# Anchoring and the Cost of Capital *

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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.

Keywords: Capital structure, Cost of Debt, Anchoring, Reference Points

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*"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."*

—Head of fixed income at Morgan Stanley on Google's bond issuance[1]

# 1 Introduction

Consider two firms 1 and 2, both currently applying for a loan. Each firm has maintained a credit rating of "A" for several years. When firm 1 last borrowed, spreads charged to A-rated firms were typically about 200 bp, higher than the current level of 160 bp. When firm 2 last borrowed, however, spreads for A-rated firms were comparatively cheap, at 120 bp. Does the fact that rates have moved against firm 2, and for firm 1, influence the spreads at which each will borrow today?

According to textbook explanations, it should not: because a firm's cost of capital is forward-looking, there should be no role for information that is *purely* historical. We find, however, that even if firms arrive at the same point, how they got there seems to matter. As the epigraph above suggests, firms that "established" attractive terms by borrowing when spreads were low enjoy abnormally low spreads in subsequent deals, even after spreads have risen. The inverse holds as well.

Panel A of Figure 1 shows this result graphically. We conduct a simple exercise, intended to show how the paths of spreads influence their levels. We consider firms that take out syndicated loans between 1987 and 2008, and evaluate how each firm's borrowing cost depends upon credit conditions at the time of its most recent prior loan.[2] To make the comparison as clean as possible, we consider only the set of repeat borrowers that have maintained the same credit rating between consecutive deals, and are accessing the same loan type (either a term loan or line of credit). For example, one group would be A-rated repeat borrowers,

[1] See `http://finance.yahoo.com/news/Google-bond-deal-sets-stage-rb-829260266.html`.

[2] For more details on construction of the sample, see Section 2.2

drawing long-term lines of credits in 2002. For each year, credit rating, and loan type, we divide borrowers into three mutually exclusive categories, those for which aggregate spreads: 1) have *risen* by 25% or more since their prior loan (e.g., between 1999 and 2002 in the current example), 2) have *dropped* by 25% or more, and 3) have been relatively stable. For comparison, the third group is taken as the reference category, and is thus omitted in the figure.

Recalling that we have fixed credit ratings and loan types, we see strong evidence that the paths of spreads influences their levels. For example, A-rated firms that last borrowed when aggregate spreads were lower enjoy an average price break of over 20% relative to firms that last borrowed when spreads were similar to today. A-rated firms that last borrowed when aggregate spreads were higher tend to have spreads that are 5% higher. For all credit ratings, firms that last borrowed when market-wide spreads were higher (lower) now pay a higher (lower) spread than equally rated firms. The *timing* of a firm's borrowing history seems to matter. This is the paper's main result and, to our knowledge, represents a novel determinant of borrowing costs.

To better appreciate this result, and highlight why it is so peculiar, it is useful to draw an analogy with the used car market. Suppose that we visit a Honda dealership, and observe two customers trading in their old Civic Hybrids for new ones. What Figure 1 essentially indicates is that a buyer who originally purchased his old Civic Hybrids when these cars were, *on average*, more expensive than today (say, five years ago) pays a current premium compared to a buyer who last bought when Civic Hybrids were, *on average*, cheaper than today (say, two years ago). Crucially, because the sorts in Figure 1 are based solely on historical values of market-wide average prices, this simply amounts to a comparison of car shoppers distinguished by the *timing* of their past purchases.

This makes clear that any plausible explanation must jointly link: 1) some feature of the current deal (e.g., the car's mileage, the buyer's budget, the car's advertised price, etc.), and 2) the *average* price level when the buyer last bought a car. A fact pertaining to only one of these will not generate the patterns shown in Figure 1. Some examples of facts that, while correct, would not explain this pattern are that wealthier people buy more expensive cars, that the econometrician might not observe all relevant attributes for pricing a car, or that car transactions are negotiated.

Returning to our context of corporate lending, the car analogy makes clear that there are only two plausible explanations for Figure 1. One is anchoring, the idea that past realized spreads serve as reference points for future transactions: past borrowers, having "established" high borrowing costs by transacting when spreads were historically high, continue to pay higher rates today. The other is that the timing of a firm's borrowing history tells us something about its current risk. For example, perhaps firms that last borrowed when spreads were high are riskier than their credit ratings would indicate. In this case, histories inform the current lender about unobservable risk factors that should be taken into account when rates are set. This latter case requires no behavioral assumptions, and would obtain in a fully rational model.

The goals of this paper are twofold. The first is to simply establish the empirical facts. Here, we present evidence of path dependence in borrowing spreads, in univariate comparisons like in Figure 1, and in a standard regression framework. The second objective is to decipher whether the path dependence we observe is most consistent with anchoring, or whether borrowing histories influence spreads because they *should*, i.e., that they tell us something about unobserved heterogeneity in borrower quality. Two sets of analyses allow us to definitely eliminate this possibility, at least as being the primary driver behind the empirical patterns we observe.

To establish the main result, we build upon the univariate patterns shown in Figure 1, and run regressions similar to those run by Beggs and Graddy (2009) in their study of (re)sales of collectible art works.[3] 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

[3]Beggs and Graddy's (2009) model is in turn adapted from Genesove and Mayer's (2001) study of loss aversion in housing markets.

acquired in a "hot" market (say, 2001), while the other one last sold in a "cold" market (say, 1997). The procedure entails two steps. First, we use thousands of loan transactions each year to obtain predicted spreads for each loan. Taking $p_{i,t}$ as the log spread of firm $i$ in year $t$, the predictive regression takes the form

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

where the vector $X$ includes credit rating dummies, firm size, firm profitability, loan purpose dummies, etc. Perhaps unsurprisingly, the typical $R^2$ from such cross-sectional regressions is high – in the neighborhood of 70% – which generates a set of relatively precise predicted spreads.

We are mainly interested in whether the *realized* log spread of a repeat borrower, denoted $s_t$ (dropping the $i$ subscript), is systematically higher or lower than the *predicted* log spread, $p_t$, based on how spreads have evolved since its last loan. That is, if the date of a firm's most recent borrowing activity is $r$, does the time series quantity $s_r - p_t$ predict $s_t$ in the cross-section, *even after controlling for all time $t$ observables through $p_t$*?

We therefore run a second regression in which we use the predicted spread from the first stage, along with the path variable $s_r - p_t$, to predict current spreads. To assure that our path variable is not capturing historical information about the firm that is still relevant for pricing debt today, we include the predicted spread at the time of the last loan as an additional regressor.[4]

Under the null that the timing of a firm's borrowing history is irrelevant, the path variable $s_r - p_t$ should have no bearing on current, realized spreads. Our regression analysis reveals that it does. Consistent with the graphical evidence in Panel A of Figure 1, the path term, $s_r - p_t$, is a strong determinant of $s_t$, with a coefficient estimated to be 0.14 (bootstrapped

[4]We difference $p_r$ by subtracting $s_r$. This differencing ensures that any non-zero coefficient on the path variable is indeed a sign of path dependence and not information. More discussion of the specification can be found in Section 2.1.

t-statistic = 9.67).[5] Because the predicted value $p_t$ is also included in the regression, the coefficient on $s_r - p_t$ has the effect of pulling realized spreads away from the contemporaneous hedonic benchmark, $p_t$, and toward the historical value, $s_r$.

How big is a coefficient of 0.14, and what does it mean? For an illustration, return to the example given in the first paragraph. Absent any path dependence, both firms 1 and 2 should be able to borrow at 160 bp, the current prevailing spread on A-rated firms. Recall that aggregate spreads have decreased 40 bp for firm 1, and increased 40 bp for firm 2. Firm 1's realized spread would thus be equal to $e^{ln(160)-0.14*ln(\frac{160}{200})} = 165$ bp, and firm 2's realized spread would be about 155 bp, generating a difference of about 10 bp between otherwise equivalent firms. As this example shows, the economic magnitude of the path dependence depends on each firm's unique history.[6] Averaged across all repeat deals, we estimate that path dependence alters realized spreads by about 10-20 basis points in the direction opposite the innovation, similar to a single credit rating shift such as moving from AA to A.

What amount to modest effects on average can occasionally have large impacts. The Financial Crisis of 2008 provides such an example. In less than a year, aggregate credit spreads increased sharply, in the neighborhood of 30% across the board, as shown in Panel A of Figure 2. The effect of path dependent spreads is apparent in Panel B: for the set of firms that borrowed immediately before the crisis (2006 or 2007) and then again during the crisis (2008), the modal spread change is exactly 0%. This part of the histogram is visually striking, especially because the remainder of the distribution appears centered roughly around 30-40%, as one would expect. The timing of a firm's borrowing history – in this case being fortunate enough to frontrun one of the largest financial crises in modern times – can meaningfully impact current borrowing costs.

[5] 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).

[6] That is, firms experiencing larger shifts in spreads between borrowing events will be more impacted by path dependence, compared to firms for which $s_r - p_t$ takes a low value.

Once we establish that borrowing costs are path dependent, we attempt to address why this might be. We explore three hypotheses. The first hypothesis – that the path provides information about current risk – is an argument about unobservables, as $p_t$ already includes a battery of firm and market variables known to influence credit spreads. The positive coefficient on the path term implies that firms with high (low) unobservable quality must disproportionately borrow when spreads are low (high).[7] We present two tests suggesting that this is not the case.

First, when we examine *observable* measures of risk, we find little relation with aggregate credit market conditions: the type of firms borrowing at a given date appears unrelated to credit conditions at that date. Second, when we consider a subset of reference loans whose initiation dates were effectively exogenous, the results do not weaken. What sort of loans are these? Suppose that we are attempting to explain the spread on a term loan initiated in 2006, as a function of the spread charged on a similar loan in 2002 (i.e., the "reference" year). Because the concern is that credit market conditions in 2002 may be correlated with unobserved risk characteristics of borrowers in 2002, we consider a subset of repeat deals not subject to this concern: term loans initiated in 2002 as refinancings of prior loans *maturing* in 2002. Under the assumption that market conditions in 2002 were not predictable when the prior loan was made, this test allows us to identify a sample of borrowing events that breaks any link between firm types and market conditions. Among this sample, we find even stronger effects, suggesting that this type of endogeneity is not a major concern.

