Essays on Financial Contracting and Asymmetric Information

by

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For God.

> "You are my God, and I will give thanks to you;
>
> you are my God; I will extol you.
>
> Oh give thanks to the LORD, for he is good;
>
> for his steadfast love endures forever!"

Psalm 118:28-29 (English Standard Version)

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A.1 **Variable Definitions**
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B.1 **Variable Definitions**
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Figure

1.1 **The Distribution of Debt/EBITDA One Year Prior to Getting Rated**
This figure presents a kernel density of the sample Debt/EBITDA ratio one year prior to getting rated. The shaded areas indicate regions where issuers are approaching a salient Debt/EBITDA threshold and thus have a high incentive to improve along this dimension, as described in Section 1.3. In the empirical tests, I refer to the shaded regions as high-incentive zones (*H-I Zones*). 28

1.2 **Issuer Characteristics by Whether the Firm Is in the High-Incentive Zone**
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1.4 **Likelihood of Cutting Investment as a Function of Distance to a Threshold**
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2.1 **Performance Pricing Grid.**
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ABSTRACT

Essays on Financial Contracting

by

Taylor Begley

Chair: Associate Professor Amiyatosh Purnanandam

This dissertation comprises three essays on financial contracting and intermediation under information asymmetry. They examine both some of the asymmetric information problems that contracts can mitigate as well as some of the distortions they can cause. The first essay examines real investment distortions that occur as a result of information intermediation through credit ratings. Credit Rating Agencies emphasize the importance of specific financial ratio thresholds in their rating process. Firms on the favorable side of these thresholds are more likely to receive higher ratings than similar firms that are not. I show that firms near these salient thresholds respond to the incentive to improve their appearance on this hard, easy to interpret dimension by distorting real investment activities during periods leading up to bond issuance. Specifically, I find that these firms are significantly more likely to reduce R&D and SG&A expenditures and, subsequently, are more likely to experience declines in profitability and innovation output than observationally similar firms not near a threshold. These distortions highlight an important cost of arms-length financing and an adverse consequence of transparency about credit rating criteria.

The second essay examines how high-quality borrowers use the design of performance-sensitive bank debt contracts to alleviate financial constraints. I show that borrowers use a

convex pricing grid (i.e., a contract where the increase in the loan spread following a decline in performance exceeds the decrease in the spread following a performance improvement) to signal their unobservable creditworthiness and receive better bank loan terms. I find that constrained firms who use convex pricing grids receive loans that are 21-28% larger with a spread that is 31-37 basis points lower than observationally similar borrowers who use fixed spread loans. Consistent with the notion that a costly signal should positively correlate with future financial health, I find that constrained borrowers who use a loan with a convex pricing grid are one third less likely to experience financial distress during the term of their loans.

The third essay examines the role of security design in mitigating informational frictions in securitization markets and is joint work with Amiyatosh Purnanandam. Using a representative sample of residential mortgage-backed security deals from the pre-crisis period, we show that deals with a higher level of equity tranche have a significantly lower foreclosure rate that cannot be explained away by the underlying loan pool's observable credit risk factors. The effect is concentrated within pools with a higher likelihood of asymmetric information between deal sponsors and potential buyers of the securities. Further, securities that are sold from high-equity-tranche deals command higher prices conditional on their credit ratings. Our study provides the first in-depth analysis of the effectiveness of the equity tranche in mitigating informational frictions in this market.

CHAPTER I

The Real Costs of Corporate Credit Ratings

1.1 Abstract

Credit rating agencies emphasize the importance of specific financial ratio thresholds in their rating process. Firms on the favorable side of these thresholds are more likely to receive higher ratings than similar firms that are not. I show that firms near these salient thresholds respond to the incentive to improve their appearance on this dimension by distorting real investment activities during periods leading up to bond issuance. These firms are significantly more likely to reduce R&D and SG&A expenditures compared to observationally similar firms not near a threshold. Subsequently, they are more likely to experience declines in innovation output, profitability, and Tobin's Q. These distortions highlight an important cost of arms-length financing and an adverse consequence of transparency in credit rating criteria.

1.2 Introduction

Arms-length financing allows firms to access a deeper pool of capital and provides investors with a broader range of investment opportunities. Information intermediaries, such as credit rating agencies (CRAs), facilitate such transactions by mitigating the inherent information asymmetry between these two groups. CRAs bridge this gap by aggregating several pieces of information about a firm into a single measure of creditworthiness. If firms know that CRAs weight specific criteria more than others in the aggregation, they may have an incentive to reallocate some of their resources toward these dimensions to achieve a better rating. Indeed, theoretical models such as Holmstrom and Milgrom (1991) show that agents distort their behavior when they know they will be evaluated based on specific, easily measurable dimensions.[1] Moreover, survey evidence shows that credit ratings are a key focus for CFOs and that the majority of managers are willing to forgo positive NPV projects to meet short-term financial objectives (Graham and Harvey, 2001; Graham et al., 2005). Building on these ideas, I ask the following question in this paper: Do firms respond to credit rating criteria by distorting their investment behavior at the expense of long-run performance?

I investigate this question by examining firms' investment behavior during periods when their rating is arguably most important to them: prior to bond issuance. The identification of rating-induced distortions, however, is difficult because a number of confounding factors could affect the firms' investment policies during these time periods. For example, a reduction in R&D expenditures in periods leading up to bond issuance could be driven by changes in a firm's product life cycle or investment opportunity set. Hence, a simple examination of changes in investment during periods prior to issuance stands little chance of separating firms' endogenous response to credit rating criteria from other potential factors. To overcome this empirical challenge, I use an institutional feature of the credit rating process that induces cross-sectional variation in the incentives for issuers to improve on a particular dimension that CRAs emphasize, which I describe below.

The CRAs publicly release guidelines and methodologies with specific criteria that they focus on when assessing a given firm's creditworthiness. One primary criterion relates to the firm's Debt/EBITDA ratio. CRAs publish mappings from Debt/EBITDA ratio to po-

[1] Holmstrom and Milgrom (1991) show that principal-agent contracting frictions go beyond the tension between incentives for effort provision and risk sharing. They show that contracts written on easily-measured dimensions (e.g., manufacturing output quantity) can lead to an overprovision of effort by the agent on these dimensions at the expense of more difficult to measure dimensions (e.g., output quality) that are important to the principal (see also Baker, 1992). The theoretical models of Hermalin and Weisbach (2012) and Edmans et al. (2013) illustrate a similar friction in the context of increased disclosure.

tential credit ratings which have jumps at particular ratio thresholds (see Table 1.1). These thresholds–which the CRAs arbitrarily place at round numbers such as 2.0 and 3.0–are unlikely to systematically coincide with changes in drivers of optimal investment policy. They do, however, generate cross-sectional variation in firms' incentives to improve their Debt/EBITDA ratio in the periods leading up to getting a bond rated. Firms in regions near thresholds the year prior to issuance, which I refer to as *High-Incentive (H-I) Zones*, face a high expected marginal benefit from Debt/EBITDA improvement. To the extent that improvement in the ratio is costly, these firms also face a lower immediate cost to cross a threshold relative to firms farther away.

In my analysis, I compare the pre-issuance investment behavior and post-issuance performance of firms near a salient threshold to firms that are farther away.[2] The identifying assumption is that these two sets of firms face different levels of incentives to improve their ratio while they remain similar on unobserved dimensions that drive optimal investment policy. The presence of multiple economically arbitrary thresholds in my sample produces an alternating sequence of "treatment" (higher incentive to improve Debt/EBITDA) and "control" (lower incentive) groups throughout the Debt/EBITDA spectrum which lends credibility to this assumption. In addition, I show that the two groups are well matched on other observable factors that potentially drive investment. This research design allows me to pin down whether firms in *H-I Zones* respond to the rating criteria by distorting their investments in innovation (R&D) and organizational capital (SG&A) in the periods leading up to getting a bond rated as compared to firms that are away from the thresholds.

Reducing these investments in long-term intangible assets provides the immediate benefit of boosting EBITDA, while the costs of forgone investments are borne in the future. This

[2]Consider the salient thresholds at Debt/EBITDA=1.5, 2.0, and 2.5 as an illustrative example (there are six such thresholds in my sample). For the Debt/EBITDA threshold of 2.0, I classify firms with Debt/EBITDA $\in [1.95, 2.20]$ a year prior to issuance as being in the treatment group (*H-I Zone*). I classify firms with Debt/EBITDA $\in [1.70, 1.95]$ and $[2.20, 2.45]$ as being in the control group since they do not fall in the *H-I Zone* around 2.0 or either of the adjacent thresholds. The timing of the measurement captures the notion that firms typically recognize their financing needs in advance and then face incentives to take actions in the periods leading up to issuance to conform to the rating criteria. Section 1.3 provides a more thorough description of the classification process and its underlying rationale.

fundamental tension between benefits now and costs later provides incentives for myopic managerial behavior (Narayanan, 1985; Stein, 1989). In my empirical tests, I first examine the effect of ratings-induced incentives on R&D and SG&A investments and then examine the long-run consequences in terms of future innovation output, profitability, and Tobin's Q.

I find that *H-I Zone* issuers are about 40% more likely to reduce R&D and 10% more likely to reduce SG&A expenditures prior to issuance than observationally similar control firms. In terms of the size of the reductions, these firms cut their R&D expenditures by 10% and SG&A expenditures by 3% relative to control firms. After documenting the average treatment effect, I estimate the impact of rating criteria on investment behavior as a continuous function of a firm's distance to a threshold. As the distance to a salient threshold increases, firms face a lower expected marginal benefit from improving their Debt/EBITDA ratio and higher total cost to reach the next threshold. Thus, the overall incentive to reduce these investments diminishes as the distance increases. The results support this notion.

The economic benefit of appearing strong on CRA-emphasized criteria is larger during periods of high yield spreads between ratings classes. Consistent with this view, I show that the main effects discussed above are strongest for periods with high credit spreads. During high-credit-spread periods, defined as above sample median Baa-Aaa spread, the likelihood of reducing investments increases by about 30% for R&D expenditures and 80% for SG&A expenditures over the baseline estimates. These results lend further credence to my main claim that economic incentives driven by credit rating criteria lead to distortions in firm behavior.

While my results so far establish a link between credit rating criteria and investment behavior, they are silent about the long-run performance effects. Standard and Poor's (S&P) recognize the potential distortions that ratings can create and state the following in their rating methodology handbook (Standard and Poor's, 2008) [emphasis added]:

> "We do not encourage companies to manage themselves with an eye toward a specific rating. The more appropriate approach is to operate for the good of the

business as management sees it and to let the rating follow. *Ironically, managing for a very high rating can sometimes be inconsistent with the company's ultimate best interests, if it means being overly conservative and forgoing opportunities.*"

In my next set of tests, I examine firms' post-issuance innovation, profitability, and firm value to study the long-run consequences of the investment changes. First, I focus on innovation because of its long-term nature, its connection to R&D, and because it is an important driver of firm value (Hall et al., 2005) and overall economic growth (Solow, 1957; Romer, 1990). I find a reduction in the raw quantity of patents produced for the first year after bond issuance for *H-I Zone* issuers, though the effects are short-lived. I next consider patent citations, which are widely considered a better measure of the quality and impact of innovation (see, e.g., Griliches, 1990; Trajtenberg, 1990). I find that issuers near the salient thresholds are about 25% more likely than control firms to see declines in patent citations. This effect persists for multiple years following bond issuance. These results suggest that although declines are not great in the quantity of patents produced, firms facing stronger ratings-induced incentives to improve their Debt/EBITDA ratio have a considerably higher likelihood of declines in the quality of their innovation output. I find similar results for future profitability. Treatment firms are about 12% and 10% more likely to experience declines in ROA (operating income/assets) and ROE (net income/shareholders equity) during the years following issuance than the control group.

To more directly examine the consequences for firm value, I compute the differential changes in industry-adjusted Tobin's Q between treatment and control firms for four years following issuance. The difference-in-differences estimates indicate a treatment effect of a 1.8% decline in industry-adjusted Q in the first year following issuance, which grows to an approximately 3-3.6% decline by year four. Combined with the results on innovation, this decline is consistent with Hall et al. (2005) who find that when a firm's quality of patents increase such that their average patent receives an additional citation, the firm's market value increases by 3%. Overall, these results show that there are real, long-term consequences as a result of incentives to look strong on credit rating criteria in the short term.

Finally, I examine how market participants interpret the issuers' changes in investment behavior around the thresholds. After confirming that crossing a salient threshold is associated with improvements in credit rating, I test whether the reductions in investment around the thresholds are penalized by the CRAs or bond investors by a lower likelihood of rating upgrade or higher at-issuance yields. I find no such evidence.

This paper contributes to several strands of literature. First, it relates to the literature that highlights the importance of credit ratings for firm financial policies. Kisgen (2006) shows that firms issue less debt when they are near a credit rating upgrade or downgrade. Hovakimian et al. (2009) and Kisgen (2009) show that firms' financial decisions are consistent with credit rating "targeting." While these papers show that credit ratings have a significant influence on capital structure decisions, my paper focuses on investment decisions. Moreover, this is the first paper to show that firms respond to credit rating *criteria* by distorting behavior on value-relevant dimensions, such as R&D investment, in efforts to look strong on the dimensions emphasized by the CRAs.

This paper also relates to the literature examining the nature of information and the tradeoffs that arise as the informational distance between contracting parties increases. With greater distance between borrower and lender, the incentives to produce soft information declines and lenders rely more on hard information (Stein, 2002; Petersen, 2004; Berger et al., 2005).[3] The use of hard information facilitates arms-length transactions and can provide firms with greater access to capital (Faulkender and Petersen, 2006b). However, as discussed earlier, Holmstrom and Milgrom (1991) show that high-powered contracts based on easily measurable outputs can have undesirable incentive effects. This issue frequently arises in the context of measuring educational outcomes with the concern that teachers may

[3] Rajan et al. (forthcomingb) show that as the mortgage market transitioned from an originate-and-hold to originate-to-distribute model, loan originators relied more on hard information such as FICO score and loan-to-value ratios for setting interest rates on loans. Liberti and Mian (2009) show that within a large bank, the sensitivity of loan terms to hard, objective information is greater as the hierarchical distance between the loan officer and the ultimate decision maker increases.

have incentives to "teach to the test."[4] In the context of this paper, the "contract" between the issuer and CRA puts weight on the hard information dimension of Debt/EBITDA and the issuer endogenously responds by focusing resources on improving this measure at the expense of investments in innovation and organizational capital, which are likely to have a large soft information component.

Finally, this paper also relates to the literature that explores potential adverse effects of increased information disclosure. Hirshleifer (1971) shows that more information can destroy ex-ante welfare-improving risk sharing opportunities and Dang et al. (2012) show that increased information production can hinder liquidity in money markets. Recent work on disclosure by Hermalin and Weisbach (2012) and Edmans et al. (2013) highlights some costs of providing more information to investors through increased disclosure. Hermalin and Weisbach (2012) show that increased disclosure can lead to greater agency problems in the form of myopic behavior; managers substitute away from long-term investments to boost short-term numbers (see also Stein, 1989). Edmans et al. (2013) present a theoretical model that shows that an increase in disclosure can produce incentives for managers to improve hard information at the expense of investment. My paper complements these theoretical papers by providing empirical evidence that pressures to appear strong on clearly-delineated rating criteria can lead to investment distortions and to long-run underperformance.

I organize the rest of the paper as follows. Section 1.3 outlines the empirical strategy and Section 1.4 describes the sample. Section 1.5 presents the main results. Section 1.6 presents additional tests and robustness checks and Section 1.7 concludes.

[4] Jacob (2005) shows that teachers in the Chicago Public Schools strategically responded to high-stakes testing by shifting more students into special education, preemptively retaining students and reallocating focus from low-stakes subjects (science and social studies) to high stakes subject (math and reading). Neal (2011) provides a helpful review of this literature.

1.3 Research Design and Identification Strategy

Credit ratings represent an opinion of debt issuers' ability and willingness to repay debt. This information about relative creditworthiness plays an important role in allocating capital to firms in the economy. Credit ratings are a key factor for firms' cost of debt capital because of the informational content they supply to investors (Kliger and Sarig, 2000; Jorion et al., 2005; Tang, 2009) and supply-side frictions induced by ratings-based regulations (Kisgen and Strahan, 2010; Ellul et al., 2011; Chernenko and Sunderam, 2012; Becker and Ivashina, 2013). In addition to their direct impact on the cost and supply of debt for firms in the bond market, benefits of a higher credit rating include better trade credit terms (Klapper et al., 2012), better access to commercial paper markets, overall financial flexibility, and reputational benefits, to name a few. Further, Jorion et al. (2005) show that stock prices have a positive response to ratings upgrades and negative response to downgrades with the effect particularly strong for downgrades (see also Hand et al., 1992; Dichev and Piotroski, 2001). In light of all this, it is not surprising that credit ratings are one of the most important factors affecting firms' financial policies and are a key point of focus for managers (Graham and Harvey, 2001).[5]

In their role as information intermediaries, CRAs condense many different pieces of information into a simple, easy to communicate grade of creditworthiness. Providing a simple measure of debt serviceability and leverage, the Debt/EBITDA ratio is a prominent ratio that CRAs emphasize, and it is the focus in this paper's analysis. In response to this emphasis, firms have an incentive to appear strong on the Debt/EBITDA dimension to economize debt costs.

While there are a number of ways that firms can affect their Debt/EBITDA ratio, I focus on two investment decisions whose payoffs are long term in nature: R&D and SG&A.[6]

[5] See Kisgen (2006) for an extensive discussion of the importance of credit ratings to firms.

[6] SG&A expenditures are often seen as investments in "organizational capital" (e.g., see Lev and Radhakrishnan, 2005; Eisfeldt and Papanikolaou, 2013) and include spending on items such as advertising, information technology, and employee training.

Because they are fully expensed the period in which they occur, reducing these expenditures allows firms to report higher EBITDA and thus have a more favorable Debt/EBITDA ratio. Because these investments in intangible capital generate benefits that are uncertain and may take years to realize, managers may have an incentive to myopically reduce such expenditures to boost EBITDA even if they would be value-increasing in the long run (Narayanan, 1985; Stein, 1989). Graham et al. (2005) provide survey evidence that 80% of managers report they would decrease discretionary expenses such as R&D, advertising, and maintenance and 55% report that they would delay starting a new project – even if it involved sacrificing NPV – in efforts to meet financial targets. Also, by examining R&D expenditures, I am able to measure the consequences of changes in investment behavior by observing future patent performance.

To study the extent to which the incentive to look strong on CRA-emphasized dimensions affects investment, I focus on firms' behavior during periods when they are likely to care about their credit rating the most: prior to bond issuance. When firms recognize there is an upcoming financing need, they can assess where they stand in relation to the CRAs' rating criteria and respond by taking actions to improve that standing.[7] To empirically identify the effects of the credit ratings process on investment, however, is challenging. Consider the following basic model:

$$Investment_{it} = f(X_{it}) + \psi(\textit{ratings-induced incentives}_{it}) + \eta_{it}$$

Even after controlling for observable drivers of investment behavior (X_{it}) of firm i at time t, a näive analysis of changes in firm investment leading up to bond issuance is problematic because the effect of ratings-induced incentives is potentially confounded by multiple unobserved factors (i.e., $Cov(\textit{ratings-induced incentives}_{it}, \eta_{it}) \neq 0$). For example, firms may reduce R&D expenditures simply because they are transitioning from development of a prod-

[7] If they wish to have assistance in this assessment, investment banks and consulting firms provide expert advice and institutional knowledge through their "ratings advisory" services.

uct to commercialization or may reduce SG&A expenditures because they have reached the end of a marketing campaign. To isolate the effect of ratings-induced investment distortions from these and other such factors that influence investment decisions, I exploit multiple discontinuities in the CRAs' mapping from Debt/EBITDA to credit rating which generate cross-sectional variation in incentives for firms to improve their Debt/EBITDA ratio.

CRAs provide specific information about the ranges of Debt/EBITDA that are consistent with different ratings. Table 1.1A presents an excerpt from S&P's published Corporate Rating Criteria that maps an issuer's Debt/EBITDA ratio to a set of credit ratings (Standard and Poor's, 2012). S&P states that their purpose in providing such guidelines is "to make explicit the rating outcomes that are typical for various business risk/financial risk combinations." Moody's and Fitch also place an emphasis on financial ratio thresholds in their published methodologies. Table 1.1B presents an example from the Moody's "Global Steel Industry" rating methodology, which shows what ranges of Debt/EBITDA are consistent with particular credit ratings for that industry.

While these correspondences are not the sole determinant of the final credit rating,[8] Table 1.1B makes clear that it behooves steel firms wishing to get an "A" rating to achieve a Debt/EBITDA ratio below 2.0. Firms are keenly aware of the importance of these key financial ratios for their ratings and, in turn, the importance of their rating for their cost and access to capital.[9]

The key to the research design is the cross-sectional variation in firms' incentives to improve their Debt/EBITDA ratio that is induced by the presence of multiple salient Debt/EBITDA thresholds. Drawn from S&P's, Moody's, and Fitch's ratings methodologies and press releases, the salient bin thresholds in the sample are 1.25, 1.50, 2.0, 2.5, 3.0, 4.0, and 5.0.[10]

[8] Fracassi et al. (2013) show that credit rating analysts' optimism or pessimism can affect ratings decisions. Griffin and Tang (2012a) provide evidence of subjectivity in the ratings for CDOs and its consequences for rating accuracy.

[9] For example, in their 2006 annual report, Textron, Inc. states: "Our credit ratings are predominantly a function of our ability to generate operating cash flow and satisfy certain financial ratios. Since high-quality credit ratings provide us with access to a broad base of global investors at an attractive cost, we target a long-term A rating from the independent debt-rating agencies."

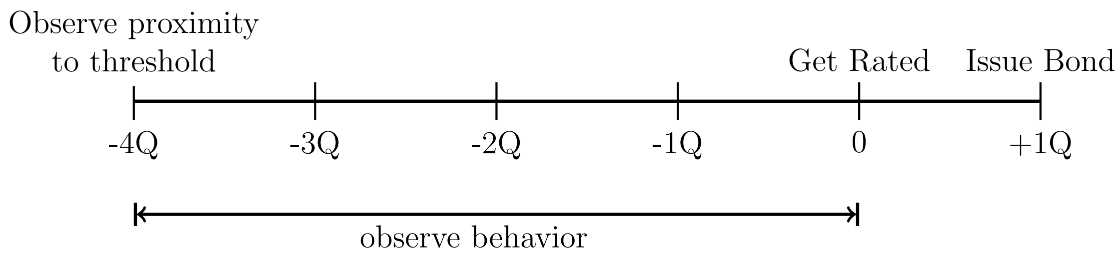
[10] There are some occasions when the rating agencies use guidance for thresholds other than the ones listed

Since the CRAs arbitrarily set these thresholds at round numbers, it is unlikely that the economic primitives that drive optimal investment policy systematically vary at precisely these points throughout the Debt/EBITDA spectrum. That is, for my identification strategy to fail, an omitted variable must drive the optimal R&D policy in this specific alternating sequence around each threshold. While all firms have an incentive to improve and appear strong on this dimension, firms on the cusp of advancing to a better bin face the highest expected marginal benefit from improvement in Debt/EBITDA. To the extent that improvement in the ratio is costly, these firms also face a lower cost since they have less distance to travel to cross a threshold relative to firms farther away.

For the empirical tests, I define whether an issuer is in a High-Incentive Zone (*H-I Zone*) in the following simple way. Consider the threshold at Debt/EBITDA = 2.0. I consider the upper bound of the *H-I Zone* around the 2.0 threshold to be 40% of the distance between 2.0 and the next worse threshold of 2.5. This equals 2.2. To capture the incentives of those with only a slim margin between their current ratio and a worse bin, I consider the lower bound to be 10% of the distance between 2.0 and the next better threshold of 1.5. This equals 1.95. Thus, I consider firms with Debt/EBITDA $\in [1.95, 2.20]$ to be the *H-I Zone* around 2.0. I follow this method for each threshold in the sample based on firms' Debt/EBITDA ratio a year prior to issuance.[11] This timing captures the notion discussed earlier that firms typically recognize their financing needs in advance and then face incentives to take actions in the periods leading up to issuance to conform to the rating criteria. The figure below illustrates the basic timeline of the analysis.

here. The presence of such lesser used thresholds in the sample may introduce noise into the estimation and partially mask the true effect.

[11]The results are robust to reasonable adjustments to this bandwidth scheme.



For each firm, I compare the relevant investments during the year leading up to getting a new bond rated to its investments in the prior year. This first difference removes within-firm time invariant unobserved drivers of investment. The timing of this measurement also ensures that results are not driven by seasonality in firm policies. Next, I take the difference between the behavior of the treatment group (those near one of the salient thresholds) and the control group to compute the average treatment effect. Similarly, I use measures of firms' profitability, innovation output, and Tobin's Q a year prior to getting rated as benchmarks for comparison when I investigate the future performance of the firms. The table below summarizes the empirical design for firm policy Y.

	$t-4$	t	Difference
Near Threshold (*H-I Zone*=1)	$Y_{t-4}^{treatment}$	$Y_t^{treatment}$	$\Delta^{treatment}$
Not Near Threshold (*H-I Zone*=0)	$Y_{t-4}^{control}$	$Y_t^{control}$	$\Delta^{control}$
	Average Treatment Effect:		$\Delta^{treatment} - \Delta^{control}$

In addition to the average treatment effects I estimate using the classification approach described above, I also perform tests that exploit finer variation in incentives using a continuous measure of the issuer's proximity to salient thresholds. Specifically, I estimate the likelihood of reducing investment as a function of the distance between the firm's Debt/EBITDA a year prior to getting rated and the next better threshold (for example, 0.15 for a firm with Debt/EBITDA = 2.15, 2.65, 3.15, etc.).

1.4 Data and Preliminary Tests

1.4.1 Sample Construction

Firm accounting and stock return data are from Compustat and CRSP. Bond issuance data are from the fixed income securities database (FISD). I merge these data to form a quarterly sample from 1990-2009. Where a firm has multiple financing observations in a single quarter (for example, a firm may issue bonds of various tenors on the same day), I combine them to a single observation by summing the issuance amounts and computing a dollar-weighted average yield.

Patent data are from the National Bureau of Economic Research (NBER) Patent Citation database.[12] This data source contains information on the owner, patent application date, patent grant date, and citation count of over three million patents granted by the United States Patent Trademark Office from 1976-2006 along with matching tables that facilitate merging these data with Compustat. I use two common measures of firm innovation: patent count and citation count. Patent count is the raw number of a firm's patent applications during a given year that are eventually granted. Raw counts, however, do not provide any differentiation in whether the innovations are marginal or new breakthroughs. Trajtenberg (1990) argues that "patents vary enormously in their importance or value, and hence, simple patent counts cannot be informative about innovative output" and proposes citation-weighted patent counts are a better measure of innovation. I correct for bias in this measure due to citation count truncation after 2006 by using the weight factors developed by Hall et al. (2001), who use an obsolesce-diffusion model to estimate future citations based on the patent's year and technology category.

After dropping financial firms, utilities, and observations that are not related to a bond issuance, the main sample contains 1770 observations from 686 firms. The sample size for tests using R&D have fewer observations because many firms do not report R&D expen-

[12] See https://sites.google.com/site/patentdataproject/

ditures. Tests involving patent productivity have fewer observations because the patent database ends in 2006. I use the maximum number of observations with complete data for each test, but my results are not substantively different if I constrain all tests to observations with complete data across all variables.

I winsorize all variables at 1% to mitigate the effects of outliers. Table B.1 in the appendix provides the details of the construction of variables. Table 1.2 presents sample summary statistics and Figure 1.1 presents the distribution of the sample along Debt/EBITDA a year prior to issuance, highlighting the *H-I Zones* near salient thresholds.

1.4.2 Comparability of Treatment and Control Groups

Before presenting the main tests, I examine the comparability of the issuers in *H-I Zones* with those that are not. To make meaningful inferences, it is important that issuers in *H-I Zones* (treatment) are observationally similar to those that are not (control) on dimensions that drive investment independent of the incentive effects. To evaluate the comparability, Figure 1.2 presents kernel densities of several such factors for both groups. The plots show that the sample is well balanced along firm characteristics that represent factors such as firm life cycle (size), financial flexibility and potential debt overhang (debt-to-asset ratio), growth opportunities (Tobin's Q), and ability to generate internal cash flows (cash flow-to-assets); it is also balanced in terms of profitability as measured by ROA (operating income/assets) and ROE (net income/shareholder equity). In unreported results, t-tests fail to reject the null of the equality of means and Kolmogorov-Smirnov tests fail to reject the null of equality of distributions for these characteristics across the two groups.

