as follows. Section 2 briefly reviews the literature in reference points and anchoring in economic decisions. We then discuss our sample and empirical methodology in Sections 3 and 4. The main results are presented in Section 5, along with several cross-sectional tests of interest. Section 6 deals with alternatives to the main hypothesis. Section 7 concludes.

2 Anchoring and reference points

It is well known in psychology that, when faced with complex tasks, individuals will often use shortcuts in judgement called heuristics. One common heuristic is *anchoring*:

> In many situations, people make estimates by starting from an initial value that is adjusted to yield the final answer. The initial value, or starting point, may be suggested by the formulation of the problem, or it may be the result of a partial computation. In either case, adjustments are typically insufficient. That is, different starting points yield different estimates, which are biased toward the initial values. We call this phenomenon anchoring. (Tversky and Kahneman, 1974)

[7] See also recent work by Baker and Xuan (2009), which shows that the stock price a CEO inherits matters for capital raising decisions.

Examples of anchoring in the economics literature are plentiful. When buying or selling a piece of art, individuals appear to anchor on past sale prices (Beggs and Graddy [2009]); when buying a house, individuals appear to anchor on a home's listing price (Northcraft and Neale [1987]); when grocery shopping, consumers anchor on historical product prices (Rajendran and Tellis [1994] and Greenleaf [1995]); and when deciding on the acquisition price for a target in a merger, firms appear to anchor on the 52-week high of the target's stock price (Baker, Pan, and Wurgler [2010]). Because buyers (sellers) may anchor on the suggested price of sellers (buyers), anchoring can also be used as a tool for manipulation in negotiations (Kahneman [1992]). For example, Babcock, Wang, and Loewenstein (1996) find evidence that a strategic choice of "comparable" school districts affects the outcome of salary negotiations between teacher unions and school boards.

The concept of anchoring is also closely related to the use of reference points in perceptions of utility following Kahneman and Tversky's (1979) prospect theory. Kahneman and Tversky argue that utility is defined over gains and losses from a reference point, rather than over absolute wealth. They propose a value function which is *S-shaped* (concave over gains and convex over losses), and kinks as it passes through the reference point at the origin. The kink at the reference point implies that individuals dislike small losses more than they like small gains, a phenomenon known as loss aversion.[8]

In our setting, we observe many repeat loan transactions. Here, the natural reference point is the interest rate (spread) agreed upon in the prior transaction. In the following sections, we describe how our data and methodology allow us to identify anchoring effects on these historical interest rates.

3 Data and Summary Statistics

We obtain loan data from the Reuters Dealscan database, firm fundamentals from Compustat, and executive officer data from ExecuComp. Each observation in our study corresponds to a separate dollar-denominated loan tranche, (also called a loan facility). Dealscan reports facilities by type, which we group into three categories: 1) short-term lines of credit, 2)

[8]Numerous examples that economic agents are influenced by loss aversion exist. Genesove and Mayer (2001) find that a homeowner's purchase price strongly affects his selling behavior because homeowners are loss averse. Sellers facing a loss select higher asking prices and incur longer selling times. Odean (1998) shows that a retail trader's purchase price affects his selling decision. Traders will hold onto stocks with paper losses and sell those with paper gains, which Shefrin and Statman (1985) call the "disposition effect." See also Grinblatt and Keloharju (2001), Dhar and Zhu (2006), Shapira and Venezia (2001) and Dorn and Strobl (2009).

long-term lines of credit, and 3) term loans.[9]

Our initial sample includes all loans from Dealscan between 1987 and 2008. From this set we exclude loans to regulated and/or financial firms (SIC 40-45, 60-64), and loans for which no spread data are reported. We also exclude any observation for which we cannot match the borrowing firm with Compustat data. This results in 19,578 observations, which we call the master sample.

An important subset of the master sample is the set of *repeat loans*, which lend themselves to tests of anchoring. A repeat loan is an observation for which there is a prior observation by the same firm and of the same loan type. For example, if Dell took out a revolver first in 2004, then in 2006, and again in 2008, we would call the last two observations repeat loans because they both have a predecessor. Of the 19,578 observations in the master sample, 7,310 are repeat loans. For each repeat loan, we can compare its pricing with that of its predecessor. For example, we can calculate the difference between the spread on Dell's 2006 line of credit with the spread on Dell's 2004 line of credit. We refer to such spread differences as *innovations*. For example, Dell's 2006 innovation in spread would be the difference between it's 2006 spread and its 2004 spread, and Dell's 2008 innovation in loan amount would be the difference between it's 2008 loan amount and it's 2006 loan amount.

Table 1 provides summary statics about the master sample (Panel A), the sample of repeat loans (Panel B) and repeat loan innovations (Panel C). The average firm in our master sample has assets of \$2.5 billion, annual sales of \$1.8 billion and an industry-median adjusted book leverage ratio of 12 percent. The average loan amount is \$228 million with a spread of 226 basis points above LIBOR.

Panel B shows that the average repeat borrower in our sample has assets of \$2.7 billion, sales of \$2.2 billion, and an industry-adjusted leverage ratio of 11 percent. On average, repeat loans have an amount of \$269 million with a spread of 208 basis points above LIBOR. Comparing the means of Panels A and B thus reveals that repeat borrowers are larger and less risky.

Panel C shows that this trend holds even for the same firm—that is, a firm that borrows a second time has generally improved its fundamentals. On average, repeat borrowers have become larger (+ \$289 million in assets), their loan amounts have become greater (+ \$37 million), and their spreads have decreased (−3 basis points). Given that a firm borrows again, it has often become a "better" firm.

[9] Short-term lines of credit are loan types that Dealscan labels "364-day facilities" and "Revolver <1 year"; long-term lines of credit are loan types that Dealscan labels "Revolver ≥1 year" and "Term/Revolver"; term loans are loan types that Dealscan labels "Term Loan", "Term Loan A" and "Term Loan B."

Of particular interest for this study will be spread innovations for repeat loans:

$$\Delta \log(spread_{i,t}) = \log(spread_{i,t}) - \log(spread_{i,t-}), \tag{3}$$

where $t-$ refers to the most recent time when the firm has borrowed.

Figure 2 plots the empirical distribution of $\Delta \log(spread_{i,t})$ for all repeat loan transactions in DealScan. The most striking feature is that although the distribution is roughly normally distributed around -20% (reflecting the fact that the typical repeat borrower is a better credit risk), there is a large discontinuity at zero. In fact, zero is the modal outcome, corresponding to about one-fifth of the total observations.

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 Gaussian kernel to estimate a smooth distribution for our sample.[10] 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.[11] Our test results reject the "no discontinuity at zero" hypothesis at less than the 0.1% level. Various alternatives for estimating the kernel density and bandwidth do not alter this conclusion.

If changes in spreads reflect changes in the riskiness of firms, or in the market price of that risk, then the distribution in Figure 2 is peculiar. For example, if there were a macro shock that increased spreads for all firms by 10%, a firm would have to have improved its risk profile to reduce its idiosyncratic spread by 10%. Such a coincidence would allow the firm to borrow at *exactly* the same spread as before and, while sometimes possible, Figure 2 suggests too many such instances. Compared to the histogram buckets immediately above (+10%) and immediately below (-10%), the empirical frequency of no change (0%) is three times greater.

[10] 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 standard normal density, and h is the kernel bandwidth chosen following Silverman (1986). In our case, $h = 0.0537$.

[11] 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.

4 Methodology

While the distribution of spread changes is suggestive, to formally establish the presence of anchoring, we must control for the effect of firm risk and market conditions on the current spread. The regression specification of Genesove and Mayer (2001) and Beggs and Grady (2009), described in Equations (1) and (2), provides a suitable framework.

The identification strategy is best illustrated with a numerical example. Suppose that a BBB firm takes out a line of credit at 250 bps above LIBOR in 1992, when a comparable BBB firm engaging in the same transaction can borrow at 200 bps. To the econometrician, our BBB firm has borrowing costs 50 bps greater than comparable contemporaneous transactions would suggest. Now, suppose that the same BBB firm takes out another line of credit in 1995 and that, in 1995, the comparable predicted spread (again, using observables) is now only 160 bps.

Our goal is to model the firm's repeat spread (i.e., that realized in 1995) as a function of three factors: 1) the predicted value based on comparable transactions in 1995 (160 bps in this example), 2) the deviation from the comparable value the last time it borrowed (250-200 bps in the year 1992), and 3) an anchoring term. The anchor term is definded as how far away current spreads are from the spread our BBB firm received in 1992 (250 - 160 = 90 bps). If anchoring is important, then the distance between the current spread and the spread at which our BBB borrowed last time will be important.

Notice that in order to predict our BBB firm's spread in 1995 with these three components, disentangling the second component (*Quality*) from the third component (*Anchor*) requires time variation in the first component (*Predicted*).[12] If typical spreads were *always* 200 bps and our BBB firm always borrowed at 250 bps, we could not tell whether our BBB was anchored to 250 bps, or was simply a below-average BBB firm that required a premium of 50 bps.

Although our formal regression analysis is more detailed, the intuition to the illustration is identical to the regression approach. The regression entails two steps. First, for each observation, we use the master sample to estimate log(*spread*) using observable firm characteristics (e.g., credit rating and leverage) and observable loan characteristics (e.g., loan amount and whether the loan is secured). This gives us our predicted spread which we label *Predicted* in Equation (1). We run this regression year-by-year to allow time variation in the effects of these variables on spreads, and we exclude all observations involving firm i when estimating $p_{i,t}$. For example, if Dell took out a line of credit in 2006, we would use

[12]This echoes the point made in Beggs and Graddy (2009) who can identify anchoring in art auctions through the time variation in the "hotness" of art markets.

only non-Dell observations to estimate the log(*spread*) Dell could expect. This first step will produce, for each observation in the master sample, both a predicted spread and a residual.