The second hypothesis – that banks implicitly or explicitly offer to smooth spreads for client firms – is directly falsifiable. Whether or not such inter-temporal smoothing is a realistic feature of corporate lending contracts (we are not aware of any such evidence), it clearly can only apply when a firm is borrowing from the same bank in repeated deals. When

[7] To see this, suppose that there are two spread regimes (high and low), and two types of firms (good and bad). Suppose that good firms are more likely to borrow when spreads are low, and bad firms borrow when spreads are high. Here, *when* a firm historically borrows gives us information about its type, and thus, should be incorporated into its risk assessment and spread. Note also that the direction is important here – if good firms were more likely to borrow when spreads were high, the coefficient on the path term would be negative.

we exclude these cases from our analysis, i.e., include repeat deals only when the lead bank is different from the prior lead bank, the coefficient on the path term remains significant.[8] In this sample, relationship lending, or any other version of spread smoothing, has little bite, and thus leaves a substantial part of the evidence unexplained.

The third hypothesis – that past spreads act as an anchor in negotiations over current spreads – parsimoniously reconciles all available evidence, in addition to providing additional predictions consistent with the patterns we observe. Beggs and Graddy's (2009) art study, mentioned previously, finds that more recent reference points are more salient, so the coefficient on the path term increases as the prior loan is more recent.[9] We find precisely the same pattern: if the reference deal is within the last year, the coefficient on the path term is 0.25, decreasing to 0.17, 0.13, and 0.11 after one, two, and three years, respectively. Reference deals older than four years have no bearing on current spreads whatsoever.

Taken together, the evidence here offers potential contributions to a large literature exploring capital structure and determinants of the costs of capital. Our innovation is to provide evidence that the cost of capital may be influenced by borrowing *history* in addition to 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.[10]

Our evidence of path dependence, however, may not be evidence of mispricing. In particular, we cannot reject the hypothesis that firms and banks "make up" for muted innovations in spreads by adjusting other deal terms. For example, suppose a firm's spread would be

[8] Note also that this test addresses the possibility that features of the Dealscan database, the source for our lending data, mechanically generate the coefficient on the path term. See Roberts (2010) for more discussion of this issue, which we directly address in Section 4.3.

[9] Indeed, in their study, they find that historical purchase prices are much more important if the reference deal is within $3\frac{1}{2}$ years of the current transaction. See Tables 4 and 5, page 1035.

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

expected to fall 50 bp because its credit rating has improved two notches since its last deal. The firm and bank may agree to reduce the spread by only 25 bp, but make up for it by increasing the borrower's credit line or eliminating restrictive covenants. While this means that we cannot quantify the dollar value of "mispricing" due to path-dependence (indeed, in this example, there is no mispricing to quantify), it does not affect the unusual nature of the main result: path dependence in credit spreads.

The paper is organized as follows. In Section 2, we discusses our sample and empirical methodology. In Section 3, we present evidence that the *path* of credit spreads influences present borrowing terms. In Section 4, we evaluate several explanations for this fact, and argue that anchoring appears to be most consistent with the evidence. Section 5 concludes.

# 2 Empirical Design

We are interested in predicting borrowing costs in the *cross-section*, using each firm's unique *time series* of credit spread evolution as the main explanatory variable. By "credit spread evolution," we are referring to how the firm's borrowing cost has evolved since its last borrowing activity. That is, if a firm was awarded a line of credit at 200 bp above LIBOR in 1997, we care about whether its expected spread has increased to, say, 250 bp in 2001, or whether it has decreased to 150 bp. We will link these time series quantities (here, +50 or -50 bp) to actual cross-sectional borrowing rates.

There are two reasons why spreads may change over time. We have already discussed one of them in the introduction, which is that aggregate credit conditions can change. Consequently, firms that access debt markets at different times can experience different spread paths, even if their own credit quality stays relatively constant. As we already saw in Panel A of Figure 1, otherwise similar firms that experience different aggregate spread trajectories pay different rates in subsequent deals.

A second type of spread evolution is due to variation in the firm's own fundamentals, regardless of any changes in aggregate credit conditions. For example, take two BB-rated firms applying for 5-year term loans in the year 2003. While the firms currently share the same rating, and presumably are of similar risk, suppose that since last borrowing, one firm has been downgraded from BBB, while the other has been upgraded from B. Given that spreads are strongly predicted by credit ratings, these firms will have experienced different trajectories due to firm-specific factors. The question is whether, as we saw for aggregate spread changes, different paths generated by a firm's rating history also influence borrowing costs.

Panel B of Figure 1 provides an answer to this question. Different spread trajectories generated by firm-specific fundamentals generate large differences in borrowing costs. A-rated firms, for example, pay nearly 45% higher spreads if they were rated below A the last time they borrowed, versus being rated above A.[11].

## 2.1 Methodology

Whether the firm's expected borrowing costs have changed due to aggregate or firm-specific factors, the goal of our regressions is to relate each firm's potentially unique spread trajectory to the level of its borrowing costs. Specifically, we are interested in whether firms having experienced upward (downward) trajectories have realized spreads that are, in some sense, lower (higher) than they "should be" according to fundamentals. Thus, the first step is to establish a but-for *benchmark* for each repeat borrower, against which we can compare the actual, *realized* spread.

As described in Equation (1) above, this benchmark we denote $p_{i,t}$, where $i$ indicates the firm and $t$ the year. There are several ways to construct these predicted values – including simply using each firm's current credit rating as in Figure 1 – but because the data are available, we use regression analysis to also incorporate firm and loan-specific features into the predictions. Although we are ultimately only interested in making predictions for the set

[11]While this is consistent with the possibility that recently upgraded firms are riskier than recently downgraded firms, the evidence suggests, if anything, the reverse. We delay this discussion until Section 4.2

of repeat borrowers, we make use of *all* loan transactions, repeat or not, to fit the hedonic model described in Equation (1).

With the set of predicted spreads $p_{i,t}$ in hand, our main estimating equation is given by

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

where $s$ refers to a log realized spread, and $p$ 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 $r$. Equation (2) is estimated only for the set of repeat borrowers, i.e., firms for which there is at least one deal at date $t$ with a preceding deal at some date $r$.

The first term captures the net impact of *current* observable characteristics like credit ratings, profitability, size, and loan purpose on realized spreads. As we will see, $p$ is a strong and unbiased predictor of $s$, so we will expect $\beta$ to be very close to one, and highly significant.

The second term is our main focus. It is the only term in Equation (2) that combines information from two dates, both today ($t$) and the time the firm last borrowed ($r$). A positive estimated coefficient on this term ($\delta$) has the effect of pulling realized spreads toward their historical values. For example, if the log spread in the most recent deal is $s_r = 4$, and the benchmark prediction in the current date is $p_t = 5$, an estimated $\delta = .2$ means that the realized spread today is pulled from 5 to 4.8.

The final term is included to capture firm quality that is unobservable to the econometrician, and therefore not picked up by the set of firm, macro, and loan characteristics in the hedonic regressions, Equation (1).[12] For example, suppose that a firm discloses private information to the bank at date $r$ about its future profitability; if this is incorporated into the risk assessment, but is not yet reflected in observable metrics, this will show up only in the date $r$ residual $s_r - p_r$. As $t - r$ becomes large, we would ultimately expect private information at $r$ to become reflected in date $t$ observables, so the coefficient on the date $t$

[12]Note that, under the null that there is no path dependence in credit spreads, $s_r = p_r + u_r + e_r$, where $u_r$ is unobserved quality and $e_r$ is an error term. Then $s_r - p_r = u_r + e_r$; i.e., the date $r$ residual term is indeed simply a noisy measure of unobserved quality.

residual should shrink as $t-r$ increases. Indeed, it does, as we analyze in Section 4. In any case, this is not a time-series term (both $p$ and $s$ are measured at date $r$), but we include it to improve the quality of the overall specification.

## 2.2 Sample

As mentioned above, there are two relevant samples. The first is a master set of all deals, which is only used to form the predictions in Equation (1). The second is a subset, which only includes deals that have at least one preceding deal, allowing us to estimate Equation (2). In either case, the relevant time period is 1987-2008.

In both cases, the source for our data come from Reuters Dealscan, which we combine with Compustat to obtain information on each firm's fundamentals at the time of each deal. We also use ExecuComp to determine CEO and CFO turnover, which we use in some of our cross-sectional tests.

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 two categories: 1) revolving lines of credit, and 2) term loans. The typical maturity for revolving lines of credit is 44 months, and for term loans is 59 months.

We do not consider loans to regulated and/or financial firms (SIC 40-45, 60-64), and loans for which no spread data are reported. We exclude lines of credit with maturities of less than 365 days, as well as any observation for which we cannot match the borrowing firm with Compustat data. This results in 18,360 observations, which we call the "master" sample.

Table 1 provides summary statistics about the master sample (Panel A) and the sample of repeat loans (Panel B). The average firm in our master sample has assets of \$2 billion, annual sales of \$1.5 billion and an industry-median adjusted book leverage ratio of 13 percent. The average loan amount is \$204 million, with a spread of 235 basis points above LIBOR.

Panel B describes the same summary statistics for the set of repeat borrowers and deals.

A repeat loan is an observation for which there is at least one 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. If Ford took out a revolver in 2004 and 2008 and a term loan in 2006, only the 2008 revolver would be a repeat loan. Of the 18,360 observations in the master sample, 6,951 are repeat loans.

Panel B shows that the average repeat borrower in our sample has assets of \$2.2 billion, sales of \$1.8 billion, and an industry-adjusted leverage ratio of 11 percent. On average, repeat loans have an amount of \$245 million with a spread of 216 basis points above LIBOR. Comparing Panels A and B reveals that repeat borrowers are a little larger, and perhaps less risky, although any differences are small.