1.5 Results

Firms near salient Debt/EBITDA thresholds face a higher expected marginal benefit from improving on this dimension (i.e., reducing the ratio) because crossing a threshold increases the likelihood of getting a credit rating upgrade.[13] Improving this likelihood gives managers incentives to take actions to increase EBITDA and/or decrease debt. In this section, I exploit the presence of multiple salient Debt/EBITDA thresholds to identify the effect of credit rating criteria on firms' R&D and SG&A investment policies and their subsequent performance.

1.5.1 The Effect of Rating Criteria on Investment

Figure 1.3 is a graphical depiction of the main results. It plots the probability that a firm reduces their R&D investment as a function of their Debt/EBITDA ratio one year prior to getting rated. I group firms based on their proximity to the salient thresholds (*H-I Zones* as outlined in Section 1.3) and plot the mean probability of reducing investment within that group. The alternating nature of the plot highlights the differential behavior of firms that are near salient thresholds from those that are not. For example, about 56% of issuers near the 2.0 threshold reduce their R&D expenditures during the year prior to getting rated, while about 40% and 47% in the adjacent comparison groups not near the threshold (for example, Debt/EBITDA $\approx$ 1.9 and 2.3) do so. This pattern emerges around each salient threshold throughout the Debt/EBITDA spectrum. The pattern for SG&A is similar, though the magnitude of the differences is smaller (not shown).

Table 1.3 aggregates the treatment group (*H-I Zone* issuers) and control group and reports the average probability of reducing their R&D or SG&A investment. The first column indicates that roughly half of the firms in the full sample reduce each type of investment in the year leading up to getting rated. The next two columns highlight the difference in investment behavior between those firms near a salient threshold and those that are not. About 64% of *H-I Zone* firms reduced R&D investment as compared to 45% of control firms. The corresponding figures for reducing SG&A are about 56% for *H-I Zone* firms compared to 51% for the controls. The results indicate that issuers in an *H-I Zone*, which face the

[13] In later tests, I explicitly show that crossing a salient threshold leads to an average rating upgrade of approximately one-fourth of a rating (see Table 1.9).

higher expected marginal benefit of improving their Debt/EBITDA ratio, are about 42% (19 percentage points) more likely to reduce R&D and about 10% (5 percentage points) more likely to reduce SG&A expenditures in the year prior to issuance.

To ensure that any residual differences in the two groups along observable dimensions or time-specific factors are not driving the differences in investment decisions, I estimate the following model.

$$\mathbb{1}\{Cut\ [R\&D, SG\&A]_{i,t\rightarrow t+4}\} = \alpha + \rho(H\text{-}I\ Zone)_{it} + \sum \beta X_{it} + q_t + \epsilon_{it} \tag{1.1}$$

I regress an indicator of whether the issuer cuts investment (R&D, then SG&A) on the issuer's proximity to a salient threshold. I use an indicator of investment reduction for the main specifications because the magnitude of the reduction is likely a function of the distance to the threshold. Firms closest to the threshold have the strongest incentives to alter their behavior, but have to reduce investment by a smaller amount to achieve their goal. Thus, an indicator variable more sharply captures the change in behavior. I also include a vector of firm characteristics (X_{it}) to control for other potential firm-level drivers of investment and year-quarter fixed effects (q_t) to capture any economy-wide fluctuations that could drive investment decisions. I also include specifications with industry fixed effects to ensure that some unobserved industry factor that is correlated with both firms' pre-issuance investments and their proximity to a salient threshold is not driving the results. I estimate regression equation (1.1) using a linear probability model[14] and cluster all standard errors at the firm level.[15] Table 1.4 presents the results.

[14]I present results from estimations using a linear probability model because it does not suffer from the incidental parameters problem in models with fixed effects (conditional logit models rely on stronger assumptions for consistency; see, e.g., Wooldridge (2002)), its parameter estimates are consistent in the face of various forms of heteroskedasticity, and the ease of interpretations of the partial effect estimates. The results are similar using a logistic regression model.

[15]I find similar results when computing White standard errors or clustering by firm, time, or industry. The relative invariance of the standard error estimates across differing clustering structures indicates that any autocorrelation in the right-hand side variables and/or residuals is likely very small and that there is not a meaningful time-specific correlation effect after controlling for year-quarter fixed effects.

The coefficient estimates on *H-I Zone* mirror the results of the simple group mean analysis presented in Table 1.3. Issuers near a salient Debt/EBITDA threshold one year prior to issuance are about 19 percentage points more likely to reduce their R&D expenditures (Columns 1-2) and 6 percentage points more likely to reduce SG&A (Columns 3-4). The striking similarity of the point estimates from the regression analysis in Table 1.4 to the simple differences presented in Table 1.3 supports the notion that the intermittent nature of salient thresholds creates a balanced comparison between firms receiving the high-incentive treatment and the control firms.

The results above highlight the average treatment effect of receiving the high-incentive treatment. The following test exploits heterogeneity in the strength of the treatment by modeling the decision to reduce investment as a function of the issuing firm's distance to the next better Debt/EBITDA threshold. Issuers closest to a salient threshold face the highest expected marginal benefit of an improvement and also the lowest cost because only a relatively small movement is necessary to improve bins. Thus the likelihood of reducing investment should be a decreasing function of distance to the next highest threshold. To test this notion, I estimate the following specification, where *dist* is the distance to the next highest bin (likewise measured a year prior to getting a bond rated) and *dist2* is its square. Columns (1)-(2) in Table 1.5 present the results.

$$\mathbb{1}\{Cut\ Inv_{i,t\rightarrow t+4}\} = \alpha + \delta(dist)_{it} + \gamma(dist2)_{it} + \sum \beta X_{it} + q_t + \epsilon_{it} \tag{1.2}$$

These results indicate that issuers closest to a threshold and thus have the highest incentives to take actions to improve their ratio are most likely to respond by reducing investment. Figure 1.4 uses the coefficient estimates to plot the change in probability of investment reduction. Similar to the baseline results above, the results are stronger for R&D investments.

The next tests examine a continuous measure of changes in investment rather than a discrete outcome of whether firms cuts their investments. Specifically, I re-estimate the baseline regression specification (1.1) using the percent change in investment policy as the dependent variable. Columns (3)-(4) of Table 1.5 present the results. The point estimates on *H-I Zone* indicate that the average firm receiving the high-incentive treatment reduces its investments in R&D and SG&A by about 10% and 3%, respectively. For the median firm with Debt/EBITDA $\in [1.25, 1.50]$, this degree of investment reduction, ceteris paribus, translates to an approximate Debt/EBITDA ratio improvement of 0.07. The effect progressively increases with each Debt/EBITDA bin, with the median firm with Debt/EBITDA $\in [4.0, 5.0]$ achieving an improvement of approximately 0.17.

1.5.2 The Effect When Credit Spreads Are High

The incentives to cross thresholds should be stronger when firms' benefit of improving their credit rating is higher. The economic benefit from an increase in credit rating is greater when the sensitivity of yield spreads to credit quality is high. That is, in high-credit-spread times, the expected benefit of crossing a salient threshold is higher than in low-credit-spread times, and the effects documented above should be stronger. To test this hypothesis, I include in the baseline regression specification an interaction of *H-I Zone* with an indicator, *High Spread*, that equals one when the Baa-Aaa yield spread exceeds the sample period median.

$$\begin{aligned} \mathbb{1}\{Cut\ Inv_{i,t\to t+4}\} &= \alpha + \rho(H\text{-}I\ Zone)_{it} + \psi(H\text{-}I\ Zone)_{it} \times (High\ Spread)_t \\ &\quad + \sum \beta X_{it} + q_t + \epsilon_{it} \end{aligned} \tag{1.3}$$

While there are other differences in the economy that could lead to differential firm investment policies between these two regimes (for example, lower credit spreads could indicate more favorable investment opportunities), the level of such effects will be absorbed by the year-quarter fixed effects–the identifying variation is still in the cross section. Along with the level effects of other macroeconomic shocks, year-quarter fixed effects absorb any level effects of the credit spread on issuers' investment decisions. Table 1.6 presents the results with Columns (1) and (3) presenting the baseline results from earlier for comparison.

For both R&D and SG&A, the point estimate on the interaction of *H-I Zone* and *High Spread* indicates that issuers near salient thresholds are even more likely to respond to incentives to reduce these investments when the economic benefit of a better rating is the higher. Compared to issuers not near a salient threshold a year prior to issuance, issuers in the *H-I Zones* are about 24 percentage points more likely to reduce R&D expenditures and 11 percentage points more likely to reduce SG&A expenditures.

Overall, these results show that issuers with higher incentives to improve their appearance on the CRA-emphasized dimension of Debt/EBITDA ratio are substantially more likely to reduce spending on real investment activities as a means to that end. Further, the likelihood of this response is stronger when the yield spreads between ratings is large and the economic benefit from crossing a salient threshold is greater.

1.5.3 Future Innovation and Profitability

The evidence thus far documents differential changes in investments for firms near salient Debt/EBITDA thresholds as compared to observationally similar control firms. This section investigates whether these firms experience subsequent declines in innovation and profitability. I use the issuer's performance as of one year prior to issuance as the baseline for comparison and construct an indicator variable equal to one when the firm's performance τ years after bond issuance experiences a decline relative to their benchmark and estimate the following specification.

$$\mathbb{1}\{Perf_{i,t+\tau} < Perf_{i,t-1}\} = \alpha + \rho(H\text{-}I\ Zone)_{i,t-1} + \sum \beta X_{i,t-1} + q_t + \epsilon_{it} \tag{1.4}$$

Investment in innovation is an important driver of long-run firm value, but its intangible and long-term nature make it particularly vulnerable to short-term cost cutting. The next tests use future patent productivity to measure the consequences of these reductions for innovation output. I use a per annum raw count of new patents and the patent citation counts over the two years prior to bond issuance as a benchmark for the firm's innovation output. To capture the long-run nature of investment on innovation, I follow the literature (see, e.g., Cornaggia et al., 2013; Seru, 2013) and examine the average innovation output over τ years following the event of interest (bond issuance). Panels A and B of Table 1.7 present the results.[16]

While the sign of the estimated coefficients on *H-I Zone* in Panel A suggest that these firms are more likely to produce less patents in the future, the point estimate is statistically different from zero only in the first year following bond issuance. This result indicates that firms in the *H-I Zone* do see declines in the raw quantity of patents produced, but the effects are relatively short-lived. Panel B presents estimates considering patent citations, which is widely considered a sharper measure of the quality and impact of innovation. These estimates provide evidence that *H-I Zone* issuers are more likely to experience a persistent future decline in innovation. With about 20% of the sample experiencing declines in this measure, the point estimate of about 0.05 indicates that these firms are roughly 25% more likely to see innovation declines that observationally similar firms not near a salient threshold. Together with the results of the tests of raw counts, this suggests though there is not a large decrease in the quantity of patents produced, firms with stronger incentives for improvement in Debt/EBITDA in the short run have a considerably higher likelihood of declines in the quality of their innovation output.

I next examine future operating performance and profitability. To measure operating performance, I use operating income scaled by assets (ROA) as suggested by Barber and Lyon (1996). This ratio measures the productivity of the firm's assets excluding items such

[16]As is typically the case in exercise such as this, the number of observations drop as the time horizon under examination increases.

as interest expense, special items, income taxes, and minority interest. Panel C in Table 1.7 presents the results. The coefficient estimates on *H-I Zone* indicate that issuers near salient Debt/EBITDA thresholds one year prior to issuance are about 5 percentage points (10%, based on the sample mean of about 0.50) more likely to experience persistent future declines in ROA compared to observationally similar issuers that are not.

In Panel D, I consider return on equity (ROE) to focus on future performance from the perspective of shareholders. Computed as the ratio of net income to shareholder equity, this ratio measures how much profit the firm generates with the money shareholders have invested. The estimates indicate that *H-I Zone* firms are more likely to have lower ROE for three years following bond issuance.

1.5.4 Future Tobin's Q

The above results highlight some important long-term consequences in terms of depressed innovation and profitability. To more directly assess the firm value implications, I extend the above analysis to examine future changes in industry-adjusted Tobin's Q (Q^{IA}) for *H-I Zone* firms compared to the control firms not near a salient threshold. I compute Q_i^{IA} by subtracting the SIC 2-digit industry median $Q_t^{Industry}$ from firm Q_{it}. Since the sample is well balanced between the treatment and control firms, I begin by computing a simple difference-in-difference estimate. Panel A of Table 1.8 presents the results.

The first column shows both the treatment and control firms have $Q^{IA} = 0.19$. Each following column computes $\Delta Q^{IA} = Q_{t+\tau}^{IA} - Q_t^{IA}$ for each group for four years following issuance. The difference-in-differences estimates indicate that *H-I Zone* firms have lower Q^{IA} in the years following issuance as compared to observationally similar control firms. While the control firms experience a modest increase in Q^{IA}, the differential performance between the groups is driven more by the falling Q^{IA} of the treatment firms. Based on a sample mean of 1.64, the treatment effect of 0.03 to 0.06 in post-issuance years 1 to 4 translates to a 1.8-3.6% decline in Tobin's Q, though the estimate for year 3 is not statistically significant.

I next turn to regression analysis to make sure that any residual differences between the two groups on observable dimensions or time effects are not driving the results. I estimate the following specification:

$$Q^{IA}_{i,t+\tau} = \alpha + \rho(\textit{H-I Zone})_{it} + \sum \beta X_{it} + q_t + \epsilon_{it} \tag{1.5}$$

Panel B of Table 1.8 presents the results. The regression estimates present a similar picture to the differences computed in Panel A; *H-I Zone* firms experience a decline in Tobin's Q of about 0.03 to 0.05 relative to control firms.

In sum, reducing investment in R&D and SG&A provides the benefit of an improved Debt/EBITDA in the short term, but it ultimately comes at the cost of reduced innovation, profitability, and long-run value. The results in this section support my main claim that firms respond to credit rating criteria by shifting resources away from value-relevant dimensions to appear strong on the dimensions emphasized by CRAs.

1.6 Additional Tests

In this section, I examine how credit ratings and at-issuance bond yields respond to the changes in investment behavior documented above. I then examine the effects of rating criteria on investment for bond issuances that are most likely to be planned in advance. Finally, I show that the results are not an artifact of firms cutting investment in response to covenant violations.

1.6.1 The Market Response to Changes in Investment Behavior

To test whether crossing a Debt/EBITDA threshold is associated with better ratings, I regress changes in credit ratings between one year prior to getting rated and the bond issuance on changes in firm characteristics that are important drivers of default risk (e.g., see Shumway, 2001b) and changes in Debt/EBITDA ratio.

$$\Delta Rating_{i,t\to t+5} = \alpha + \phi(Improve\ bin)_{i,t\to t+4} + \sum \beta \Delta X_{i,t\to t+4} + \epsilon_{i,t}$$

The variable of interest is a dummy variable (*Improve bin*) equal to one when the firm has crossed a salient Debt/EBITDA threshold. The coefficient estimate on this variable indicates the additional boost in credit rating a firm receives from crossing a salient threshold above and beyond the general effect of reducing Debt/EBITDA. For example, this estimates the benefit an issuer gets from decreasing Debt/EBITDA from 2.05 to 1.95 (crossing 2.0) above and beyond the effect of an improvement of Debt/EBITDA from 1.9 to 1.8 or 2.2 to 2.1. Columns (1-2) in Table 1.9 present the results.[17]

Column (1) presents the results without including the threshold-crossing indicator. Consistent with previous literature, firms with higher stock returns, increases in profitability, and decreases in leverage are more likely to be upgraded. Consistent with intuition, decreases in Debt/EBITDA are also positively related to credit rating upgrades. Column (2) presents the full specification. The coefficient estimate on *Improve bin* of 0.26 indicates that crossing a salient threshold is associated with a upgrade of about one-fourth of a rating. While it is sufficient that managers believe that improving Debt/EBITDA bin is associated with better ratings, these results show that firms benefit from crossing a salient threshold.

If the CRAs observe that a firm advances to a better Debt/EBITDA bin, but view the behavior that facilitated the move as a poor signal of creditworthiness, then the ratings may not react to the firm crossing a salient threshold. To test this supposition, I regress the changes in credit rating in the periods leading up to bond issuance on fundamental drivers of credit ratings, a variable that indicates an improvement in Debt/EBITDA bin (Improve bin), an indicator of whether the firm cut investment (Cut Inv), and the interaction of the latter two terms in the following specification.

[17] These estimations require the issuer have an S&P rating five months prior to issuance, which leads to a roughly 12% reduction in sample size.

$$\Delta Rating = \alpha + \phi(Improve\ bin) + \rho(Cut\ Inv) + \theta(Improve\ bin \times Cut\ Inv) + \Gamma\Delta X + \epsilon \tag{1.6}$$

Columns (3-4) of Table 1.9 present the results. For both R&D and SG&A, firms that are reduce their level of investments are less likely to receive an upgrade. However, the point estimates on the interaction terms ($\hat{\theta}$) indicate the firms that cut investments and crossed a salient threshold were not assigned significantly different ratings than those that cut investments but did not cross a salient threshold. Next, I examine whether at-issuance bond yields respond to this behavior.

If bond market participants observe this behavior and view it as a negative signal, they will demand a higher yield on the bonds. To test this hypothesis, I regress the bond yield at issuance on variables that reflect credit risk including dummy variables for each rating class, Debt/EBITDA bin, and year-quarter fixed effects. Similar to the spirit of the previous test, I include an indicator of whether the firm recently improved their Debt/EBITDA bin, whether the firm recently cut investment, and the interaction of these two variables. If bond buyers identify and penalize this behavior, the point estimate on the interaction term will be positive to indicate a higher demanded yield. Table 1.10 presents the results.[18]

The point estimates on the interaction terms are not statistically different from zero. These results indicate that firms that cut investment and cross a salient threshold do not receive significantly different yield on their bonds at offering beyond the effects the actions may have on credit rating. Overall, the lack of price response is consistent with the notion that investors rely on credit ratings and that changes in investment policies are not unambiguously interpreted by the bond market. These findings are consistent with those of Cohen et al. (2013), who provide evidence that stock market investors do not differentiate between high quality and low quality R&D investment.

[18]Some observations are dropped because of missing yield data.

1.6.2 Subsample Analysis and Robustness

Refinancing Bonds

An underlying assumption of the tests in the paper is that management knows in advance that there is a financing need. In anticipation of bond issuance, management has some time to take actions to conform to the standards of the CRAs. While issuing a bond is a major financial event for most firms and is typically planned well in advance, there are also cases when firms may issue bonds very quickly to fund, for example, a strategic acquisition. If such an opportunity arises unexpectedly, a firm does not have time to take actions to improve their appearance and simply issues the bond in their current state. The presence of such observations in the data adds noise to the estimations and could mask the true effect. The following test focuses on a subset of observations where management is more likely to be planning the issuance in advance. Specifically, I focus on debt issuances that are more likely to be refinancing transactions by computing a ratio of the amount of debt in current liabilities (debt due within a year) the quarter before issuance to the eventual bond issuance amount. Because of the relatively large amount of debt due soon, firms with a higher ratio are more likely to be planning in advance of their financing need. Table 1.11A presents the results of the base specification for the subset of observation where $\frac{debt\ in\ current\ liabilities_{t-1Q}}{bond\ amount_t} \geq 1$.

For this subsample of observations, the point estimates of the coefficient on *H-I Zone* is greater than the estimates from the base specification for each investment category. This finding supports the notion that firms that foresee an approaching financing need are more likely to take actions to strengthen their appearance leading up to getting a bond rated.

Covenant Violations

In addition to being a key metric of creditworthiness in the eyes of CRAs, the Debt/EBITDA ratio is also used in financial covenants in bank loan contracts. When borrowers violate loan covenants, they are in technical default and creditors then have the right to accelerate the loan. This gives creditors a great deal of influence on the actions of the firm during renegotiation. Chava and Roberts (2008) show that capital expenditures decline following violations of financial covenants. In light of their results, a possible concern may be that firms near salient thresholds happen to be firms that have recently violated covenants and the findings in this paper are an artifact of the effects of covenant violations on investment. To rule out this possibility, I augment my dataset with covenant violation data generously provided by Nini et al. (2012). Their data record whether a firm is in violation of a financial covenant violation data during a given quarter for Compustat non-financial firms from 1996-2009. Because the data begin in 1996 and my sample begins in 1990, these tests have fewer observations than the baseline results.

For each investment variable, I estimate two specifications to examine whether covenant violations drive the findings and present the results in Panel B of Table 1.11. First, I estimate the base regression specification (1.1) including an indicator variable, *Cov Violation*, equal to one if the issuer breaches a covenant during the periods leading up to getting rated (columns 1 and 3). Second, I estimate the base specification excluding the observations for which *Cov Violation* equals one (columns 2 and 4).

The coefficient estimate for *Cov Violation* is positive for each investment type indicating that firms in violation of a covenant are more likely to reduce R&D and SG&A investments, but the estimates are not statistically significant. Turning to the coefficient of interest in this paper, the size and statistical significance for coefficient estimates for *H-I Zone* are virtually unaffected by covenant violation considerations.

1.7 Conclusion

Credit ratings have emerged as a key mechanism to bridge the fundamental information asymmetry problem between firms and investors. Ratings give better access to debt markets for firms, expand the universe of investment opportunities for investors, and are deeply interwoven into financial regulation. Because credit ratings are an important factor in firms' level of access to and cost of debt capital, firms have incentives to take potentially costly actions to improve their rating.

I use an institutional feature of the credit rating process that generates cross-sectional variation in the incentives of firms to improve on a specific dimension that CRAs emphasize: Debt/EBITDA ratio. I show that firms that are near salient Debt/EBITDA thresholds–effectively receiving a high-incentive "treatment" to improve on this dimension–respond by reducing R&D and SG&A investments in the periods leading up to getting a bond rated. Further, I show that these firms are more likely to experience declines in innovation output, profitability, and firm value as measured by Tobin's Q in the years following bond issuance than observationally similar control firms. These results highlight an important cost of arms-length financing and suggest that the benefits of policies requiring increased transparency and disclosure of credit rating criteria should be carefully balanced against the corporate behavioral distortions they may induce.[19]

[19]See the "Credit Rating Agency Reform Act of 2006" and Title IX, Subtitle C – Improvements to the Regulation of Credit Rating Agencies of the "Dodd-Frank Wall Street Reform and Consumer Protection Act."

1.8 Figures

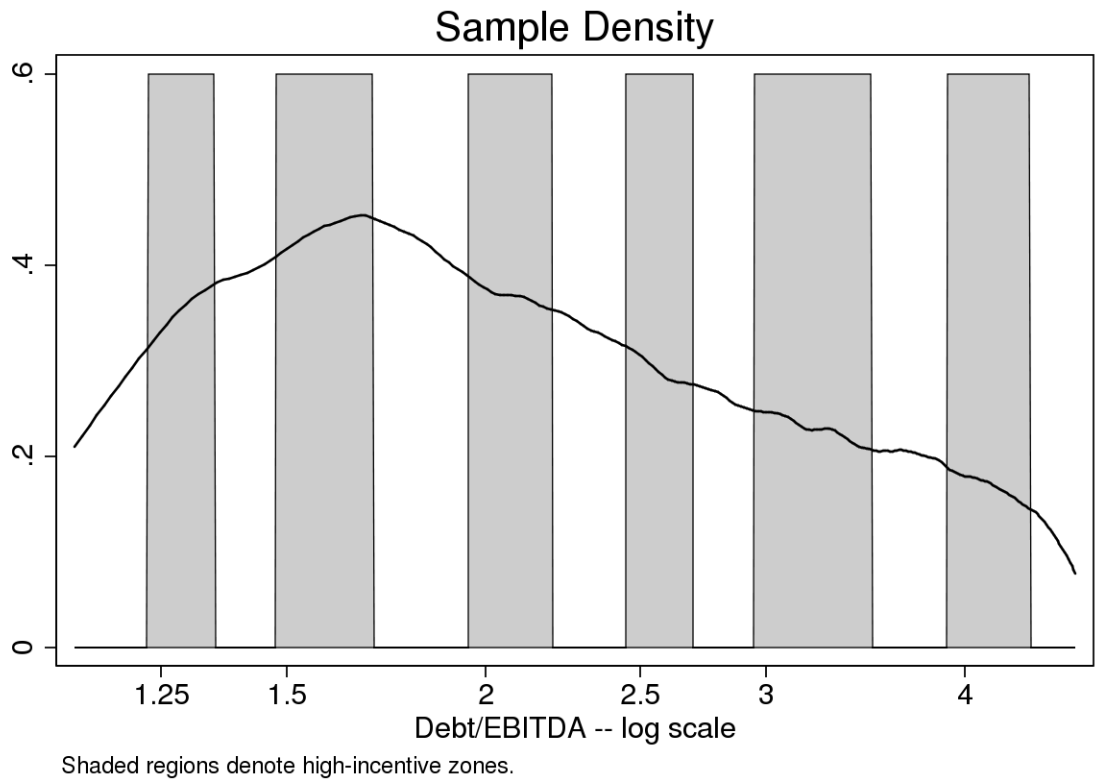


Figure 1.1: The Distribution of Debt/EBITDA One Year Prior to Getting Rated
This figure presents a kernel density of the sample Debt/EBITDA ratio one year prior to getting rated. The shaded areas indicate regions where issuers are approaching a salient Debt/EBITDA threshold and thus have a high incentive to improve along this dimension, as described in Section 1.3. In the empirical tests, I refer to the shaded regions as high-incentive zones (*H-I Zones*).

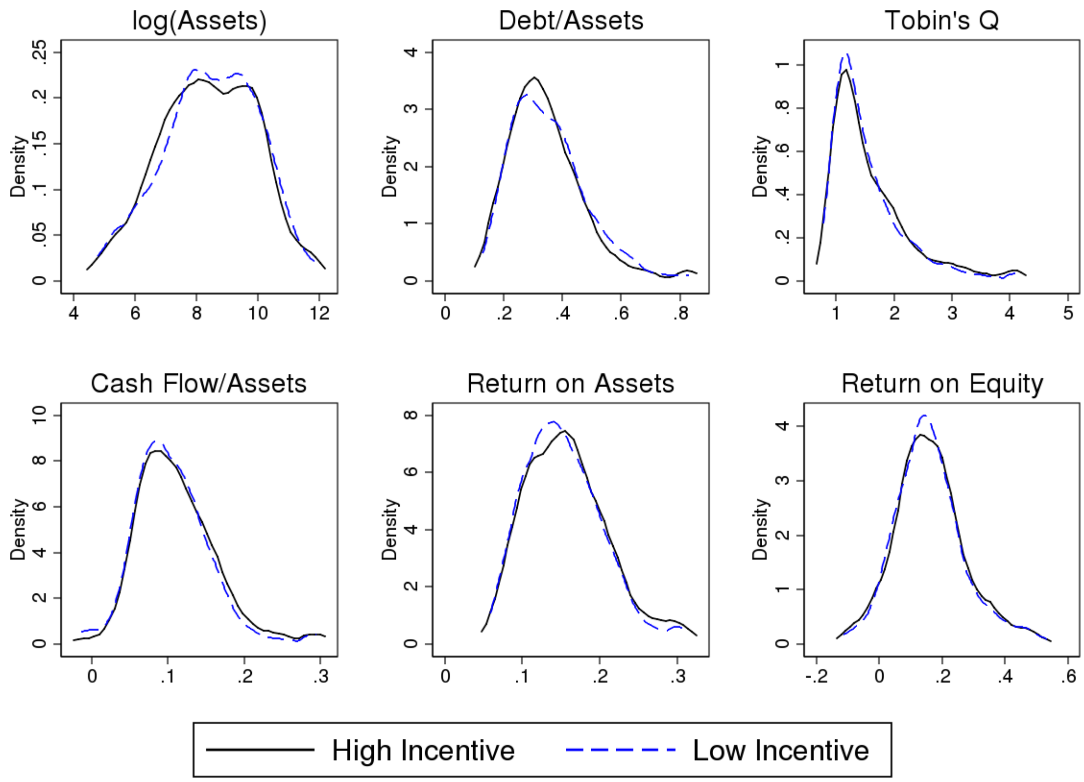


Figure 1.2: Issuer Characteristics by Whether the Firm Is in the High-Incentive Zone

This figure presents kernel densities of the sample separately for those near salient Debt/EBITDA thresholds (*H-I Zone*=1) and those that are not. Table A.1 in the Appendix outlines the construction of the variables.