In the second step, we only consider the set of repeat loan observations. By definition, each repeat loan will have a predecessor loan for which we have calculated a residual. This term captures unobserved *Quality* and is denoted by $(s_{i,t-} - p_{i,t-})$ in Equation (2). For example, the spread residual from Dell's 2004 line of credit would correspond to its unobserved quality when predicting the spread in Dell's 2006 transaction. For each repeat loan observation, we can also calculate the "distance" between the current predicted spread, and the spread received in the last transaction. This is the *Anchor* term, and is denoted $(s_{i,t-} - p_{i,t})$ in Equation (2).

Having calculated *Predicted*, *Quality*, and *Anchor* for each repeat loan observation, we then regress log(*spread*) on all three variables in Equation (2). The main variable of interest is the coefficient λ on *Anchor*. A positive coefficient would imply that, *after controlling for the effect of current fundamentals and time-invariant unobserved quality*, the nominal value of a firm's historical spread pulls current spreads towards it. As in Genesove and Mayer (2001) and Beggs and Graddy (2009), we interpret this as evidence of anchoring.

5 Empirical Results

5.1 First-Stage Predictive Regressions

Table 2 displays the results of the first stage predictive regressions, shown in Equation (1). There are 22 such cross-sectional regressions, one for each year from 1987-2008. In this step we use the master sample to obtain year-specific coefficient estimates, which are then used to predict spreads for the smaller sample of repeat loans.

Rather than present complete, and largely redundant, results from each cross-sectional regression, Table 2 simply reports summary statistics for the estimated coefficients. Most estimates are intuitively sensible, with size (*Sales* and *Assets*), profitability (*ROA*), and restrictions (*Covenants* and *Performance Pricing*) reducing spreads. By contrast, larger spreads are required for firms with more *Leverage*, and when *Collateral* is pledged.[13]

For our purposes, the more relevant metric is the distribution of R^2. The average (median) R^2 is 0.68 (0.71), and in ninety percent of the cases, we are explaining at least half of the cross-sectional variation in spreads. This is important because in the second stage, we will rely on the predictions from this model to control for observed firm quality. Specifically, when explaining spreads during any year t, we will use the hedonic regression in year t

[13]One explanation for this is that lenders require collateral in unusually risky situations.

to form a predicted spread, which is intended to control for the effects of observable firm characteristics on spreads.

Indeed, inspection of Equation (2) makes clear that δ and λ can be separately identified only when $p_t \neq p_{t-}$. Because s_{t-} shows up in both terms, variation in predicted borrowing costs is required to make the distinction between anchoring and unobserved firm quality. Unless we can accurately measure changes in p, we have no hope of detecting the use of reference point in lending negotiations. However, Table 1 indicates that this is unlikely to be a serious concern.

5.2 Anchoring to Historical Spreads

With the predicted spreads in hand, we are in a position to estimate Equation (2), the results of which are shown in Table 3. The first column aggregates all three transaction types together, while the subsequent columns present them separately. The coefficient on the predicted spreads from the first stage hedonic regression is nearly one in each case, confirming that the larger estimation sample is not systematically different than the holdout sample of repeat loans.

The predicted log spread explains, by itself, 69.6%, 71.9%, 68.1%, and 41.1% of the variation in log spreads. The remainder of the explanatory power comes from the other two terms, *Anchor* and *Quality*. The coefficient on *Quality* indicates that about one-sixth of the deviation from the most recent transaction translates to current spreads. For instance, suppose that in 2004, a petroleum firm takes out a line of credit at 160 basis points, despite the fact that the hedonic prediction (from 2004) indicated a spread of only 100 basis points. The next time it borrows, a spread increase of roughly 10% ($0.16 \times \frac{160-100}{100}$) would be expected.

While the positive coefficient on $(s_{t-} - p_{t-})$ suggests that past residuals likely contain some information about current firm quality, most of it appears to have dissipated ($\delta << 1$). To continue with the example, the petroleum firm might have been able to convince its lenders in 2004 that oil reserves not yet proven soon would be, which might have justified a lower borrowing rate. However, as this information becomes public, it will become reflected in the firm's observable characteristics. In the limit, all private information will be ultimately be captured by p_t, rendering the residual from past regressions worthless. On average, this aggregate "depreciation" is in the neighborhood of 80%. Inclusion of the quality term increases the R^2 by 0.04 in the aggregate (first column), and by 0.09, 0.04, and 0.16 in the second, third, and fourth columns respectively.

Our primary interest is in coefficient on *Anchor*, the difference between the past realized spread and the current predicted spread (both in logarithms). The point estimate on the anchoring term is 0.16 (bootstrapped t statistic = 10.66), indicating the amount by which current spreads are biased toward past realized spreads. Continuing with the same petroleum firm we discussed above, suppose that instead of being able to borrow at 160 basis points (as it did in 2004), deteriorating fundamentals in 2006 indicate a higher borrowing cost of 240 basis points. Without anchoring, the firm's 2006 spread would fully incorporate the impact of its poor fundamentals, increasing the spread it is charged by 50 percent. With anchoring, this adjustment is mitigated, so that only 1-0.16=0.84 of this innovation is reflected in the 2006 spread. Net of anchoring, the realized spread increases by only .84*80=67 bps.

Moving across Table 3, we see that while anchoring effects are present for all three loan types, they are much stronger for short-term lines of credit than for long-term facilities and term loans. The magnitude of the anchoring coefficient for lines of credit less than a year is 0.43 (bootstrapped t statistic = 7.02), indicating that only about half of predicted spread innovations actually become reflected in realized spreads. The standard deviation of spread innovations, $(s_{t-} - p_t)$, is 0.58 for short-term lines of credit. With anchoring of the magnitude seen in the second column, a 58 percent increase in *predicted* borrowing costs translates to an *actual* increase of only 31 percent.

The third and fourth columns report the results for lending facilities of longer duration. For long-term credit lines and term loans respectively, the anchoring term is estimated at 0.14 and 0.17. As before, this means that innovations in borrowing costs will be muted, although not as dramatically as for short-term lines of credit. The standard deviation of of the anchoring variable is 0.55 and 0.49 in columns three and four respectively, which, against the average spreads for each (191 basis points and 297 basis points respectively), implies a standard deviation of predicted borrowing costs in the neighborhood of 0.55*191=105 bp to 0.49*297=145 bp, for long-term lines of credit and term loans respectively. However, anchoring reduces the *expression* of these innovations in actual borrowing costs, so that a typical long-term line of credit will be mispriced by about 14 bp, and a term loan by about 19 bp.

Our empirical specification assumes that anchoring is symmetric—borrowers benefit when spreads rise and lenders benefit when spreads fall—but to this point, we have not shown whether this assumption is valid. Results presented in Table 4 confirm the assumption. In panel A of Table 4, we compare anchor terms in situations where the predicted spreads should have increased or decreased for a firm.[14] The first column presents the results when $(p_t - p_{t-}) < 0.5$, or when the firm's predicted spread has fallen by 50% or more. These are

[14]We consider only on repeat loans within four years after their immediate predecessors. This is because, as we will show in Section 5.2.1, the power of anchoring weakens as the time between loans increases. Since the time between loans is clearly higher when a firm's fundamentals have changed more, we must select only loans with a recent predecessor to reduce confounding.

infrequent events, but among this sample, past borrowing costs still matter considerably for current spreads. A nearly identical effect is seen in the second column, where we restrict attention to observations where borrowing costs have decreased, but less dramatically. The third and fourth columns correspond to increases in spreads. The third column shows a strong anchoring effect, however the effect is nonexistent in the rightmost column. Panel B of Table 4 presents a similar picture, except that we have segregated the sample based on credit rating changes rather than on innovations in p. The results are similar. When a firm's condition has deteriorated substantially, anchoring appears less prominant, but in all other cases, anchoring is approximately equally powerful. Because the sample is dominated by the latter cases, our symmetry assunmption appears valid.

Thus far, we have found evidence of anchoring in our sample and that its effect is largely symmetric. In the following three sections we consider cross-sectional predictions. In particular, anchoring should be strongest when the reference point is most salient (e.g., Kahneman [1992]). We investigate this prediction with three measures of salience: whether the prior loan is recent, whether the lead bank is the same for both loans (because the same bankers are likely to have negotiated both the current and previous loan terms) and whether the CEO and CFO of the borrower are the same for both loans.

5.2.1 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. Table 5 tests for this directly, breaking up the sample of repeat deals into those where the most recent deal was less than one year prior (column one), between one and two years prior (column two), and so forth. The last column shows deals whose most recent predecessor is more than five years in the past.

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 0.29 (bootstrapped t statistic = 8.53), approaching twice the magnitude observed for the overall sample. Advancing forward a year (next column), we see the effect cut by over a third, to 0.18 (bootstrapped t statistic = 5.88). The effect continues to decrease at 2–3 and 3–4 years, and it becomes only marginally significant following the end of the fourth year. Deals five years or beyond appear to have no impact on the current borrowing cost.[15]

It is also worth noting that the explanatory power of the residual from past deals (*Quality*)

[15]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.

also drops over time. As discussed above, this is consistent with the idea that these residuals may capture private information exchanged during lending negotiations, but that eventually this information becomes reflected in observable characteristics. At short horizons, roughly 20 percent of the residual from a past deal makes its way into future transactions, but after three years, this fades as well.

5.2.2 Same vs. Different Lead Bank

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: 1) the borrower transacts with the same lead bank, and 2) the borrower transacts with a different lead bank.