# 3 Spread histories and borrowing costs

This section presents the results when we allow spread evolutions to predict cross-sectional differences in borrowing costs. We first present the results of the hedonic benchmark regressions in Section 3.1, and then the main results in Section 3.2.

## 3.1 First-Stage Predictive Regressions

Table 2 displays the results of the first stage predictive regressions, shown in Equation (1). The explanatory variables used here are similar to those in Ivashina (2009). 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.

For our purposes, the more relevant metric is the distribution of $R^2$. The average (median) $R^2$ is 0.63 (0.65), 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 (which directly follows), we will rely on the predictions from this model to control for observed firm quality.

## 3.2 Path Dependence and Credit Spreads

With 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 both transaction types together, while the subsequent columns present them separately. Unsurprisingly, the coefficient on $p_t$ is nearly one in each case, indicating that Equation (1) provides an unbiased estimate of $s_t$.

Our main interest is the second term, called *Spread evolution* in the table. On average, the coefficient is 0.14, indicating that approximately $\frac{1}{7}$ of would-be evolutions are not incorporated into realized spreads. Roughly speaking, if a firm's expected spread is 50% higher than its spread at the time of its last loan, – either because aggregate spreads have increased or because its own credit quality has decreased – its actual spread will increase only 43% on average. The effect is slightly larger among term loans (point estimate of 0.17, $t$-statistic = 4.32).

For comparison, we also show the estimated magnitude on the third term, which we call *Previous residual*. Recall that while the second term is a time-series term, combining information from both dates $t$ and $r$, the third term is simply the static residual from date $r$. The third term allows for the possibility that the firm's date $r$ lender had private information (or at least information not included in the hedonic model) when negotiating a spread. For example, perhaps the firm had recently hired a new management team, whose quality investors and rating agencies had not yet observed. The coefficient on the date $r$

spread residual can be interpreted as the percentage of this residual that is still not reflected in current observables. Table 3 shows that on average, about 17% of a firm's previous spread residual remains unaccounted for.

Equation (2) is symmetric, in the sense that spread increases and decreases are treated equally. Furthermore, the marginal effects of large and small spread changes are assumed to be equal. We relax these assumptions in regressions whose results are presented in Table 4.

The columns are ranked in ascending order, based on how much spreads have evolved since the firm's last transaction. In column 1, firms' predicted spreads have declined by 50% or more since their most recent loans. Likewise column 4 shows the case where predicted spreads have increased at least 50%. The middle two columns show intermediate cases.[13] As a group, the four columns in Table 4 suggest that the effect of histories on borrowing costs is roughly the same for increases and decreases. Columns 2 and 3 feature similar point estimates and statistical significance, and although the coefficient in column 1 is not statistically significant, the point estimate is similar to the overall sample (0.12). Only among the small set of observations where predicted spreads have increased by more than half do histories not appear to matter for borrowing costs.

## 3.3 Unchanged Spreads: A Special Case

Before attempting to understand *why* borrowing histories seem to matter for cross-sectional borrowing costs, we conclude this section by considering a special case of path dependence. The most extreme path dependence one could imagine is that a firm's spread is set precisely at its prior level, even if its fundamentals or aggregate spreads have changed. Figure 3 plots the empirical distribution of $\Delta \log(spread_{i,t})$ for all repeat loan transactions in DealScan. Although the distribution is roughly normally distributed around -20% (reflecting the fact that the typical repeat borrower is a better credit risk at the time of the later loan), there is a an unusually large number of cases where $\Delta \log(spread_{i,t})$ is zero. The zero cases are

[13] We consider only on repeat loans within four years after their immediate predecessors. We save the discussion of why for Section 4.1.1.

borrowers whose spread today is *exactly* as it was in the previous loan. In fact, zero is the modal outcome, corresponding to about one-fifth of the total observations.

If changes in spreads reflect changes in the riskiness of firms, or in the market price of that risk, then the distribution in Figure 3 is peculiar. For example, if there were a macro shock that increased spreads for all firms by 10%, then a firm would have to have improved its risk profile to reduce its idiosyncratic spread by exactly 10% 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 and, while sometimes possible, Figure 3 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.[14]

Figure 2 and Figure 4 demonstrate an unusual number of zero cases *even when credit conditions and firm characteristics have changed*. Figure 2 plots spread changes for firms that borrowed in 2006 or 2007 and then again during the Financial Crisis in 2008. Although Panel A demonstrates a 30-40% increase in spreads across credit categories during the crisis, Panel B indicates that the most common spread change, by a factor of two, was 0%. Thus, even when the market price of risk rose dramatically during the crisis, we find an unusual number of firms who borrow at exactly the same spread as they did pre-crisis.

Figure 4 plots spread changes for three types of firms: those which have experienced a

[14] 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. 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, $\phi$ is the standard normal density, and $h$ is the kernel bandwidth chosen following Silverman (1986). In our case, $h = 0.0537$. 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 0.1% level. Various alternatives for estimating the kernel density and bandwidth do not alter this conclusion.

credit rating upgrade between loans (Panel A), those which have experienced a credit rating downgrade between loans (Panel B) and those with no change in credit rating between loans (Panel C). No change in spreads is the modal outcome in each panel. Taken together, Figures 2 and 4 demonstrate instances of extreme path dependence. That is, even when the market price of risk has changed or the credit worthiness of the firm has changed so that spreads should adjust, we find an unusual number of cases where no adjustment is made.

# 4 Why do borrowing histories matter?

Having established that prior deal terms appear to influence current spreads, over and above any information about past risk that they may contain, we turn to the question of why this occurs. We offer three potential explanations. Past deal terms may influence current spreads because they 1) serve as a heuristic/anchor for the firm and lender in negotiating deal terms, 2) provide information about risk (i.e., our regression framework may have been misspecified), or 3) are intentionally smoothed by bankers as part of a implicit or explicit agreement with firms.

## 4.1 Anchoring

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)

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.

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. If firms and banks use old transactions as reference points, this will generate path dependence in spreads.

The anchoring hypothesis has several testable implications. Perhaps most obvious is that anchoring effects should be stronger when the reference point is more salient. We consider three dimensions in which salience may vary and, in each case, find stronger path dependence in spreads when the prior deal's terms were more salient.

#### 4.1.1 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 "clean the slate" 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 5 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 5 tests for this directly: Panel A breaks 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. Panel B repeats the same regressions as our main specification in Table 3 including interactions of the *Spread evolution* and *Previous residual* terms with dummy variables for the time between repeat loans.

What is immediately clear is that the effects of anchoring degrade with time. Focusing on Panel A, we see that for deals completed within the most recent year, the *Spread evolution* coefficient is 0.25 (bootstrapped $t$ statistic = 6.81), approaching twice the magnitude observed for the overall sample. Advancing forward a year, we see the effect cut by over a third, to 0.17 (bootstrapped $t$ statistic = 6.00). 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 (*Previous residual*) 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.

Panel B shows results similar to those in Panel A, though the effects appear to taper-off slightly sooner at the 2–3 year mark. Additionally, Panel B shows that these time effects are pervasive across all loan types. For all repeat loans, the coefficient on the interaction between *Spread evolution* and the dummy variable for loans made within 1 year of one another is 0.22 (bootstrapped $t$ statistic = 4.59). This same coefficient is 0.16 for revolving lines of credit (bootstrapped $t$ statistic = 3.24), and 0.53 for term loans (bootstrapped $t$ statistic = 4.38).

[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.

### 4.1.2 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 to assess the relevance of reference points when *both* sides of the initial negotiation transact again. That is, we can compare path-dependence when the borrower transacts with 1) the same lead bank, and 2) a different lead bank.[16]

Both Figure 6 and Table 6 show the results for this segregation, and each highlights sharp differences. Figure 6 shows that 29% of repeat loans have the same spread when the same lead arranger is used, but only 18% of loans have the same spread when the lead arranger is changed. In the first column of Table 6, we see that the *Spread evolution* term has a magnitude of 0.31 when the lead arranger is the same as for the previous loan, but only 0.13 when the lead arranger is different. The second column shows that, although path-dependence still appears relevant when the lead bank has changed, it is less important. Because it's possible that changes in the lead arranger may be proxying for longer time passing between repeat loans, care is taken to disentangle these two effects. To this end, 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.

### 4.1.3 Same vs. Different Loan Purpose

Our final cross-sectional test concerns the purpose of the loan. When firms and banks negotiate current spreads, historical spreads may be most salient when the purpose of the historical loan is identical to that of the current loan. The last two columns of Table 6

[16] 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.

estimate the main model among loans with the same purpose (Column 3) and a different purpose (Column 4).[17] Consistent with the intuition behind the test, we find that when the loan purpose is the same the *Spread evolution* coefficient is 0.23, but only 0.15 when the loan purpose has changed.

Taken together, these results suggest that path dependence is strengthened when reference points are most salient.[18] This provides cross-sectional support for the anchoring hypothesis.

## 4.2 The Path Informs the Present

We next consider the possibility that the timing of a firm's loan is informative about its quality. Thus far, we have implicitly assumed that after controlling for $p_t$ and $(s_r - p_r)$, 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, for example, 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 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 of a borrower – then this will generate path dependence in spreads.

To see why, 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 regardless of the cost, and high quality firms only raising capital when it is comparatively cheap. While firm quality may not be 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, borrowing histories

[17] Results where the stated loan purpose is "Other" are excluded from the sample of reference and repeat loans. Additionally, the sample is limited to repeat loans that have occurred within the last four years of one another.

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

will matter. To this point, our regressions have ignored this issue altogether, having taken historical credit market conditions as exogenous.