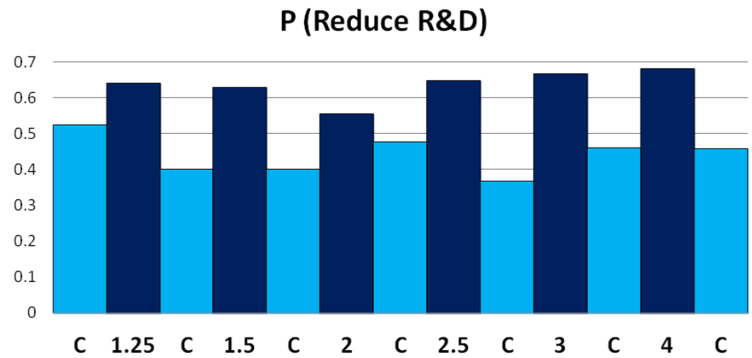


Figure 1.3: Proximity to a Salient Debt/EBITDA Threshold and Subsequent R&D Investment
This figure presents the mean issuer's decision to reduce R&D investment policies during the year leading up to getting a bond rated, based on issuer's proximity to a salient Debt/EBITDA threshold one year prior to getting rated. Each bin illustrates the mean of the binary behavior response of the issuers in that bin with regard to reducing investment (corresponding to a value of one) or not reducing investment (corresponding to a value of zero). The darker bins represent issuers near a salient threshold (e.g., 2.0, 2.5, etc.), which I refer to as high-incentive zones (*H-I Zones*) throughout the paper, and the lighter bins represent issuers who are not (denoted in the figure with a "C" to represent *Control*).

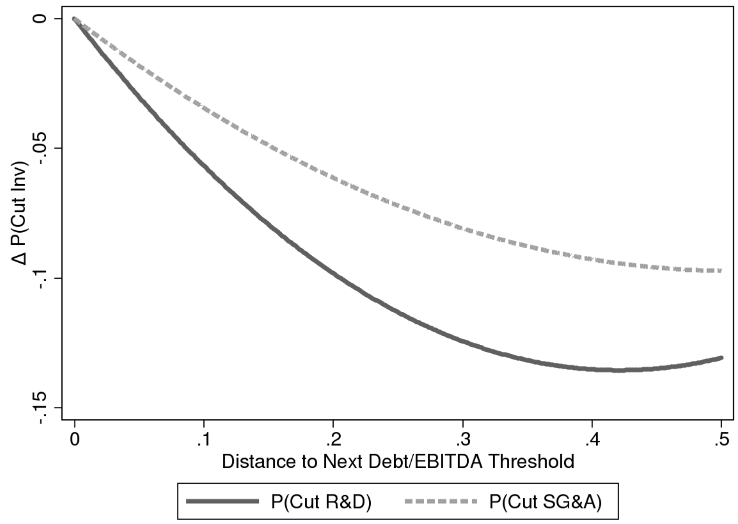


Figure 1.4: Likelihood of Cutting Investment as a Function of Distance to a Threshold

This figure presents the plot of the change in likelihood of cutting R&D and SG&A investment policies as a function of the issuer's distance to a salient Debt/EBITDA threshold a year prior to getting a bond rated as estimated in Table 1.5. For example, 0.1 on the x-axis represents firms with Debt/EBITDA = (2.0 + 0.1=) 2.1, 2.6, 3.1, etc.

1.9 Tables

Table 1.1: Business and Financial Risk Profile Matrix
This table presents excerpts from the credit ratings methodologies published by major credit rating agencies. Panel A presents Standard & Poor's Corporate Credit Rating Methodology Business Risk/Financial Risk Profile Matrix (Standard and Poor's, 2012) and Panel B presents the Debt/EBITDA to credit rating mapping for the global steel industry by Moody's (Moody's Investor Service, 2012).

Panel A: Standard & Poor's Business Risk/Financial Risk Matrix

	Financial Risk Profile					
	Minimal	Modest	Intermediate	Significant	Aggressive	Highly Leveraged
Debt/EBITDA	< 1.5	1.5-2.0	2.0-3.0	3.0-4.0	4.0-5.0	> 5.0
Business Risk Profile						
Excellent	AAA/AA+	AA	A	A-	BBB	–
Strong	AA	A	A-	BBB	BB	BB-
Satisfactory	A-	BBB+	BBB	BB+	BB-	B+
Fair	–	BBB-	BB+	BB	BB-	B
Weak	–	–	BB	BB-	B+	B-
Vulnerable	–	–	–	B+	B	B- or below

Panel B: Moody's Example Rating Grid from the Global Steel Industry Methodology

Debt/EBITDA	<0.75	0.75-1.25	1.25-2.0	2.0-3.0	3.0-4.0	4.0-5.5	5.5-7.5	>7.5
Rating	Aaa	Aa	A	Baa	Ba	B	Caa	Ca

Table 1.2: Sample Summary Statistics

This table presents summary statistics for the sample. All variables are winsorized at 1% prior to regression analysis. Table A.1 in the Appendix outlines the construction of the control variables.

	mean	sd	p25	p50	p75	count
Debt/EBITDA	2.47	1.08	1.59	2.24	3.22	1770
log(Assets)	8.32	1.62	7.19	8.31	9.54	1770
Book Leverage	0.35	0.14	0.25	0.33	0.42	1770
Tobin's Q	1.64	0.72	1.15	1.43	1.91	1770
Cash flow/Assets	0.11	0.06	0.08	0.11	0.14	1770
log(Firm Age)	3.39	0.66	2.94	3.61	3.89	1770
Return on Assets	0.16	0.06	0.12	0.16	0.19	1770
Return on Equity	0.18	0.43	0.08	0.16	0.24	1770
R&D/Assets	0.03	0.04	0.01	0.02	0.04	807
SG&A/Assets	0.22	0.20	0.08	0.16	0.29	1770

Table 1.3: Proximity to Salient Thresholds and Investment – Difference in Means
This table presents the percentage of firms that reduce R&D or SG&A investment policies during the year leading up to getting a bond rated. Column (1) presents overall sample means, Columns (2) and (3) present mean investment decisions for those not near a salient Debt/EBITDA threshold (*H-I Zone* = No) and those that are near a salient threshold (*H-I Zone* = Yes). Columns (4) and (5) present the difference in means for these two groups in percentage points (pps) and percent difference. *H-I Zone* is a dummy variable equal to one if the issuer is near a salient Debt/EBITDA threshold one year prior to getting a bond rated (see Section 1.3 for details).

	Overall	H-I Zone		Difference	
Investment	$\mathbb{P}$(Cut Investment)	No	Yes	pps	%
R&D	51.4%	44.9%	63.5%	18.6***	41.5%
SG&A	52.4%	50.5%	55.5%	5.0**	9.9%

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.4: Proximity to Salient Thresholds and Investment
This table presents OLS estimates from regressions of an indicator of whether the firm reduces R&D (Columns (1)-(2)) or SG&A (Columns (3)-(4))investment during the year leading up to getting a bond rated on the issuer's proximity to a salient Debt/EBITDA threshold one year prior to getting a bond rated and firm characteristics. *H-I Zone* is a dummy variable equal to one if the issuer is near a salient Debt/EBITDA threshold one year prior to getting a bond rated (see Section 1.3 for details). Table A.1 in the Appendix outlines the construction of the control variables. Columns (2) and (4) include SIC 2 digit industry code dummies. All standard errors are clustered by issuer.

	Cut R&D		Cut SG&A	
	(1)	(2)	(3)	(4)
H-I Zone	0.184***	0.188***	0.055**	0.062**
	(0.00)	(0.00)	(0.03)	(0.02)
log(Assets)	-0.000	-0.007	0.015	0.009
	(0.98)	(0.69)	(0.21)	(0.49)
Tobin's Q	-0.028	-0.038	0.035*	0.022
	(0.45)	(0.33)	(0.09)	(0.32)
Cash flow	-0.842**	-0.871**	-0.029	0.002
	(0.05)	(0.04)	(0.89)	(0.99)
log(Debt/EBITDA)	-0.082	-0.136	0.030	0.027
	(0.35)	(0.12)	(0.55)	(0.62)
log(Firm Age)	0.067*	0.080**	-0.009	-0.031
	(0.07)	(0.05)	(0.69)	(0.24)
Industry FE	No	Yes	No	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Observations	807	807	1770	1770
R^2	0.186	0.236	0.097	0.129

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.5: Tests Using Continuous Variables

Columns (1)-(2) present OLS estimates from regressions of an indicator of whether the firm reduces R&D or SG&A during the year leading up to getting a bond rated on the issuer's proximity to a salient Debt/EBITDA threshold one year prior to getting a bond rated and firm characteristics. The dependent variables for columns (3)-(4) are the percent change in the relevant investment policy during the year leading up to getting a bond rated. *dist* equals the distance between the firm's Debt/EBITDA and the adjacent better threshold (e.g., for Debt/EBITDA = 2.1, *dist* = 0.1) and *dist2* is its square. *H-I Zone* is a dummy variable equal to one if the issuer is near a salient Debt/EBITDA threshold one year prior to getting a bond rated (see Section 1.3 for details). Table A.1 in the Appendix outlines the construction of the control variables. All specifications include SIC 2 digit industry code dummies. All standard errors are clustered by issuer.

	$\mathbb{1}$\{Cut Investment\}		%Δ Investment	
	(1) R&D	(2) SG&A	(3) R&D	(4) SG&A
HI_Zone			-0.099***	-0.028***
			(0.00)	(0.00)
dist	-0.644**	-0.382**		
	(0.02)	(0.04)		
dist2	0.765**	0.374		
	(0.04)	(0.15)		
log(Assets)	-0.004	0.010	-0.002	-0.01***
	(0.81)	(0.44)	(0.85)	(0.01)
Tobin's Q	-0.060	0.041**	0.032	-0.019**
	(0.10)	(0.04)	(0.21)	(0.03)
Cash flow	-1.025**	-0.003	0.108	0.183*
	(0.02)	(0.67)	(0.24)	(0.06)
log(Debt/EBITDA)	0.041	0.101*	-0.003	-0.057***
	(0.70)	(0.07)	(0.95)	(0.01)
log(Firm Age)	0.083**	-0.028	-0.027	0.015
	(0.04)	(0.28)	(0.33)	(0.15)
Industry FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Observations	807	1770	807	1770
R^2	0.211	0.129	0.306	0.146

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.6: Proximity to Salient Thresholds and Investment When Credit Spreads Are High

This table presents OLS estimates from regressions of an indicator of whether the firm reduces R&D (Columns (1)-(2)) or SG&A expenditures (Columns (3)-(4)) during the year leading up to getting a bond rated on the issuer's proximity to a salient Debt/EBITDA threshold one year prior to getting a bond rated and firm characteristics. *H-I Zone* is a dummy variable equal to one if the issuer is near a salient Debt/EBITDA threshold one year prior to getting a bond rated (see Section 1.3 for details), and *High Spread* is a dummy variable equal to one for time periods when the Baa-Aaa spread exceeds the median for the sample period. Table A.1 in the Appendix outlines the construction of the control variables. Columns (1) and (3) reproduce results from Table 1.4 for comparison. All specifications include SIC 2 digit industry code dummies. All standard errors are clustered by issuer.

	Cut R&D		Cut SG&A	
	(1)	(2)	(3)	(4)
H-I Zone	0.188***	0.007	0.062**	-0.013
	(0.00)	(0.93)	(0.02)	(0.78)
H-I Zone * High Spread		0.241***		0.113**
		(0.01)		(0.04)
Controls, Industry FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Observations	807	807	1770	1770
R^2	0.236	0.246	0.129	0.132

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.7: Future Declines in Operating Performance and Innovation Output

This table presents OLS estimates from regressions of an indicator of future performance declines on whether the issuer is near a salient threshold one year prior to issuing a bond and a vector of firm controls.

$\mathbb{1}\{Perf_{t+\tau} < Perf_t\} = \alpha + \rho(\textit{H-I Zone})_{it} + \sum \beta X_{it} + q_t + \epsilon_{it}$

The dependent variables in Panels A and B are dummy variables equal to one when the issuers average patent productivity, measured as number of patents and citation-weighted patents, respectively, in the τ years after bond issuance is lower than its average patent productivity one year prior to getting a bond rated. The dependent variable in Panels C and D are dummy variables equal to one when the issuer's ROA (operating income/assets) or ROE (net income/shareholder equity) in τ years is lower than its respective value one year prior to getting a bond rated. *H-I Zone* is a dummy variable equal to one if the issuer is near a salient Debt/EBITDA threshold one year prior to getting a bond rated (see Section 1.3 for details). The vector of controls includes log(Assets), Tobin's Q, Cash flow/Assets, log(Debt/EBITDA) and SIC 2 digit industry code dummy variables. R&D/Assets is included as a control in Panels A and B. Table A.1 in the Appendix outlines the construction of the control variables. All standard errors are clustered by issuer.

Panel A: P(Lower Future Number of Patents)

	+1yr	+2yr	+3yr	+4yr
H-I Zone	0.085*	0.024	0.006	0.031
	(0.07)	(0.57)	(0.90)	(0.47)
Controls	Yes	Yes	Yes	Yes
Observations	557	548	496	450
R^2	0.161	0.227	0.302	0.323

Panel B: P(Lower Future Patent Citation)

	+1yr	+2yr	+3yr	+4yr
H-I Zone	0.041	0.056**	0.053**	0.052*
	(0.16)	(0.03)	(0.04)	(0.05)
Controls	Yes	Yes	Yes	Yes
Observations	557	548	496	450
R^2	0.454	0.563	0.571	0.521

Panel C: P(Lower Future ROA [operating income/assets])

	+1yr	+2yr	+3yr	+4yr
H-I Zone	0.055**	0.042*	0.052**	0.064**
	(0.01)	(0.09)	(0.04)	(0.03)
Controls	Yes	Yes	Yes	Yes
Observations	1691	1528	1429	1205
R^2	0.162	0.180	0.199	0.219

Panel D: P(Lower Future ROE [net income/shareholder equity])

	+1yr	+2yr	+3yr	+4yr
H-I Zone	0.045*	0.048**	0.051*	0.036
	(0.06)	(0.04)	(0.05)	(0.20)
Controls	Yes	Yes	Yes	Yes
Observations	1691	1528	1429	1205
R^2	0.177	0.195	0.171	0.169

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.8: Future Changes in Tobin's Q

This table presents estimates of changes in future industry-adjusted Tobin's Q (Q^{IA}). Panel A presents the difference in Q^{IA} for the treatment (*HI-Zone*) and control firms for four years following issuance, followed by the difference in differences across these groups. Panel B presents OLS estimates from regressions of future Q^{IA} on whether the issuer is near a salient threshold one year prior to issuing a bond and a vector of firm controls: $Q^{IA}_{i,t+\tau} = \alpha + \rho(H\text{-}I\ Zone)_{it} + \sum \beta X_{it} + q_t + \epsilon_{it}$. Q^{IA} is the firm's Tobin's Q minus the industry median Tobin's Q for that time period. *H-I Zone* is a dummy variable equal to one if the issuer is near a salient Debt/EBITDA threshold one year prior to getting a bond rated (see Section 1.3 for details). Table A.1 in the Appendix outlines the construction of the variables. All regression specifications include SIC 2-digit industry dummy variables and all standard errors are clustered by issuer.

Panel A: Future Changes in Q – Raw Differences

		ΔQ^{IA}			
	Baseline Q^{IA}	+1yr	+2yr	+3yr	+4yr
Control (*H-I Zone=0*)	0.19	0.01	0.02	0.00	0.01
Treatment (*H-I Zone=1*)	0.19	-0.02	-0.02	-0.03	-0.05
Difference-in-Differences	0.00	-0.03**	-0.04**	-0.03	-0.06**
p-value	(0.97)	(0.03)	(0.04)	(0.15)	(0.04)

Panel B: Future Q – Regression Results

	(1) +1yr	(2) +2yr	(3) +3yr	(4) +4yr
HI-Zone	-0.028*	-0.043**	-0.029	-0.048*
	(0.06)	(0.02)	(0.18)	(0.07)
log(Assets)	-0.001	0.004	0.007	0.009
	(0.95)	(0.73)	(0.59)	(0.56)
Industry-Adjusted Q	0.831***	0.753***	0.714***	0.595***
	(0.00)	(0.00)	(0.00)	(0.00)
log(Debt/EBITDA)	0.014	-0.014	-0.049	-0.115
	(0.74)	(0.79)	(0.39)	(0.14)
log(Firm Age)	0.002	0.012	0.019	0.008
	(0.89)	(0.57)	(0.48)	(0.80)
Leverage	-0.054	0.018	0.123	0.178
	(0.54)	(0.89)	(0.45)	(0.41)
ROA	0.765**	0.659	0.229	0.307
	(0.02)	(0.11)	(0.64)	(0.65)
Industry FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Observations	1691	1528	1429	1205
R^2	0.767	0.664	0.590	0.461

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.9: Crossing Thresholds, Cutting Investment and Credit Rating Improvement
This table presents OLS estimates from regressions of changes in the issuer's credit rating from one year prior to getting rated and bond issuance on the key drivers of corporate credit ratings. $\mathbb{1}$\{Improve Debt/EBITDA bin\} is a dummy variable equal to one if the issuer crossed a salient threshold into a better Debt/EBITDA bin during the year leading up to getting a bond rated. *Cut R&D, SG&A* are dummy variables equal to one if the issuer cut the respective investment in the year prior to getting rated. Table A.1 in the Appendix outlines the construction of the control variables. Include SIC 2 digit industry code dummies. All standard errors are clustered by issuer.

	(1) ΔRating	(2) ΔRating	(3) ΔRating	(4) ΔRating
Stock Return	0.339***	0.343***	0.442***	0.365***
	(0.00)	(0.00)	(0.01)	(0.00)
Δlog(Assets)	0.288	0.313	0.312	0.337*
	(0.14)	(0.11)	(0.39)	(0.10)
ΔLeverage	-2.480***	-2.483***	-2.540**	-2.464***
	(0.00)	(0.00)	(0.02)	(0.00)
ΔProfitability	0.599**	0.505**	1.658	0.307
	(0.04)	(0.04)	(0.20)	(0.14)
ΔDebt/EBITDA	-0.061***	-0.053**	-0.064	-0.053**
	(0.00)	(0.01)	(0.23)	(0.01)
$\mathbb{1}$\{Improve bin\}		0.260***	0.281**	0.270***
		(0.00)	(0.01)	(0.00)
Cut R&D			-0.155*	
			(0.06)	
$\mathbb{1}$\{Improve bin\} * Cut R&D			-0.031	
			(0.81)	
Cut SG&A				-0.130**
				(0.03)
$\mathbb{1}$\{Improve bin\} * Cut SG&A				0.053
				(0.58)
Year-Quarter FE	Yes	Yes	Yes	Yes
Observations	1498	1498	687	1498
R^2	0.236	0.252	0.311	0.263

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.10: Crossing Thresholds, Cutting Investment and Yields
This table presents OLS estimates from regressions of the yield of newly issued bonds on the key drivers of default risk. For firms that issue multiple bonds in the same quarter, I use a dollar-weighted average yield of the bonds. $\mathbb{1}$\{Improve Debt/EBITDA bin\} is a dummy variable equal to one if the issuer crossed a salient threshold into a better Debt/EBITDA bin during the year leading up to getting a bond rated. *Cut R&D, SG&A* are dummy variables equal to one if the issuer cut the respective investment in the year prior to getting rated. Table A.1 in the Appendix outlines the construction of the control variables. Include SIC 2 digit industry code dummy variables, dummy variables for each credit rating, and dummy variables for each salient Debt/EBITDA bin (described in Section 1.3. All standard errors are clustered by issuer.

	(1) Yield	(2) Yield	(3) Yield
Log(Assets)	-0.146**	-0.131*	-0.162**
	(0.03)	(0.09)	(0.02)
Leverage	0.027	-0.675	0.083
	(0.96)	(0.33)	(0.88)
Profitability	-2.978***	-2.998	-2.917***
	(0.00)	(0.22)	(0.00)
Stock Return	-0.028	-0.755***	0.011
	(0.88)	(0.01)	(0.95)
log(Bond Amount)	-0.098	-0.276**	-0.067
	(0.35)	(0.02)	(0.53)
$\mathbb{1}$\{Improve bin\}	0.090	0.096	0.177
	(0.31)	(0.60)	(0.16)
Cut R&D		0.059	
		(0.66)	
$\mathbb{1}$\{Improve bin\} * Cut R&D		0.036	
		(0.88)	
Cut SG&A			0.091
			(0.43)
$\mathbb{1}$\{Improve bin\} * Cut SG&A			-0.125
			(0.48)
Debt/EBITDA Bin FE	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes
Observations	1140	551	1096
R^2	0.619	0.685	0.626

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.11: Additional Tests

This table presents OLS estimates from regressions of an indicator of whether the firm reduces R&D or SG&A expenditures during the year leading up to getting a bond rated on the issuer's proximity to a salient Debt/EBITDA threshold one year prior to getting a bond rated and firm characteristics. Panel A presents estimates using the subsample of bonds that are more likely to be used for refinancing existing debt. These are observations where the debt in current liabilities one quarter prior to issuance is at least as large as the size of the bond issuance. Panel B presents the baseline regression, controlling for whether the issuer is in violation of a financial covenant during the year leading up to getting a bond rated (*Cov Violation*=1). Columns (1) and (3) use all observations that can be matched to the covenant violation data and columns (2) and (4) re-estimate the specification with violating issuers dropped from the sample. *H-I Zone* is a dummy variable equal to one if the issuer is near a salient Debt/EBITDA threshold one year prior to getting a bond rated (see Section 1.3 for details). All the control variables used in the main specification are included in these regressions. Table A.1 in the Appendix outlines the construction of the control variables. All standard errors are clustered by issuer.

Panel A: Refinancing Bonds

	(1) Cut R&D	(2) Cut SG&A
H-I Zone	0.261***	0.120**
	(0.00)	(0.01)
Controls, Industry FE	Yes	Yes
Year-Quarter FE	Yes	Yes
Observations	417	634
R^2	0.395	0.264

Panel B: Covenant Violations

	Cut R&D		Cut SG&A	
	(1)	(2)	(3)	(4)
H-I Zone	0.198***	0.196***	0.077**	0.080**
	(0.00)	(0.00)	(0.02)	(0.02)
Cov Violation	0.107		0.076	
	(0.36)		(0.36)	
Controls, Industry FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Observations	512	490	1116	1063
R^2	0.259	0.258	0.125	0.127

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

CHAPTER II

Signaling, Financial Constraints, and Performance-Sensitive Debt

2.1 Abstract

This paper examines how good borrowers use the design of performance-sensitive debt contracts to alleviate financial constraints. I show that borrowers use a convex pricing grid (i.e., a contract where the increase in the loan spread following a decline in performance exceeds the decrease in the spread following a performance improvement) to signal their unobservable creditworthiness and receive better bank loan terms. I find that constrained firms who use convex pricing grids receive loans that are 21-28% larger with a spread that is 31-37 basis points lower than observationally similar borrowers who use fixed spread loans. Consistent with the notion that a costly signal should positively correlate with future financial health, I find that constrained borrowers who use a loan with a convex pricing grid are one third less likely to experience financial distress during the term of their loans.

2.2 Introduction

Asymmetric information causes high quality, but opaque, borrowers to face costly financing constraints if they are unable to convince investors of their creditworthiness. These constraints distort both the allocation of capital in the economy and borrowers' real and financial policies.[1] While there are multiple potential channels through which financial constraints may be relieved (e.g., better governance, different ownership structure, improved legal systems and investor protection, etc.), this paper focuses on security design. Given that information asymmetry plays a large role in the activities of the firm, it is important to understand if, and by what means, borrowers design contracts to alleviate financial constraints to move them closer to their first best policies.

A large theoretical literature suggests that signaling may mitigate information asymmetry.[2] In the spirit of the classic Leland and Pyle (1977) paradigm, I examine whether good borrowers use contract design as a costly signal to separate themselves from bad borrowers in the context of bank lending. If good borrowers can achieve this separation, they should receive better terms of credit than if lenders pool them with bad borrowers. Specifically, I test whether good borrowers use the presence and design of performance pricing in bank loans as a credible signal of creditworthiness.

A state contingent mapping from performance to loan spread, which I refer to as the pricing grid, is the defining feature of performance-sensitive debt (PSD) bank loans. This feature is present in 54% of the 11,297 loans in my sample. As the firm performs better (worse), the spread on the loan decreases (increases). A borrower exposes himself to more risk by accepting a loan with a pricing grid instead of using a fixed spread loan. Looking only at the *presence* of pricing grids, though, ignores the heterogeneity in their shape across contracts. To exploit this variation in the design of the pricing grids, I construct a measure of the pricing grid's convexity. I define a PSD loan to be convex if the pricing grid has the following property: the loan spread is more sensitive to decreasing borrower performance than to increasing performance. Said differently, the punishment for diminishing performance is more severe than the reward for improvement. Accepting a contract with this potentially costly asymmetric response to changes in performance can signal good borrowers' private

[1]For example, Stein (2003) reviews how asymmetric information influences investment behavior and Campello et al. (2010) provide survey evidence on the role of financial constraints from the global financial crisis in 2008.

[2]See Riley (2001) for a survey of signaling models in economics.

information that they are less likely to arrive in lower performance states in the future. Thus, I interpret higher convexity as a stronger signal of creditworthiness. Figure 2.1 presents a graphical example a convex grid from a 1998 term loan to Central Parking Corp.

I classify all loans in my Dealscan-Compustat sample into one of three categories – fixed spread (weakest signal), low convexity, and high convexity (strongest signal) – and I propose that good borrowers can use a more convex contract as a costly signal to alleviate financial constraints. Following specific predictions of this signaling hypothesis, I estimate the relationship between signal strength and loan size and spread and then between signal strength and the borrower's future financial health. By revealing themselves as good borrowers, those that send a strong signal should receive more favorable terms of credit and be less likely to experience future financial distress relative to observationally similar borrowers. These effects should be strongest among opaque, financially constrained borrowers since lenders are most uncertain about these borrowers' creditworthiness.

First, I examine how PSD use and pricing grid convexity relate to loan size and spread. While controlling for observable borrower credit risk characteristics, I find that borrowers that use PSD contracts receive loans that are 16% larger and have a spread that is 22 basis points lower than those that use fixed spread loans. I then exploit differences in pricing grid convexity and find that the more costly high convexity signal is associated with the best terms of credit. Controlling for observable credit risk, high convexity loans are 20% larger and have a spread that is 27 bps lower than those that use fixed spread loans. To test for differential effects for constrained versus unconstrained borrowers, I use various sorting schemes to divide the sample according to ex ante financial constraint measures. High convexity loans in the unrated, low payout policy, and "no prior relationship with the lender" subsamples are 21-28% larger and have spreads that are 31-37 bps lower than observationally similar fixed spread loans. These estimates are statistically different from and economically larger than the corresponding effects in the unconstrained subsamples.

Next, I examine borrowers' ex post financial health. Signaling mitigates information asymmetry only if it communicates information that is not otherwise observable. The hypothesis that strong borrowers use convex contracts to signal otherwise unobservable creditworthiness predicts that signaling borrowers are less likely to experience financial distress in the future. I define borrowers as entering financial distress based upon their interest coverage ratio during the term of the loan. I include convexity along with other observable measures of credit risk from the default literature in regressions that predict distress to test this hypothesis. The regression estimates show that opaque, financially constrained borrowers that use high convexity PSD are approximately one third less likely to experience financial distress during the stated term of the loan compared to those that use a fixed spread. The relationship is not present for more transparent, less financially constrained borrowers. These results support the notion that signaling matters most for transactions with more severe information asymmetry.

This paper contributes to the bank loan contracting, signaling, and financial distress literatures. The bank loan contracting literature examines how banks use monitoring and security design to mitigate market imperfections and alleviate financial constraints. Much of this literature focuses on the roles of collateral, maturity, and loan covenants. Despite its widespread use, extant research largely overlooks the presence and heterogeneity of PSD contracts. The studies that do acknowledge performance pricing typically consider its presence a binary outcome. I exploit heterogeneity in the shape of these contracts to demonstrate that a “dummy variable” approach misses much of the richness and versatility of PSD. A clear picture of how borrowers and lenders design and use PSD is important to better understand bank lending and PSD’s role in the financial contracting toolbox.

Related, but more broadly, this paper contributes to the literature on signaling in finance. While there is a large, rich theoretical literature on the role of signaling in finance, empirical evidence supporting these theories is somewhat scarce. Much of the empirical contracting literature studies the effects of contract design on ex post firm behavior or how observable firm characteristics affect loan spreads.[3] The literature is mostly silent on how borrowers can use financial contract design as a signal to mitigate information asymmetry between themselves and lenders to ease financial constraints. To help fill this gap, I provide empirical support for the signaling hypothesis that borrowers can use the convexity of a performance pricing grid as a costly signal of creditworthiness.

Finally, this paper contributes to the literature on predicting financial distress. I show that constrained borrowers can design contracts that reveal information about their likelihood of future distress beyond what is explained by observable credit risk variables from previous literature. Specifically, I show that when opaque borrowers use convex PSD contracts, they reveal otherwise unobservable positive information about their creditworthiness that variables such as leverage, volatility, size, profitability, and credit rating do not capture.