Table 6 shows the results for this segregation, which highlights sharp differences. Starting with short-term lines of credit, we see that the anchoring term has a magnitude of 0.62, meaning that reference points prevent the *majority* of new information about borrowing costs from being incorporated into current spreads. Indeed, the anchor term by itself increases the explanatory power of the regression by 7%, relative to the other variables. The second column shows that although the effects of anchoring still appear relevant when the lead bank has changed, they are much smaller.

The same comparison for loans of longer duration is shown in columns three through six. For long-term lines of credit, the coefficient on the *Anchor* variable is 0.22 when the lead arranger is the same, and 0.11 when it is different. Similarly, for term loans, the coefficient on the *Anchor* variable is 0.34 when the lead arranger is the same, and 0.12 when it is different.

5.2.3 Same vs. Different CEO/CFO

Using the same argument, one might expect for changes in the firm's top management to influence the relevance of past deal terms as they relate to current ones. However, because the probability that a CEO or CFO is replaced increases with time, and we already know that longer time between successive deals reduces the salience of reference points, the analysis in Table 7 focuses only on repeat deals completed four years after the reference transaction. This breakpoint is arbitrary, but similar results are found if we move the date forward or backward one or two years. Because changes in top management are not particularly common at four years, occurring in approximately 20 percent of the cases for both CEOs and CFOs, the results are presented for all deals together, rather than broken up separately by tranche type.

Comparing the magnitude on the *Anchor* coefficient between the first two columns, we see that it is almost twice as high for instances where the CEO is unchanged. Evidence for CFOs is substantially weaker, although the difference in point estimate goes in the same direction.

These results confirm that the anchor is more powerful when it is more salient. The next section analyzes whether anchoring is large in dollar terms, and how the loan mispricing varies over time.

5.2.4 Discussion

With $\lambda > 0$ (anchoring), loans will be either too cheap or too expensive, but in either case mispriced. Moreover, because anchoring can be identified only from time series changes in p_t, it is clear that increases in borrowing costs ($s_{t-} - p_t < 0$) imply loans that are too cheap, whereas decreases ($s_{t-} - p_t > 0$) imply the opposite. In this section, we focus on one particular source of variation in p_t, changes in aggregate credit spreads, and ask how this affects anchor-induced loan mispricing.

Figure 3 shows graphically the results of an exercise meant to illustrate this relation. First, in each panel of Figure 3, we plot the average spread averaged across all repeat loan observations by year. Spreads were low and stable through the mid 1990s, and then widened considerably beginning with the Russian default and LTCM crisis in 1998. Spreads then narrowed from 2002 until 2006, where they began to increase again.

The idea underlying Figure 3 is that when aggregate spreads widen, for example, the typical firm's expected borrowing cost has increased, which would predict underpricing.[16] To estimate the average mispricing for each year, we calculate the mispricing of loan i at time t, $m_{i,t}$, as the the realized spread minus the counterfactual spread implied by anchoring as estimated over our entire sample. That is,

$$m_{i,t} = \exp(s_{i,t}) - \exp\left(s_{i,t} - \lambda \cdot (s_{i,t-} - p_{i,t})\right), \tag{4}$$

with $\lambda = 0.16$, as estimated in column one of Table 3. The plots in Figure 3 simply capture time series variation in the anchor term, which we compare to aggregate spreads for reference.

Panel A of Table 3 plots the yearly average mispricing per loan in basis points (i.e., the average value of $|m_{i,t}|$ by year.) We see that the absolute level of mispricing is strongly

[16] Of course, this does not necessarily have to be the case, as p_t is sensitive to both firm and market variables.

and positively correlated with aggregate spreads. The typical loan is mispriced by about 16 basis points at the beginning of the cycle, dipping to almost 10 basis points, and then rising to a maximum of 20 basis points in 2002. While it is easy to understand why mispricing would be high after a fierce run-up in spreads (e.g., 2002), it is not obvious why the effect would not be symmetric. One might imagine, for example, that although loans would be mispriced negatively after periods of rising spreads, that they would be mispriced positively after drop-offs in aggregate spreads. Together, this would imply that past spread *trends*, not contemporaneous *levels* (as we see) would be correlated with aggregate mispricing.

Panels B and C of Figure 3 break up loans into ones with positive ($m_{i,t} > 0$) and negative ($m_{i,t} < 0$) realizations of the anchor term, respectively. Consistent with Panel A, Panel B shows that *conditional on the anchor term being positive*, loans are more underpriced when spreads are high. Specifically, in the mid 1990s when spreads were low and stable, anchor-induced average mispricing is in the neighborhood of -10 basis points. However, as spreads widened through 2002, the average loan in this group was underpriced by over 22 basis points, approaching ten percent of the average spread. It is as though firms that borrowed in low spread regimes (e.g., 1996) have created an asset—an anchor—that pays off when the reference point it establishes is quite valuable (e.g., 2001).

The same argument would appear to apply to firms with negative realizations of the anchor term, so that past reference points would imply positively mispriced current loans. However, Panel C shows that this intuition is not confirmed in the data. Instead, we see that falling rates are associated with *less* positive mispricing, although the effect is much weaker. In any case, the net effect is dominated by those observations where the firm benefits (i.e., spreads are underpriced), which is responsible for the aggregate mispricing pattern seen in Panel A of Figure 3.

Figure 4 converts these percentage calculations to dollar amounts. For each year, we conduct two related calculations. The first is to simply multiply the amount of the anchoring (in bps) by the average size of each loan (i.e., $\frac{|m_{i,t}|}{10{,}000} \times \textit{tranche amount}$), as a rough estimate of the annual, mispriced interest flow. This will thus then depend both on the size of the average loan, as well as the amount by which the interest rate is distorted. The blue bars show that generally, mispriced interest flow increases over our sample, mostly a reflection of average deal size increasing. By the end of the sample, this figure approaches \$500,000 per loan.

Perhaps a more meaningful calculation is how these annual amounts convert to present values. Shown in the same figure are approximations for the present values of these annual amounts (black bars).[17] As we see, in the typical case, the present value of anchoring effects

[17]The present value of mispricing for a loan is approximated by discounting the loan's dollar amount of

is around $1 million, growing toward the end of the sample. Whether this effect is big or small depends largely on the comparison. Against the typical firm's total assets, it is usually small. However, compared to figures such as CEO compensation, annual costs (or benefits) are comparable.

6 Alternative Explanations for the Results

The evidence presented thus far supports the claim that anchoring is at work when lenders and firms negotiate loan terms. We have presented two types of evidence. First, Figures 1 and 2 provide distributional evidence that firms and banks have a strong tendency to leave interest spreads unchanged, even when credit conditions change substantially. Second, Tables 3 through 9 establish the anchoring effect more broadly: the previous spread is a point to which the new spread is drawn. For each piece of evidence, there are alternative explanations which we consider below.

6.1 Renegotiation Costs

Figure 1 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 this case, the regression results in Table 3 would 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. In Table 8 we reproduce Table 3 with all observations removed in which the time t and time $t-$ spreads are identical. That is, we only include observations in which there is renegotiation. The anchoring effect is still present. While costly renegotiation may be at play, it is at best a second-order effect.

6.2 Differential Impact of a Credit Market Shock

Figure 1 also is consistent with some firms being affected by the Financial Crisis and others not. Suppose that some firms are affected by the crisis, and the increase in credit risk for

mispricing annually for the life of the loan using a fixed 5% discount rate.

firms in this group is widely distributed. Also suppose that some firms are not affected, and their increase in credit risk is tightly distributed about 0%. Then we would expect a distribution for the population as a whole quite similar to the one shown in Figure 2. While this is indeed an alternative explanation for Figure 1, it is not consistent with our regression results. First, our *Quality* variable in each regression would absorb unobserved quality differences regardless of the distribution of spread changes. The fact that the change in credit spreads is not normally distributed is irrelevant. Second, as previously discussed, Table 8 repeats our baseline analysis with observations removed if they feature no change in credit spread. This limits the regressions to the affected group, and results are little changed.

6.3 Rounding/Increments

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. Then small changes in the "correct" spread for the firm could lead to no change in the loan terms.[18] 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 $t-$ 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 t 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.[19] 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.

Figure 5 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.

In Panel A, the rounding is greater in period $t-$ than period t. Our empirical specification will include this rounding as "unobserved quality": the firm is getting a lower interest

[18] 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.

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

rate, due to rounding, than one would expect just based on fundamentals observable to the econometrician. The amount of rounding is lower in period t, but because the previous rounding is included as unobserved quality, our error is more positive by an amount equal to the difference. In panel B the timing is reversed, so the error is more negative by an amount equal to the difference. There is no reason we would expect more rounding in the earlier or later period, so these biases should cancel on average.

In Panel C, the spread is rounded up to 3/8 in period t and rounded down to 3/8 in period $t-$. As before, rounding down in period $t-$ is interpreted as unobserved quality. Since rounding up takes place in period t, the error is more positive by an amount equal to the sum of the absolute values of the rounding. In panel D the timing is reversed, so the error is more negative by an amount equal to the sum of the absolute values of the rounding. There is no reason we would expect the direction of rounding to switch up to down or down to up on average, so these biases should cancel on average.

Since there is no bias on average, if there is an effect on the coefficient of the anchoring term, it must be because the error term is correlated with the anchoring term. That is, the difference between predicted spread at time t and spread at time $t-$ would have to be larger when: 1) there was rounding down at times t and $t-$ but the rounding was smaller at time t, 2) there was rounding up at times t and $t-$, but the rounding was larger at time t, and 3) there was rounding down in period $t-$ and up in period t. This seems arbitrary and unlikely.

To be sure that rounding does not affect our results, we again refer to Table 8. By removing outcomes in which the spread is unchanged from period $t-$ 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.