There are two pieces of evidence that suggest that this argument cannot explain the results. First, in regression results analogous to Figure 1, we find that the performance of firms who previously borrowed at higher (lower) spreads (i.e., when aggregate spreads were higher or their credit ratings were lower) is not typically better (worse) in the quarter leading up to the next loan. Specifically, Panel A of Table 7 reports regression estimates for the following specification:

$$Performance_{i,t-1} = \lambda_1 Spreads\ Fell_{i,t} + \lambda_2 Spreads\ Rose_{i,t} + \alpha(L)\, Year/Rating/Tranche_{i,t} + \epsilon_{i,t-1}.$$

where $Spreads\ Fell_{i,t}$ ($Spreads\ Rose_{i,t}$) is a dummy variable that equals one if aggregate spreads have decreased (increased) more than 25% since the last time firm $i$ borrowed, and $Year/Rating/Tranche_{i,t}$ is vector representing debt rating-year-tranche type fixed effects for firm $i$ at quarter $t$

Regardless of the performance measure used, we find little evidence suggesting that firms that borrow following aggregate spread increases (decreases) are systematically better (worse) than firms that borrow following small changes in aggregate spreads. For example, when sales growth is used as the performance measure we find that firms that last borrowed when aggregate spreads were lower actually have lower sales growth in the quarter previous to their next loan relative to the firms that previous borrowed when aggregate spreads were approximately the same. If firms were sorting on sales growth, we would expect them to have higher sales growth, justifying the better loan terms that these firms receive. Panel B presents similar results looking at firms which have experienced credit rating increases or decreases since their last loan. Similar to aggregate spreads, we find little evidence that the timing of a firm's loan is informative about its quality.

Second, we consider a subset of firms that do not appear to time the market. Table 8 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, path dependence 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 8 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. Because we can reasonably assume that when IBM originally borrowed in 1997, it could not foresee credit market conditions in 2002, we can reasonably conclude that the firm's borrowing activity in 2002, and therefore credit market conditions in 2002, are unrelated to its risk.

Table 8 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 *Spread evolution* coefficient is 0.15, compared to 0.14 for the full sample. More precise identification is shown in the final column, where we examine the 69 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 spread evolution coefficient is the same as the coefficient in column two using the sample matching only on year; the estimate is statistically insignificant (bootstrapped $t$ statistic = 1.19), however – a fact attributable to our much smaller sample size.

## 4.3 Relationship Banking

Finally, we consider the possibility that path dependence arises out of existing relationships between firms and banks. It is possible that a bank and a firm could have an implicit or explicit 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 intertemporal 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 is a potential explanation.[19]

One manifestation of smoothing may come about in renegotiation. That is, when firms and banks revisit terms of an existing deal, some feature of the relationship may lead then to change few, if any, deal terms. 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.

Table 9 suggests such "intradeal" transactions, however, cannot explain our main result. Table 9 considers a subset of transactions which are almost certainly new deals rather than renegotiations of existing deals. These are deals where the loan origination date of the second

[19]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 (a) asked to join future syndicates, and (b) allowed to share in the rewards in future deals—in order to agree to below-market spreads today. While it is possible that syndicate members have their own implicit relationships with lead banks, it seems unlikely. It is difficult to maintain a pair-wise implicit contract in real-world settings, in which unexpected events, noisy indicators that promises have been broken, etc. often cause agreements to break down. Adding an additional implicit contract with a third-party (or, as there are many banks in a syndicate, third-parties) is likely to make the problem far more difficult. The problem could be resolved by the lead-bank bearing the entire burden: perhaps syndicate-members receive market spreads regardless of the actual spread charged the firm. As we do not observe these terms, we do not take a stand on this issue.

loan occurs *after* the maturity date of the first loan (see Figure 7, Panel A, for a timeline). These cannot be loan renegotiations *because the original loans have expired*. The coefficient on the *Spread evolution* term is 0.11 (bootstrapped $t$ statistic = 2.00). Figure 7 also provides complimentary evidence from the empirical distribution of spread changes between repeat loans where the second loan originates after the expiration of the first loan. The histogram of log spread changes in Panel B clearly demonstrates a discontinuity at zero among this set of deals.

Perhaps the strongest evidence that relationship banking cannot explain the cross-section of evidence comes from Table 6. There we found that path dependence was stronger among repeat deals where the lead bank was the same in both transactions. However, we still found significant effects among deals where the lead bank had changed. It is implausible for a relationship banking story to explain path dependance among this subset of deals. In such cases, *there is no relationship* because the bank is changing.

# 5 Conclusion

We provide evidence that the interest rate spreads paid by firms on syndicated bank loans are influenced by – not simply correlated with – the rates they have paid in the past: firms whose most recent loan spread was high (low) tend to pay a higher (lower) spread today. This is true whether the past spread was high (low) because aggregate spreads at that time were high (low), or because the firm was higher (lower) risk in the past. We evaluate several explanations for the evidence and argue that the heuristic known as *anchoring* is the most likely candidate.

Our evidence of anchoring comes in two forms: simple non-parametric analyses, and regressions following the methodology of Genesove and Mayer (2001) and Beggs and Graddy (2009). The non-parametric analyses reveal a clear presence of path-dependence in deal terms, including an extreme form in which a firm's prior spread is identical to the spread on its current loan. That is, the most likely change in spreads for a firm from one loan to the next is zero, regardless of changes in firm risk or the market-wide cost of debt. The regression analyses allow us to quantify the effect of path dependence: on average, we find that roughly 14% of innovations in expected credit spreads for a firm do not materialize. That is, if a firm's spread would be expected to rise 100 bp from its last loan, the actual increase is only 86 bp.

While the magnitude of this effect may be of interest to those studying the determinants of credit spreads, the existence of an anchoring effect should be of interest to a much broader audience. Although there is overwhelming experimental and laboratory evidence that people violate the standard economic assumptions of full rationality, critics of behavioral economics remain skeptical about the extent to which observations in the lab translate into the "real world."[20] That we find evidence of anchoring in a trillion-dollar debt market suggests that behavioral forces may manifest even when the stakes are large, market forces are strong, and agents are sophisticated.

[20]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 theres 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."

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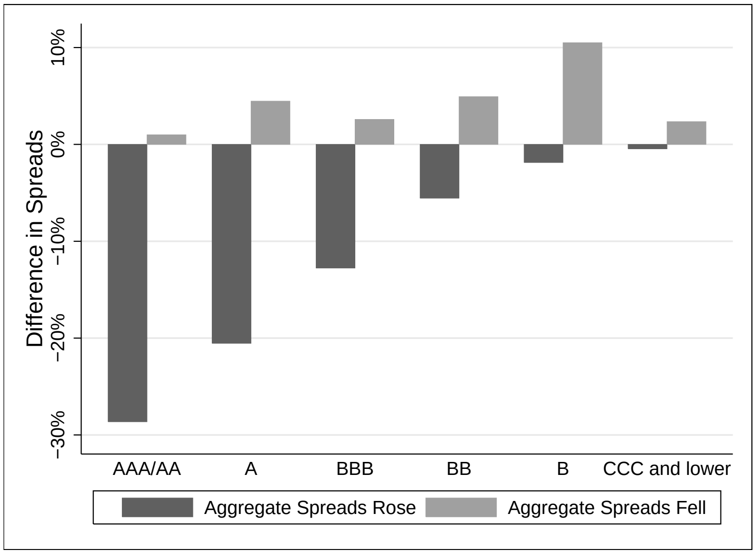
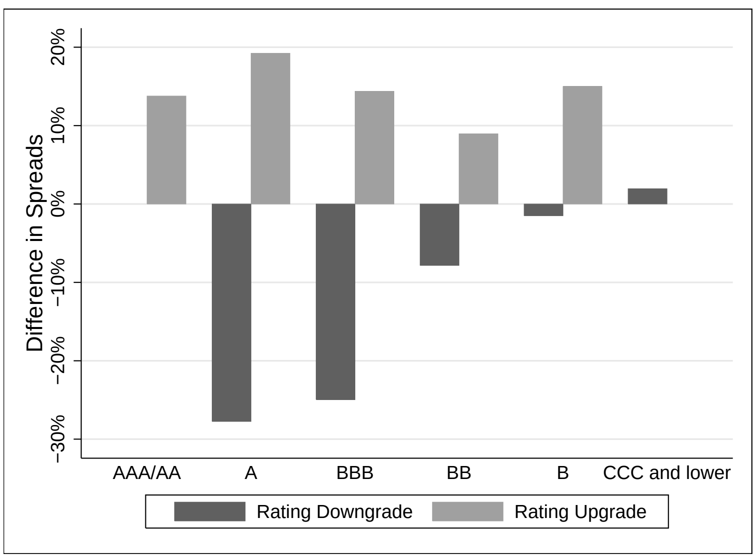
# Figure 1: Loan spreads sorted on rating changes and aggregate spread changes