The papers that most closely relate to mine are Manso et al. (2010), Asquith et al. (2005), and Tchistyi et al. (2011). Manso et al. (2010) model PSD as a screening device used to separate high growth firms from low growth firms. They provide empirical evidence supporting the predictions of their theoretical model by using a sample of contracts from Thomson Financial's SDC database that use credit rating based PSD. They show that these firms were more likely to improve their credit rating than firms using fixed spread loans. My paper is distinct from theirs in two key ways. First, while they shed light on some of the determinants of PSD use as a binary outcome, I exploit the heterogeneity in the shape of the pricing grid to study the implications of PSD use in alleviating financial constraints. I use the degree of PSD contract convexity as a measure of the costliness of a signal to quantify the economically significant relationship between signal strength and loan size and spread. Second, while credit rating based PSD contracts constitute an important portion of the PSD universe (18% of the PSD loans in my sample), their use is by definition restricted to those that have a credit rating. Though the presence of a credit rating is endogenous, firms with a credit rating are quite different from those without a credit rating (Faulkender and Petersen,

[3]See Roberts and Sufi (2009a) and the references therein for a recent review of this literature.

2006a). Rated firms are less likely to face problems of asymmetric information to the extent of unrated firms, so the value of the signal to these borrowers is lower. My study includes performance-sensitive debt that is tied to accounting ratios (with debt-to-cash flow being by far the most prevalent) as well as credit ratings, which allows for a more general analysis of PSD use.

Asquith et al. (2005) provide exploratory analysis of the determinants of "interest-increasing" performance pricing (i.e., interest spread increases when credit quality deteriorates) and "interest-decreasing" performance pricing. Their tests show that interest-increasing performance pricing is correlated with contracting environments where renegotiation is likely to be more costly and where adverse selection may be a concern. They also argue that interest-decreasing performance pricing is more prevalent when moral hazard costs are higher. While their study provides plausible predictions, they do not study the convexity of the pricing grids and do not provide evidence that corroborates their predictions with ex post outcomes.

While I argue that signaling is one reason for borrowers to use PSD, this is surely not its only use. Tchistyi et al. (2011) consider a subset of PSD contracts based on credit ratings and show how managers can exploit the leveraging characteristic of PSD to increase financial risk for their own personal gain through their compensation structure.

I organize the rest of the paper as follows. Section 2.3 discusses theoretical motivations and empirical predictions. Section 2.4 describes the sample and the construction of my measure of convexity. Section 2.5 tests the predictions and presents the results and Section 2.6 concludes.

2.3 Background and Predictions

High quality, but opaque, borrowers may have difficulty communicating their ex ante unobservable quality to potential capital providers. The uncertainty about the these borrowers' creditworthiness may lead lenders to ration or completely deny them credit. A lower quantity and higher cost of capital can constrain investment or lead to other suboptimal policies. Effective financial contracting minimizes the impact of asymmetric information and allows capital to flow more smoothly to productive users, allowing borrowers to move closer to their first best policies.

Costly signaling is one way to solve this "unobservable type" problem. To separate themselves, good borrowers need a mechanism through which they can credibly signal private information about their type to lenders. Theoretical signaling models explain many phenomena in finance including capital structure (Leland and Pyle, 1977; Ross, 1977), IPO underpricing (Allen and Faulhaber, 1989; Welch, 1989; Grinblatt and Hwang, 1989) and payout policy (Bhattacharya, 1979; John and Williams, 1985; Miller and Rock, 1985). Within the lending literature, theory suggests that high quality borrowers can signal their type through, for example, collateral pledging (Besanko and Thakor, 1987) and debt maturity choice (Flannery, 1986; Diamond, 1991). Though this theoretical literature is rich and the idea of signaling is intuitively appealing, there is little empirical support for signaling models.[4]

PSD's key feature is that it allows the interest rate spread on the debt to adjust without intervention or renegotiation. The structure of the pricing grid is set during origination and then the spread follows a mapping from a performance variable (e.g., debt-to-cash flow ratio) to the loan spread during the life of the loan. Financial covenants are the most closely related contract feature to PSD in that covenants make control contingent on a performance variable. As opposed to having the spread automatically adjust as with PSD, control rights effectively shift to the creditor if the borrower passes the covenant threshold. Following a covenant violation, the borrowers and lenders typically renegotiate the contract and the lender often waives the violation in exchange for concessions such as an increase in spread, an increase

[4]Jiménez et al. (2006) provide a recent exception. Using a sample of Spanish business loans, the authors show that while the overall driver of collateral use is observable credit risk, among young borrowers with no previous record of financial or commercial activity, collateral is more likely to be pledged by high quality borrowers. Thus among those with the largest information asymmetry, they find that collateral serves as a signal of low credit risk.

in collateral, or restricting the borrower's credit access or investments (Chava and Roberts, 2008; Nini et al., 2009; Sufi, 2009). It would seem that PSD's state contingent pricing may substitute for covenants as a means to reduce the likelihood of costly renegotiation (Asquith et al., 2005). However, Roberts and Sufi (2009b) show that the presence of performance pricing is largely unrelated to the likelihood of renegotiation. Alternatively, I propose that PSD contract design can serve as a costly signal of creditworthiness.

A hallmark of signaling models is that the "good type" must take an action that is too costly for the "bad type" to mimic. For a contract to be a credible signal, it should include a potentially costly feature that good borrowers can accept to separate themselves. From a risk sharing perspective of contracting, the more diversified lender should bear more of the risk than the smaller, relatively risk averse borrower. That is, the optimal risk sharing contract would give the borrower a fixed interest rate. By accepting a PSD contract, the borrower increases their exposure to their own specific risk since the borrower's spread varies inversely with their performance. Instead of smoothing outcomes across high and low performance states of the world, PSD contracts amplify shocks to performance and increase the risk borne by its users.

A *convex* pricing grid further increases the potential cost to the borrower. Convexity causes the spread on the loan to asymmetrically respond to changes in performance. Specifically, convexity mutes the rewards in high performance states but maintains the penalties in low performance states. By accepting a convex contract, the borrower can signal his private information that he is less likely to arrive in low performance states. Creditors value this information because it is the likelihood that borrowers will fall into a low performance state that defines the borrower's creditworthiness and directly relates to the success of the lender's investment.

Since borrowers' types are inherently unobservable (hence the information asymmetry), I examine observable outcomes to test the signaling hypothesis. The signaling hypothesis motivates three testable predictions. First, the signaling borrowers should receive better loan terms. By using the costly signal, borrowers reveal themselves as good types. This action should lead to an outward shift in the supply curve of credit, effectively providing the signaling borrowers with access to larger loans and a lower interest rate spread.

Prediction 1: *Borrowers that use a convex pricing grid receive larger loans and lower spreads than observationally similar borrowers.*

Second, signaling borrowers' ex post outcomes should be consistent with the information their signal communicates. If borrowers are indeed signaling higher creditworthiness by accepting a convex pricing grid, they should have a lower incidence of future financial distress.

Prediction 2: *Borrowers that use a convex pricing grid are less likely than observationally similar borrowers to become financially distressed during the term of the loan.*

Finally, the value of the signal hinges on the ex ante level of information asymmetry. If lenders already know a borrower's type, then signaling is informationally vacuous in addition to being inefficient in terms of optimal risk sharing. On the other hand, where there is more uncertainty about the borrower's type, any information about creditworthiness is valuable and increasingly so in transactions where the informational gap between the borrower and lender is larger. Thus opaque, financially constrained borrowers should derive the most value from signaling.

Prediction 3: *Predictions 1 and 2 are strongest for opaque, financially constrained borrowers.*

2.4 Sample Construction and Key Variables

Data Sources

Loan data are from the Loan Performance Corporation's Dealscan database. The database contains detailed information on the performance pricing grids when they are present in a loan in addition to the other standard loan terms. To be included in the sample, a loan must be originated in the USA, dollar denominated, and have a LIBOR base rate. Accounting data are from the Compustat database. I match the Dealscan data to Compustat data using a linking table from Michael Roberts (Chava and Roberts, 2008). Since the tests in this paper require the borrower's accounting variables, the final sample only includes loans where I can match the borrower to Compustat. As a result, the sample consists of borrowers that are, on average, larger than the universe of Dealscan borrowers as a whole. I winsorize all variables at 1 percent to minimize the impact of outliers and I adjust all dollar amounts to year 2000 price levels. Dealscan began recording pricing grid data in 1994, so the sample period is 1994-2010.

Pricing Grid Convexity

Dealscan provides detailed data on the pricing grids for loans that have a performance pricing feature. The database provides the complete mapping from the performance variable (e.g., debt-to-cash flow ratio) to a corresponding loan spread. Figure 2.1 from earlier illustrates an example pricing grid. The solid line traces out the pricing grid, while the dotted line simply connects the grid's endpoints. To measure convexity, I compute the difference between the level of the spread in the contract and the predicted value of the spread based on the linear interpolation of the pricing grid endpoints.

Figure 2.2 presents a kernel density of the convexity measure for the sample of loans with a debt-to-cash flow performance pricing grid. The densities using other performance measures (e.g., leverage, credit rating, fixed charge coverage ratio, etc.) are similar. I set a dummy variable *High Convexity* equal to one if the convexity measure is in the top tercile of the distribution (shaded in Figure 2.2) for that performance variable.[5] I set a dummy variable *Low Convexity* equal to one for the remaining loans with performance pricing. For the regression analysis, the omitted category is loans with a fixed interest rate spread (i.e.,

[5] I discretize the measure to provide a simpler interpretation. The results are robust to adjustments to this threshold, which I use for the analysis because it is a natural cutoff in the empirical distribution. Using continuous measures of convexity for the analysis yields qualitatively similar results.

no performance pricing feature).

Ex Ante Financial Constraints Sorting

I focus on three ex ante financial constraint measures to test if the signaling mechanism has differential effects for borrowers with different degrees of financing frictions (Prediction 3): whether the borrower (i) has a credit rating, (ii) paid a dividend during the prior year and (iii) had a prior relationship with the lender. I present the results using these sorting schemes for their simplicity in interpretation, but the results of the analysis are robust to various other ex ante measures of financial constraint.[6]

First, borrowers without a credit rating are more likely to be financially constrained. These firms are less likely to have easy access to public debt markets and they lack the information production and certification that comes with a credit rating (Faulkender and Petersen, 2006a).

Second, to the extent that external funds are more costly than those generated internally, firms that pay dividends are likely to be less constrained than those that do not. Dividend payers always have the option to redirect funds to investment rather than pay them out.

Third, no prior relationship between the borrower and lender indicates a larger information gap. While borrowing from a lender for the first time is not a sufficient condition for financial constraint, the first time that the borrower and lender enter into a lending transaction, the lender knows less about the borrower than if there has been prior dealings. As such, an informative signal is more valuable in this setting. Following the relationship banking literature (see Bharath et al., 2011), I code a loan as having no prior relationship if the borrower and lender have not transacted a loan package during the last five years.

Measuring Financial Distress

I use the borrower's future interest coverage ratio as a measure of financial distress to investigate ex post outcomes (see Asquith et al., 1994; Andrade and Kaplan, 1998). I define borrowers as financially distressed if they satisfy at least one of the following two conditions:

[6]Section 2.5.3 presents these robustness checks.

(i) the borrower has an interest coverage ratio, defined as EBITDA divided by interest expense, below 1.0 for two consecutive years or (ii) the borrower has an interest coverage ratio below 0.8 for one year. In case (i), I consider the first of the two years to be the year that the firm begins to experience financial distress. The outcome measure I use in the specifications, *Future Distress*, is an indicator variable equal to one if the borrower enters financial distress prior to the stated maturity date of the loan. I drop observations where the borrower is in financial distress at the time of origination, loans with a maturity of less than 12 months to allow time for the measurement of distress, and loans that have a stated maturity after 2011 since these data are incomplete for the financial distress analysis.

Control Variables

For the multivariate analysis of the terms of credit, I include several variables that are important in determining loan size and spreads. Previous research shows that characteristics such as the borrower's asset size, profitability, leverage, market-to-book ratio, and prior relationships with the lender are important drivers of these outcomes. I lag all accounting variables two quarters prior to the origination date. Though simultaneously determined, the presence of collateral and financial covenants and the maturity of the loan could also affect the pricing and size of the loan. I include these loan characteristics in the full specifications to better isolate the impact of PSD. All results are robust to their exclusion.

The financial distress literature provides guidance on predictors of future financial distress. I include those variables as controls when investigating the ability of pricing grid convexity to explain future distress. The structural modeling approach (see Merton, 1974) and the hazard modeling approach (see Shumway, 2001a) suggest that firms with higher leverage, higher volatility, lower profitability and smaller size are more likely to experience financial distress. To further capture available information, I also include a full set of credit rating dummies in the distress specifications.

Summary Statistics

The final sample consists of 11,297 loans with 3,592 unique borrowers from 1994-2010. Table

2.1 presents summary statistics. While the sample attributes are standard for papers in this literature, it is worth noting that 54% of the loans in the sample have the performance pricing feature. The sample is also split relatively well between constrained and unconstrained borrowers: 55% do not have an S&P credit rating, 63% have not paid a cash dividend in the year prior to the loan, and 39% have no prior lending relationship with their lender. These statistics also reveal that the borrowers experience financial distress prior to maturity in about 12% of the loans in the sample.

2.5 Results

2.5.1 Bivariate Analysis

Table 2.2 presents average borrower and loan characteristics for loans that have a fixed spread (i.e., no pricing grid), use low convexity PSD, and use high convexity PSD. The last column presents differences in means between loans with high convexity and those with a fixed spread. The mean (median) size of loans with high convexity pricing grids is about 50% (over 100%) larger than loans with a fixed spread and 15% (45%) larger than PSD loans with low convexity. The mean (median) spread for loans with a high convexity pricing grid is about 93 bps (100 bps) lower than loans with a fixed spread and 40 bps (50 bps) lower than contracts with low convexity pricing grids. Loans with high convexity also tend to have longer maturity, are less likely to be secured, and contain more financial covenants.

While the differences in loan size and spread are sizable, Table 2.2 also illustrates non-trivial differences in the average borrowers that use of each style of contract. Borrowers using high convexity contracts tend to be larger, more profitable firms with slightly lower leverage and higher market-to-book ratio. They also are more likely to have a credit rating, pay dividends and have a prior relationship with their current lender. These characteristics suggest that, on average, these borrowers may face less severe financing frictions. Finally, despite having a longer average maturity than fixed spread loans, borrowers with high convexity loans are about half as likely to find themselves in financial distress prior to the stated maturity of the loan.

These statistics show that loans with high convexity pricing grids tend to be larger, have a lower spread, and their borrowers are less likely to find financial distress. However, these statistics also suggest that observable credit risk characteristics could drive these differences. I now turn to multivariate analysis to better separate the impact of pricing grid design from observable credit risk factors.

2.5.2 Multivariate Analysis

2.5.2.1 Ex Ante Outcomes: The Terms of Credit

In the ensuing tests, I estimate the relationship between PSD and loan size and spread while controlling for observable borrower and loan characteristics using the following specification:

$$\textit{Loan Size} \text{ or } \textit{Spread} = \alpha + \beta_{HC}(\textit{High Convexity}) + \beta_{LC}(\textit{Low Convexity}) + \Gamma X_{borrower} + \Lambda X_{loan} + \epsilon$$

High Convexity (*Low Convexity*) is a dummy variable equal to one when the loan includes a performance pricing grid with high (low) convexity as defined in Section 2.4. The coefficient estimates on these variables represent the difference in loan size and spread for these styles of loans compared to fixed spread loans, which is the omitted category in the specification. I include year dummies to control for macroeconomic conditions, industry dummies for persistent differences in risks across industries, loan type and purpose dummies since different loan types and purposes may be priced differently and credit rating dummies to more fully control for observable information about creditworthiness. I cluster all standard errors by borrower.

Prediction 1 implies $|\hat{\beta}_{HC}| > |\hat{\beta}_{LC}| > 0$ with the point estimates positive when the dependent variable is *Loan Size* and negative when the dependent variable is *Spread*. That is, after controlling for observable credit risk characteristics, PSD users should receive larger loans and lower spreads and the magnitude of the PSD effect should increase monotonically with the strength of the signal. Table 2.3 presents the results.

Models (1)-(3) present regression estimates with *Loan Size*, defined as the log of the size of the loan (in millions), as the dependent variable. Before considering the differential impacts of high and low convexity PSD, model (1) presents estimates of the impact of the basic presence of PSD on loan size. The estimate indicates that PSD loans are about 16% larger than fixed spread loans after controlling for borrower characteristics. Model (2) separates PSD into high convexity and low convexity categories to test the relationship between signal strength and loan size. The estimates of $\hat{\beta}_{HC} = 0.25$ and $\hat{\beta}_{LC} = 0.11$ represent a large differential impact of high convexity contracts over both low convexity and fixed spread contracts. These estimates are economically significant and statistically different from both zero and each other with $p < 0.01$. Model (3) includes loan maturity, number of financial covenants, and a dummy for secured loans to better isolate the specific correlation between the PSD variables and loan size. The magnitude of the point estimates for *High Convexity* and *Low Convexity* are smaller in this model, but they remain economically large and statistically different from both zero and each other. Overall, models (1)-(3) show that borrowers who use PSD receive much larger loans than those who do not and that high convexity loans drive this effect. Controlling for borrower and loan characteristics, the average loan with a high convexity pricing grid is about 20% larger than one with a fixed spread and low convexity loans are about 7% larger than fixed spread loans. For the average loan in the sample, the coefficient estimate on *High Convexity* represents an increase of about \$48 million in loan size from \$241 million to \$289 million.

Models (4)-(6) present estimates with *Spread* as the dependent variable. Model (4) shows that PSD loans have a spread that is 22 bps lower than fixed spread loans after controlling for borrower characteristics. Model (6) presents the estimate of the full specification. Both $\hat{\beta}_{HC} = -27$ bps and $\hat{\beta}_{LC} = -12$ bps are economically significant and statistically different from both zero and each other. At the sample mean loan size of $241 million, contracts with high convexity have an annual interest expense that is $650 thousand less than fixed spread loans, which adds up to $2.68 million over the life of the average maturity loan. Along with the results of the *Loan Size* estimates in model (3), these results support Prediction 1 that a stronger signal is associated with better terms of credit.

The coefficient estimates on the other covariates shown in Table 2.3 are consistent with previous studies in this literature. Larger, more profitable borrowers with higher growth prospects and stronger lending relationships tend to get better terms of credit. Whereas PSD is associated with stronger borrowers and better terms of credit, the presence of collateral and more financial covenants is associated with worse terms of credit.

Prediction 3 states that the effects of signaling should be larger for transactions where the informational gap is more severe. I estimate the models separately for the financially constrained and financially unconstrained observations to test this prediction.[7] Empirically, Prediction 3 implies $|\hat{\beta}_{HC}^{Constrained}| > |\hat{\beta}_{HC}^{Unconstrained}|$. Tables 2.4 and 2.5 present the subsample regression estimates of the impact of convexity on loan size and spread, respectively. As discussed in Section 2.4, I sort observations according to the presence of a credit rating (models (1)-(2)), dividend policy (models (3)-(4)), and prior lending relationship (models (5)-(6)).

Table 2.4 shows that the presence of a high convexity pricing grid has a larger impact on loan size in the constrained subsample relative to the unconstrained subsample for each sorting scheme. Borrowers who are unrated, non-dividend payers, and those in a new lending relationship that use a high convexity pricing grid have, on average, loans that are 21%, 23%, and 28% larger than those with a fixed spread. The increase in loan size with high convexity for the constrained subsample ($\hat{\beta}_{HC}^{Constrained}$) exceeds that of the corresponding unconstrained subsample ($\hat{\beta}_{HC}^{Unconstrained}$), though the estimates are economically large for both. Wald tests

[7]I estimate the models separately because lenders may have a different lending model for these different subsets of firms. In unreported results, I estimate a full specification that includes the measures of convexity interacted with the financial constraint sorting variable and find quantitatively similar and qualitatively identical results.

reject $\hat{\beta}_{HC}^{Constrained} = \hat{\beta}_{HC}^{Unconstrained}$ at conventional levels for each sorting scheme (I present p-values from these tests at the bottom of the table). The difference is particularly large for borrowers entering their first loan with a lender. High convexity has twice the impact for borrowers with no prior relationship compared to those that are in repeat relationships ($\hat{\beta}_{HC}^{Constrained} = 0.28$ versus $\hat{\beta}_{HC}^{Unconstrained} = 0.14$). Within the constrained subsamples, the weaker signal of low convexity is also associated with larger loans, though the effect is less than half as large as high convexity in all cases and there is no effect of low convexity among unconstrained borrowers.

Table 2.5 presents similar results from regressions where *Spread* is the dependent variable. High convexity pricing grids are associated with a much larger reduction in loan spread in the constrained subsamples relative to the unconstrained subsamples, though the impact is economically significant for both groups. Borrowers that are unrated, non-dividend payers, and those with no prior lending relationship that use a high convexity loans have, on average, a spread that is 36 bps, 31 bps, and 37 bps lower than those with fixed spread loans. These estimates are both statistically different from and 20 bps, 8 bps, and 14 bps greater in magnitude than the point estimates for *High Convexity* in the corresponding unconstrained subsamples.

Overall, the results in Tables 2.3-2.5 support Predictions 1 and 3. A strong signal through a high convexity pricing grid has a large positive impact on terms of credit and the results are largest for opaque, financially constrained borrowers about whom lenders face more ex ante uncertainty.

2.5.2.2 Ex Post Outcomes: Predicting Financial Distress

There is certainly no random assignment of the presence and shape of PSD among good type borrowers in this study. Because of this, the results of the previous section may be subject to the endogeneity concerns that pervade nearly all studies in this literature. For example, some worries may include that it may be loan demand that drives the results (though a positive shift in the demand curve would predict higher, not lower, spreads) or that PSD users, despite their small size relative to large diversified lenders, get better loan terms as a premium for bearing additional risk. This section focuses on ex post outcomes to alleviate some of these worries.

If pricing grid convexity signals otherwise unobservable creditworthiness, then those that signal should be less likely to experience future financial distress (Prediction 2). As I described in Section 2.4, I follow previous literature and use thresholds of the borrower's interest coverage ratio for the periods following loan origination as a measure of financial distress. For each loan, I set the variable *Future Distress* equal to one if the borrower's interest coverage ratio falls below that threshold prior to the stated maturity of the loan.

The bivariate results from Table 2.2 show that borrowers who use high convexity loans are about half as likely to experience financial distress as those that use fixed spread contracts. These simple averages, however, do not control for observable credit risk variables. To make a more fitting comparison, I use multivariate regression to control for observable borrower specific characteristics at the time of origination that relate to credit risk with the following specification:[8]

$$Future\ Distress = \alpha + \delta_{HC}(High\ Convexity) + \delta_{LC}(Low\ Convexity) + \Psi X_{credit\ risk} + \epsilon$$

The financial distress literature has shown that borrowers with higher leverage, higher volatility, lower profitability and smaller size are more likely to experience financial distress. In addition to these variables, I include a full set of credit rating dummy variables to incorporate the observable opinion of credit rating agencies on borrowers' creditworthiness. I include the loan maturity as a control variable since loans with longer maturities have more time to find trouble. I also include year, industry, loan type and loan purpose dummies in $X_{credit\ risk}$. If a high convexity pricing grid indeed signals unobservable creditworthiness, then its presence

[8]I present results using a linear probability model for ease of interpretation. Estimation using logit and probit regression models provide similar results.

should be negatively related to future financial distress after controlling for other observable measures of credit risk. Empirically, Prediction 2 states $\hat{\delta}_{HC} < 0$.

A potential concern with the distress specification is the effect of the incremental interest expense burden from the new loan on future distress. As earlier results showed that high convexity loans have a lower spread, these loans will have an interest expense that is smaller for each dollar borrowed. However, it is not clear that the total interest expense burden will be lower on net since the earlier results also show that high convexity loans are larger. In any case, I control for incremental impact of the current loan's interest expense by including the ratio of the new interest payment to total assets (*New Interest Expense*) in the full specification. To ensure that the results reflect *future* distress, I drop all observations where the borrower is below the distress threshold at loan origination. Table 2.6 presents the results.

Model (1) indicates that borrowers with loans that have a performance pricing feature are about 2 percentage points (or about 17% based on the sample average of 12%) less likely to enter financial distress than those with loans that have a fixed spread loan. In model (3), with high convexity and low convexity indicators in the place of the generic PSD indicator, the *High Convexity* point estimate $\hat{\delta}_{HC}$ is negative, but not statistically significant at conventional levels with $p = 0.11$. Models (2) and (4) include the incremental interest expense of the loan. A larger incremental interest expense burden is associated with a higher likelihood that the borrower will experience future financial distress, but including this variable has little effect on the point estimates of the PSD signaling variables.

The full sample regression estimates in Table 2.6, however, ignore the level of ex ante information asymmetry. A signal is only useful to the extent that it conveys new information, so if there exists a relationship between the high convexity signal and future financial distress, it should be strongest in transactions where there is more uncertainty about the borrower's creditworthiness (Prediction 3). To test this, I estimate the specification separately for the constrained and unconstrained subsamples. Predictions 2 and 3 imply

$\hat{\delta}_{HC}^{Constrained} < \hat{\delta}_{HC}^{Unconstrained} \leq 0$ since there is more uncertainty about the prospects of constrained borrowers. Table 2.7 presents results.

Estimates from all three ex ante sorting schemes support the notion that the high convexity signal is more informative for opaque, financially constrained borrowers. For borrowers who are unrated, non-dividend paying, or have no prior relationship with the lender, high convexity is associated with a 4, 5, and 6 percentage point lower likelihood of experiencing financial distress prior to maturity. These estimates are statistically distinct from both zero and *Low Convexity* point estimates. In the unconstrained subsamples, the estimates of the PSD variable coefficients are not statistically different from zero. The point estimates on *High Convexity* support the prediction that loans using high convexity pricing grids communicate information about constrained borrowers' creditworthiness that conventional predictors of financial distress do not capture. With mean future distress rates that range from 13% to 15% for the constrained subsamples, borrowers who use high convexity PSD contracts are approximately one-third less likely to experience financial distress during the stated term of their loan than are observationally similar fixed spread borrowers.

2.5.3 Robustness: Alternative Ex Ante Sorting Schemes

In the main results section, I use the presence of a credit rating, borrower payout policy and prior lending relationship to divide the sample between borrowers who are more or less likely to face informational frictions and financial constraints. To test the sensitivity of the results to the sorting schemes, I repeat the regressions with the sample sorted along the following dimensions: (i) Size (total assets), (ii) age (number of years in Compustat), (iii) Whited and Wu (2006) index, and (iv) analyst estimates dispersion.[9] Table 2.8 presents the results of the subsample analysis for loan size, spread, and likelihood of distress. I include the full set of covariates from the earlier analysis, but only display the estimates for *High Convexity* and *Low Convexity* to save space.

[9]All results are also robust to using the SA index (Hadlock and Pierce, 2010) which combines size and age and analyst coverage to categorize the sample.

The results broadly support the earlier findings. While the results when dividing by age are not as strong for the *Spread* specification and not statistically significant for the distress specification (p-value of 0.12), high convexity is associated with much larger loans for young firms. Overall, Table 2.8 shows that the earlier results do not critically depend on specific ex ante sorting schemes.

2.6 Conclusions

Previous research has shown that asymmetric information and financial constraints can cause suboptimal firm policies and an inefficient allocation of capital in the economy. In this paper, I show how contracting parties design performance pricing grids in PSD bank loans mitigate informational frictions and ease financial constraints.

After establishing that PSD contracts, in general, receive more favorable terms of credit, I exploit the rich heterogeneity in the shape of the pricing grids to construct a measure of convexity which I interpret as the strength of the signal. High convexity contracts are costly for borrowers because they expose the borrower to additional risk and provide a smaller reward for improved performance with a larger punishment for decreased performance. I find that good borrowers, by subjecting themselves to these costly convex pricing grids, can effectively signal their otherwise unobservable high quality and receive larger loans and lower spreads. The effects of this costly signal are largest among opaque, financially constrained borrowers. I then test the relationship between convexity and ex post financial health. Consistent with the signaling hypothesis, I find that opaque, constrained borrowers who use convex PSD are about one third less likely to face financial distress than observationally similar borrowers. Together, these results highlight the ability of security design to communicate ex ante unobservable credit quality to ease financing frictions.