6.4 Smoothing of Terms in the Lending Relationship

One could imagine that a banker and CEO/CFO may believe that they have a long-term relationship in which it is in the interest of both parties to reduce the variability of credit spreads. That is, there may be a *quid pro quo* in which the lender offers lower spreads when market spreads are high and, in return, the firm pays higher spreads when market spreads are low. On average spreads are correct, but the variability is reduced, reducing risk for both parties. This is consistent with results in Table 7, which indicate that when the relationship between banker and CEO/CFO is severed, the anchoring effect is reduced or eliminated. This argument has some theoretical flaws, not least of which is that other banks in a lending syndicate would not be happy to go along with the arrangement, but we offer an alternative test in order to empirically reject it.

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. Table 6 shows that the anchoring effect is present even when the lead arranger in the syndicate changes. The effect is weaker, as anchoring theory says it should be, but it is still present.

6.5 Endogeneity of Timing

Throughout our analysis, we have implicitly assumed that after controlling for p_t and $(s_{t-} - p_{t-})$, changes in a firm's borrowing cost (relative to its historical costs) do not provide additional about its current risk. In other words, the fact that, e.g., credit market conditions 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 $(s_{t-} - p_t)$ as anchoring.

As a simple example, divide the world into two credit spread regimes: high and low. Suppose that firms differ in how dependent they are on debt finance, with low quality firms being forced to access credit markets in both high and low times, but high quality firms only raising capital when it is comparatively cheap. When firm quality is not directly observable, it is clear that *when* a firm chooses to raise debt provides information about its risk. Even when other observable characteristics are informative about borrowing costs, it is clear that borrowing histories will matter, but not for anchoring reasons. To this point, our regressions have ignored this issue altogether, having taken historical credit market conditions as exogenous.

Fortunately, this assumption is innocuous. Table 9 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, Table 9 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 would have been maturing in 2002, requiring it to refinance. Here, we assume that when IBM originally borrowed in 1997, it could not foresee credit market conditions in 2002, and thus, takes the borrowing environment at this time as 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.

Table 9 shows the results. If we restrict the sample to observations where the "forced refinancing" occurs in the same year as the maturity date of the previous loan, the anchoring coefficient is 0.22, compared to 0.16 for the full sample. If anything, the results are stronger among the sample for which financing choices are more exogenous. More precise identification is shown in the final column, where we examine the 141 loans where refinancing and maturity are matched by both month and year. Here, it is almost a certainty that refinancing is exogenous with respect to prevailing market conditions. Among this smaller sample, the anchoring effect is significantly increased, with a point estimate of 0.28 (bootstrapped t statistic = 2.84).

7 Conclusion

We provide evidence that the interest rates paid by firms with syndicated bank loans are strongly influenced by the rates they have paid in the past. We begin by pointing out a curious number of repeat bank loans which have rate terms that are identical to the prior loan, even when credit market conditions have drastically changed. We then formally disentangle the anchoring effects from other effects using the regression methodology of Genesove and Mayer (2001) and Beggs and Graddy (2009). The anchoring effects we find using this approach are sizable, particularly for short-term lines of credit.

We also find strong evidence of anchoring in the cross-section. Prior research suggests that anchoring will be stronger when the anchor (here, the spread from a prior deal) is more salient. Consistent with this intuition, we find that anchoring is stronger when the prior deal is more recent, when the lead bank is the same, and when the firm's CEO and CFO are the same. These cross-sectional tests also rule out a number of alternative explanations of our evidence.

Taken together, the results are surprising for two reasons. First, nearly all theories of corporate finance require a firm's cost of capital to be forward-looking: interest rates on new loans should depend upon the riskiness of the firm and the market-price of that risk. Because risk is determined by states of the world that will occur in the future, there is no role for historical variables. And yet this is precisely what we find: historical interest rates matter for current pricing.

Second, many have criticized behavioral economics because some of its strongest results are found in small-scale experiments. That anchoring is relevant for human decision-making has been known for some time, but the degree to which market pressure or large stakes dampen the effect has not been determined. Our work suggests that even when the stakes are high, and when there is significant competition from both the firm's and lender's sides, anchoring can affect critical drivers of profitability like interest spreads.

References

Babcock, Linda, Xianghong Wang, and George Loewenstein. "Choosing the wrong pond: Social comparisons in negotiations that reflect a self-serving bias." *Quarterly Journal of Economics* 116: (1996) 1–19.

Baker, Malcolm, and Yuhai Xuan. "Under new management: Equity issues and the attribution of past returns." *Harvard Business School Working Paper* .

Baker, Malcolm P., Xin Pan, and Jeffrey A. Wurgler. "A Reference Point Theory of Mergers and Acquisitions." *Working Paper* .

Barberis, Nicholas, Ming Huang, and Tano Santos. "Prospect theory and asset prices." *Quarterly Journal of Economics* 116: (2001) 1–53.

Becker, Gary. Quoted in "NOVA: Mind over money." PBS, 2010.

Beggs, Alan, and Kathryn Graddy. "Anchoring Effects: Evidence from Art Auctions." *American Economic Review* 99: (2009) 1027–39.

Bollen, Nicolas, and Veronika Pool. "Do hedge fund managers misreport returns? Evidence from the pooled distribtion." *Journal of Finance* 64: (2009) 2257–2287.

Camerer, Colin F., George Loewenstein, and Matthew Rabin, editors. *Advances in Behavioral Economics*. Princeton University Press, 2003.

Chen, M. Keith, Venkat Lakshminarayanan, and Laurie R. Santos. "How Basic Are Behavioral Biases? Evidence from Capuchin Monkey Trading Behavior." *Journal of Political Economy* 114: (2006) 517–537.

Cochrane, John. Quoted in "NOVA: Mind over money." PBS, 2010.

Daniel Dorn, Günter Strobl. "Rational Disposition Effects: Theory and Evidence." *Working Paper* .

Dhar, Ravi, and Ning Zhu. "Up Close and Personal: An Individual Level Analysis of the Disposition Effect." *Management Science* 52: (2006) 726–740.

Freedman, David. "On bootstrapping two-stage least-squares estimates in stationary linear models." *The Annals of Statistics* 12: (1984) 827–842.

Genesove, David, and Christopher Mayer. "Loss aversion and seller behavior: Evidence from the housing market." *Quarterly Journal of Economics* 116: (2001) 1233–1260.

Greenleaf, Eric A. "The Impact of Reference Price Effects on the Profitability of Price Promotions." *Marketing Science* 14: (1995) 82–104.

Grinblatt, Mark, and Matti Keloharju. "What makes investors trade?" *The Journal of Finance* 56: (2001) 589–616.

Kahneman, Daniel. "Reference points, anchors, norms, and mixed feelings." *Organizational Behavior and Human Decision Processes* 51: (1992) 296–312.

Kahneman, Daniel, and Amos Tversky. "Prospect theory: An analysis of decision under risk." *Econometrica* 47: (1979) 263–291.

Northcraft, Gregory B., and Margaret Ann Neale. "Experts, amateurs, and real estate: An anchoring-and-adjustment perpective on property pricing decisions." *Organizational Behavior and Human Decision Processes* 39: (1987) 228–241.

Odean, Terrance. "Are investors reluctant to realize their losses?" *Journal of Finance* 53: (1998) 1775–1798.

Rajendran, K. N., and Gerard J. Tellis. "Is Reference Price Based on Context or Experience? An Analysis of Consumers' Brand Choices." *Journal of Marketing* 58: (1994) 10–22.

Rubinstein, Ariel. *Advances in Economics and Econometrics: Volume 2: Theory and Applications, Ninth World Congress*, Cambridge University Press, 2006, chapter Discussion of "Behavioral Economics".

Shapira, Zur, and Itzhak Venezia. "Patterns of Behavior of Professionally Managed and Independent Investors." *Journal of Banking and Finance* 25: (2001) 1573–87.

Shefrin, Hersh, and Meir Statman. "The disposition to sell winners too early and ride losers too long: Theory and evidence." *Journal of Finance* 40: (1985) 777–90.

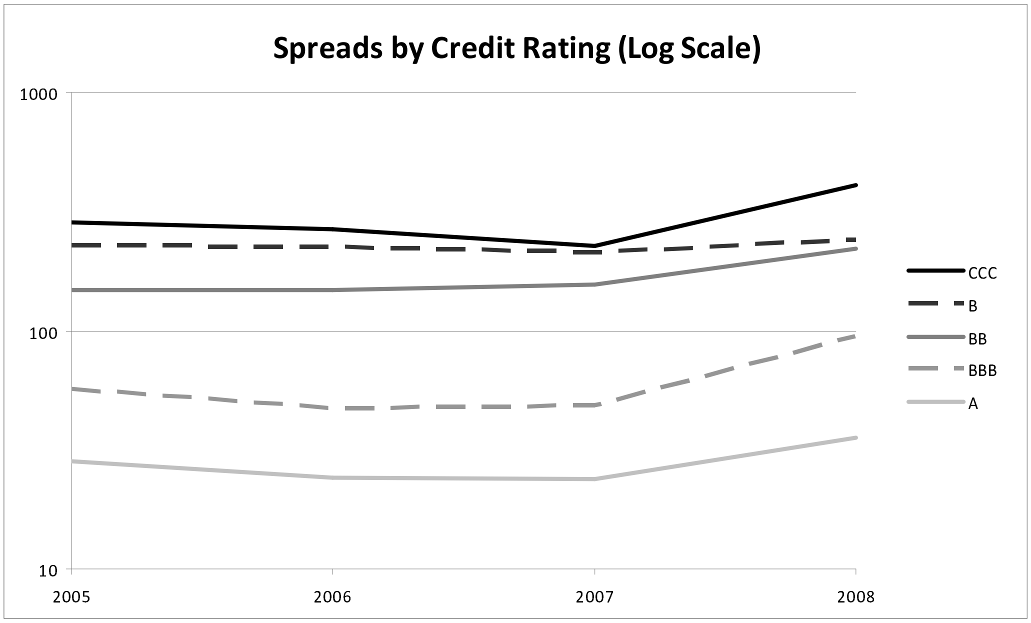
Silverman, Bernard W. *Density estimation for statistics and data analysis*. Chapman Hall, 1986.