**Panel A: Spread Difference Sorted on Previous Aggregate Spread**



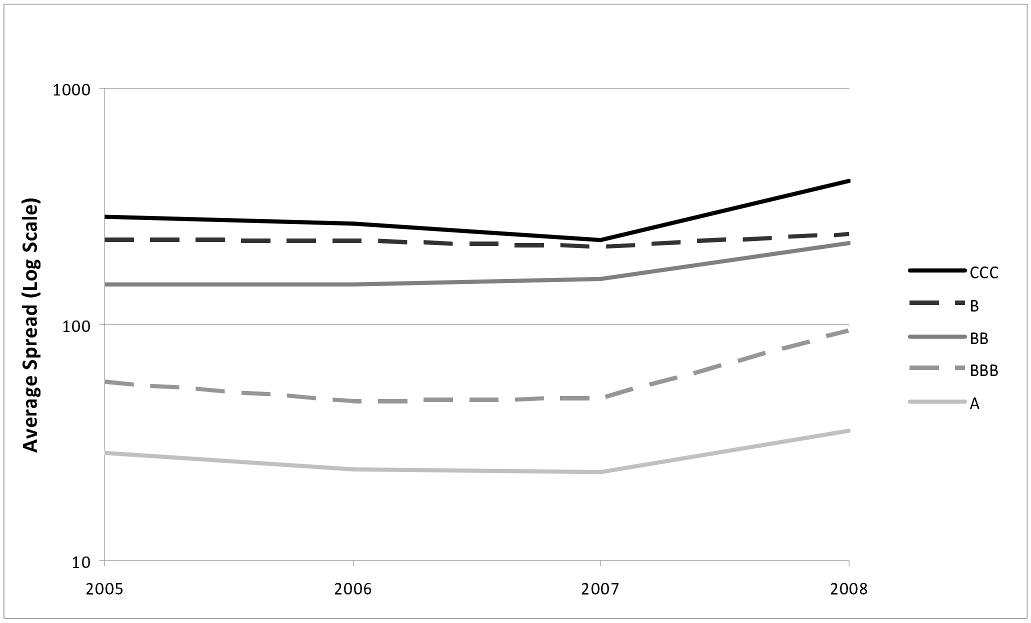

**Panel B: Spread Difference Sorted on Previous Debt Rating**



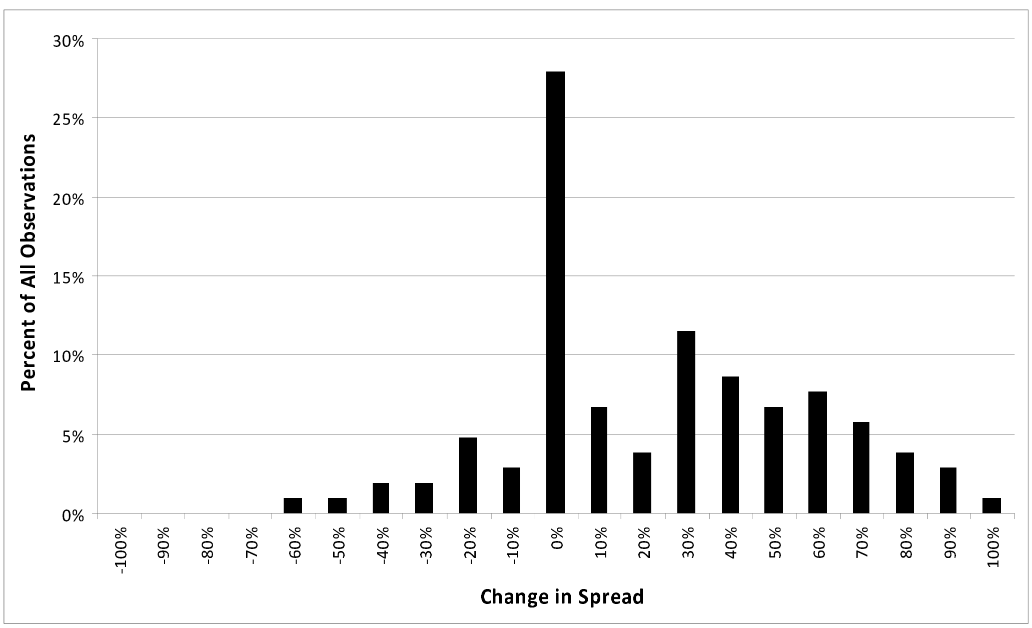

Panel A of this figure plots the average percent difference in the spreads of firms that previously borrowed when aggregate spreads were 25% higher or 25% lower than the average spread of firms currently borrowing in the same year, with the same credit rating and loan type. Panel B plots the average percent difference in the spreads of firms whose credit ratings have been upgraded or downgraded since the last time they borrowed, relative to the average spread of firms currently borrowing in the same year with the same credit rating and loan type, whose credit ratings have not changed since the most recent loan. Both plots use only repeat deals completed within four years of the reference transaction.

## Figure 2: Spreads before and during the financial crisis of 2008

**Panel A: Average spreads over time by debt rating**

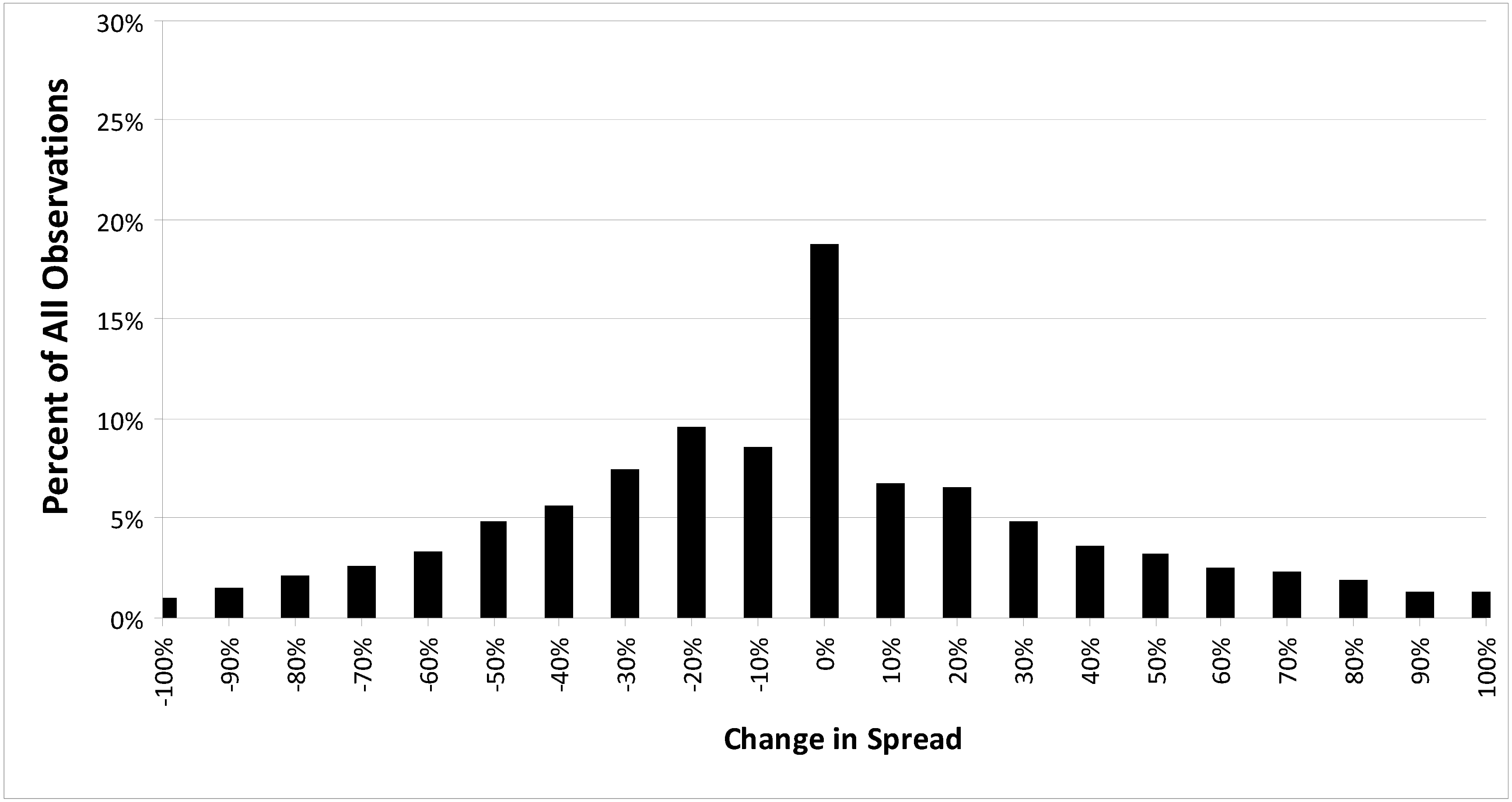



**Panel B: Change in spreads before/during the crisis**




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.

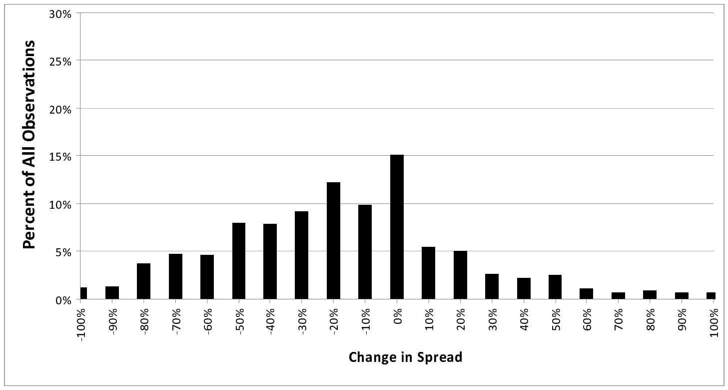
## Figure 3: Histogram of log(spread) differences



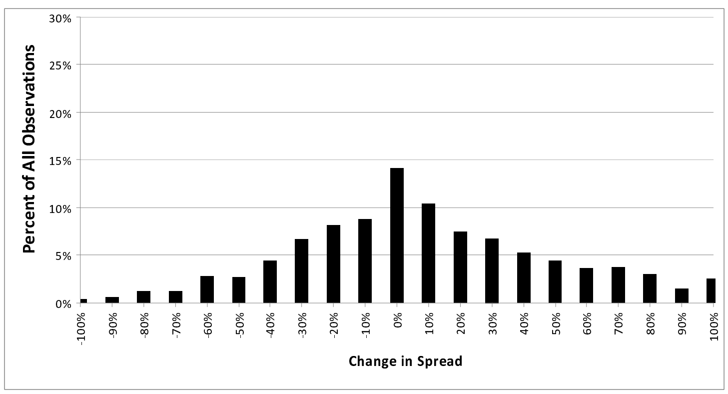

This figure plots the empirical distribution of $\Delta \log(spread_{i,t}) = \log(spread_{i,t}) - \log(spread_{i,r})$ for all repeat loan transactions in DealScan.