2.7 Figures

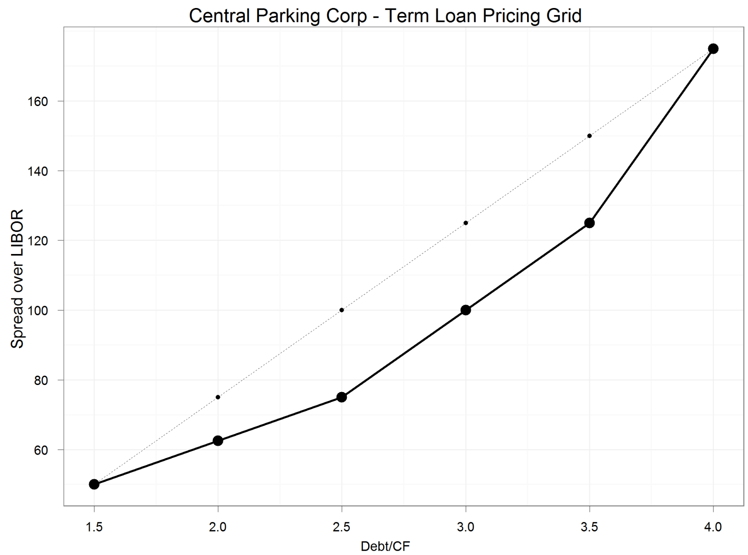


Figure 2.1: Performance Pricing Grid.
This figure presents an example pricing grid from a 1998 term loan to Central Parking Corp that maps their debt-to-cash flow ratio to a loan spread (basis points). The bold line is the actual grid and the dotted line represents a linear interpolation of the endpoints. The actual pricing grids are flat in each interval, with a step increase at each intermediate point (e.g., at 2.0, 2.5, and so on. I simply connect the points in the figure for illustrative purposes. With an initial spread of 100 bps and the linear interpolation predicting 125 bps, the convexity measure is 25 bps.

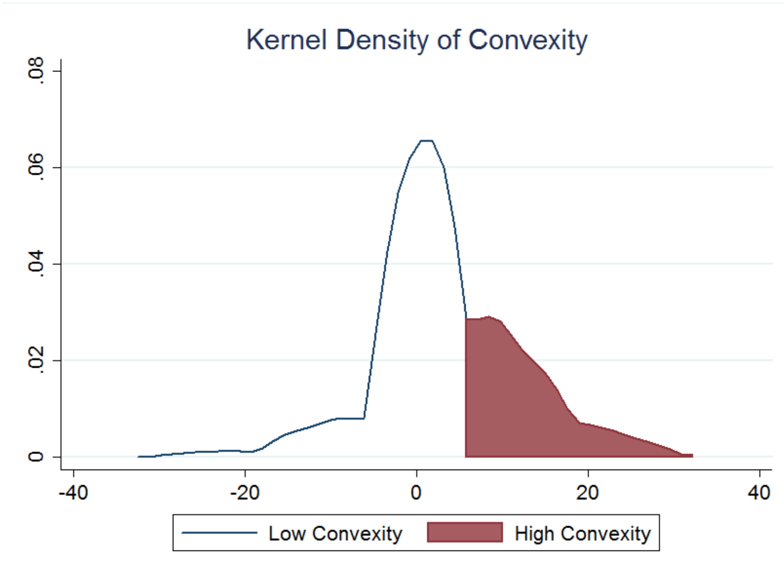


Figure 2.2: Sample Density of Pricing Grid Convexity: Debt-to-Cash Flow.
This figure presents the empirical distribution of the measure of convexity (in basis points) which I define as the difference between the level of the spread in the contract and the predicted value of the spread based on the linear interpolation of the pricing grid endpoints. For the analysis, I code the PSD loans that fall in top tercile of the distribution (the shaded region) as "High Convexity" and the remainder of the PSD loans as "Low Convexity."

2.8 Tables

Table 2.1: Sample Summary Statistics

This table presents summary statistics for loans from 1994-2010. See Table B.1 in the appendix for variable definitions.

	mean	sd	min	p25	p50	p75	max	count
Loan Characteristics:								
Spread (bps)	200.79	119.85	17.50	112.50	200.00	275.00	750.00	11297
Loan Size (mil)	241.39	385.28	1.00	35.00	100.00	262.00	2250.00	11297
Maturity (months)	49.31	22.79	5.00	36.00	58.00	60.00	180.00	11297
Secured	0.76	0.43	0.00	1.00	1.00	1.00	1.00	11297
Financial Covenants	1.68	1.17	0.00	1.00	2.00	2.00	6.00	11297
PSD	0.54	0.50	0.00	0.00	1.00	1.00	1.00	11297
Borrower Characteristics:								
Assets (mil)	2260.55	5371.48	8.56	194.89	594.25	1836.68	49221.90	11297
Leverage	0.34	0.27	0.00	0.18	0.31	0.46	6.92	11297
Market-to-Book	1.45	0.97	0.31	0.87	1.18	1.69	7.07	11297
Profitability	0.16	0.13	0.00	0.07	0.13	0.20	0.69	11297
Interest Coverage	16.79	44.85	-61.57	2.63	5.51	12.14	310.21	11297
EBITDA Volatility	0.02	0.02	0.00	0.01	0.01	0.02	0.15	11297
Constrained & Future Distress:								
Prior Lender Relationship	0.61	0.49	0.00	0.00	1.00	1.00	1.00	11297
Not Rated	0.55	0.50	0.00	0.00	1.00	1.00	1.00	11297
Non-Dividend Payer	0.63	0.48	0.00	0.00	1.00	1.00	1.00	11297
Future Distress	0.12	0.33	0.00	0.00	0.00	0.00	1.00	7208

Table 2.2: Summary Statistics by Contract Type

This table presents subsample means [medians] of loans from 1994-2010. *Fixed Spread* represents loans with no performance pricing grid. *High Convexity* dummy variable equal to one if the loan has a performance pricing grid that is in the top tercile of the convexity measure (see Section 2.4 for details). *Low Convexity* dummy variable equal to one if the loan has a performance pricing grid that is not in the top tercile of the convexity measure. *HC-Fixed* is the difference in means of the *High Convexity* and *Fixed Spread* subsamples. See Table B.1 in the appendix for variable definitions.

	Fixed Spread	Low Convexity	High Convexity	HC-Fixed
Loan Characteristics:				
Spread (bps)	236.72	183.84	143.91	-92.82***
	[225.00]	[175.00]	[125.00]	
Loan Size (mil)	198.85	264.58	303.06	104.21***
	[75.00]	[110.00]	[160.00]	
Maturity (months)	46.92	51.52	51.11	4.19***
	[47.00]	[60.00]	[60.00]	
Secured	0.85	0.73	0.60	-0.25***
	[1.00]	[1.00]	[1.00]	
Financial Covenants	1.29	2.02	2.02	0.73***
	[1.00]	[2.00]	[2.00]	
Borrower Characteristics:				
Assets (mil)	1916.12	2555.15	2564.38	648.26***
	[434.01]	[673.54]	[806.59]	
Leverage	0.36	0.34	0.32	-0.04***
	[0.32]	[0.31]	[0.30]	
Market-to-Book	1.39	1.48	1.55	0.16***
	[1.08]	[1.25]	[1.30]	
Profitability	0.13	0.18	0.18	0.04***
	[0.10]	[0.14]	[0.14]	
Interest Coverage	14.76	17.94	19.66	4.90***
	[4.17]	[6.36]	[7.36]	
EBITDA Volatility	0.02	0.02	0.01	-0.01***
	[0.01]	[0.01]	[0.01]	
Constrained & Future Distress:				
Prior Lender Relationship	0.54	0.65	0.71	0.17***
	[1.00]	[1.00]	[1.00]	
Not Rated	0.59	0.50	0.52	-0.07***
	[1.00]	[0.00]	[1.00]	
Non-Dividend Payer	0.69	0.59	0.54	-0.15***
	[1.00]	[1.00]	[1.00]	
Future Distress	0.17	0.11	0.08	-0.09***
	[0.00]	[0.00]	[0.00]	
Observations†	5239	3915	2143	

† *Observations* applies to all variables with the exception of *Future Distress*, whose proportions are similar, but has less observations due to data restrictions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.3: Terms of Credit - Full Sample

This table presents OLS estimates from regressions of *log(Loan Size)* in Models (1)-(3) and *Spread* in Models (4)-(6) on borrower and loan characteristics. *log(Loan Size)* is the log of the loan facility amount in millions of dollars. *Spread* is the interest rate spread in basis points. *High Convexity* dummy variable equal to one if the loan has a performance pricing grid that is in the top tercile of the convexity measure (see Section 2.4 for details). *Low Convexity* dummy variable equal to one if the loan has a performance pricing grid that is not in the top tercile of the convexity measure. Table B.1 in the appendix presents the construction of the other covariates. All specifications include a full set of year, 2 digit SIC industry, loan type, loan purpose, and credit rating dummies. All standard errors are clustered by borrower.

	log(Loan Size)			Spread		
	(1)	(2)	(3)	(4)	(5)	(6)
PSD	0.16***			-21.57***		
	(0.00)			(0.00)		
High Convexity		0.25***	0.20***		-34.96***	-27.01***
		(0.00)	(0.00)		(0.00)	(0.00)
Low Convexity		0.11***	0.07***		-14.51***	-12.38***
		(0.00)	(0.00)		(0.00)	(0.00)
log(Assets)	0.70***	0.70***	0.67***	-14.55***	-14.27***	-2.18
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.13)
Leverage	0.21***	0.21***	0.18***	25.23***	25.39***	26.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Market-to-Book	0.03**	0.02**	0.02**	-6.28***	-6.03***	-4.74***
	(0.02)	(0.03)	(0.05)	(0.00)	(0.00)	(0.00)
Profitability	0.27**	0.27**	0.23*	-44.16***	-44.82***	-38.82***
	(0.03)	(0.02)	(0.05)	(0.00)	(0.00)	(0.01)
log(1+Interest Coverage)	0.03***	0.03***	0.01	-24.19***	-24.03***	-20.10***
	(0.00)	(0.00)	(0.21)	(0.00)	(0.00)	(0.00)
CF Volatility	2.25***	2.30***	2.55***	180.23***	173.18***	190.06***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Relationship	0.14***	0.13***	0.14***	-5.45**	-4.93**	-4.22**
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.03)
log(Maturity)			0.35***			-10.93***
			(0.00)			(0.00)
Financial Covenants			-0.02**			2.56**
			(0.04)			(0.03)
Secured			-0.20***			55.95***
			(0.00)			(0.00)
log(Loan Size)						-11.62***
						(0.00)
Constant	-0.47**	-0.45*	-1.42***	537.66***	534.44***	495.30***
	(0.05)	(0.06)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	10842	10842	10842	10842	10842	10842
R^2	0.74	0.74	0.75	0.56	0.56	0.59

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.4: Loan Size - Subsample Analysis

This table presents OLS estimates from regressions of *log(Loan Size)* on borrower and loan characteristics within subsets of the sample sorted by ex ante measures of financial constraint and information asymmetry. *log(Loan Size)* is the log of the loan facility amount in millions of dollars. *High Convexity* dummy variable equal to one if the loan has a performance pricing grid that is in the top tercile of the convexity measure (see Section 2.4 for details). *Low Convexity* dummy variable equal to one if the loan has a performance pricing grid that is not in the top tercile of the convexity measure. Table B.1 in the appendix presents the construction of the other covariates. $\hat{\beta}_{HC}^{Const} = \hat{\beta}_{HC}^{Unconst}$ presents the p-values for the Wald test statistic of equality of the coefficients on *High Convexity* between complementary subsamples. All specifications include a full set of year, 2 digit SIC industry, loan type, loan purpose, and credit rating dummies. All standard errors are clustered by borrower.

	Credit Rating		Dividend Payer		Prior Relationship	
	(1) Yes	(2) No	(3) Yes	(4) No	(5) Yes	(6) No
High Convexity	0.14***	0.21***	0.14***	0.23***	0.14***	0.28***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Low Convexity	0.03	0.09***	0.05	0.07***	0.02	0.13***
	(0.34)	(0.00)	(0.17)	(0.01)	(0.34)	(0.00)
Relationship	0.12***	0.16***	0.10***	0.16***	0.11***	
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	
log(Assets)	0.60***	0.71***	0.64***	0.69***	0.68***	0.66***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Leverage	0.12**	0.16**	0.17***	0.20***	0.20***	0.17***
	(0.02)	(0.02)	(0.00)	(0.00)	(0.01)	(0.00)
Market-to-Book	0.07***	-0.01	0.05***	0.01	0.04**	0.01
	(0.00)	(0.49)	(0.00)	(0.52)	(0.01)	(0.64)
Profitability	0.18	0.35**	0.18	0.21	0.17	0.25
	(0.32)	(0.02)	(0.38)	(0.13)	(0.25)	(0.11)
log(1+Interest Coverage)	0.04*	-0.00	0.04**	0.01	0.01	0.01
	(0.05)	(0.87)	(0.03)	(0.68)	(0.45)	(0.40)
CF Volatility	1.86*	3.18***	0.76	3.11***	2.39***	2.62**
	(0.07)	(0.00)	(0.41)	(0.00)	(0.00)	(0.01)
log(Maturity)	0.32***	0.35***	0.40***	0.32***	0.33***	0.36***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Financial Covenants	-0.02	-0.02	-0.00	-0.03***	-0.03**	-0.01
	(0.11)	(0.15)	(0.80)	(0.01)	(0.02)	(0.30)
Secured	-0.29***	-0.13***	-0.25***	-0.13***	-0.15***	-0.27***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	-0.50*	-1.81***	-1.42***	-1.48***	-1.03***	-1.72***
	(0.07)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	5046	5796	4133	6709	6710	4132
R^2	0.61	0.70	0.74	0.73	0.73	0.74
$\hat{\beta}_{HC}^{Const} = \hat{\beta}_{HC}^{Unconst}$		0.07		0.04		0.00

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.5: Interest Rate Spread - Subsample Analysis

This table presents OLS estimates from regressions of *Spread* on borrower and loan characteristics within subsets of the sample sorted by ex ante measures of financial constraint and information asymmetry. *Spread* is the interest rate spread in basis points. *High Convexity* dummy variable equal to one if the loan has a performance pricing grid that is in the top tercile of the convexity measure (see Section 2.4 for details). *Low Convexity* dummy variable equal to one if the loan has a performance pricing grid that is not in the top tercile of the convexity measure. Table B.1 in the appendix presents the construction of the other covariates. $\hat{\beta}_{HC}^{Const} = \hat{\beta}_{HC}^{Unconst}$ presents the p-values for the Wald test statistic of equality of the coefficients on *High Convexity* between complementary subsamples. All specifications include a full set of year, 2 digit SIC industry, loan type, loan purpose, and credit rating dummies. All standard errors are clustered by borrower.

	Credit Rating		Dividend Payer		Prior Relationship	
	(1) Yes	(2) No	(3) Yes	(4) No	(5) Yes	(6) No
High Convexity	-16.07***	-35.91***	-22.81***	-30.98***	-22.43***	-36.95***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Low Convexity	-5.52	-18.50***	-11.85***	-12.38***	-9.18***	-17.07***
	(0.12)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Relationship	-3.53	-4.40	-0.03	-6.63**	-4.73	
	(0.15)	(0.13)	(0.99)	(0.01)	(0.18)	
log(Assets)	4.36**	-5.08**	-2.19	0.18	-0.28	-3.87*
	(0.05)	(0.01)	(0.29)	(0.93)	(0.87)	(0.09)
Leverage	21.72**	45.09***	5.79	41.16***	33.83***	20.25*
	(0.01)	(0.00)	(0.32)	(0.00)	(0.00)	(0.09)
Market-to-Book	-8.17***	-3.70**	-3.07	-6.10***	-6.01***	-4.07**
	(0.00)	(0.02)	(0.14)	(0.00)	(0.00)	(0.04)
Profitability	-42.96***	-34.17	-43.20**	-33.22*	-44.22**	-22.44
	(0.01)	(0.10)	(0.02)	(0.08)	(0.01)	(0.28)
log(1+Interest Coverage)	-7.87***	-19.74***	-17.84***	-19.78***	-19.30***	-19.95***
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
CF Volatility	12.73	257.99***	314.73***	192.07***	151.12**	244.41***
	(0.90)	(0.00)	(0.00)	(0.01)	(0.02)	(0.00)
log(Maturity)	-11.54**	-10.30***	-4.60	-14.07***	-15.70***	-4.19
	(0.02)	(0.00)	(0.26)	(0.00)	(0.00)	(0.35)
Financial Covenants	1.92	3.04*	2.94*	2.75*	2.27	3.72**
	(0.21)	(0.05)	(0.05)	(0.07)	(0.12)	(0.03)
Secured	42.94***	56.45***	57.27***	49.40***	53.61***	60.23***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
log(Loan Size)	-13.44***	-10.43***	-10.64***	-12.64***	-12.10***	-11.14***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	427.51***	485.78***	432.44***	510.36***	527.03***	431.41***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	5046	5796	4133	6709	6710	4132
R^2	0.71	0.50	0.70	0.51	0.64	0.52
$\hat{\beta}_{HC}^{Const} = \hat{\beta}_{HC}^{Unconst}$		0.00		0.09		0.01

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.6: Predicting Financial Distress - Full Sample

This table presents OLS estimates from regressions of *Future Distress* on borrower and loan characteristics typically associated with predicting financial distress. *High Convexity* dummy variable equal to one if the loan has a performance pricing grid that is in the top tercile of the convexity measure (see Section 2.4 for details). *Low Convexity* dummy variable equal to one if the loan has a performance pricing grid that is not in the top tercile of the convexity measure. *New Interest Expense* is the increase in the borrowers interest expense burden from the current loan. Table B.1 in the appendix presents the construction of the other covariates. All specifications include a full set of year, 2 digit SIC industry, loan type, loan purpose, and credit rating dummies. All standard errors are clustered by borrower.

	(1) All	(2) All	(3) All	(4) All
PSD	-0.02**	-0.02*		
	(0.04)	(0.05)		
High Convexity			-0.02	-0.02
			(0.11)	(0.12)
Low Convexity			-0.01	-0.01
			(0.50)	(0.54)
log(Assets)	-0.01**	-0.01**	-0.01**	-0.01**
	(0.01)	(0.04)	(0.01)	(0.04)
Leverage	0.05	0.05	0.05	0.05
	(0.14)	(0.15)	(0.13)	(0.14)
Profitability	-0.34***	-0.33***	-0.34***	-0.33***
	(0.00)	(0.00)	(0.00)	(0.00)
EBITDA Volatility	1.71***	1.56***	1.70***	1.55***
	(0.00)	(0.00)	(0.00)	(0.00)
Market-to-Book	0.01	0.01	0.01	0.01
	(0.20)	(0.16)	(0.17)	(0.14)
log(Maturity)	0.05***	0.04***	0.05***	0.04***
	(0.00)	(0.00)	(0.00)	(0.01)
New Interest Expense		1.20***		1.21***
		(0.00)		(0.00)
Constant	-0.16	-0.17	-0.15	-0.17
	(0.14)	(0.11)	(0.16)	(0.12)
Observations	7208	7208	7208	7208
R^2	0.14	0.15	0.14	0.15

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.7: Predicting Financial Distress - Subsample Analysis

This table presents OLS estimates from regressions of *Future Distress* on borrower and loan characteristics typically associated with predicting financial distress within subsets of the sample sorted by ex ante measures of financial constraint and information asymmetry. *High Convexity* dummy variable equal to one if the loan has a performance pricing grid that is in the top tercile of the convexity measure (see Section 2.4 for details). *Low Convexity* dummy variable equal to one if the loan has a performance pricing grid that is not in the top tercile of the convexity measure. *New Interest Expense* is the increase in the borrowers interest expense burden from the current loan. Table B.1 in the appendix presents the construction of the other covariates. $\hat{\delta}_{HC}^{Const} = \hat{\delta}_{HC}^{Unconst}$ presents the p-values for the Wald test statistic of equality of the coefficients on *High Convexity* between complementary subsamples. The *Mean Distress* statistics at the bottom of the table presents the sample mean *Future Distress.* All specifications include a full set of year, 2 digit SIC industry, loan type, loan purpose, and credit rating dummies. All standard errors are clustered by borrower.

	Credit Rating		Dividend Payer		Prior Relationship	
	(1) Yes	(2) No	(3) Yes	(4) No	(5) Yes	(6) No
High Convexity	0.00	-0.04**	-0.00	-0.05***	0.00	-0.06**
	(0.81)	(0.03)	(0.91)	(0.01)	(0.87)	(0.01)
Low Convexity	0.01	-0.02	-0.01	-0.01	-0.01	-0.02
	(0.59)	(0.35)	(0.71)	(0.35)	(0.61)	(0.46)
log(Assets)	0.00	-0.01	-0.03***	-0.00	-0.02**	0.01
	(0.68)	(0.13)	(0.00)	(0.89)	(0.02)	(0.43)
Leverage	0.09	0.02	0.16**	-0.01	0.06	0.03
	(0.12)	(0.67)	(0.01)	(0.71)	(0.17)	(0.49)
Profitability	-0.11	-0.46***	-0.34***	-0.34***	-0.16	-0.49***
	(0.34)	(0.00)	(0.00)	(0.00)	(0.11)	(0.00)
EBITDA Volatility	0.99	1.58***	1.85***	1.39***	2.02***	0.90*
	(0.12)	(0.00)	(0.00)	(0.00)	(0.00)	(0.07)
Market-to-Book	0.02	0.01	0.02	0.01	0.00	0.01
	(0.14)	(0.16)	(0.21)	(0.37)	(0.62)	(0.27)
log(Maturity)	0.00	0.07***	0.04*	0.05**	0.04**	0.06**
	(0.99)	(0.00)	(0.08)	(0.01)	(0.03)	(0.01)
New Interest Expense	2.88	1.10***	-0.97	1.30***	1.01**	1.33***
	(0.29)	(0.00)	(0.68)	(0.00)	(0.02)	(0.00)
Constant	-0.32**	-0.09	0.08	-0.14	0.06	-0.41***
	(0.03)	(0.35)	(0.54)	(0.28)	(0.63)	(0.01)
Observations	3099	4109	2604	4604	4378	2830
R^2	0.27	0.17	0.26	0.17	0.19	0.21
$\hat{\delta}_{HC}^{Const} = \hat{\delta}_{HC}^{Unconst}$		0.02		0.09		0.01
Mean Distress	0.08	0.14	0.09	0.13	0.10	0.15

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.8: Robustness - Alternate Sorting Schemes

This table presents OLS estimates from regressions of *log(Loan Size)* (Panel A), *Spread* (Panel B) and *Future Distress* (Panel C) on the full set of covariates, whose point estimates are omitted to save space, used in Tables 2.4, 2.5, and 2.7, respectively. The sort the sample to compare the top and bottom terciles of *Size*, *Age*, *Whited-Wu index*, and *Analyst Dispersion*. *High Convexity* dummy variable equal to one if the loan has a performance pricing grid that is in the top tercile of the convexity measure (see Section 2.4 for details). *Low Convexity* dummy variable equal to one if the loan has a performance pricing grid that is not in the top tercile of the convexity measure. $\hat{\beta}_{HC}^{Const} = \hat{\beta}_{HC}^{Unconst}$ presents the p-values for the Wald test statistic of equality of the coefficients on *High Convexity* between complementary subsamples. The *Subsample Mean Distress* statistics at the bottom of the table presents the sample mean *Future Distress*. All specifications include a full set of year, 2 digit SIC industry, loan type, loan purpose, and credit rating dummies. All standard errors are clustered by borrower.

Panel A: Loan Size

	Size		Age		Whited-Wu		Analyst Dispersion	
	Small	Large	Young	Old	Low	High	Low	High
High Convexity	0.20***	0.10**	0.24***	0.11***	0.10***	0.19***	0.13***	0.24***
	(0.00)	(0.02)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
Low Convexity	0.12***	0.01	0.06*	0.04	0.04	0.09***	0.02	0.07*
	(0.00)	(0.81)	(0.10)	(0.22)	(0.31)	(0.01)	(0.57)	(0.09)
Observations	3177	3514	3372	3671	3370	3201	2242	2470
R^2	0.53	0.43	0.71	0.78	0.52	0.61	0.75	0.68
$\hat{\beta}_{HC}^{Const} = \hat{\beta}_{HC}^{Unconst}$		0.08		0.02		0.11		0.10

Panel B: Spread

	Small	Large	Young	Old	Low	High	Low	High
High Convexity	-32.87***	-17.37***	-22.98***	-21.00***	-22.25***	-36.75***	-14.38***	-31.99***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Low Convexity	-17.96***	-8.52**	-11.75***	-9.16**	-14.22***	-16.72***	0.57	-15.09**
	(0.00)	(0.03)	(0.01)	(0.03)	(0.00)	(0.00)	(0.89)	(0.02)
Observations	3177	3514	3372	3671	3370	3201	2242	2470
R^2	0.42	0.73	0.52	0.69	0.74	0.45	0.67	0.54
$\hat{\beta}_{HC}^{Const} = \hat{\beta}_{HC}^{Unconst}$		0.02		0.74		0.03		0.01

Panel C: Distress

	Small	Large	Young	Old	Low	High	Low	High
High Convexity	-0.06**	0.00	-0.05	-0.01	-0.02	-0.09**	-0.03	-0.07*
	(0.05)	(0.85)	(0.12)	(0.74)	(0.24)	(0.01)	(0.16)	(0.07)
Low Convexity	-0.03	0.00	0.00	0.01	-0.02	-0.00	0.01	-0.04
	(0.27)	(0.85)	(0.99)	(0.52)	(0.33)	(0.94)	(0.61)	(0.20)
Observations	2563	1909	2426	2166	1825	2170	1510	1385
R^2	0.18	0.35	0.22	0.28	0.29	0.22	0.26	0.32
$\hat{\delta}_{HC}^{Const} = \hat{\delta}_{HC}^{Unconst}$		0.03		0.08		0.16		0.27
Subsample Mean Distress	0.16	0.06	0.18	0.08	0.06	0.17	0.07	0.16

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

CHAPTER III

Design of Financial Securities: Empirical Evidence From Private-Label RMBS Deals

3.1 Abstract

Using a representative sample of residential mortgage-backed security (RMBS) deals from the pre-crisis period, we show that deals with a higher level of equity tranche have a significantly lower foreclosure rate that cannot be explained away by the underlying loan pool's observable credit risk factors. The effect is concentrated within pools with a higher likelihood of asymmetric information between deal sponsors and potential buyers of the securities. Further, securities that are sold from high-equity-tranche deals command higher prices conditional on their credit ratings. Our study provides the first in-depth analysis of the effectiveness of the equity tranche in mitigating informational frictions in this market.

3.2 Introduction

Securitization provides numerous economic benefits to borrowers and lenders such as more favorable terms of credit for borrowers and better liquidity and risk sharing for lenders. However, each step in the securitization process also introduces potentially costly conflicts of interest.[1] At the root of these frictions is the information asymmetry between different agents along the securitization chain. Understanding the various institutional mechanisms and security design solutions that can overcome these problems and facilitate the functioning of these markets has important implications for both the economic theory underlying securitization markets and ongoing policy debates.[2] However, there is surprisingly little empirical work in this area. To fill this gap in the literature, we examine the role of the equity tranche in residential mortgage-backed security (RMBS) deals in mitigating informational frictions between deal sponsors and investors.

RMBS sponsors create financial securities by pooling several mortgages together and then issuing marketable tranches against the pool's combined cash flows. Security design, therefore, is at the very core of the existence of this market. RMBS sponsors can convey their private information to potential investors by retaining a larger financial interest in the asset's performance (Leland and Pyle, 1977). Motivated by theoretical models such as Gorton and Pennacchi (1990), Boot and Thakor (1993), Riddiough (1997), DeMarzo and Duffie (1999), DeMarzo (2005), and Hartman-Glaser et al. (2011), we analyze three main questions in this paper. First, conditional on observable risk metrics of the underlying pool, does the size of the equity tranche increase with the degree of information asymmetry between deal sponsors and potential buyers of these securities? Second, conditional on the degree of information asymmetry, do pools with a higher level of equity tranche perform better ex-post as compared to observationally similar pools with lower levels of equity tranche? And third, do security buyers pay higher prices for securities sold in high-equity-tranche deals as compared to a

[1] See, for example, Keys et al. (2012), Gorton and Metrick (2012) for recent surveys; Keys et al. (2010), Mian and Sufi (2009), Purnanandam (2011), Demyanyk and Van Hemert (2011), Je et al. (2012), Loutskina and Strahan (2011), Acharya et al. (2009) for work related to the subprime mortgage crisis; and Ashcraft and Schuermann (2008) for a detailed analysis of the securitization process.

[2] For example, issues surrounding the equity tranche of securitization deals form an important part of the Dodd-Frank Reform Act. In discussing the effects of risk retention requirements pursuant to the Section 946 of the Dodd-Frank Wall Street Reform and Consumer Protection Act, the treasury secretary stresses the importance of this tool in mitigating some contracting frictions and notes that: "...the academic literature on risk retention with respect to asset-backed securitization is limited." Scharfstein and Sunderam (2011) examine some other recent policy proposals and provide suggestions for the more broad reform of the housing finance system.

similarly rated security in low-equity-tranche deals?