Tversky, Amos, and Daniel Kahneman. "Judgement under uncertainty: Heuristics and biases." *Science* 185: (1974) 1124–1130.

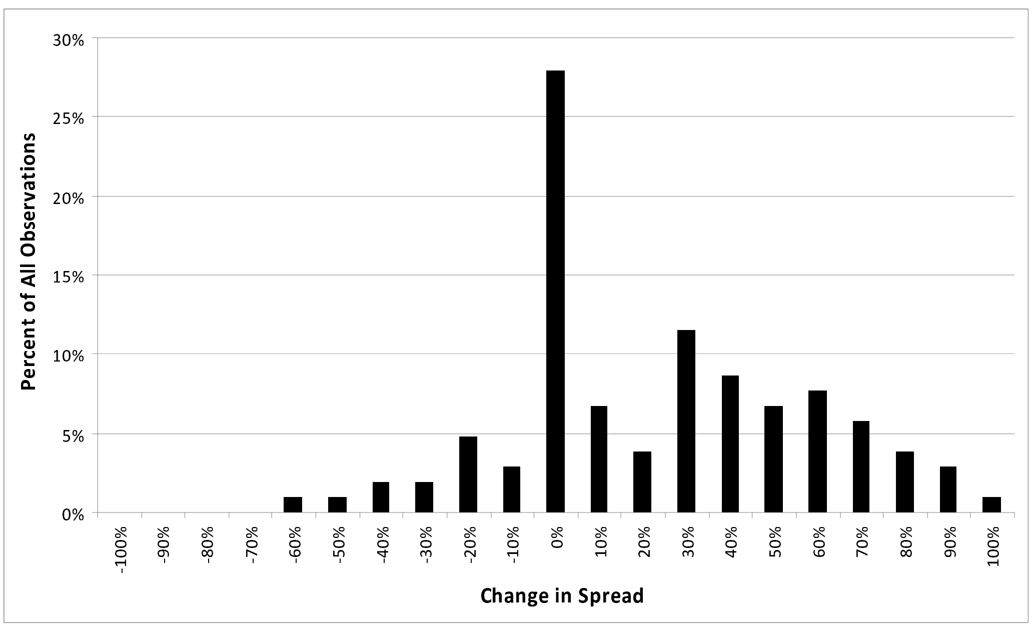
Wurgler, Malcolm Baker Jeffery. "Market timing and capital structure." *Journal of Finance* 57: (2002) 1–32.

Figure 1: Financial crisis of 2008

Panel A: Average spread by debt rating

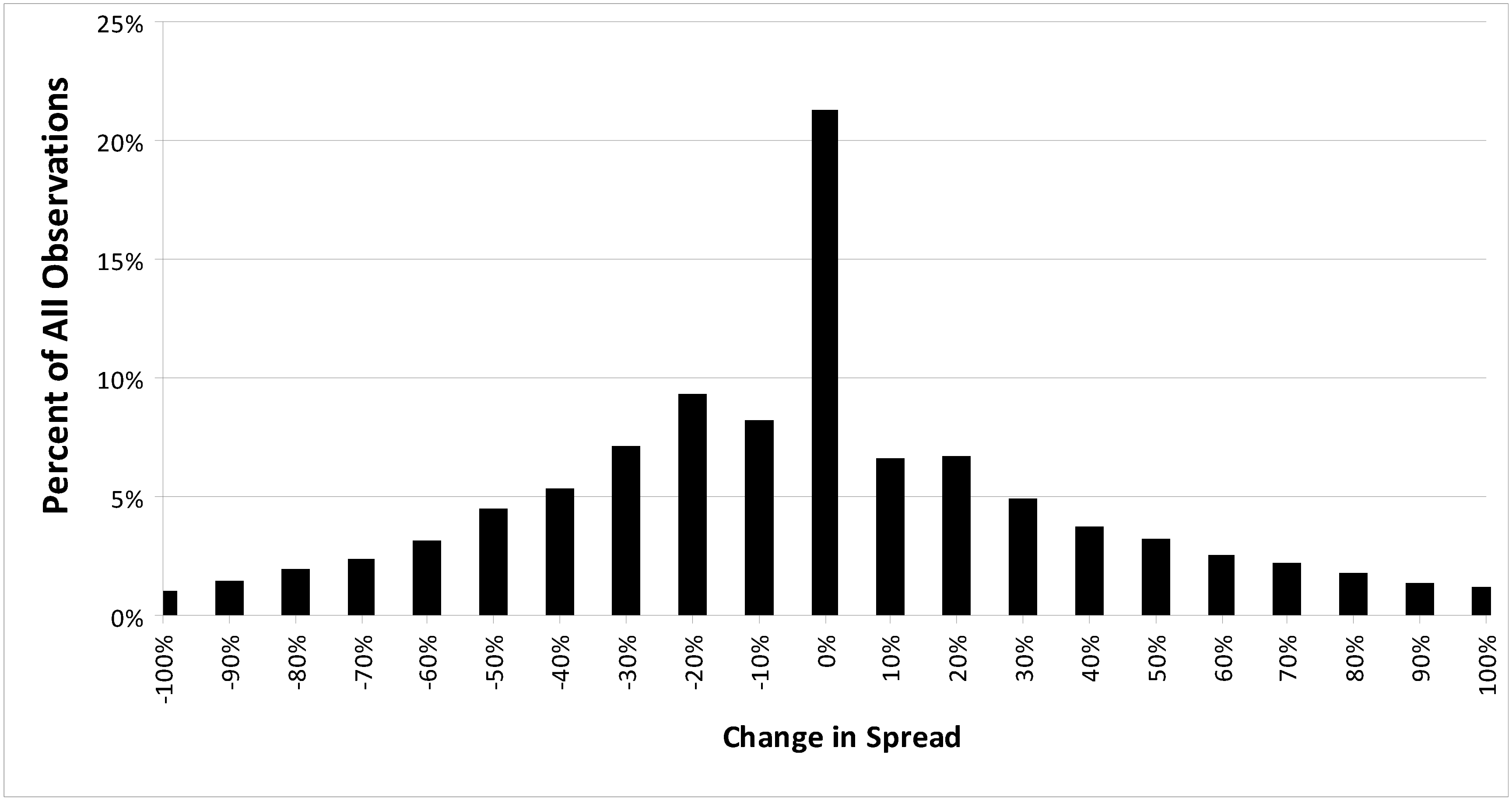


Panel B: Histogram of spread changes



Panel A plots the average spread above LIBOR for long-term lines of credit by firm debt rating 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. To keep the comparison as clean as possible, we consider 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.

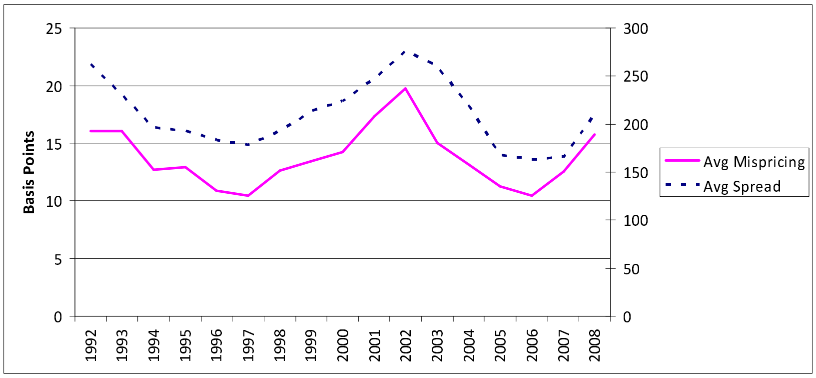
Figure 2: Histogram of log(spread) differences



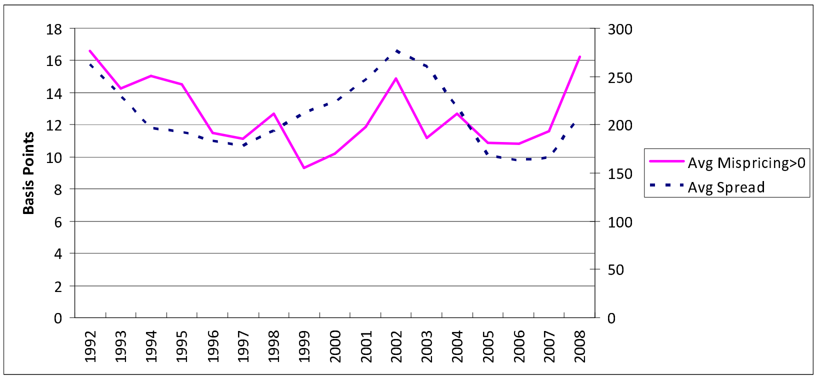
This figure plots the empirical distribution of $\Delta \log(spread_{i,t}) = \log(spread_{i,t}) - \log(spread_{i,t-})$ for all repeat loan transactions in DealScan.