## Figure 4: Histogram of spread changes sorted on credit rating changes

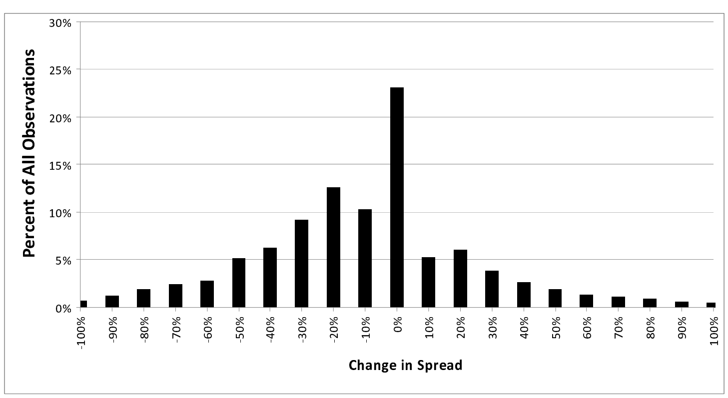
**Panel A: Rating Upgrade**




**Panel B: Rating Downgrade**



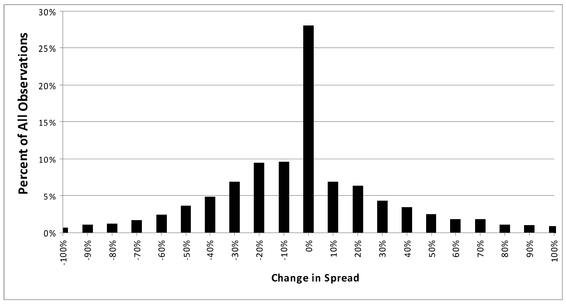

**Panel C: Rating No Change**



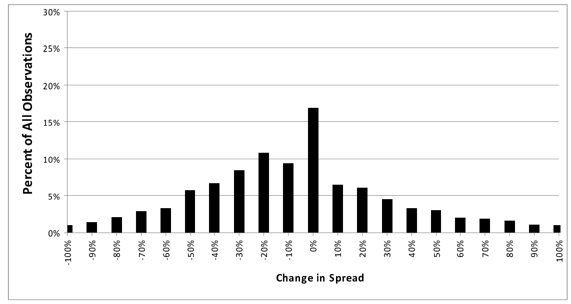

This figure plots the empirical distribution of $\Delta \log(spread_{i,t}) = \log(spread_{i,t}) - \log(spread_{i,r})$ for all repeat loan transactions according to whether the firm's credit rating has been upgraded, downgraded, or remained unchanged since the date the previous loan was made. All plots use only repeat deals completed within four years of the reference transaction.

## Figure 5: Histogram of spread changes sorted on years between loans

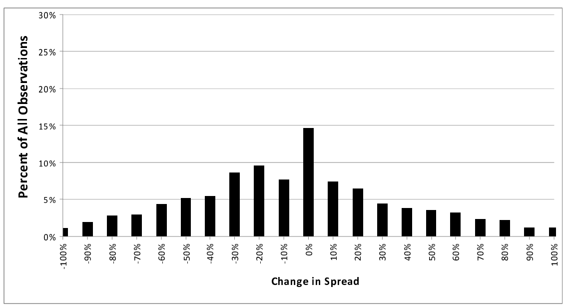
### Panel A: $\leq$ 1 year between loans



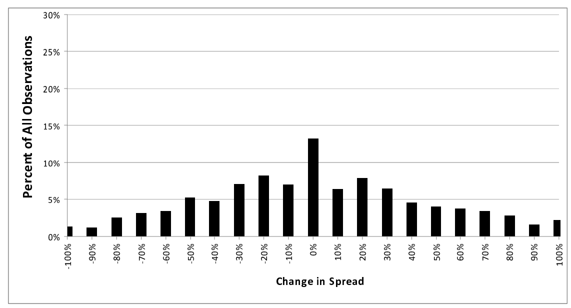

### Panel B: 1$-$2 years between loans



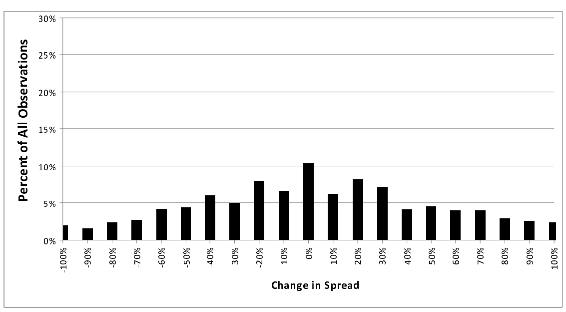

### Panel C: 2$-$3 years between loans



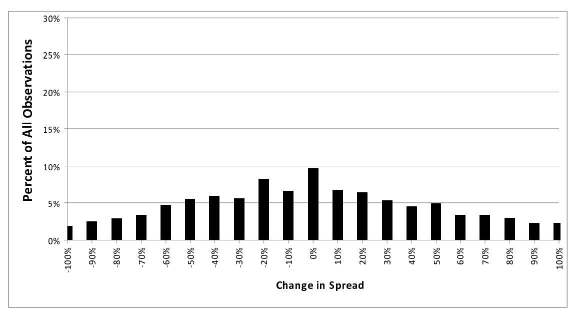

### Panel D: 3$-$4 years between loans




### Panel E: 4$-$5 years between loans



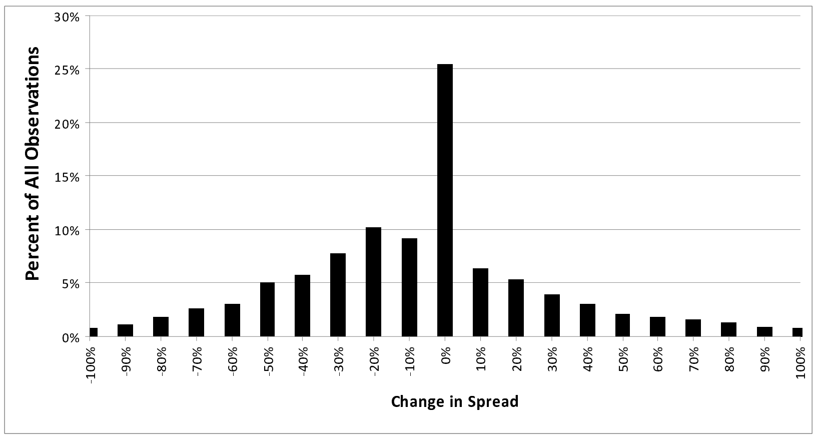

### Panel F: $\geq$ 5 years between loans



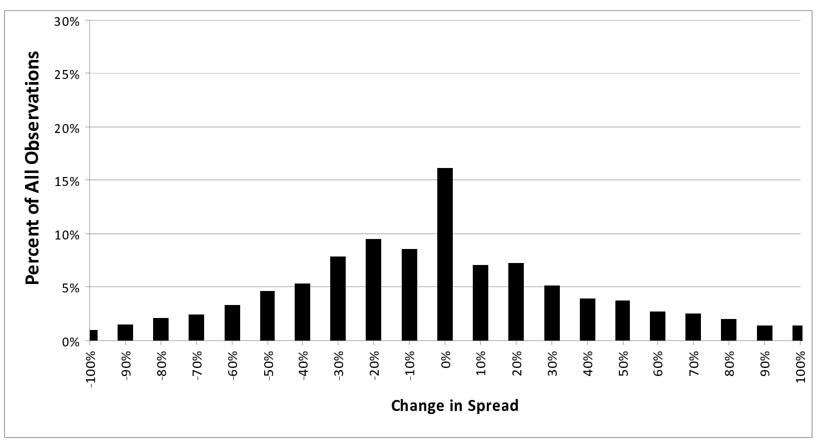

This figure plots the empirical distribution of $\Delta \log(spread_{i,t}) = \log(spread_{i,t}) - \log(spread_{i,r})$ for all repeat loan transactions according to the number of years in between transactions.

# Figure 6: Histogram of spread changes sorted on lead arranger

**Panel A: Same lead arranger**



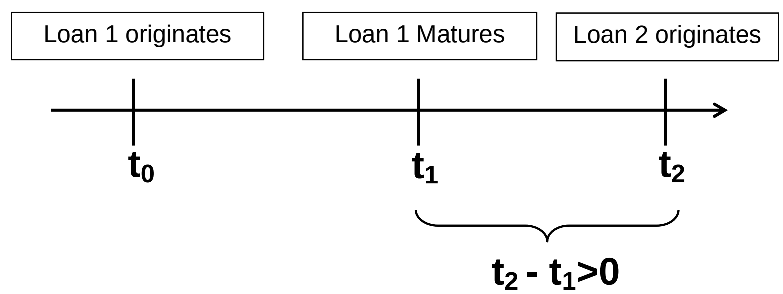

**Panel B: Different lead arranger**



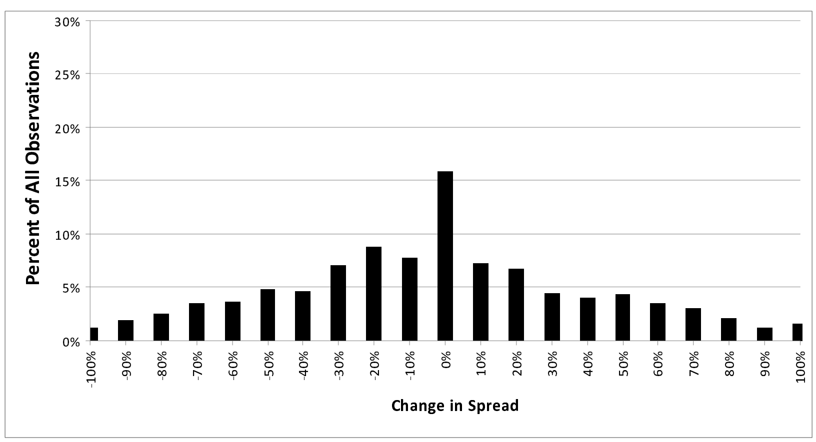

This figure plots the empirical distribution of $\Delta \log(spread_{i,t}) = \log(spread_{i,t}) - \log(spread_{i,r})$ for all repeat loan transactions according to whether the repeat loan has the same or different lead arranger than the previous loan. All plots use only repeat deals completed within four years of the reference transaction.