We carefully assemble a representative sample that comprises about 500,000 loans bundled into 196 private-label RMBS deals from 2001-02 and 2005. Our sample covers a wide cross-section of banks and borrowers. We combine tranche-level security data with the underlying pool characteristics at the time of RMBS issuance, and track the default performance of each loan in these pools through December 2011. This comprehensive information on the loan characteristics of the underlying pool, tranche-level security data, and the ex-post foreclosure status of each loan in the pool allows us to examine the three questions posed above.

We use the percentage of no-documentation loans in a pool as a cross-sectional measure of information asymmetry between the deal sponsors and investors. There is no verification of the borrower's income or assets for these loans, and unlike full-documentation loans, they are not accompanied by key information sources like federal and state income tax filings. This leaves a great degree of discretion with the originating institutions in terms of verifying employment and the level and stability of the borrower's income. Soft pieces of information like these are lost as loans pass through the securitization chain, widening the information gap between the sponsor and the investor.[3] We find that deals with a higher proportion of no-documentation loans have significantly higher levels of equity tranche after controlling for the effects of observable pool characteristics such as FICO score and Loan-to-Value ratio (LTV). This finding is consistent with the key idea that investors are likely to have higher adverse selection concerns in relatively opaque deals, which in turn motivates the sponsors to create a larger informationally sensitive first-loss equity tranche (DeMarzo and Duffie, 1999). We also find that measures of observable credit risk, such as FICO score and loan-to-value (LTV) ratio, are unrelated to the size of the equity tranche. However, these variables, and *not* the proportion of no-documentation loans in the pool, drive the division of the sold tranches

[3]The use of this measure is also in the spirit of the "opacity" measure of theoretical papers by Skerta and Veldkamp (2009) and Sangiorgi, Sokobin, and Spatt (2009). Our assumption is that the information asymmetry between sponsors and buyers increases with the loan opacity.

between AAA and mezzanine groups. These results suggest that concerns about asymmetric information explain the split between sold and initially unsold (equity) tranches, whereas observable and easier to price characteristics of the pool explain the relative distribution between AAA and mezzanine tranches.[4]

We next turn to our main question: does the size of the equity tranche serve as a signal of the sponsor's private information about the pool quality? While we do not, by definition, observe the sponsor's private information at the time of RMBS issuance, we do observe the ex-post default performance (i.e., the foreclosure status) of every loan in our pools. The ex-post default performance of a loan can be decomposed into three parts: (a) a component that is entirely driven by observable information such as the borrower's FICO score, LTV ratio, the geographical location of the property, and the nature of interest rate on the loan; (b) a component that is entirely driven by common macroeconomic shocks affecting all loans in the economy; and (c) a residual component. We relate the level of equity tranche created at the time of security issuance to the residual component to assess the relationship between private information at issuance and subsequent loan performance. If the level of equity tranche serves as a signal of the sponsor's private information about the pool, then we should find lower abnormal default (i.e., lower residual component) for loans from high-equity-tranche pools. In contrast, if the level of equity tranche is unrelated to the seller's private information, it should not correlate with the residual component of default.

We implement this idea using two models of default prediction. In the first model, we compute the expected default rate for each loan in the pool by fitting a default prediction model that accounts for the component of default that is driven by observable loan and property characteristics along with the year of loan origination. The difference between the actual pool-level default rate we observe and the expected default rate of the pool from the fitted model is our first measure of abnormal default rate. In the second model, we use

[4]Consistent with this idea, we also find that the hard pieces of information explain the pricing of individual mortgages very well, whereas the extent of no-documentation loans has no effect on pricing measures (see also Rajan et al., forthcominga).

our sample of about 500,000 loans to create an observationally similar matched pool for each actual pool. We use a loan-by-loan matching algorithm, which is then aggregated to the pool level, that ensures that the actual and matched pools are similar on dimensions such as FICO scores, LTV ratio, loan product type, and the property location. The default rate of the actual pool over and above its match is our second measure of abnormal default rate. By comparing the pool with their match, we effectively difference out the effects of observable credit and macroeconomic risks as well as the correlation structure of the pool to the extent that it is driven by geographical diversity. Critically, the matched pool, by construction, lacks the private information component that is present in the actual pool. Thus, the difference in the realized default rate of actual and matched pools' default rates provides our second measure of abnormal default.

We find that deals with higher equity tranche have significantly lower abnormal foreclosure rate, and this effect is concentrated among pools with a higher proportion of no-documentation loans. Said differently, for relatively opaque pools, higher equity tranche predicts better performance in future. In economic terms, pools with above-median level of equity tranche have 24-27% lower foreclosure rates that cannot be explained away by observable credit risk characteristics and macroeconomic conditions. The effect of the equity tranche for relatively transparent pools is statistically indistinguishable from zero. These results are consistent with the idea that sponsors create a larger equity tranche in deals with favorable information on unobservable dimensions.

Are our results driven by asymmetric information concerns or simply a lack of information with all agents in the opaque pools? To answer this question more precisely, we conduct an important sub-sample test based on the originator's affiliation with the sponsors. Prior research suggests that sponsors are more likely to possess private information about the underlying loan pools when they are also the loan originators (e.g., see Keys et al., 2010). Consistent with our asymmetric information argument, we find that the effect of equity tranche on ex-post loan performance is stronger in deals where the sponsors are also the top loan originators, i.e., in deals where the sponsors are likely to have better access to private information.

We provide further evidence in support of private information content of equity tranche by exploiting the passage of Anti-Predatory Lending (APL) laws across several states during our sample period. These laws put stricter requirements on the lenders in terms of their lending practices and disclosure policy which, on the margin, made it more difficult for the lenders to originate poor-quality loans. Such a government regulation should reduce the lemons problem in the market, making the use of private contracting mechanisms less important. Therefore, prior to the passage of this law, the equity tranche is likely to serve as a more important signal of private information for loans originated in APL states. At the same time, the states that do not pass such laws should experience no systematic change in the relationship between the equity tranche and abnormal default rate. Consistent with this idea, we show that loans originated in APL states in the pre-passage period default at disproportionately lower rate if they are backed by higher equity tranche.

In our third test, we study the pricing implications of equity tranche. If a higher level of equity tranche conveys the sponsors' positive private information about the pool, then the market should respond to this signal by paying a higher price for the sold tranches of the deal. To separate out the mechanical leverage effect of a higher level of equity tranche, we condition our analysis on the credit ratings of sold tranches. Thus, we estimate the effect of equity tranche on the yield spread of sold tranches after controlling for the credit rating of the security. Since security prices are not directly available, following earlier literature we take yield spread, defined as the markup over a risk-free benchmark rate, as the measure of pricing (see Je et al. (2012)). We find that sold tranches command higher prices (i.e., lower yield spread) for the same credit rating class if they are backed by higher equity tranche. Again, the effect is concentrated within opaque pools, giving further support for the interpretation that the result is not driven by the mechanical leverage effect of equity tranche. In addition, the effect is stronger for the more informationally sensitive non-AAA-rated tranches. Together, these results show that opaque pools with a higher level of equity tranche have lower abnormal default rate ex-post, and ex-ante, they command a higher price. These findings are consistent with the idea that the equity tranche serves as a mechanism to convey the sponsor's private information to potential buyers.

Some have argued that the equity tranche lost its signaling role during the pre-crisis period because deal sponsors are free to sell them to other entities. Our empirical tests provide evidence contradicting these claims. While we cannot track the ownership of the equity tranche over time directly, sponsors did have a considerable amount of retained interest in mortgage-backed securities on their balance sheets during our sample period.[5] In addition, the buyers of equity tranches in the secondary market were often active hedge funds or CDO managers whereas the more senior tranches were typically bought by less sophisticated investors such as retirement funds.[6] Such a segmentation in this market is likely to provide incentives to deal sponsors to retain relatively larger portion of better deals since equity tranches are sold to relatively more informed buyers. In addition, some of the sales of equity tranches were motivated by regulatory capital arbitrage considerations in which the sponsor retained residual interest in the risk (see Acharya et al. (2013)). Our analysis shows that, despite the possibility of subsequent sale, a higher level of equity tranche at issuance predicts better future performance beyond what can be explained by observed credit risk factors, and markets reflected this quality by paying a higher price for securities from these deals.

Our study connects to several strands of literature in banking, securitization, and real estate finance. Griffin and Tang (2012b) study rating inflation in a large sample of CDOs from 1997 to 2007 and conclude that rating agencies used their subjective assessment to

[5]For example, Goldman Sachs' 2005 annual report states, "During the years ended November 2005 and November 2004, the firm securitized \$92.00 billion and \$62.93 billion, respectively, of financial assets, including \$65.18 billion and \$47.46 billion, respectively, of residential mortgage-backed securities." The report also shows the value of their retained interests in mortgage-backed securities to be \$2.928 billion and \$1.798 billion, respectively, for those time periods. A back of the envelope calculation suggests that (2.928-1.798)/62.93 = 1.73% was retained during this time period. While this is only a rough approximation, it clearly shows that deal sponsors did retain at least a piece of these securities. A similar computation using information from Merrill Lynch's annual reports gives an estimate of 2.84%.

[6]For example, see the representative deal from CitiBank in Financial Crisis Inquiry Commission, Figure 7.2 on page 116.

increase the size of AAA-rated tranche beyond the model-implied objective level. Ashcraft et al. (2010) report a significant decline in RMBS subordination levels between 2005 and mid-2007 and show that the ratings are correlated with ex-ante credit risk measures and they do explain subsequent deal performance.[7] Our study is related to Demiroglu and James (2012) who show that linkages between syndicate members, namely the originators and sponsors, can result in better ex-post performance of the securitization deals. Hartman-Glaser (2012) studies the effect of seller's reputation capital in these contracts. Je et al. (2012) show the influence of large sponsors on credit rating agencies. An et al. (2011) study the role of conduit lenders in mitigating informational problems in CMBS deals. Our work also relates to a growing and large literature regarding the conflicts of interest in the securitization market (see Je et al., 2012; Keys et al., 2010; Purnanandam, 2011; Downing et al., 2009).[8] Unlike these studies, our paper does not study the motivations behind and differences in securitized versus retained loans, or the possibility of originator moral hazard that comes with securitization.[9] Instead we highlight the effect of informational frictions within the set of securitized deals and the RMBS contract's ability to mitigate some of these frictions.

Much of the extant literature focuses on the informativeness of ratings, the optimal subordination level, the effect of syndicate structure on deal performance, and the possibility of rating inflation during the years leading up to the crisis. Our paper is the first to provide an in-depth examination of the role of the equity tranche in mitigating informational frictions between deal sponsors and investors in securitization markets. The rest of the paper is organized as follows. Section 2 discusses the theoretical motivation and develops the main hypotheses of the paper. Section 3 describes the data. Section 4 presents the results and Section 5 concludes the paper.

[7]See Cornaggia and Cornaggia (forthcoming), Becker and Milbourn (2011) and Bongaerts et al. (2012) for some recent studies on credit ratings for corporate bonds.

[8]See Benmelech et al. (2012) on securitization in the case of Collateralized Loan Obligations and Nadauld and Weisbach (2012) for the effect of securitization on the cost of debt.

[9]An originate-to-hold model of lending can be viewed as a limiting case of an RMBS deal where the entire stake is kept by the originating bank. From that perspective, our empirical findings are consistent with the basic idea of this literature: as the sellers stake in the deal increases, the underlying loans perform better in future.

3.3 Hypothesis Development

Absent any market frictions, the pooling and tranching of securities cannot be a value enhancing security design. Theoretical research, therefore, focuses on frictions such as information asymmetries, transactions costs, and market incompleteness to explain a financial intermediary's motivations behind asset-backed securitization. At a broad level, the optimal design of financial securities serves as a mechanism to resolve inefficiencies through costly signaling (e.g., Leland and Pyle, 1977; DeMarzo and Duffie, 1999), allocation of cash flow rights (e.g., Townsend, 1979; Gale and Hellwig, 1985), or allocation of control rights (e.g., Aghion and Bolton, 1992).[10] We focus on the asymmetric information-based theories in the paper for two main reasons. First, in recent years there has been considerable discussion and debate among academics, practitioners, and regulators regarding the presence of information problems in this market. Second, information-based theories provide testable cross-sectional hypotheses that have important policy implications for this market.

We do not attempt to test any specific theoretical model in this paper. Instead, we develop our hypotheses based on the collective insight of theoretical models of security sales in the presence of asymmetric information. When an uninformed agent buys financial securities from an informed seller, he faces an adverse selection problem which, in turn, imposes a cost on the informed seller. This problem becomes more severe as the fraction of the asset the seller desires to sell increases. However, by selling a higher fraction of assets to outsiders, sponsors are able to redeploy their capital at attractive rates. Optimizing sellers, therefore, face a trade-off between the benefits from selling a larger fraction of assets with the cost of an adverse selection, or "lemons," discount demanded by the buyer.[11] In equilibrium, sellers retain a fraction of the risky assets to signal the quality of the asset (Leland and Pyle, 1977).

[10]This is not a comprehensive list of design solutions. There are other motivations for security design such as transaction costs and market incompleteness. For example, in an incomplete markets setting, Allen and Gale (1988) argue that optimal security design assigns state-contingent cash flows to the agents that values it the most in that state.

[11]Financial institutions face considerable regulatory capital charge for retaining equity tranche on their balance sheets. This provides a direct justification for the use of equity tranche as a costly signal.

Consider a mortgage i in pool p and denote its payoff by a random variable $\tilde{Y}_{ip}$. Let X_{ip} be a set of publicly observable loan characteristics such as FICO score and loan-to-value ratio. We can then express the loan's payoff conditional on observable signals as follows:

$$\tilde{Y}_{ip}|X_{ip} = \tilde{I}_{ip} + \tilde{z}_{ip} \tag{3.1}$$

$\tilde{I}_{ip}$ is the private information of the sponsor and $\tilde{z}_{ip}$ represents a random shock to the loan's performance. I_{ip} is a known quantity to the sponsor, but remains a random variable to outside investors.

As the distribution of $\tilde{I}_{ip}$ widens, the asymmetric information concerns increase and investors of debt securities issued against this payoff become more concerned about the adverse selection problem (DeMarzo and Duffie, 1999). In such pools, outside investors require the sponsor to hold higher level of equity tranche in equilibrium. Therefore, considering two pools with observationally similar loans (i.e., similar X_{ip}), the pool with wider support of $\tilde{I}_{ip}$ is likely to have a larger equity tranche. This argument forms the basis of our first test that more opaque pools (i.e., those with a higher level of no-documentation loans) should have larger equity tranche.

The optimal quantity of the security sold to outside investors depends on the sponsor's private information. Conditional on the degree of information asymmetry, sponsors sell a relatively smaller fraction of claims on the pool to outsiders if their private information is positive. Thus, an implication of the signaling models is that conditional on observable characteristics, pools that are backed by a higher level of equity tranche should perform better ex-post. This forms the basis of our second test that relates the level of the equity tranche to ex-post default performance of loans.

Finally, an important implication of these models is that the demand curve for security is downward sloping: as sponsors sell higher fraction of security to outsiders, outsiders rationally infer the sponsor's private information to be worse and demand a liquidity discount

(DeMarzo and Duffie, 1999). This forms the basis of our third test that, after controlling for leverage effects of the equity tranche size, the yield spread at issuance is lower for tranches from deals backed by higher equity tranche.

The securitization of a pool of assets adds additional complexity to this standard lemons-discount model. However these basic predictions apply equally well to the sale of securitized assets. DeMarzo and Duffie (1999) show that the quantity of assets retained by the seller serves as a costly signal of the asset's cash flows in a similar manner as in the case of single security sale. DeMarzo (2005) extends this model to address the sponsor's choice between selling assets individually versus selling them as a pool and then studies the optimal tranching decisions. In addition to these key hypotheses, his model also provides some novel predictions specific to the pooling and tranching of securities. We postpone the tests of these specific predictions for future work.

3.4 Sample and Descriptive Statistics

We construct a novel dataset of RMBS pools and tranches using hand-collected data from relevant SEC filings and matching them with loan-level data obtained from CoreLogic, a private data vendor. We hand-collect the security level data from the SEC filings to ensure that we do not miss any tranche in a specific deal. In addition, we hand-collect several important pieces of information such as the proportion of no-documentation loans in a pool and the identity of key players in the securitization chain from the SEC filings that are not easily available from other sources. Our loan-level data contain information on characteristics such as FICO scores and LTV ratios at the time of the deal as well as each loan's ex-post performance. In particular, we have information on whether the property entered into foreclosure any time from the deal date through December 31, 2011. Since we do not have data on the entire universe of RMBS deals during the pre-crisis period, we take special care in ensuring that our sample is representative. We use a stratified random sampling method to collect private-label RMBS deals covering a wide cross-section of banks and borrowers. We provide detailed description of sample selection criteria and data collection exercise in the Appendix C.1.

Figure 3.1 presents a schematic diagram of a representative deal and the relevant data sources. Our random sample begins with 196 securitization deals from 2001-02 and 2005 covering a wide range of sponsors, originators, and servicers. Our main empirical tests are based on a sample of 163 deals that have all the necessary information needed for the analysis. These deals have approximately 3000 tranches issued against cash flows from approximately 500,000 loans. The sample is approximately equally balanced between early and late periods (defined as 2001-02 and 2005, respectively). Our sample represents about 12% of the dollar volume of securities issued in the market during the sample period. Thus, we have a representative as well as an economically meaningful sample of deals from the pre-crisis period. It is worth emphasizing that we draw our sample randomly from the universe of all possible deals. This, in turn, provides confidence in the external validity of our results.

Table 3.1 presents summary statistics. We winsorize all variables at 1% from both tails to remove any outlier effects. Panel A of the table presents overall loan-, pool-, and tranche-level descriptive statistics. Based on 501,131 loans that enter our full sample, the average loan's FICO score is 656 with an LTV ratio of 77%. These numbers are broadly in line with Keys et al. (2012), who present detailed statistics on this market during 1998-2007. As expected, there is considerable cross-sectional heterogeneity in these two key measures of credit risk across loans. About 66% of the loans are classified as Adjustable-Rate Mortgages (ARM) and 89% of loans are owner occupied residences. Turning to pool-level statistics, the average pool has $776 million in principal amount and is backed by 3,150 loans.

We measure geographical diversification as the complement of one-state concentration of the loan. We first compute the percentage of loans in a pool that comes from each state and then identify the state with maximum share of loans in the pool. Our measure of geographical diversification (*GeoDiverse*) is simply one minus this share.[12] The average pool

[12]We perform several robustness tests using alternative measures of geographical diversification such as Herfindahl index across states and concentration in top-three states. Our key results remain similar.

in our sample has *GeoDiverse* score of 59, representing one-state concentration of 41%. Our sample contains a wide variety of institutional players covering commercial banks, investment banks, and mortgage companies. The full sample contains 22 unique sponsors and 32 unique top originators. We present the list of institutions that are most frequently involved in the deals in our sample in Table C.1 the Appendix.

The key measure of future performance of these loans is their foreclosure status. 16% of the loans in the sample enter foreclosure anytime from the deal origination until December 2011. The dollar-weighted pool-level foreclosure rate has a mean of 12% which varies from 3% for the 25th percentile pool to 18% for the 75th percentile.[13] Panel B in Table 3.1 provides some basic statistics relating borrower credit risk factors and eventual foreclosure. Consistent with intuition and past literature, we show that borrowers with higher FICO scores, lower LTV ratios, and fixed-rate mortgages default at lower rates. Also, loans from the earlier period are about half as likely to end up in foreclosure, showing strong vintage effect. We now describe the construction of our key variables that measure information asymmetry and the level of the equity tranche.

3.4.0.1 No-documentation loans

We obtain the percentage of no-documentation (*NoDoc*) loans in a pool directly from the deal prospectus. No-documentation loans are defined as loans that document neither the income nor the assets of the borrowers. Since different originators label these loans differently, we read through all the deal prospectuses to ensure consistency in our definition across deals. Originators classify these loans under various categories such as "stated documentation," "LITE," and "stated income, stated asset." The prospectus provides further details on the originator-specific underwriting criteria and terminologies, including the details on the various documentation classifications and verification undertaken by the originator. Based on this disclosure, we classify a loan under the no-documentation category if the originator

[13]The foreclosure information is available for a slightly lower number of deals because it is based on the sample formed by the intersection of our hand-collected data with CoreLogic foreclosure data.

has not verified both the borrower's income and assets. We provide an example of these differences in the classification of *NoDoc* loans in Appendix C.2. As shown in the Appendix, the ABFC Mortgage Loan series has three categories of loans in it: "full documentation loans," "stated income, stated asset loans," and "lite documentation loans." Under the full documentation loans, the lender obtains detailed documentations on information such as borrower's employment status, tax returns for the past two years, and pay-stubs. The originator also performs a telephonic verification of employment for salaried employees. No such attempt for income verification is made under the "stated income, stated asset" program, leaving a great deal of discretion with the originator.[14] We classify these loans under the *NoDoc* category. Finally, under the "LITE" category the originator reviews the deposit activity in the borrower's bank account for the past six to twenty-four consecutive months. We classify these loans as "limited documentation" category, and not "no documentation" in our study. We follow such classification strategy for all deals in our sample. Based on this classification scheme, *NoDoc* loans make up about 19% of all loans in the average pool. There is significant variation in this measure as it ranges from about 3% of the pool in the 25th percentile to 35% of the pool in the 75th percentile.

3.4.0.2 Equity Tranche

Our main variable of interest is the level of the equity tranche in a deal. We collect this information from the deal prospectuses that provide detailed security-level data on the notional amount of each tranche in the deal, their credit ratings, and the offered yield spread. We combine all tranches that are rated AAA by at least two rating agencies as the AAA-rated tranche. All tranches that are rated below AAA but above the equity tranche are clubbed together into the mezzanine tranche. Equity tranche is defined as the difference

[14]Specifically, the prospectus states, "The applicant's income as stated must be reasonable for the applicant's occupation as determined in the discretion of the loan underwriter; however, such income is not independently verified. Similarly the applicant's assets as stated must be reasonable for the applicant's occupation as determined in the discretion of the loan underwriter; however, such assets are not independently verified."

between the principal amount of loans in the pool and the sum of AAA and Mezzanine tranche sold to outside investors. In effect, we create a balance sheet of each deal in our sample and take the difference between the dollar value of assets and debt liabilities as the equity tranche. Thus, our definition of equity tranche represents the residual interest of the sponsors, which is precisely in line with the theoretical papers discussed earlier. In practice, sponsors use two different deal structures for tranching: (i) a six-pack structure, and (ii) an overcollateralization (OC) structure (see Gorton, 2010). In the six-pack structure, the junior most tranche is a well-specified unrated tranche that provides protection to all the senior tranches sold to the investors. In such deals the sum of sold tranches and the equity tranche equals the principal amount of loans in the pool. In the OC structure, the principal amount of loans in the pool exceeds the sum of securities on the liability side. The excess amount – the overcollateralization – provides an additional level of residual interest to the sponsor. Economically, the OC amount is the equity interest of the sponsor.[15] Our construction ensures that we capture the true economic interest of the sponsor, regardless of whether it comes in the form of a well-specified security or by having additional residual interest in the pool. It is worth emphasizing that investors are able to observe this measure of equity tranche at the time of deal issuance since this information is readily available in the deal-prospectus. We provide an example from each of these structures and the computation of the equity tranche in each case in Table C.2 in the Appendix.

Panel C of Table 3.1 provides descriptive statistics on the tranche structure. Overall, 90.40% of the average deal is tranched into AAA-rated security, while only 1.20% of the average deal is in the equity tranche. Panel C also illustrates the evolution of the average deal structure over our sample period. The size of the average AAA-rated tranche drops from 92.56% in 2001-02 to 88.32% in 2005. The level of equity tranche more than doubled from 0.72% to 1.63% over the same time period. To give these numbers some perspective, Benmelech and Dlugosz (2009) find that about 71% of CLO pools are rated AAA and 11%

[15]As noted by Gorton (2010): "The overcollateralization reverts to an equity claim if it remains at the end of the transaction".

are unrated while Stanton and Wallace (2011) find about 84-87% of CMBS pools are rated AAA and 3-4% are unrated equity tranche. Not surprisingly, RMBS tranching structure is closer to the numbers reported by Stanton and Wallace (2011) as compared to the summary statistics of Benmelech and Dlugosz (2009), who include several other types of assets in the pool.

We use the level of equity tranche at the time of security sale as the measure of the sponsor's retained interest in the pool. Some observers have argued that if sponsors offload a bulk of this risky tranche in the secondary market, then it has no value as a signal of private information. Ideally, we want the amount of securities retained by the sponsors for a long time after the initial deal creation as the measure of retained interest. Unfortunately, this information is not available due to limited disclosure requirements. In the absence of this proxy, the unsold equity tranche at the time of security sale provides the most natural alternative measure. There are several economic reasons to support the use of equity tranche for our empirical exercise. First, anecdotal evidence suggests that banks often retained part of this exposure on their balance sheet. For example, the Financial Crisis Inquiry Commission's Report presents a case study of an MBS deal issued by Citi Bank in 2006 called CMLTI 2006-NC2. They provide details on the identity of the holders of different tranches of this deal (see page 116 of the report). The AAA-tranches were bought by foreign banks and funds in China, Italy, France, and Germany, the Federal Home Loan Bank of Chicago, the Kentucky Retirement Systems and a few other parties. The mezzanine tranches were mostly bought by the sponsors of CDOs. More relevant to our work, Citi Bank did retain a part of the equity tranche in the deal sharing the rest with Capmark Financial Group, a real-estate investment firm. Similarly, Demiroglu and James (2012) provide an example from a deal sponsored by Bear Stearns that shows the sponsor's commitment to initially hold the residual interest: *"The initial owner of the Residual Certificates is expected to be Bear Stearns Securities Corp."*

Second, as suggested by the Citi Bank sponsored deal above, the buyers of equity tranches are on average more informed than the buyers of safer tranches. The asymmetric information problem between the buyers and sellers in this market is likely to be relatively lower than the corresponding problem at the time of initial sale. Thus the sponsors' incentive to keep higher proportion of deals with favorable private information remains preserved.

Third, even though the sponsors can subsequently offload this risk in the secondary market in the medium to long run, in the immediate aftermath of the deal the risk remains with the sponsor. Indeed there have been numerous commentaries on the role of warehousing risk in this market during the sub-prime mortgage crisis. Thus the extent of equity tranche at the time of security sale provides a clean proxy for risk exposure during the initial period. Fourth, as shown by Acharya et al. (2013), there are several instances of securitization motivated by regulatory capital arbitrage. In such deals the residual credit risk stayed with the sponsors.

Finally, we check the annual reports of major sponsors in our sample and find significant equity tranche retention on their balance sheets. For example, Lehman Brothers had approximately $2 billion of non-investment grade retained interests in residential mortgaged-backed securitization as of November 30, 2006. We obtain similar evidence from the annual reports of Goldman Sachs and Merrill Lynch during this period (see footnote 5). While this method does not allow us to get pool level retention amount, it does show that in aggregate the sponsors were holding significant amount of unrated tranches on their balance sheets. Overall, these arguments suggest that equity tranche created at the time of RMBS issuance imposes significant cost on the sponsor consistent with the underlying theoretical assumption of the signaling models.

Ultimately, the relationship between the level of the equity tranche and loan quality remains an empirical question. If the deal sponsors did not care about the risk of equity tranche because of the possibility of future sale, then we should find no correlation between the level of the equity tranche and future default performance. In contrast, if they did care about this risk, then we expect to observe better performance for deals with high equity tranche. Our empirical analysis allows us to test these competing hypotheses in the paper.

3.5 Empirical Results

In this section, we present the results of our empirical tests for the key predictions outlined in Section 3.3. Our main interest lies in estimating the effect of equity tranche on future default performance of the underlying pool. However, for expositional simplicity, we begin our analysis by relating the level of equity tranche to asymmetric information concerns of RMBS buyers. Next, we relate the level of the equity tranche to ex-post foreclosure performance of the entire pool. Our final set of tests examine the ex-ante pricing effect of equity tranche.

3.5.1 Cross Sectional Determinants of Tranche Structure

One of the key predictions of information-based models is that the level of the equity tranche should increase with the asymmetric information concerns about the underlying pool. In such deals, debt security buyers are more likely to demand a higher level of equity tranche to mitigate their concerns about adverse selection. We estimate the following pool-level regression model to examine this:

$$EquityTranche_p = \alpha + \beta(InfoAsym_p) + \theta(Late_p) + \gamma(Credit_p) + \delta(GeoDiverse_p) + \epsilon_p \quad (3.2)$$

As discussed earlier, we use the percentage of *NoDoc* loans in the pool as the proxy for the extent of asymmetric information ($InfoAsym_p$), or opacity of the underlying pool, faced by the investors.