Figure 3: Average mispricing per loan in basis points

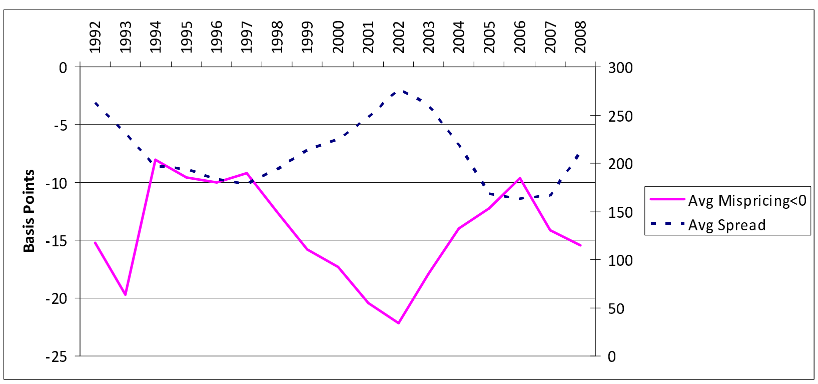
Panel A: All loans



Panel B: Positive mispricing

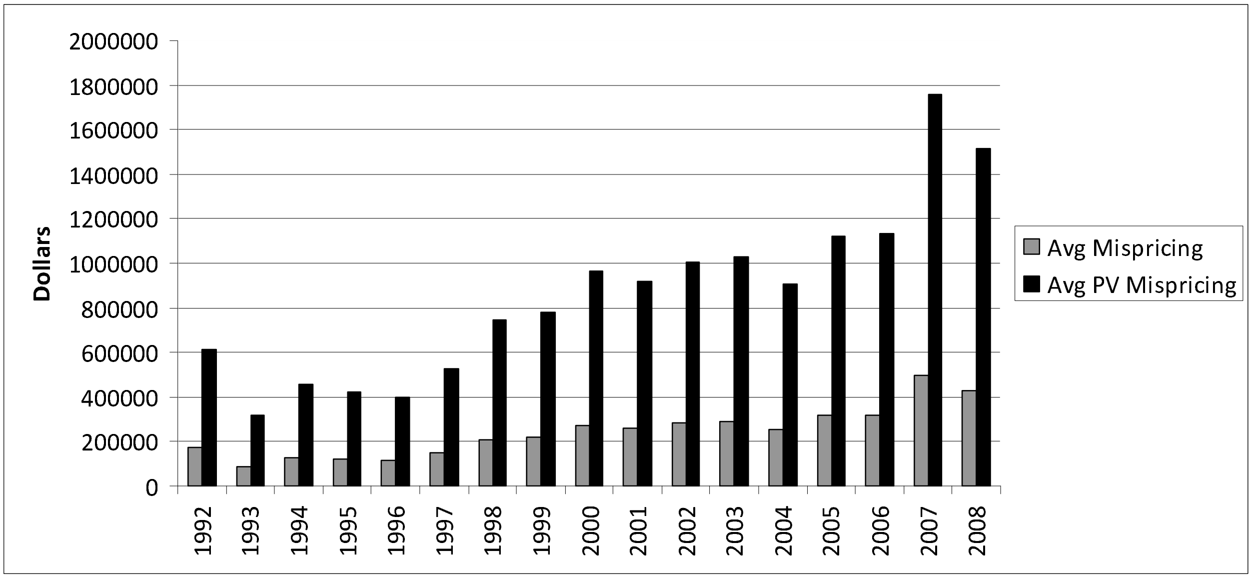


Panel C: Negative mispricing



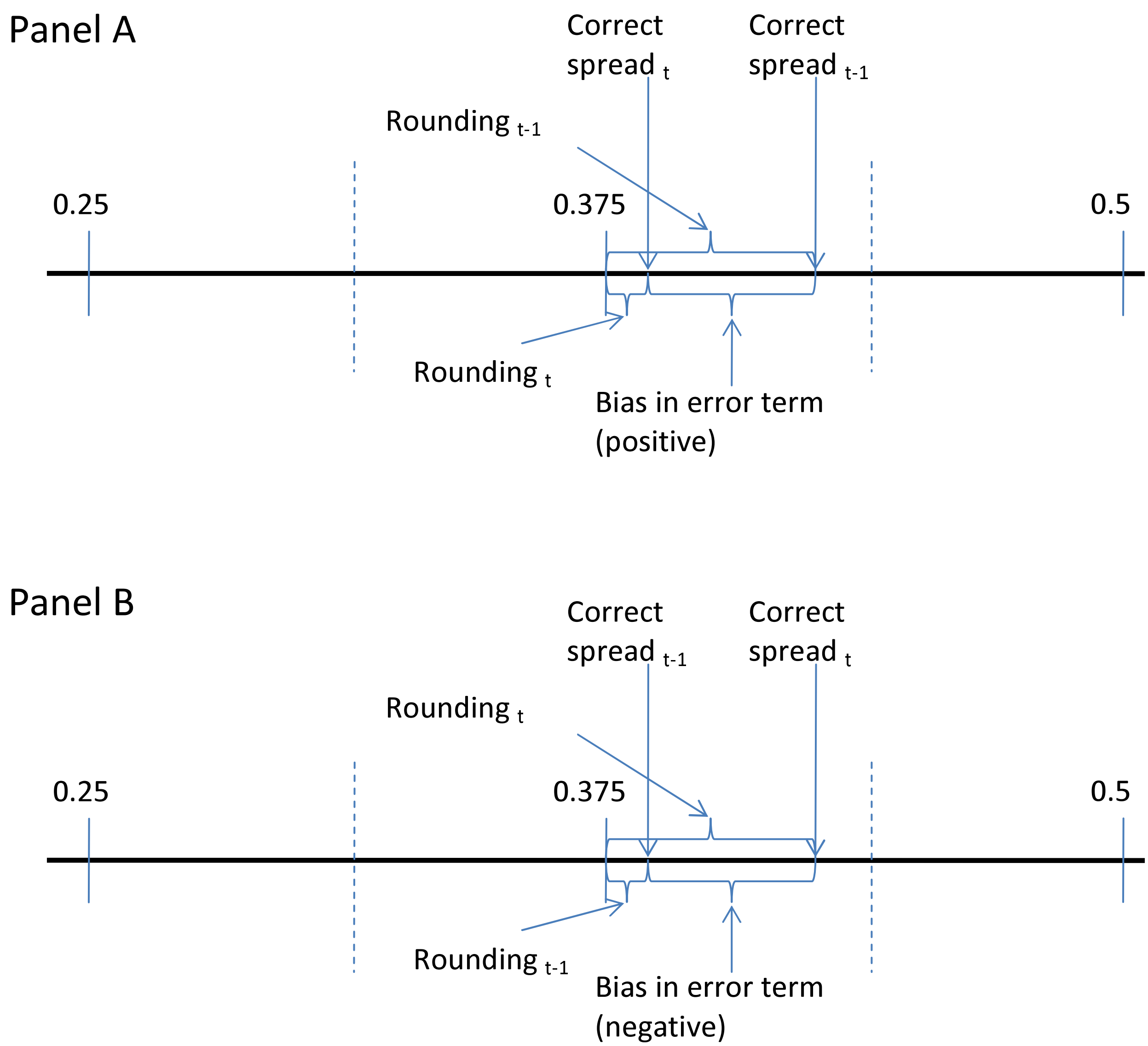
This figure plots the average loan mispricing by year for all repeat loans in our sample (left scale). Panel A plots the average absolute value of mispricing per loan, by year. Panel B plots the average only for loans that are positively mispriced, and Panel C plots the average only for loans that are negatively mispriced. Additionally, in each panel the average spread for all repeat loans is also plotted for comparison (right scale).

Figure 4: Average mispricing per loan in dollars



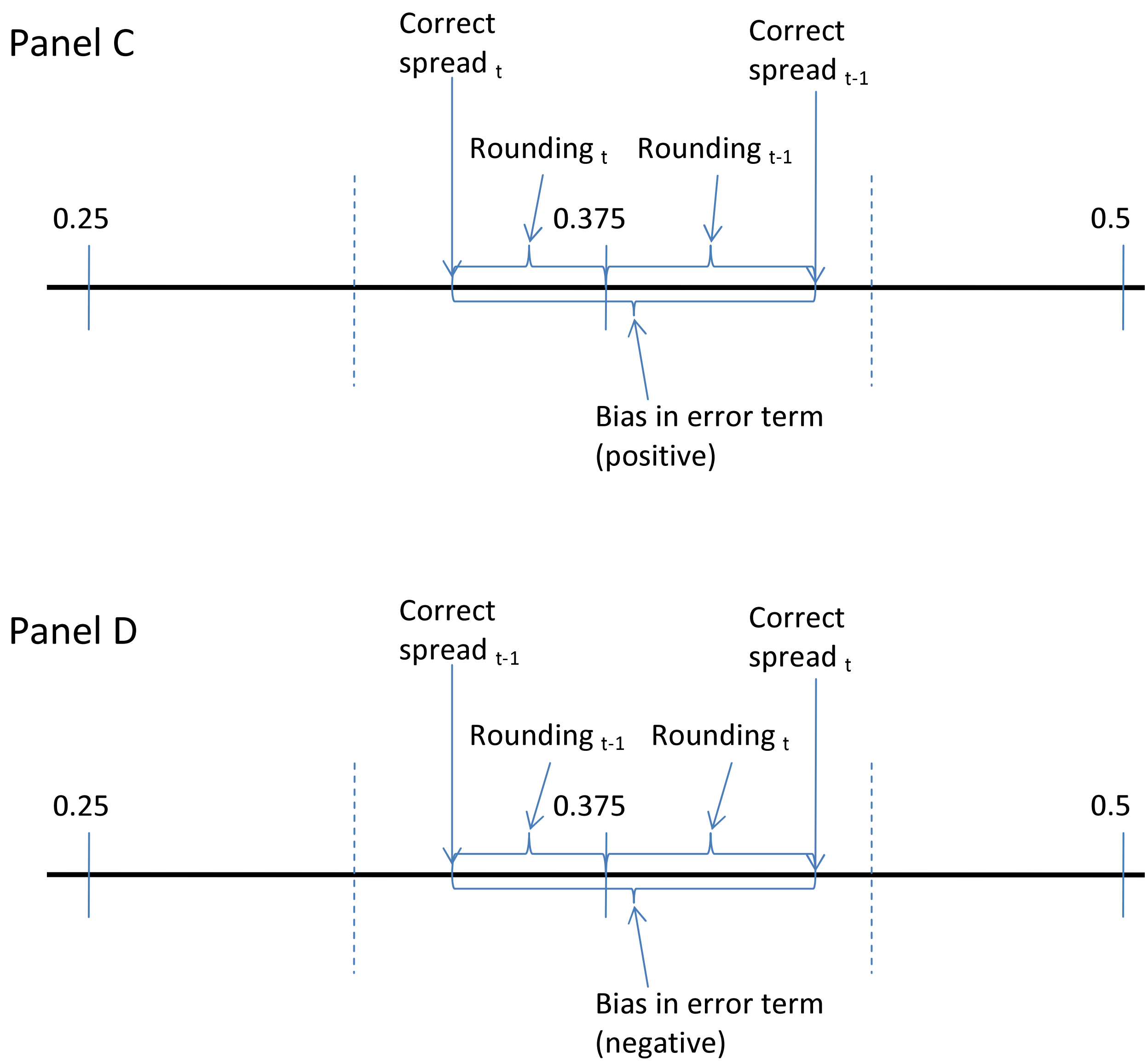
This figure plots the average dollar amount of loan mispricing per loan for all repeat loans in our sample (grey bars), and the average present value of loan mispricing per loan (black bars).