# Figure 7: Histogram of spread changes for "New Deals"

**Panel A: Timeline**




**Panel B: Empirical distribution**




Panel A of this figure illustrates the timeline we use for defining "new" loans, i.e., loans where the origination date of the second loan is after the maturity date of the first loan. Panel B plots the empirical distribution of $\Delta \log(spread_{i,t}) = \log(spread_{i,t}) - \log(spread_{i,r})$ for all repeat loan transactions following the timing structure mentioned in Panel A. This plot uses only repeat deals completed within four years of the reference transaction.

# Table 1: Summary statistics

This table provides summary statics for the master sample (Panel A), and 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). 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 | 18360 | 1480 | 4670 | 30 | 30 | 90 | 301 | 1000 | 3110 |
| Assets | 18360 | 2036 | 12326 | 209 | 28 | 85 | 305 | 1061 | 3360 |
| Leverage | 18360 | 0.13 | 2.02 | 0.16 | -0.25 | -0.11 | 0.05 | 0.22 | 0.45 |
| ROA | 18360 | -0.01 | 0.99 | 0 | -0.11 | -0.04 | 0 | 0.06 | 0.12 |
| Tranche Amount | 18360 | 204 | 532 | 100 | 5 | 17 | 65 | 200 | 500 |
| Spread | 18360 | 235 | 172 | 250 | 60 | 130 | 225 | 300 | 400 |
| **Panel B: Sample of repeat loans** | | | | | | | | | |
| Sales | 6951 | 1780 | 5100 | 30 | 60 | 155 | 462 | 1350 | 3970 |
| Assets | 6951 | 2243 | 11012 | 384 | 60 | 162 | 483 | 1447 | 4052 |
| Leverage | 6951 | 0.11 | 0.36 | 0.16 | -0.22 | -0.09 | 0.05 | 0.22 | 0.44 |
| ROA | 6951 | 0 | 0.11 | 0 | -0.08 | -0.04 | 0 | 0.05 | 0.11 |
| Tranche Amount | 6951 | 245 | 473 | 100 | 10 | 35 | 100 | 275 | 585 |
| Spread | 6951 | 216 | 147 | 225 | 55 | 125 | 200 | 275 | 355 |

# 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 | -0.03 | 0.16 | -0.21 | -0.15 | -0.01 | 0.09 | 0.13 |
| Public | -0.02 | 0.09 | -0.12 | -0.11 | 0.00 | 0.03 | 0.09 |
| Log(Sales) | -0.03 | 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 | 0.14 | 0.10 | 0.03 | 0.06 | 0.15 | 0.20 | 0.26 |
| ROA | -0.33 | 0.21 | -0.57 | -0.50 | -0.34 | -0.15 | 0.01 |
| Log(Tranche Amount) | -0.08 | 0.02 | -0.10 | -0.09 | -0.08 | -0.06 | -0.05 |
| Maturity | -0.18 | 0.15 | -0.37 | -0.28 | -0.23 | -0.06 | -0.01 |
| # of lenders | 0.02 | 0.33 | -0.31 | -0.23 | 0.07 | 0.20 | 0.40 |
| Collateral | 0.53 | 0.11 | 0.41 | 0.47 | 0.50 | 0.59 | 0.63 |
| Covenants | -0.03 | 0.14 | -0.15 | -0.06 | -0.01 | 0.03 | 0.08 |
| Performance Pricing | -0.13 | 0.11 | -0.24 | -0.19 | -0.13 | -0.11 | -0.01 |
| **Panel B: $t$ statistics** | | | | | | | |
| Commercial paper rating | -0.09 | 1.44 | -1.98 | -1.44 | -0.18 | 1.22 | 2.09 |
| Public | -0.26 | 1.32 | -1.93 | -1.25 | -0.06 | 0.40 | 1.02 |
| Log(Sales) | -2.23 | 1.84 | -5.33 | -2.90 | -2.43 | -0.93 | 0.13 |
| Log(Assets) | -0.66 | 1.30 | -2.58 | -1.28 | -0.80 | 0.57 | 0.95 |
| Leverage | 3.15 | 1.87 | 0.84 | 1.80 | 3.37 | 4.19 | 5.71 |
| ROA | -2.27 | 2.22 | -4.33 | -3.49 | -2.92 | -1.43 | 0.25 |
| Log(Tranche Amount) | -4.60 | 1.68 | -6.89 | -5.42 | -4.33 | -3.49 | -2.77 |
| Maturity | -2.08 | 1.65 | -3.86 | -3.73 | -2.24 | -0.51 | -0.14 |
| # of lenders | 0.12 | 1.35 | -1.50 | -1.08 | 0.42 | 0.93 | 1.60 |
| Collateral | 10.22 | 3.12 | 6.28 | 8.12 | 9.99 | 11.83 | 14.87 |
| Covenants | -0.47 | 1.38 | -2.78 | -1.26 | -0.21 | 0.60 | 1.09 |
| Performance Pricing | -4.06 | 2.00 | -6.84 | -5.46 | -4.12 | -3.11 | -1.26 |
| **Panel C: Other** | | | | | | | |
| Observations | 1002 | 325 | 482 | 827 | 1020 | 1220 | 1275 |
| Adjusted $R^2$ | 0.63 | 0.10 | 0.48 | 0.55 | 0.65 | 0.69 | 0.74 |

## 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, *Revolvers* (tranches labeled "Revolver <1 year", "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 $t$. *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 $r$. *Observations* is the number of observations per regression.

| | (1)<br>All<br>Log(Spread) | (2)<br>Revolver<br>Log(Spread) | (3)<br>Term Loan<br>Log(Spread) |
|---|---|---|---|
| Predicted spread | 0.99*** | 0.99*** | 0.98*** |
| | (111.29) | (102.75) | (27.37) |
| Spread evolution | 0.14*** | 0.14*** | 0.17*** |
| | (9.67) | (8.89) | (4.32) |
| Previous residual | 0.17*** | 0.15*** | 0.23*** |
| | (9.79) | (7.63) | (5.67) |
| Constant | 0.07 | 0.05 | 0.09 |
| | (1.53) | (1.00) | (0.43) |
| Observations | 6951 | 5423 | 1528 |
| $R^2$ | 0.701 | 0.711 | 0.491 |

$t$ 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 $r$. 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 ratings have increased more than one notch since their previous loans. Similarly, column two shows results for firms whose ratings have increased exactly one notch, column three shows results for firms whose debt ratings have not changed, and so on. The loan sample for these regressions is limited to only repeat loans made within four years of one another. For variable definitions see Table 3.

**Panel A: Percent change in average spread**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| $p_t - p_{t-}$ | $\Downarrow$ >50% | $\Downarrow$ ≤50% | $\Uparrow$ ≤50% | $\Uparrow$ >50% |
| | Log(Spread) | Log(Spread) | Log(Spread) | Log(Spread) |
| Predicted spread | 0.95*** | 1.01*** | 1.02*** | 1.01*** |
| | (30.62) | (94.18) | (54.60) | (22.15) |
| Spread evolution | 0.12 | 0.21*** | 0.18*** | −0.08 |
| | (1.21) | (4.19) | (2.80) | (−1.50) |
| Previous residual | 0.07 | 0.25*** | 0.23*** | 0.14** |
| | (0.61) | (5.11) | (3.28) | (2.27) |
| Constant | 0.29 | −0.05 | −0.12 | −0.18 |
| | (1.56) | (−0.92) | (−1.17) | (−0.75) |
| Observations | 587 | 2899 | 2085 | 456 |
| $R^2$ | 0.658 | 0.739 | 0.675 | 0.608 |

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

**Panel B: Change in debt rating**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Rating Change | $\Uparrow$ >1 notch | $\Uparrow$ 1 notch | No Change | $\Downarrow$ 1 notch | $\Downarrow$ >1 notch |
| | Log(Spread) | Log(Spread) | Log(Spread) | Log(Spread) | Log(Spread) |
| Predicted spread | 0.90*** | 0.96*** | 0.95*** | 0.96*** | 0.93*** |
| | (16.58) | (31.17) | (64.76) | (28.84) | (29.22) |
| Spread evolution | 0.24*** | 0.22*** | 0.21*** | 0.29*** | 0.09* |
| | (2.72) | (2.79) | (6.02) | (3.43) | (1.74) |
| Previous residual | 0.07 | 0.15 | 0.20*** | −0.03 | 0.05 |
| | (0.67) | (1.59) | (4.91) | (−0.25) | (0.89) |
| Constant | 0.42 | 0.18 | 0.20*** | 0.25 | 0.40** |
| | (1.40) | (1.10) | (2.65) | (1.44) | (2.27) |
| Observations | 217 | 315 | 1332 | 268 | 317 |
| $R^2$ | 0.678 | 0.782 | 0.824 | 0.800 | 0.749 |

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

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

Panel A of 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. Panel B presents results for the second-stage regression with additional regressors for interactions of the *Spread evolution* and *Previous residual* terms with dummy variables for the time between repeat loans. Results are presented for *All* repeat loans, and for sub-samples of repeat loans sorted by tranche-type, including *Revolvers* and *Term loans*.