We separate out the effect of observable risk factors in this regression model by including several pool-specific measures of credit risk, $Credit_p$, as explanatory variables. These variables include the weighted average FICO score, the weighted average LTV ratio, and the fraction of adjustable rate mortgages (ARM) in the pool. The first two variables directly measure the credit risk and leverage of the deal, and hence are predictors of future default by the borrower. We include percentage of ARM in the pool as an additional control variable for both credit and interest rate risks of the pool. We control for the time effect by including an indicator variable *Late* that equals one for deals from 2005, and zero for the earlier period.[16] Inclusion of this variable in the regression model allows us to separate the effect of aggregate macroeconomic shocks such as the level of interest rate and the demand of such securities from the outside investors. We include a measure of geographical diversification ($GeoDiverse_p$) of the pool as an additional variable to capture the effect of correlations of loans within the pool.

Columns (1) and (2) of Table 3.2 present the results. In column (1), which only includes *Late* as a control variable, we find a positive and significant (at 1%) coefficient on the $\%NoDoc$ variable. In economic terms, one standard deviation increase in no-documentation loans (17.8 percentage points) is associated with an increase of about 0.45 percentage points, or a 60% increase in the equity tranche level for the median deal. The coefficient estimate on *Late* shows that the extent of equity tranche increased in later periods. In column (2), we include all the control variables and find that the estimate on $\%NoDoc$ remains virtually unaffected. In Column (3) we include sponsor fixed effects in the model. This specification ensures that our results are not driven by sponsor's unobserved characteristics such as its reputation in the market. Our results remain robust to this specification. Overall, these estimates show that the opacity of the loan pool is a key driver of the size of the equity tranche. Observable credit risk characteristics of the pool such as FICO score and LTV ratio do not explain significant variation in equity tranche across deals. These results are consistent with our first prediction that the level of equity tranche increases with the size of the wedge between sponsors' and buyers' information sets.

We next turn to the division of sold tranches (i.e., the complement of the equity tranche) into AAA and Mezzanine categories. The dependent variable in these specifications measures

[16] In unreported regressions we control for even finer time-periods such as the month or quarter of the deal. Our results do not change.

the ratio of Mezzanine tranche to the sum of AAA-rated and mezzanine tranche in the deal. The *Mezzanine-to-Sold* ratio is 8.57% for the average deal in our sample with significant cross-sectional variation. Using the same modeling approach as above, we regress explanatory variables capturing credit risk and information concerns on this dependent variable. Columns (4), (5) and (6) in Table 3.2 present the results.

While $\%NoDoc$ has no effect on the division of sold tranches across Mezzanine and AAA category after controlling for observable measures of credit risk in the full specification in column (5), this division is explained well by observable credit risk factors such as FICO score and LTV ratio. As expected, pools with lower FICO score and higher LTV ratio have relatively higher proportion of Mezzanine (lower AAA) tranche within the sold portion of the deal. Loan pools with more geographical diversity have relatively higher proportion of AAA-rated tranche. These results show that pools with lower observable credit risk and higher risk diversification have relatively higher AAA-rated tranche.

Taken together with the earlier results, we find that concerns about private information drive the cross-sectional dispersion in the level of the equity tranche, whereas hard pieces of information such as FICO score, LTV ratio, and geographical diversification drive the division of the sold tranche into AAA and mezzanine categories. In addition to the slope coefficients, the R^2 of the models provides an interesting insight as well. For the equity tranche regression, inclusion of observable credit risk variables improves the model's R^2 from 26.8% to a marginally higher 31.8% (columns 1 and 2), whereas the corresponding R^2 improves from 33.4% to 85.7% for the *Mezzanine-to-Sold* regression (columns 4 and 5). Hard pieces of information are easier to price and therefore can be incorporated in the security pricing relatively easily. In contrast concerns about information asymmetry are harder to price and the level of the equity tranche emerges as an additional contracting tool in such settings. Our results provide evidence in support of these arguments.

A potential concern with our analysis is the omission of some observable credit risk factors that correlate both with $\%NoDoc$ and the extent of equity tranche. Note that after controlling for FICO score, LTV ratio, $\%ARM$, geographical diversity, time effects, and sponsor fixed effects, $\%NoDoc$ does not have any explanatory power in explaining the division of sold tranches between Mezzanine and AAA categories. If we miss a correlated omitted variable from the model that is observed to the investors, then it is likely to influence both the level of equity tranche and the division of sold tranches across Mezzanine and AAA category. In light of our results on Mezzanine-to-Sold tranches, it is unlikely that our results suffer from any serious omitted variable bias. As an additional test (unreported), we include the weighted average interest rate on mortgages in the pool as an explanatory variable in the regression. Interest rates are likely to capture a bulk of the publicly available information about the credit risk of the borrowers. Thus the inclusion of interest rate in the model provides a reasonable control for the measures of credit risk that may be known to the investors, but not to us as econometricians. The estimate shows that the coefficient on %*NoDoc* remains unaffected. We repeat the same exercise for the division between AAA and mezzanine tranche in column (6) and show that our results remain unchanged for that model as well.

As an alternative estimation technique, we also estimate a seemingly unrelated regression model for the proportion of AAA, mezzanine, and equity tranche in a deal, which we do not tabulate for brevity. Our key results are stronger for this specification. We also perform our tests with standard errors clustered at the sponsor level and find that our inferences are unaffected. However, we need a sufficiently large number of clusters to obtain consistent standard errors using this method. Since we only have 22 clusters, we present our results without clustering.

3.5.2 Ex-Post Performance of Pools

We have shown that more opaque pools have a relatively larger equity tranche. While consistent with the broad idea behind adverse selection models, this test is not conclusive in terms of evaluating the role of the equity tranche as a signal of the underlying pool quality. Does the creation of a larger equity tranche indicate deal sponsors' favorable private information about the underlying loans in the pool? Are these effects mainly concentrated in pools with higher concerns about asymmetric information? We exploit the cross-sectional variation in equity tranche along with data on ex-post performance of mortgages to answer these questions. If sponsors with favorable private information about the underlying pool create a larger equity tranche, then we expect to observe relatively better ex-post default performance by such pools after conditioning on observable pool characteristics. In other words, we expect *abnormal* default performance of high equity tranche pools to be better, where *abnormal* default performance measures the actual default rate of the pool against a benchmark default rate based on ex-ante observable information. We use a standard default model and then a matched pool exercise to create two benchmarks of expected default rates to test these predictions. We first describe the empirical design and then discuss the construction of abnormal default performance measures in greater detail.

We want to estimate the relationship between the equity tranche and abnormal default rate, conditional on the degree of asymmetric information concerns. We do so by estimating the following empirical model:

$$AbDefault_p = \beta_0 + \beta_1(Opaque_p) + \beta_2(HighEq_p) + \beta_3(Opaque_p \times HighEq_p) + \sum \gamma X_p + \epsilon_p \quad (3.3)$$

$AbDefault_p$ is the abnormal default rate of pool p. *Opaque* equals one for pools that have an above-median percentage of no-documentation loans, and zero otherwise. *HighEq* equals one for pools that have an above-median level of equity tranche, and zero otherwise. X_p measures some pool level control variables such as the pool's weighted average FICO scores and the LTV ratio and the year of the deal. We include them in the regression model to capture any remaining pool specific variation that does not get captured by the loan-level default model described later. Our regression model uses a difference-in-differences design to estimate the effect of the equity tranche across opaque and transparent pools. Consistent with theoretical models, this empirical approach estimates the relationship between the level of the equity tranche and abnormal default, conditional on the degree of the buyer's asymmetric information concerns. That is, the model allows us to separately examine the relationship of interest for both high-information-asymmetry pools (*Opaque*) and transparent pools. For easier economic interpretation, we use indicator variables for opaque and transparent pools as well as high and low equity tranche pools in the regression. The regression coefficients in this model estimate the abnormal default rate across different pools as shown below:

	Transparent Pool	Opaque Pool
Low Equity Tranche	β_0	$\beta_0 + \beta_1$
High Equity Tranche	$\beta_0 + \beta_2$	$\beta_0 + \beta_1 + \beta_2 + \beta_3$
Difference	β_2	$\beta_2 + \beta_3$

Our interest lies in both $\hat{\beta}_3$, the difference-in-differences estimator, and the sum of coefficients $\hat{\beta}_2 + \hat{\beta}_3$. The sum of these coefficients provides an estimate of the difference in abnormal default rates across high and low equity tranche deals for opaque pools. If sponsors used equity tranche as a tool to signal their favorable private information, then we expect the sum of these coefficients to be negative. $\hat{\beta}_3$ provides an estimate of the differential effect of equity tranche on default rate for opaque pools as compared to the corresponding difference for the transparent pools. In other words, $\hat{\beta}_3$ differences out the effect of equity tranche on abnormal default rate that we observe within the relatively transparent pools. We expect this difference-in-difference estimate to be negative as well. If the equity tranche correlates with abnormal default rate of pools for reasons unrelated to information asymmetry at the time of issuance, then this estimator provides a useful way to separate out those effects. For example, factors such as economy wide abundance of capital or investment opportunity set of the sponsors can potentially affect both the level of equity tranche and the ex-post default rates for all the pools. Our estimator is able to remove all such effects as long as they affect opaque and transparent pools in similar manner.

3.5.2.1 Standard Default Model

Our goal is to parse out the effect of observable loan and property characteristics from the default performance (i.e., foreclosure rate) of the loans, which we do as follows. We create a benchmark model of loan level foreclosure probability based on publicly available information at the time of issuance. Next, we aggregate this at the pool level to compute the expected default rate of the pool. We then take the ratio of actual foreclosure rate we observe ex post to the expected foreclosure rate as the measure of abnormal default.

We estimate a benchmark model of foreclosure probability for every loan based on the following logistic regression model:

$$Pr(foreclosure_i = 1) = \frac{1}{1 + e^{-\beta X_i}} \tag{3.4}$$

$foreclosure_i$ equals one for loans that enter foreclosure any time up to December 31, 2011. X_i is a set of observable loan and property characteristics that are likely to predict the loan's default rate. We choose these variables based on economic intuition and previous research in the area (see e.g., Demyanyk and Van Hemert, 2011). They include the borrower's FICO score, LTV ratio, state of the property's location, the purpose of the loan, the year of loan origination, and the type of the loan product. FICO score and LTV ratio are the main drivers of the loan's default risk. The property location and the year of origination control for geographical and temporal variations in the house price appreciation and credit risk of the borrowers. In addition, we include a rich set of control variables based on the purpose of the loan and the type of mortgage product. CoreLogic classifies loans into six buckets based on their purpose. The six categories are: (i) purchase, (ii) refinancing without cash out, (iii) refinancing with cash out, (iv) the remaining refinancing loans (i.e., loans with no information on cash out), (v) second mortgage, and (v) others. Finally, we include the nature of interest rate offered on the loan and its reset terms as additional explanatory variables. Based on CoreLogic data, loans are divided into categories based on the following broad criteria: (i) the nature of interest rate (Fixed Rate, ARM, or Balloon), (ii) the number of years for the reset of loan terms (such as 5 year ARM or 7 year Balloon), and (iii) the special structure of the interest or principal payments (such as interest only loans for the first n number of years). Different combinations of these attributes result in 46 distinct categories of product type, and we include them as fixed effects in the model. In addition to default risk, these variables also control for the cross-sectional differences in the loan's prepayment risk in our sample.

The estimated default model uses roughly 500,000 loans originated primarily during the years of 2001-2005.[17] Of these loans, about 16% enter foreclosure during our sample period. As noted earlier, the foreclosure rates of loans based on observable metrics such as FICO score, LTV ratio, loan interest rate type and time period correlate in the expected directions as shown in Panel B of Table 3.1.

The estimates of the logistic regression are consistent with previous findings in the literature (see Appendix 3 for the estimates). Borrowers with higher FICO scores and lower LTV ratios are significantly less likely to get into foreclosure. Loans with features such as ARM and Balloon payments are significantly more likely to default. All other categorical variables (state, year of origination, and the product type) have significant predictive power in explaining the foreclosure rate as well.[18]

After estimating the model, we use the fitted values from the model to obtain the predicted default likelihood $\widehat{foreclosure}_{ip}$ of each loan i in pool p. The predicted foreclosure rate provides us with an in-sample benchmark for the expected default rate of the loan con-

[17]The results we present are based on a pooled model using all available data. Our results are unchanged if we compute our benchmark default model separately for the early and late time periods as well as using different performance horizons (e.g., two years, five years, etc.).

[18]We consider several alternative models of default prediction in unreported robustness tests. In particular, we include additional variables such as: (a) the second LTV ratio of the loan (if any), (b) whether the loan is for single-family housing or other assets such as condominiums or manufactured housing, (c) whether the loan has a negative amortization feature, (d) the debt-to-income ratio of the borrower, and (e) the margin on the interest rate. Our results remain robust to these alternative specifications. Some of the additional variables are not available for all loans in the sample. Thus, when we include these covariates in the model, we lose some observations. More important, addition of these variables do not considerably improve the fit of our model as compared to the base case analysis that captures all the key drivers of default. Therefore, we only report results based on the base case model to save space.

ditional on key observable characteristics. We aggregate this measure at the pool level to compute a predicted foreclosure rate of the pool. The abnormal default rate for the pool ($AbDefault_p$) with N_p loans in it is then calculated as follows:

$$AbDefault_p = \frac{\sum_{i=1}^{N_p} w_i(foreclosure_{ip})}{\sum_{i=1}^{N_p} w_i(\widehat{foreclosure_{ip}})} \tag{3.5}$$

Our measure computes the dollar-weighted ratio (weights w_i with $\sum_{i=1}^{N_p} w_i = 1$) of number of loans in a pool that actually defaulted to the number of loans that were expected to default based on observable characteristics at the time of issuance.[19] We plot the kernel density of *AbDefault* measure in Figure 3.2a. As expected, the average number is centered around one with significant cross-sectional variation. The $75th$ percentile pool has abnormal default ratio of 1.18, indicating that the pool's actual default rate is 18% higher than the expected default rate based on observable characteristics. In contrast, the $25th$ percentile pool has a ratio of 0.47 indicating 53% lower default rate than its benchmark.

We estimate regression equation (3.3) based on this measure of abnormal default and report the results in column (1) of Table 3.3. Since the signaling value of the equity tranche should be most useful when there is more opacity about the underlying asset, the key variable of interest is the interaction of the strength of the signal, *HighEq*, and pool opacity, *Opaque* ($\hat{\beta}_3$ from the earlier discussion). Column (1) reports an estimate of $\hat{\beta}_3 = -0.244$, which is significant at 1% level. Opaque pools with a higher level of equity tranche have significantly lower abnormal default rates than those with lower equity tranche, as compared to the corresponding difference for the transparent pools. The difference-in-differences coefficient translates into a lower default rate of 24.4% for higher equity tranche pool. The second estimate of interest is the difference in abnormal default between deals with higher and lower levels of equity tranche within opaque pools, which is $\hat{\beta}_3 + \hat{\beta}_2 = -0.134$ and significant at the 6% level (not tabulated). Thus, pools with higher level of equity tranche have 13%

[19]Alternatively, we compute our default benchmark measures of abnormal default based on the *number* of loans that enter foreclosure (i.e., equal weighting) and find similar results for our tests.

lower abnormal default rate within opaque pools. These results show that equity tranche predicts better future performance conditional of ex-ante loan characteristics.

In Column (2) of the Table we show that these results are driven by the level of equity tranche, and not by the level of Mezzanine tranche in the same deal. We do so by including an indicator variable *HighMezz* that equals one for deals with higher than median level of Mezzanine tranche and its interaction with *Opaque* as additional regressors in the model. The coefficients on these additional variables are not significant, while the estimates on $HighEq * Opaque$ are strengthened both economically and statistically. This result contrasts the difference between equity and mezzanine tranches and emphasizes the importance of equity tranche, not just the level of AAA subordination, in predicting the future performance of the entire pool. Finally, in Column (3) we include the sponsor fixed effects in the model, and show that our results remain practically unchanged, both in statistical and economic terms.

3.5.2.2 Matched Sample Benchmark Model of Default

One of the basic rationales behind the creation of mortgage-backed securities is the benefit of diversification that can be achieved by pooling several loans together. Indeed, a key input to the RMBS pricing models is the underlying correlation matrix of the loans in a pool. Our default risk model in the previous section ignores the within pool correlation of default risk of loans. We now account for this effect as well the effects of macroeconomic shocks through a matching exercise which we describe below.[20]

For every loan in a given pool, we find a matching loan with similar observable characteristics from the universe of all loans in our sample *excluding* the loans in the loan's own pool. The matched loan is similar on key dimensions of default and interest rate risk such as FICO

[20] An alternative approach to parse out the effect of latent macroeconomic shocks is to use a frailty correlated default model. Duffie et al. (2009) propose such a model and estimate it for a sample of U.S. nonfinancial firms. They find strong evidence for the presence of common latent factor even after controlling for commonly used firm specific default predictors. In unreported robustness test, we estimate a maximum likelihood based frailty model and obtain similar results. We prefer the matching based approach for our exercise as it allows us to account for correlation structure of the loans in a relatively straightforward manner.

score, LTV ratio, loan amount, year of origination, type of interest rate on the loan (e.g., ARM, balloon or fixed rate) and geographical location. We outline the precise matching algorithm in Appendix C.3. The key idea is to match the actual pool created by the informed sponsor to a hypothetical pool that is, by construction, from an uninformed sponsor. Our matched pool lacks the sponsor's pool-specific private information component, while retaining the similarity along observable dimensions. Loans in the hypothetical pool are likely to have similar correlation structure as the actual pool, especially since we match these loans based on the geographical location as well. Since the hypothetical pool is observationally similar and the loans in the pool are subjected to similar macroeconomic shocks as the actual pool, the foreclosure rate on hypothetical pool provides us with a benchmark that accounts for ex-ante loan characteristics, macroeconomic shocks and the correlation structure of the loans in a non-parametric way. In particular, the matched sample approach allows us to difference out the effect of house price appreciation and the prepayment risk of the loans on default rates, since these factors are likely to be driven by geographical location of the property, the timing of the loan origination, and the nature of interest rate on the loan.[21] As before, we take the ratio of the actual pool's default rate to its matched hypothetical pool's default rate as the measure of abnormal default.

A kernel density of the abnormal default rate based on this measure is provided in Figure 3.2b. Like our first measure, the average performance is centered around 1 with a large cross-sectional variation. The ratio ranges from 0.67 to 1.21 as we move from the 25th to the 75th percentile of the distribution.

We estimate regression equation (3.3) based on this measure of abnormal default and report the results in Column (4) of Table 3.3. The estimates on our variables of interest largely mirror our findings from our first measure of abnormal default. Column (3) reports an estimate of $\hat{\beta}_3 = -0.221$, which indicates that opaque pools with a higher equity tranche

[21]In unreported tests, we also consider matching the property location at the zip-code level. Our results remain similar. However, we are unable to find a match in the same zip-code for several loans in our sample. Hence we prefer the state-level matching for our main analysis.

have a 22.1% lower default rate as compared to the corresponding difference for relatively transparent pools. The difference in abnormal default rate between deals with higher and lower levels of equity tranche within opaque pools is (i.e., $\hat{\beta}_3 + \hat{\beta}_2$) -0.188 which is significant at the 2% level (not tabulated). Thus, pools with higher level of equity tranche have an 18.8% lower abnormal default rate within opaque pools. We re-estimate these models with *HighMezz* and its interaction with *Opaque* as additional regressors and present the results in column (5). We find that the difference-in-differences estimator is strengthened with $\hat{\beta}_3 = -0.270$. Finally, in Column (6) we include the sponsor fixed effects in the model, and show that our results remain practically unchanged, both in statistical and economic terms. These findings indicate that equity tranche created at the time of security sale forecasts better than expected foreclosure outcomes for loans in the underlying pool.

Our results also highlight an important distinction across pools with varying degree of no-documentation loans. The effect of equity tranche on future loan performance is concentrated within the opaque pools, i.e., pools with higher proportion of no documentation loans. Prior literature has argued that soft information is more important for loans with poor documentation (e.g., see Keys et al. (2010) and Demiroglu and James (2012)). Our results extend the literature by showing that in such deals, the equity tranche conveys important information about the underlying loan quality.

3.5.3 The Channel of Private Information

Where do sponsors get information about the underlying loan quality? As a part of the securitization chain, sponsors are likely to have access to much more detailed documents from the originators as compared to the buyers in addition to other informal channels of information exchange. If the loans are originated by the sponsors themselves, the information advantage over potential RMBS investors increases even more (e.g., see Keys et al. (2010)). We collect the identity of top originators for each pool in our sample from the deal prospectus. In almost half the cases, sponsors are also the top originators of the loan pool. In such cases, sponsor's information advantage over the buyers is likely to be higher, and we expect equity tranche to play an even more meaningful role here. This sub-sample test allows us to establish that our results are driven by asymmetric information concerns, and not by a pure absence of information with the sponsors. Thus, it establishes an important economic channel of private information.

In Table 3.4, we re-estimate our main regression specification (3.3) across these two groups. For expositional clarity, we reproduce the full sample results in Column (1), and the sub-sample results in Columns (2) and (3). While the estimated coefficient on ($Opaque_p \times HighEq_p$) remains negative for both subgroups, it is statistically and economically significant only in the sub-group where sponsors are also the top originators. In fact the economic magnitude of the coefficient increases by almost 70% for this sub-group. When the information advantage of sponsors is likely to be higher, equity tranche forecasts the default rate better. This is consistent with our main assertion that the equity tranche captures the sponsor's private information in RMBS deals.[22]

3.5.4 Identification Using Anti-Predatory Lending Laws

A potential concern with our analysis may be that opaque pools with a higher level of equity tranche are systematically *better* on observable dimensions that we, as econometricians, are unable to control for. If that be the case, we would find lower ex-post default for such pools even without any private information component of the equity tranche. Note that we have already controlled for some of the most important observable loan characteristics such as FICO score, LTV ratio, the purpose of the loans, the nature of interest rate, year of origination, and geographical location of the property in our default model. Earlier research has shown that these variables explain most of the variation in ex-post default of mortgages. Second, our matched sample exercise eliminates the effect of any observable characteristics that remains similar across the actual and matched pool. Third, we focus on

[22]In unreported tests, we estimate this model in a triple-interaction framework as well and obtain similar results. We do not report them for brevity.

the difference-in-differences coefficient in our empirical tests. This design ensures that we eliminate the effect of any differences in missing observables that correlate with the levels of loan opacity and equity tranche on an unconditional basis. Therefore, it is unlikely that our results are simply an artifact of missing observable characteristics. To further support this claim, we exploit the passage of state-level Anti-Predatory Lending Laws (APLs) as a source of exogenous variation in concerns about lenders' private information.

Several states passed these laws during our sample period to protect homeowners from predatory lending practices. These laws are structured along the lines of Federal Home Ownership and Equity Protection Act (HOEPA), and they typically impose more stringent restrictions on lending practices at the state level as compared to the Federal Act. APLs vary across states in terms of the type of loans they cover and the restrictions they impose on the lenders in terms of required lending practices and information disclosure rules. For example, some of these restrictions include limits on allowable prepayment penalties and balloon payments, borrower counseling requirements, and restriction on mandatory arbitration. Ho and Pennington-Cross (2005, 2006) provide detailed explanations of these laws and the timing of their passage by different states.

The passage of the law is likely to decrease the lenders' ability to originate and package predatory or abusive loans at the margin (see Agarwal et al., 2012). Such a government regulation should make the use of private contracting mechanisms less important. Therefore, prior to the passage of this law, the equity tranche is likely to serve as a more important signal of private information. Said differently, if the equity tranche indeed conveys private information, then it should have a higher impact in the pre-APL period, i.e., during the period with relatively less government regulation on information disclosure rules.

Ho and Pennington-Cross (2005, 2006) provide an index of the strength of APLs across states as well as the date of the law passage. Their index varies from 4 to 17 with a median score of 10, where a higher index level indicates stronger laws in the state. Based on this measure, we classify all states with index value of 10 or above as the states with strong APL. These states are California, Colorado, Connecticut, Georgia, Illinois, Indiana, Massachusetts, New Jersey, New Mexico, North Carolina, and Washington DC. Of these states, all but Massachusetts and Connecticut, passed their law during our sample period (i.e., between 2002 and 2004), providing us with data on both before and after the law passage. For our test, we create an indicator variable *APL* that takes a value of one for states with strong APL, and zero otherwise. We create an indicator variable *Before* that equals one for loans that belong to states before the passage of law, and zero after that. As in our earlier tests, we create an indicator variable for high equity tranche (*HighEq*) based on the median level of this variable in our sample. With these three variables, namely *APL*, *HighEq*, and *Before*, we estimate a triple-difference model to estimate the difference in the effect of high equity tranche on future loan foreclosure rate for the APL states before and after the passage of the law as compared to the corresponding difference for states without the law. In this estimation strategy, we separate out the unconditional level effects of each one of these variables on the foreclosure rate as well as all the double-interaction effects. The coefficient on the triple-interaction term presents us with the estimate of interest.

We first estimate the model at loan-level. In this specification, we fit a loan level logistic regression model with foreclosure status as the dependent variable. Column (1) of Table 3.5 presents results estimated with the entire sample. Since *HighEq* is a pool-level variable, we cluster all standard errors at the pool level.[23] We find a negative and significant coefficient on *APL * HighEq * Before* indicating that equity tranche conveys stronger information about the future loan performance for APL states before the passage of the law. This is consistent with our prediction outlined above. In column (2), we restrict our sample to only

[23]The use of clustered standard errors for logistics regressions requires a caveat. The estimates produced by standard maximum likelihood estimates (i.e., the ones produced by statistical packages such as STATA) may not be the true estimates when we have clustered observations. This happens because the observations are no longer independent within clusters. Hence the joint distribution function for the sample may no longer be the product of the distribution functions for each observation. Without a precise knowledge of the correlation structure within clusters, one cannot write down the true likelihood of the sample. Thus the estimates are consistent only under special cases. Considering these limitations, we also estimate the model by collapsing loan-level observations to pool level. While we lose the loan-level granularity in this approach, we are able to avoid the econometric concerns with clustered logistics regression model. Our main results remain similar across these modeling approaches.

Opaque pools and find the point estimate on the triple interaction term to be over twice as large, which indicates that the effect is especially strong for pools with higher concern about information asymmetry.

In columns (3) and (4), we collapse the data at the pool level. In the process we lose the loan-level variation since loans in a pool often come from different states. We compute the fraction of loans that comes from APL states and classify pools as *HighAPL* if the fraction of loans from APL states in the pool exceeds the sample median (59%). With this definition of pool-level APL, we estimate the triple-interaction model using an OLS approach. Despite the loss in variation arising out of pool-level aggregation, we find a negative and significant coefficient on the triple-interaction term.

Overall, these findings are consistent with equity tranche being an indicator of sponsors' favorable private information. Did investors recognize and respond to this indicator? To answer this question, we look at the pricing effect of the level of equity tranche in the following section.

3.5.5 Pricing Effect of Equity Tranche

Did investors pay higher prices for securities backed up by higher equity tranche? An important prediction of signaling models is the presence of a downward sloping demand curve: as sponsors sell more of their assets, investors demand lower prices (e.g., see DeMarzo and Duffie, 1999). Sponsors trade off the resulting liquidity discount from selling more of their assets with the cost of retaining higher equity tranche. Since pricing data for sold tranches is unavailable, following the prior literature we use yield spread on these securities to test this prediction (Je et al., 2012). It is relatively straightforward to compute yield spread for floating rate coupons. It is estimated as the spread over LIBOR benchmark reported in the deal prospectus. For the fixed rate tranches, we need to know the duration of these securities to be able to compute the benchmark rate more precisely. Absent this information, we only focus on floating rate tranches for this part of the analysis. Despite this limitation, we are able to cover about 70% of tranches in our sample.[24]

We want to estimate the effect of equity tranche on the pricing of sold tranches in the same deal. An immediate implication of higher equity tranche is that there is less leverage in the deal. In such deals, superior tranches that are sold to the investors are safer and therefore they should command attractive prices. This effect is independent of any information revelation via the equity tranche that we are interested in. To separate out the leverage effect, we condition our analysis on the credit rating of sold tranches. We compare the pricing of two similarly rated tranches coming from deals with different levels of equity tranche. We maintain our basic empirical design that estimates the effect of equity tranche separately across opaque and transparent pools. If the effect of equity tranche on prices come entirely due to the leverage effect, then we should find no difference across opaque and transparent pools. On the other hand, if the effect comes via the revelation of private information, then we expect to see higher prices for tranches backed by higher equity tranche only in the opaque pools.