Figure 5: 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 $t-$, but the rounding was larger in period $t-$. 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.

Figure 5: Examples of rounding bias - cont'd



Panel C shows the case where the direction of rounding changes from "down" to "up" from period $t-$ 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 $t-$. The error term will be positively biased. Panel D shows the reverse case, in which case the error term is negatively biased.

Table 1: Summary statistics

This table provides summary statics for the master sample (Panel A), the sample of repeat loans (i.e, observations in which there is a prior observation by the same firm of the same loan type, Panel B) and repeat loan innovations (i.e., variable differences between loans at time t and $t-$, Panel C). Statistics presented include the number of observations (N), the *Mean*, the standard deviation (SD), the *Mode*, and the 10^{th}, 25^{th}, 50^{th}, 75^{th}, and 90^{th} percentiles. *Sales* is the borrower's sales in millions of US dollars recorded at the time of loan origination. *Assets* is the borrower's total assets in millions of US dollars recorded at fiscal year-end previous to the time of loan origination. *Leverage* is calculated as the book value of debt (total liabilities+preferred stock-convertible debt) to total assets; and profitability (ROA) is calculated as operating income before depreciation to total assets. Both variables are calculated using data recorded at the fiscal year-end previous to the time of loan origination. Both leverage and ROA are industry-median adjusted. *Tranche Amount* is the size of the tranche recorded in millions of US dollars. *Spread* is the all-in-drawn spread, i.e. the margin paid over LIBOR net of upfront fees.

	N	Mean	SD	Mode	10^{th}	25^{th}	50^{th}	75^{th}	90^{th}
Panel A: Master sample									
Sales	19578	1810	5500	1000	32	96	332	1170	3900
Assets	19578	2509	13170	209	29	92	338	1300	4404
Leverage	19578	0.12	1.96	0.16	-0.24	-0.11	0.04	0.22	0.44
ROA	19578	-0.01	0.96	0	-0.1	-0.04	0	0.06	0.12
Tranche Amount	19578	228	609	100	5	20	75	225	500
Spread	19578	226	171	250	50	125	225	300	400
Panel B: Sample of repeat loans									
Sales	7310	2150	5950	1000	62	166	499	1560	4790
Assets	7310	2749	11975	209	63	170	535	1705	5563
Leverage	7310	0.11	0.35	0.16	-0.21	-0.08	0.05	0.21	0.43
ROA	7310	0.01	0.11	0	-0.08	-0.03	0	0.05	0.11
Tranche Amount	7310	269	589	100	12	35	100	300	600
Spread	7310	208	147	250	48	110	200	275	355
Panel C: Repeat loan innovations									
Sales	7310	331	3370	0	-103	0	23	210	888
Assets	7310	289	5139	0	-85	0	39	224	831
Leverage	7310	-0.01	1.86	0	-0.21	-0.06	0	0.08	0.22
ROA	7310	0	0.15	0	-0.09	-0.03	0	0.02	0.07
Tranche Amount	7310	37	462	0	-100	-9	5	60	200
Spread	7310	-3	133	0	-125	-50	0	38	125

Table 2: First-stage regression estimates

This table displays results for first-stage predictive regressions, shown in Equation (1). First-stage regressions are run year by year from 1987-2008, excluding observations of the same firm in the same year. This table presents sample statistics for the coefficient estimates (Panel A), t statistics (Panel B), and *Observations* per regression and *Adjusted* R^2 (Panel C). Statistics presented include the number of observations (N), the *Mean*, the standard deviation (SD), and the 10^{th}, 25^{th}, 50^{th}, 75^{th}, and 90^{th} percentiles. *Commercial Paper Rating* is a dummy variable that equals one if the firm has a commercial paper rating, and zero otherwise. *Public* is a dummy variable that equals one if the firm is publicly traded, and zero otherwise. *Log(Sales)* is the log of *Sales*. *Log(Assets)* is the log of *Assets*. *Leverage* and *ROA* are the industry-median-adjusted leverage and profitability of the borrower. *Log(Tranche Amount)* is the log of the tranche size. *Maturity* is the maturity of the loan measured in months divided by 100 (for readability). *# of Lenders* is the number of members of the loan syndicate divided by 100 (for readability). *Collateral* is a dummy that equals one if the loan is secured, and zero if the loan is unsecured. *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. Additional control variables used in our first-stage regressions not shown in Table 2 include a set of dummy variables for firm S&P long-term debt rating, a set of dummy variables for loan type, and and a set of dummy variables for loan purpose.

	Mean	SD	10^{th}	25^{th}	50^{th}	75^{th}	90^{th}
Panel A: Coefficients							
Commercial Paper Rating	-0.05	0.14	-0.18	-0.15	-0.03	0.06	0.11
Public	-0.03	0.08	-0.12	-0.10	-0.02	0.01	0.06
Log(Sales)	-0.04	0.03	-0.07	-0.05	-0.04	-0.02	0.00
Log(Assets)	-0.01	0.03	-0.05	-0.02	-0.01	0.01	0.02
Leverage	0.13	0.10	0.03	0.06	0.10	0.20	0.25
ROA	-0.34	0.22	-0.56	-0.50	-0.36	-0.17	0.00
Log(Tranche Amount)	-0.08	0.02	-0.10	-0.10	-0.08	-0.06	-0.05
Maturity	-0.19	0.15	-0.38	-0.30	-0.20	-0.06	-0.02
# of Lenders	0.04	0.34	-0.26	-0.19	0.11	0.31	0.41
Collateral	0.53	0.11	0.40	0.48	0.50	0.58	0.65
Covenants	-0.03	0.14	-0.15	-0.07	0.00	0.03	0.06
Performance Pricing	-0.12	0.11	-0.22	-0.18	-0.12	-0.09	-0.05
Panel B: t statistics							
Commercial Paper Rating	-0.35	1.54	-2.72	-1.60	-0.36	0.48	2.06
Public	-0.39	1.13	-1.88	-1.23	-0.16	0.26	1.07
Log(Sales)	-2.39	1.84	-5.47	-3.29	-2.49	-1.18	0.10
Log(Assets)	-0.70	1.39	-2.75	-1.36	-0.92	0.49	1.47
Leverage	3.03	1.92	0.83	1.71	2.70	4.25	5.78
ROA	-2.41	2.31	-4.53	-3.66	-2.44	-1.90	0.06
Log(Tranche Amount)	-4.77	1.73	-7.56	-5.70	-4.49	-3.72	-2.37
Maturity	-2.15	1.67	-4.11	-3.88	-2.16	-0.54	-0.22
# of Lenders	0.25	1.44	-1.42	-1.17	0.50	1.16	1.83
Collateral	10.70	3.17	7.07	8.23	10.53	12.04	15.67
Covenants	-0.48	1.41	-2.97	-0.92	-0.06	0.38	1.18
Performance Pricing	-3.76	1.86	-6.28	-5.10	-3.77	-2.40	-1.17
Panel C: Other							
Observations	1079	346	503	861	1178	1259	1329
Adjusted R^2	0.68	0.09	0.50	0.67	0.71	0.73	0.76

Table 3: Second-stage regression by tranche type

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, including *Short-term* lines of credit (tranches labeled "364-day facilities" and "Revolver <1 year"); *Long-term* lines of credit (tranches labeled "Revolver ≥ 1 year" and "Term/Revolver"); and *Term loans* (tranches labeled "Term Loan", "Term Loan A" and "Term Loan B"). 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. *Anchor* represents the coefficient on the difference between the spread at time $t-$ and the predicted value for the spread at time t. *Quality* represents the residual value, or unobserved quality, from the first-stage regression for the loan at time $t-$. *Observations* is the number of observations per regression.