**Panel A: Regressions sorted on years between loans**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Years b/t Loans | $\leq 1$ | $1-2$ | $2-3$ | $3-4$ | $4-5$ | $\geq 5$ |
| | Log(Spread) | Log(Spread) | Log(Spread) | Log(Spread) | Log(Spread) | Log(Spread) |
| Predicted spread | 0.98*** | 0.98*** | 0.99*** | 1.06*** | 0.99*** | 1.03*** |
| | (61.41) | (67.85) | (45.07) | (42.31) | (27.21) | (19.70) |
| Spread evolution | 0.25*** | 0.17*** | 0.13*** | 0.11*** | 0.06 | 0.05 |
| | (6.81) | (6.00) | (4.08) | (2.84) | (1.32) | (1.10) |
| Previous residual | 0.18*** | 0.18*** | 0.21*** | 0.07 | 0.04 | 0.02 |
| | (4.40) | (5.30) | (5.51) | (1.57) | (0.67) | (0.44) |
| Constant | 0.10 | 0.10 | 0.06 | −0.32** | 0.06 | −0.18 |
| | (1.21) | (1.31) | (0.50) | (−2.42) | (0.32) | (−0.65) |
| Observations | 1994 | 2121 | 1276 | 639 | 389 | 532 |
| $R^2$ | 0.710 | 0.713 | 0.718 | 0.748 | 0.684 | 0.597 |

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

**Panel B: Regressions by tranche type with time between loan interactions**

| | (1) | (2) | (3) |
|---|---|---|---|
| | All | Revolver | Term Loan |
| | Log(Spread) | Log(Spread) | Log(Spread) |
| Predicted spread | 0.99*** | 1.00*** | 0.98*** |
| | (114.23) | (104.76) | (27.58) |
| Spread evolution | 0.05 | 0.07** | −0.03 |
| | (1.53) | (1.98) | (−0.46) |
| Spread evolution × 1 Years b/t Loans | 0.22*** | 0.16*** | 0.53*** |
| | (4.59) | (3.24) | (4.38) |
| Spread evolution × 2 Years b/t Loans | 0.13*** | 0.10** | 0.24** |
| | (3.28) | (2.38) | (2.39) |
| Spread evolution × 3 Years b/t Loans | 0.08* | 0.06 | 0.15* |
| | (1.90) | (1.26) | (1.65) |
| Spread evolution × 4 Years b/t Loans | 0.03 | −0.01 | 0.17* |
| | (0.59) | (−0.20) | (1.66) |
| Previous residual | 0.04 | 0.01 | 0.13* |
| | (1.08) | (0.17) | (1.67) |
| Previous residual × 1 Years b/t Loans | 0.13** | 0.14** | −0.02 |
| | (2.39) | (2.26) | (−0.13) |
| Previous residual × 2 Years b/t Loans | 0.13*** | 0.17*** | 0.00 |
| | (2.68) | (3.03) | (0.00) |
| Previous residual × 3 Years b/t Loans | 0.17*** | 0.18*** | 0.19* |
| | (3.19) | (3.01) | (1.68) |
| Previous residual × 4 Years b/t Loans | 0.07 | 0.10 | −0.01 |
| | (1.26) | (1.57) | (−0.04) |
| Constant | 0.04 | 0.02 | 0.06 |
| | (0.86) | (0.32) | (0.32) |
| Observations | 6951 | 5423 | 1528 |
| $R^2$ | 0.707 | 0.716 | 0.516 |

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

# Table 6: Second-stage regression sorted on loan differences

The first two columns of this table show 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. The final two columns present results sorted on whether the repeat loan has the same or different loan purpose from from its previous loan. For all regressions, the sample used is limited to only repeat loans made within four years of one another. For variable definitions see Table 3.

| | (1)<br>Same Lead<br>Log(Spread) | (2)<br>Diff Lead<br>Log(Spread) | (3)<br>Same Purpose<br>Log(Spread) | (4)<br>Diff Purpose<br>Log(Spread) |
|---|---|---|---|---|
| Predicted spread | 1.00*** | 0.98*** | 0.99*** | 0.98*** |
| | (93.49) | (67.27) | (72.45) | (76.11) |
| Spread evolution | 0.28*** | 0.13*** | 0.23*** | 0.15*** |
| | (9.70) | (6.44) | (7.46) | (7.12) |
| Previous residual | 0.20*** | 0.15*** | 0.22*** | 0.15*** |
| | (6.48) | (5.54) | (6.56) | (5.83) |
| Constant | −0.01 | 0.10 | 0.01 | 0.10 |
| | (−0.14) | (1.29) | (0.12) | (1.46) |
| Observations | 2668 | 3362 | 2132 | 3332 |
| $R^2$ | 0.748 | 0.689 | 0.754 | 0.691 |

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

## Table 7: Firm performance sorted on rating changes and aggregate spread changes

Panel A of this table reports results for regressions of firm performance on a dummy variable that equals one if aggregate spreads have decreased (increased) more than 25% since the last time firm borrowed, *Spreads Fell* (*Spreads Rose*), and debt rating-year-tranche type fixed effects. Firm performance 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 ($Sales_t = \text{SALEQ}_t - \text{SALEQ}_{t-4}$), investment ($Investment_t = \text{CAPXY}_t/\text{ATQ}_{t-4}$), returns ($Return_t = \log(\text{PRCCQ}_t) - \log(\text{PRCCQ}_{t-1})$), return on assets ($ROA_t = \text{OIADPQ}_t/\text{ATQ}_{t-4}$), and future returns ($Future\ Return_t = \log(\text{PRCCQ}_{t+1}) - \log(\text{PRCCQ}_t)$). All performance measures are calculated in the quarter previous to when the loan is obtained except for future returns which are calculated in the following quarter. Panel B reports results for regressions similar to Panel A except instead of dummy variables for aggregate spread increases and decreases the regressors of interest are dummy variables for if a firm's credit rating has been upgraded (downgraded) a full rating (e.g., A to B) since the time of their previous loan.

**Panel A: Aggregate spread changes**

| | (1)<br>Log(Spread) | (2)<br>M/B | (3)<br>Earnings | (4)<br>Sales | (5)<br>Investment | (6)<br>ROA | (7)<br>Return | (8)<br>Future Returns |
|---|---|---|---|---|---|---|---|---|
| Spreads Rose | −0.08*** | 0.03 | −0.02 | −0.06*** | −0.05** | −0.05 | 0.14 | −0.21 |
| | (−5.01) | (0.50) | (−1.57) | (−4.55) | (−2.00) | (−1.42) | (0.46) | (−0.62) |
| Spreads Fell | 0.06*** | 0.13*** | −0.02 | −0.03** | −0.05 | −0.05 | 0.27 | 0.31 |
| | (4.13) | (2.60) | (−1.32) | (−2.39) | (−1.10) | (−1.02) | (0.91) | (0.90) |
| Constant | 4.86*** | 13.20*** | 0.03* | 0.14*** | 0.10** | 0.08* | 0.36*** | −0.01 |
| | (870.29) | (677.71) | (1.83) | (29.11) | (2.40) | (1.75) | (3.10) | (−0.06) |
| Observations | 8687 | 5516 | 7214 | 7409 | 6947 | 7157 | 6721 | 6710 |
| $R^2$ | 0.751 | 0.169 | 0.008 | 0.059 | 0.009 | 0.008 | 0.038 | 0.055 |

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

**Panel B: Rating changes**

| | (1)<br>Log(Spread) | (2)<br>M/B | (3)<br>Earnings | (4)<br>Sales | (5)<br>Investment | (6)<br>ROA | (7)<br>Return | (8)<br>Future Returns |
|---|---|---|---|---|---|---|---|---|
| Downgrade | −0.08*** | −0.05 | −0.03* | −0.06*** | −0.06* | −0.06 | −0.43* | −0.19 |
| | (−5.22) | (−0.81) | (−1.71) | (−4.09) | (−1.82) | (−1.48) | (−1.68) | (−0.54) |
| Upgrade | 0.15*** | 0.14** | −0.03 | 0.01 | −0.07 | −0.08 | 0.15 | 0.26 |
| | (7.69) | (2.13) | (−1.26) | (0.58) | (−1.00) | (−1.01) | (0.44) | (0.71) |
| Constant | 4.85*** | 13.22*** | 0.03* | 0.13*** | 0.10** | 0.08* | 0.46*** | 0.01 |
| | (948.83) | (764.33) | (1.85) | (30.66) | (2.41) | (1.77) | (4.37) | (0.09) |
| Observations | 8687 | 5516 | 7214 | 7409 | 6947 | 7157 | 6721 | 6710 |
| $R^2$ | 0.752 | 0.169 | 0.008 | 0.059 | 0.009 | 0.008 | 0.038 | 0.055 |

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

## Table 8: 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 of 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 depend upon spreads 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 variable definitions see Table 3.

| | (1)<br>All<br>Log(Spread) | (2)<br>Same Year<br>Log(Spread) | (3)<br>Same Month/Year<br>Log(Spread) |
|---|---|---|---|
| Predicted spread | 0.99*** | 0.99*** | 1.06*** |
| | (111.83) | (22.44) | (12.85) |
| Spread evolution | 0.14*** | 0.15** | 0.15 |
| | (9.52) | (2.11) | (1.19) |
| Previous residual | 0.17*** | 0.20** | 0.12 |
| | (9.46) | (2.51) | (0.78) |
| Constant | 0.07 | 0.06 | −0.36 |
| | (1.54) | (0.25) | (−0.86) |
| Observations | 6951 | 417 | 69 |
| $R^2$ | 0.701 | 0.684 | 0.743 |

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

## Table 9: Second-stage regression using only new deals

This table presents second-stage regression results for the sub-sample of our repeat loan master sample which excludes all repeat loans where the origination date of the second loan is after the maturity date of the first loan (see Figure 7). The sample used is limited to only repeat loans made within four years of one another. For variable definitions see Table 3.

| | New Deals<br>Log(Spread) |
|---|---|
| Predicted spread | 0.98*** |
| | (21.41) |
| Spread evolution | 0.11** |
| | (2.00) |
| Previous residual | 0.08 |
| | (1.27) |
| Constant | 0.13 |
| | (0.53) |
| Observations | 550 |
| $R^2$ | 0.551 |

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