	(1) All Log(Spread)	(2) Short-term Log(Spread)	(3) Long-term Log(Spread)	(4) Term Loan Log(Spread)
Predicted	0.99***	1.00***	1.00***	0.98***
	(139.03)	(52.35)	(107.72)	(27.48)
Anchor	0.16***	0.43***	0.14***	0.17***
	(10.66)	(7.02)	(8.16)	(4.67)
Quality	0.18***	0.13**	0.16***	0.22***
	(10.01)	(2.20)	(7.57)	(5.27)
Constant	0.04	0.03	0.02	0.07
	(1.15)	(0.43)	(0.43)	(0.36)
Observations	7310	450	5332	1528
R^2	0.732	0.824	0.712	0.486

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Second-stage regression sorted on borrowing costs

This table shows second-stage regression estimates for repeat loans sorted on changes in firm borrowing costs due to changes in predicted spreads (Panel A) or changes in firm debt ratings (Panel B). For both panels, borrowing costs are increasing moving rightwards across columns. Panel A sorts repeat loans according to the difference in the predicted log(*spread*) from time t to time $t-$. Column one reports estimates for the sample of repeat loans where predicted spreads have decreased more than 50%, column two reports results where predicted spreads have decreased by less than 50%, and so on. Column one of Panel B shows second-stage regression results for firms whose debt rating has increased more than one notch since their previous loan. Similarly, column two shows results for firms whose rating has increased exactly one notch, column three shows results for firms whose debt rating has not changed, and so on. Additionally, the loan sample for these regressions is limited to only repeat loans made within four years of one another. For regression variable definitions see Table 3.

Panel A: Percent change in average spread

$p_t - p_{t-}$	(1) $\Downarrow$ >50% Log(Spread)	(2) $\Downarrow$ ≤50% Log(Spread)	(3) $\Uparrow$ ≤50% Log(Spread)	(4) $\Uparrow$ >50% Log(Spread)
Predicted	0.98***	1.00***	1.01***	1.03***
	(40.33)	(99.70)	(76.75)	(25.92)
Anchor	0.17*	0.19***	0.21***	−0.05
	(1.82)	(3.68)	(3.39)	(−0.78)
Quality	0.06	0.26***	0.21***	0.24***
	(0.67)	(4.92)	(3.19)	(2.79)
Constant	0.14	0.00	−0.09	−0.29
	(1.04)	(0.03)	(−1.26)	(−1.36)
Observations	617	3018	2222	506
R^2	0.690	0.756	0.743	0.623

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel B: Change in debt rating

Rating Change	(1) $\Uparrow$ >1 notch Log(Spread)	(2) $\Uparrow$ 1 notch Log(Spread)	(3) No Change Log(Spread)	(4) $\Downarrow$ 1 notch Log(Spread)	(5) $\Downarrow$ >1 notch Log(Spread)
Predicted	0.90***	0.96***	0.96***	0.98***	0.95***
	(20.60)	(34.30)	(84.44)	(40.22)	(34.23)
Anchor	0.25***	0.21***	0.25***	0.24***	0.12**
	(3.07)	(2.93)	(6.55)	(4.67)	(2.23)
Quality	0.10	0.15*	0.18***	0.06	0.06
	(1.05)	(1.75)	(4.22)	(0.87)	(1.05)
Constant	0.41*	0.19	0.14**	0.12	0.30*
	(1.68)	(1.26)	(2.55)	(1.00)	(1.94)
Observations	229	330	1514	318	345
R^2	0.730	0.802	0.843	0.841	0.779

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Second-stage regression sorted on years between loans

This table shows estimates for the second-stage regression (see Equation 2) sorted on the number of years between loans. For example, column one shows regression results for repeat loans taking place within the same year, column two shows results for repeat loans taking place between one and two years apart, and so on. The last column shows deals where the repeat loan occurs more than five years after the previous loan. For regression variable definitions see Table 3.

	(1)	(2)	(3)	(4)	(5)	(6)
Years b/t Loans	≤ 1	$1-2$	$2-3$	$3-4$	$4-5$	≥ 5
	Log(Spread)	Log(Spread)	Log(Spread)	Log(Spread)	Log(Spread)	Log(Spread)
Predicted	1.00***	0.97***	1.00***	1.06***	0.98***	1.03***
	(82.31)	(74.70)	(48.66)	(44.22)	(28.15)	(26.13)
Anchor	0.29***	0.18***	0.14***	0.09***	0.07	0.04
	(8.53)	(5.88)	(4.20)	(2.69)	(1.62)	(0.80)
Quality	0.17***	0.20***	0.19***	0.09**	0.06	0.06
	(4.53)	(6.06)	(5.17)	(2.08)	(0.98)	(1.05)
Constant	0.03	0.13*	0.01	−0.30**	0.13	−0.17
	(0.43)	(1.88)	(0.09)	(−2.40)	(0.72)	(−0.81)
Observations	2252	2173	1295	646	400	544
R^2	0.770	0.729	0.725	0.753	0.686	0.632

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Second-stage regression sorted on lead arranger

This table shows estimates for the second-stage regression sorted on whether the repeat loan has the same lead arranger (*SAME LEAD*), or a different lead arranger (*DIFF LEAD*) than the previous loan. Results are shown sorted by tranche-type. Columns one and two show results for *Short-term* lines of credit, columns three and four show results for *Long-term* lines of credit, and column five and six show results for *Term loans*. For regression variable definitions see Table 3.

	Short-term		Long-term		Term Loan	
	(1)	(2)	(3)	(4)	(5)	(6)
	SAME LEAD	DIFF LEAD	SAME LEAD	DIFF LEAD	SAME LEAD	DIFF LEAD
	Log(Spread)	Log(Spread)	Log(Spread)	Log(Spread)	Log(Spread)	Log(Spread)
Predicted	1.02***	0.95***	0.99***	1.01***	0.98***	0.99***
	(50.23)	(27.67)	(77.43)	(72.37)	(23.03)	(18.24)
Anchor	0.62***	0.27***	0.22***	0.11***	0.34***	0.12***
	(7.64)	(3.49)	(7.71)	(5.69)	(4.43)	(2.67)
Quality	0.05	0.15*	0.20***	0.10***	0.25***	0.17***
	(0.58)	(1.93)	(6.27)	(3.92)	(3.59)	(3.19)
Constant	−0.08	0.26*	0.02	−0.00	0.08	0.06
	(−0.97)	(1.76)	(0.29)	(−0.06)	(0.35)	(0.19)
Observations	219	231	2238	3094	547	981
R^2	0.886	0.763	0.732	0.701	0.594	0.439

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Second-stage regression sorted on executive officer

This table shows estimates for the second-stage regression sorted on whether the borrower has the same executive officer, CEO or CFO, as when they made their most previous loan. Columns one and two are sorted on whether a firm has the same CEO (*SAME CEO*) or different CEO (*DIFF CEO*) relative to their most recent previous loan. Similarly, columns three and four are sorted on whether a firm has the same CFO (*SAME CFO*) or different CFO (*DIFF CFO*) from their previous loan. The loan sample for these regressions is limited to only repeat loans made within four years of one another. For regression variable definitions see Table 3.

	(1) SAME CEO Log(Spread)	(2) DIFF CEO Log(Spread)	(3) SAME CFO Log(Spread)	(4) DIFF CFO Log(Spread)
Predicted	0.98***	0.95***	0.99***	0.94***
	(79.93)	(44.62)	(76.47)	(32.62)
Anchor	0.18***	0.10**	0.18***	0.16***
	(6.54)	(2.31)	(6.14)	(3.16)
Quality	0.18***	0.29***	0.20***	0.10
	(5.62)	(4.82)	(5.48)	(1.52)
Constant	0.04	0.23**	0.04	0.26*
	(0.75)	(2.26)	(0.68)	(1.82)
Observations	2060	578	1707	434
R^2	0.780	0.772	0.784	0.745

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Second-stage regression excluding repeat loans with same spread

This table presents the same regressions as Table 3, but excludes all repeat loans for which $\log(spread_t) = \log(spread_{t-})$ from the estimation sample.

	(1) All Log(Spread)	(2) Short-term Log(Spread)	(3) Long-term Log(Spread)	(4) Term Loan Log(Spread)
Predicted	0.99***	0.99***	1.00***	0.96***
	(130.86)	(46.26)	(102.76)	(24.42)
Anchor	0.14***	0.41***	0.13***	0.14***
	(9.92)	(7.49)	(7.92)	(3.50)
Quality	0.17***	0.15**	0.16***	0.21***
	(9.77)	(2.53)	(7.46)	(4.78)
Constant	0.06	0.05	0.03	0.20
	(1.52)	(0.61)	(0.56)	(0.93)
Observations	6672	408	4892	1372
R^2	0.737	0.813	0.720	0.469

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Second-stage regression conditional on forced-rollover

This table shows second-stage regression estimates for a sample of loans whose previous loan is the result of forced-rollover or a prior loan. 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). This table examines whether spreads set for loans made in time 2 are anchored to spreads obtained for loans made in time 1 that are the product of forced rollover. Column two presents results for the sample where the previous loan is rolled over in the same year as specified by the maturity the loan made at time 0. Column three presents results for the sample where the previous loan is rolled over in the same month and year as specified by the maturity of the loan made at time 0. Column 1 uses the full sample of repeat loans and is here for comparison purposes only. For regression variable definitions see Table 3.

	(1) All Log(Spread)	(2) Same Year Log(Spread)	(3) Same Month/Year Log(Spread)
Predicted	0.99***	0.99***	0.97***
	(128.51)	(44.69)	(26.09)
Anchor	0.16***	0.22***	0.28***
	(10.55)	(3.55)	(2.84)
Quality	0.18***	0.20***	0.22*
	(10.28)	(2.98)	(1.70)
Constant	0.04	0.04	0.10
	(1.06)	(0.33)	(0.66)
Observations	7310	510	141
R^2	0.732	0.794	0.820

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$