We divide all tranches into broad credit rating classes: AAA, AA, A, and BBB.[25] For deals with multiple tranches within one rating class, we compute a dollar-weighted average yield spread and consolidate them into one observation. This aggregation leads to 549 sold tranches in our sample, out of which 379 are floating rate. We break all pools into two categories based on whether they have above or below the median level of equity tranche. Table 3.6 presents the cross-tabulation of the average yield spread of sold tranches across high and low equity tranche groups for every credit rating category. There is a clear pattern in the data: within each credit rating class, the yield spread is lower for pools with higher equity tranche. As sponsors sell more of their pool's cash flows to outside investors, the price decreases (yield spread increases).

We estimate a regression model relating yield spread to level of the equity tranche in

[24]In a robustness exercise, we include fixed rate tranches as well, and obtain similar results. For this analysis, we subtract the 5-year risk-free treasury rate from the fixed coupon rate of the tranche.

[25]There are a very small number of sold tranches below the BBB rating. We include them in the BBB category.

the deal after controlling for the credit rating fixed effects. Columns (1)-(2) of Table 3.7 present our base results. The significant negative coefficient on *HighEq* indicates that after controlling for the credit rating class, high-equity-tranche deals have 27 basis points lower yield spread. More important, the effect comes entirely from the *Opaque* deals. This is precisely the group where we find a considerably lower foreclosure rate in our earlier tests. We further break our analysis down to AAA-rated and non-AAA rated securities and report the results in columns (5)-(6). The effect is concentrated among the non-AAA rated tranche backed by opaque deals. With their higher informational sensitivity, we expect the pricing effect to be higher for these pools and the empirical results confirm this intuition. Taken together with the abnormal default rate results, our results show that equity tranche did contain the sponsor's private information, and market prices reflect this ex ante in a cross-sectional sense.

3.5.6 Alternative Channels

It has been recognized in the literature that in addition to tranche structure, concerns such as sponsor's reputation, servicing rights, and influence over credit rating agencies can play important roles in the way participants contract in this market. These considerations could potentially interact with the retention of equity tranche, creation of AAA-tranche and other related features of the RMBS design. While we do not explore these interactions in detail, this section presents several tests to establish the robustness of our analysis even in the presence of these competing influences. We first consider the possibility that our results are driven by deals where sponsors and originators have more "skin in the game" by holding servicing contracts (e.g., Piskorski et al., 2010; Demiroglu and James, 2012). In addition to earning fees from the origination of loans, lenders sometimes retain servicing rights on loans that provide them with an additional stream of income for the life of the loan. This income averages about 37 basis points per year for the deals in our sample. If the sponsors hold servicing rights on the loans, this implicit equity stake may provide stronger incentives for them to ensure that the pool is populated with higher quality loans. If deals with higher servicing "skin in the game" coincide with those with higher equity tranche, then our inferences maybe contaminated. To empirically separate out this alternative channel, we collect data on the identity of primary servicer for the loans in the pool. We create a dummy variable that indicates if the sponsor is also the servicer (*SellAndService*) and a dummy variable that indicates if the top originator for the pool is also the servicer (*TopOrigAndService*).[26]

Another mechanism that can potentially confound our results is the reputational concerns of the members of the syndicate (Hartman-Glaser, 2012). In our main tests, we already include sponsor fixed effects in the estimation exercise. This ensures that we are able to separate out time-invariant reputational effect of the sponsors. Since we consider a short time-period (2002-2005) for our analysis, it is reasonable to assume that a sponsor's reputation remained practically constant during the sample period. As an alternative test in a similar spirit, we consider the heterogeneity in the sponsor-type to control for the reputational concerns. We expect that long-lived and established commercial banks such as JP Morgan Chase have different concerns about protecting their franchise values as compared to specialized mortgage originating institutions such as Ameriquest. Also, large commercial and investment banks may be able to exert more influence over the credit rating agencies to receive inflated ratings relative to smaller stand-alone mortgage lenders (Je et al., 2012). To address these issues, we classify each sponsor as a commercial bank, investment bank, savings and loan institution, or mortgage lender and then include dummy variables for these categories in the regression model.

Table 3.8 reproduces the main results from earlier sections of the paper alongside a specification that includes the variables mentioned above. All our key results remain qualitatively similar. Among the additional control variables, we do find some effect consistent with "skin in the game" hypothesis as deals where the top originator is also the servicer have better ex-post performance. However, inclusion of this control variable does not change any of our

[26]We perform the same tests using a dummy variable that indicates if the servicer is any of the top four originators and get qualitatively identical results.

results. In unreported tests, we repeat these analyses for other tests of the paper as well, and our main results remain robust to these controls. Overall, our results are unlikely to be affected by these alternative channels.

3.6 Discussions and Conclusion

This paper empirically examines the role of equity tranche in residential mortgage-backed securities during the build-up to the sub-prime mortgage crisis. We document that the level of equity tranche conveys the sponsor's private information in opaque pools. Within such pools, higher levels of equity tranche is associated with significantly lower future foreclosure rates after parsing out the effects of loan characteristics, macroeconomic shocks and the correlation structure of loans in the pool. Further, investors paid higher prices for sold securities in such deals. These pieces of evidence provide support for some of the fundamental predictions of security design models based on asymmetric information (e.g., Leland and Pyle (1977) and DeMarzo and Duffie (1999)).

Overall, our findings show that market participants understood informational frictions in the RMBS market to some extent and incorporated them in the design of these securities. In other words, the design of mortgage-backed securities was able to mitigate some of the contracting frictions as predicted by extant theoretical models in the literature. By design, our study is cross-sectional in nature. Therefore, we are able to comment on the ability of equity tranche in explaining economic outcomes only in a relative sense. Our study does not rule out the possibility that the absolute level of equity tranche supporting these deals was too low during the sample period. Indeed, Stanton and Wallace (2011) show that in the period leading up to the crisis, the rating agencies allowed subordination levels in CMBS markets to fall to suboptimal levels. The key contribution of our paper is to show that cross-sectional pattern in securitization design does follow the predictions of asymmetric information models. This finding has important implications for the development of future theoretical models in this area as well as for informing policy debates surrounding this market.

3.7 Tables

Table 3.1: Full Sample Summary Statistics

This table presents summary statistics for our sample. Panel A presents various loan level, pool level, and tranching structure characteristics, Panel B presents ex-post foreclosure rates, divided by various loan characteristics and Panel C presents the tranche structure of deals in our sample across time periods.

Panel A: Loan, Pool, and Tranche Structure Summary Statistics

	Mean	Std Dev	Min	25%	50%	75%	Max	N
Loan Level:								
Loan Amount	259781.43	206639.42	3150.00	110000.00	196000.00	365000.00	4350000.00	501131
FICO	656.26	76.92	496.00	599.00	657.00	716.00	799.00	501131
LTV	77.27	13.59	31.25	71.93	80.00	85.00	100.00	501131
ARM	0.66	0.47	0.00	0.00	1.00	1.00	1.00	501126
Single Family Residence	0.76	0.42	0.00	1.00	1.00	1.00	1.00	501131
Owner Occupied	0.89	0.30	0.00	1.00	1.00	1.00	1.00	501131
Foreclosure	0.16	0.37	0.00	0.00	0.00	0.00	100.00	501131
Pool Level:								
PrincipalPoolAmount (mil)	775.85	507.28	151.84	422.34	664.12	1000.08	3267.41	196
NumLoans	3150.46	2535.52	340.00	1343.50	2269.00	4409.75	12202.00	196
% NoDoc	18.77	17.84	0.00	2.94	14.34	34.68	79.13	172
GeoDiverse	59.47	17.26	0.00	49.48	61.31	74.15	87.54	196
Late	0.52	0.50	0.00	0.00	1.00	1.00	1.00	196
Subprime (FICO<660)	0.36	0.48	0.00	0.00	0.00	1.00	1.00	194
Foreclosure (dollar weighted)	0.12	0.10	0.00	0.03	0.10	0.18	0.41	152
Tranche Structure:								
% AAA Tranche	90.40	7.17	72.40	82.80	93.52	96.51	98.75	196
% Mezzanine Tranche	8.40	6.70	0.00	2.77	5.48	15.67	27.60	196
% EquityTranche	1.20	1.27	0.00	0.50	0.75	1.70	7.43	196
Mezzanine-to-Sold	8.57	6.83	0	2.80	5.49	15.86	21.99	196

Panel B: Ex-post Default Probabilities Across Risk Factors (loan counts in brackets)

	No	Yes
Above median FICO	0.22	0.11
	[251,350]	[249,781]
Above median LTV	0.15	0.20
	[346,616]	[154,515]
Fixed-rate Mortgage	0.19	0.11
	[353,342]	[147,789]
Late period (2005)	0.09	0.19
	[135,474]	[365,657]

Panel C: Tranche Structure Across Time

Piece	All	Early (2001-02)	Late (2005)
AAA	90.36	92.59	88.32
Mezzanine	8.44	6.69	10.05
Equity	1.20	0.72	1.63
Observations	196	94	102

Table 3.2: Cross-Sectional Determinants of Deal Structure

This table presents OLS estimates from regressions of *%Equity Tranche* (columns (1)-(3)) and *Mezzanine-to-Sold* (columns (4)-(6)) on loan pool characteristics. *%Equity Tranche* is the percent of the principal pool amount that is not publicly offered, *Mezzanine-to-Sold* is computed as the ratio of principal dollar amount of the mezzanine tranche to the total principal dollars amount publicly offered (mezzanine plus AAA), *Late* is a dummy variable equal to 1 for deals from 2005, *% NoDoc* is the percent of the loan pool with no documentation, *FICO* is the pool's weighted average FICO score, *LTV* is the pool's weighted average loan-to-value ratio, *% ARM* is the percent of the loan pool with adjustable rate mortgage loans, *GeoDiverse* measures the geographic diversity and is 100 - (percent of largest one state origination concentration) in the mortgage pool. All standard errors are heteroskedasticity robust.

	%Equity			Mezzanine-to-Sold		
	(1)	(2)	(3)	(4)	(5)	(6)
Late	0.886***	0.943***	0.761**	2.751**	3.181***	3.167**
	(0.00)	(0.00)	(0.01)	(0.02)	(0.00)	(0.01)
% NoDoc	0.025***	0.023**	0.016*	0.200***	-0.007	-0.007
	(0.00)	(0.04)	(0.06)	(0.00)	(0.67)	(0.77)
FICO		-0.004	0.004		-0.101***	-0.101***
		(0.23)	(0.29)		(0.00)	(0.00)
LTV		-0.025	0.034		0.301***	0.275***
		(0.32)	(0.20)		(0.00)	(0.01)
% ARM		0.005	0.006**		-0.015***	-0.013**
		(0.14)	(0.04)		(0.01)	(0.04)
GeoDiverse		-0.008	-0.005		-0.054***	-0.056***
		(0.42)	(0.62)		(0.00)	(0.00)
Constant	0.295**	5.268	-3.850	3.847***	58.015***	61.686***
	(0.04)	(0.11)	(0.35)	(0.00)	(0.00)	(0.00)
Sponsor FE	No	No	Yes	No	No	Yes
Observations	163	163	163	163	163	163
R^2	0.268	0.318	0.577	0.334	0.857	0.869

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.3: Ex-Post Outcomes: Abnormal Default

This table presents OLS estimates from regressions of *AbDefault* on loan pool characteristics. In columns (1) and (2), *AbDefault* is the ratio of the actual ex-post pool default rate to a predicted default rate based on a default model calibrated using the full sample. In columns (3) and (4), *Abnormal Default* is the ratio of the actual ex-post pool default rate to the default rate on a pool of loans that are matched, loan by loan, to the actual pool based on observable characteristics. *Opaque* is a dummy variable equal to 1 for deals with *%NoDoc* greater than that of the median deal, *HighEq* is a dummy variable equal to 1 for deals with *%Equity Tranche* greater than that of the median deal, *HighMezz* is a dummy variable equal to 1 for deals with *%Mezzanine Tranche* greater than that of the median deal, *Late* is a dummy variable equal to 1 for deals from 2005, *FICO* is the pool's weighted average FICO score, and *LTV* is the pool's weighted average loan-to-value ratio. All standard errors are heteroskedasticity robust.

	Default Model			Matched Pool		
	(1)	(2)	(3)	(4)	(5)	(6)
Late	0.321***	0.293***	0.293***	-0.006	0.008	0.037
	(0.00)	(0.00)	(0.00)	(0.95)	(0.92)	(0.68)
FICO	0.001	0.002*	0.002	0.002**	0.002*	0.001
	(0.38)	(0.08)	(0.13)	(0.05)	(0.10)	(0.58)
LTV	0.052***	0.051***	0.046***	0.069***	0.069***	0.060***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Opaque	0.163*	0.109	0.140	0.099	0.057	0.123
	(0.06)	(0.26)	(0.20)	(0.49)	(0.73)	(0.47)
HighEq	0.110	0.139*	0.202**	0.033	0.062	0.138
	(0.13)	(0.06)	(0.02)	(0.72)	(0.50)	(0.14)
HighEq * Opaque	-0.244**	-0.263***	-0.241**	-0.221*	-0.265**	-0.267**
	(0.01)	(0.01)	(0.02)	(0.07)	(0.03)	(0.05)
HighMezz		0.080	0.155		-0.115	-0.027
		(0.51)	(0.27)		(0.36)	(0.86)
HighMezz * Opaque		0.115	0.113		0.141	0.088
		(0.31)	(0.32)		(0.32)	(0.54)
Constant	-3.783***	-4.477***	-4.315***	-5.588***	-5.473***	-4.130**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Sponsor FE	No	No	Yes	No	No	Yes
Observations	151	151	151	151	151	151
R^2	0.650	0.659	0.723	0.440	0.444	0.518

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.4: Ex-Post Outcomes: The Channel of Private Information

This table presents OLS estimates from regressions of *AbDefault* on loan pool characteristics. *Abnormal Default* is the ratio of the actual ex-post pool default rate to the default rate on a pool of loans that are matched, loan by loan, to the actual pool based on observable characteristics. *Opaque* is a dummy variable equal to 1 for deals with *%NoDoc* greater than that of the median deal, *HighEq* is a dummy variable equal to 1 for deals with *%Equity Tranche* greater than that of the median deal, *HighMezz* is a dummy variable equal to 1 for deals with *%Mezzanine Tranche* greater than that of the median deal, *Late* is a dummy variable equal to 1 for deals from 2005, *FICO* is the pool's weighted average FICO score, and *LTV* is the pool's weighted average loan-to-value ratio. *Sponsor is Top Originator* indicates deals where the deal sponsor originated more loans in pool than any other originator. All standard errors are heteroskedasticity robust.

	(1) All	(2) Sponsor not Top Originator	(3) Sponsor is Top Originator
Late	0.008	0.035	-0.050
	(0.92)	(0.74)	(0.71)
FICO	0.002*	0.000	0.003**
	(0.10)	(0.92)	(0.05)
LTV	0.069***	0.070***	0.080***
	(0.00)	(0.00)	(0.00)
Opaque	0.057	0.021	0.027
	(0.73)	(0.93)	(0.90)
HighEq	0.062	-0.062	0.126
	(0.50)	(0.71)	(0.21)
HighEq * Opaque	-0.265**	-0.074	-0.421***
	(0.03)	(0.72)	(0.01)
HighMezz	-0.115	-0.370	-0.060
	(0.36)	(0.19)	(0.65)
HighMezz * Opaque	0.141	0.241	0.149
	(0.32)	(0.33)	(0.34)
Constant	-5.473***	-4.368**	-7.325***
	(0.00)	(0.05)	(0.00)
Observations	151	73	78
r2	0.444	0.398	0.538

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.5: Anti-Predatory Lending Laws, Equity Tranche, and Ex-Post Outcomes
Columns (1) and (2) present loan-level logistic regression estimates where the dependent variable is an indicator variable equal to one if the loan ends up in foreclosure. Columns (3) and (4) present pool-level OLS regression estimates where the dependent variable is the pool level abnormal default, *AbDefault*, which is the ratio of the actual ex-post pool default rate to a predicted default rate based on a default model calibrated using the full sample. We compute heteroskedasticity robust standard errors for these specifications. Columns (2) and (4) present estimates from regressions including only *Opaque* pools, which are those with *%NoDoc* greater than that of the median deal. *HighEq* is a dummy variable equal to 1 for deals with *%Equity Tranche* greater than that of the median deal, *APL* is a dummy variable equal to 1 for loans from states that enact anti-predatory lending laws, *HighAPL* is a dummy variable equal to 1 for pools where the proportion of loans from APL states is greater than sample median, *Before* is a dummy variable equal to 1 for the time period prior to the passage of APL laws. *FICO* is the FICO score and *LTV* is the loan-to-value ratio, where we use dollar-weighted averages for the pool-level specifications. The loan level regressions also include dummies for different loan purposes and loan types (coefficients not reported).

	Loan Level		Pool Level	
	(1) All	(2) Opaque	(3) All	(4) Opaque
FICO	-0.006***	-0.005***	0.002*	0.002
	(0.00)	(0.00)	(0.06)	(0.19)
LTV	0.018***	0.009***	0.054***	0.067***
	(0.00)	(0.00)	(0.00)	(0.00)
HighEq	-0.153	-0.208**	-0.077	-0.162
	(0.10)	(0.02)	(0.29)	(0.11)
Before	-0.053	-0.086	-0.342**	-0.407**
	(0.86)	(0.74)	(0.02)	(0.02)
HighEq * Before	0.611***	0.434**	0.346**	0.396**
	(0.00)	(0.01)	(0.04)	(0.05)
APL	0.337	0.451		
	(0.22)	(0.15)		
APL * HighEq	0.256***	0.293***		
	(0.00)	(0.00)		
APL * Before	-0.297***	-0.021		
	(0.01)	(0.86)		
APL * HighEq * Before	-0.226*	-0.463***		
	(0.09)	(0.00)		
HighAPL			-0.050	0.061
			(0.60)	(0.55)
HighAPL * HighEq			0.036	-0.234
			(0.78)	(0.12)
HighAPL * Before			-0.085	0.027
			(0.62)	(0.91)
HighAPL * HighEq * Before			-0.443**	-0.486*
			(0.04)	(0.09)
Constant	-13.685	0.621*	-4.034***	-5.041***
	(0.21)	(0.07)	(0.00)	(0.00)
Observations	497367	254774	151	72
Pseudo R^2	0.110	0.092		
R^2			0.688	0.569

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.6: Equity Tranche and Yield Spreads Cross-tabulation

This table presents the mean yield spread for variable rate tranches in the sample according to the size of the equity tranche and the tranche's rating class. For deals with multiple tranches within a rating class, the observation is the dollar-weighted average of the coupons. *High Equity* indicates that the pool under consideration has *%Equity Tranche* greater than that of the median deal.

	Tranche Rating			
Equity Tranche Size	AAA	AA	A	$\leq$ BBB
Low Equity	0.44	1.27	1.50	2.43
	(0.06)	(0.30)	(0.25)	(0.22)
High Equity	0.34	0.77	1.25	2.24
	(0.03)	(0.10)	(0.11)	(0.12)

Standard errors in parentheses

Table 3.7: Price Response to Equity Tranche

This table presents OLS estimates from regressions of the yield spread (in percentage points) on loan pool characteristics. Each observation represents a *Pool* × *Rating Class* dollar-weighted spread for variable rate tranches, where we define *Rating Class* as *AAA*, *AA*, *A*, and *BBB and below*. *Late* is a dummy variable equal to 1 for deals from 2005, *Opaque* is a dummy variable equal to 1 for deals with *%NoDoc* greater than that of the median deal, and *HighEq* is a dummy variable equal to 1 for deals with *%Equity Tranche* greater than that of the median deal. All standard errors are heteroskedasticity robust.

	(1) All	(2) All	(3) non-AAA	(4) AAA	(5) non-AAA	(6) AAA
Late	-0.49***	-0.58***	-0.62***	-0.24***	-0.76***	-0.25***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
HighEq	-0.27**		-0.38**	-0.09		
	(0.01)		(0.02)	(0.13)		
Opaque		-0.18			-0.35	-0.07
		(0.44)			(0.38)	(0.48)
HighEq * Opaque		-0.34***			-0.46***	-0.09
		(0.00)			(0.00)	(0.34)
HighEq * Not Opaque		-0.08			-0.21	-0.06
		(0.74)			(0.58)	(0.49)
Rating Class FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	379	379	262	117	262	117
R^2	0.43	0.45	0.30	0.15	0.34	0.17

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.8: Robustness – Alternate Channels

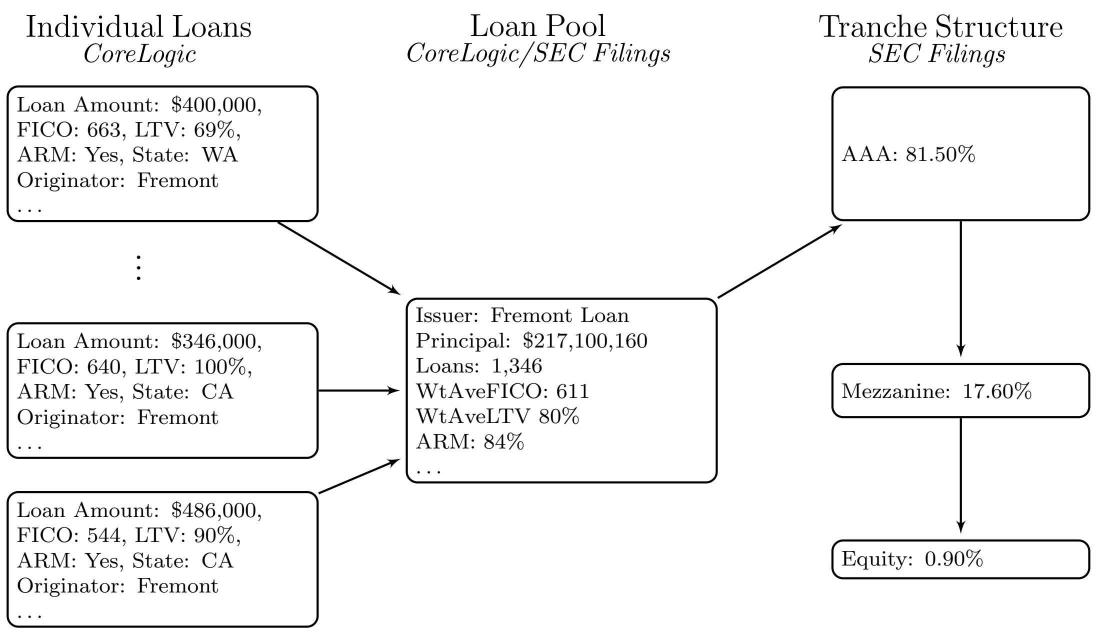
This table presents our main results from earlier tables alongside specification that include other variables that capture the roles and connections of the various agents in the securitization chain. *Late* is a dummy variable equal to 1 for deals from 2005, *%NoDoc* is the percent of the loan pool with no documentation loans, *FICO* is the pool's weighted average FICO score, *LTV* is the pool's weighted average loan-to-value ratio, *% ARM* is the percent of the loan pool with adjustable rate mortgage loans, *GeoDiverse* measures the geographic diversity and is computed as 100 - (percent of largest one state origination concentration) in the mortgage pool, *Opaque* is a dummy variable equal to 1 for deals with *%NoDoc* greater than that of the median deal, *HighEq* is a dummy variable equal to 1 for deals with *%Equity Tranche* greater than that of the median deal, and *SellAndService* is a dummy variable equal to 1 for deals where the issuer is also the primary servicer. *TopOrigAndService* is a dummy variable equal to 1 for deals where the top originator in the pool is also the primary servicer. Institution-Type effects refers to the inclusion of a set of dummy variables that identify sponsors as a commercial bank, investment bank, savings and loan or strictly mortgage lender. All standard errors are heteroskedasticity robust.

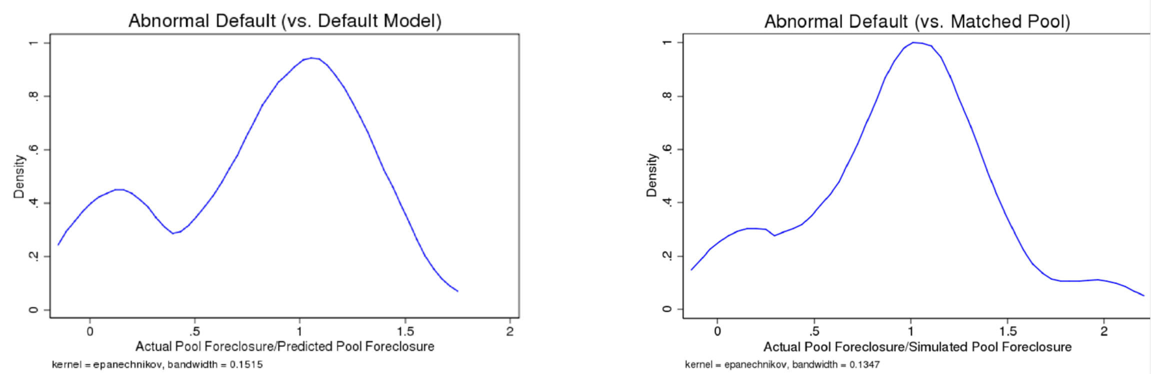
	%Equity		Mezzanine-to-Sold		Abnormal Default Match	
	(1)	(2)	(3)	(4)	(5)	(6)
Late	0.943***	0.840***	3.181***	3.235***	-0.006	-0.044
	(0.00)	(0.00)	(0.00)	(0.00)	(0.95)	(0.56)
NoDoc	0.023***	0.019**	-0.007	-0.012		
	(0.01)	(0.04)	(0.55)	(0.38)		
FICO	-0.004	0.004	-0.101***	-0.102***	0.002**	0.001
	(0.16)	(0.29)	(0.00)	(0.00)	(0.05)	(0.25)
LTV	-0.025	0.009	0.301***	0.288***	0.069***	0.062***
	(0.23)	(0.74)	(0.00)	(0.00)	(0.00)	(0.00)
ARM	0.005**	0.006**	-0.015***	-0.016***		
	(0.01)	(0.01)	(0.01)	(0.01)		
GeoDiverse	-0.008	-0.010	-0.054***	-0.058***		
	(0.27)	(0.10)	(0.00)	(0.00)		
Opaque					0.099	0.138
					(0.49)	(0.29)
HighEq					0.033	0.074
					(0.72)	(0.46)
HighEq * Opaque					-0.221*	-0.300**
					(0.07)	(0.02)
SellAndService		-0.453*		-0.632		0.218**
		(0.10)		(0.29)		(0.02)
TopOrigAndService		-0.210		0.196		-0.361***
		(0.41)		(0.70)		(0.00)
Constant	5.268*	-2.111	58.015***	60.044***	-5.588***	-4.546***
	(0.08)	(0.59)	(0.00)	(0.00)	(0.00)	(0.00)
Institution Type FE	No	Yes	No	Yes	No	Yes
Observations	163	163	163	163	151	151
R^2	0.318	0.494	0.857	0.860	0.440	0.540

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.8 Figures

Figure 3.1: Example Deal: Fremont Home Loan Trust Series 2002-1
This figure provides an example deal from our sample to illustrate the construction of a typical deal and the sources of our data. Loan specific characteristics such as FICO score, loan amount, loan type, LTV, etc. are from CoreLogic. Aggregate deal statistics, including the tranche structuring of the deal, were hand collected from the Form 424(b)(5) filings to the SEC.





(a) Default Model

(b) Matched Pools

Figure 3.2: Measures of Abnormal Default

This figure presents kernel densities of our measures of abnormal default. Panel (a) presents a kernel density of our first measure of abnormal default which we calculate as the ratio of the actual ex-post pool default rate to a predicted default rate based on a default model calibrated using the full sample. Panel (b) presents our second measure of abnormal default which we calculate as the ratio of the actual ex-post pool default rate to the default rate on a pool of loans that are matched, loan by loan, to the actual pool based on observable characteristics.

APPENDICES

APPENDIX A

The Real Costs of Corporate Credit Ratings

A.1 Variable Construction

Table A.1: Variable Definitions

This table identifies the data sources and describes the construction of variables used in the analysis. Company financial data are from Compustat, returns are from CRSP, and bond data are from the fixed income securities database (FISD). For firm financial data, quarterly data are used if the firm reports at that frequency.

Variable	Definition
Assets	Total assets [atq].
Leverage	Total debt [dlcq + dlttq] / assets [atq].
Tobin's Q	(Assets [atq] + market value of equity [prccq * cshoq] - common equity [ceqq] - deferred taxes [txditcq]) / assets [atq].
Debt/EBITDA	Total debt [dlcq + dlttq] / trailing four quarters EBITDA [oibdpq].
Return on Assets	Trailing four quarters of EBITDA [oibdpq] / lagged assets [atq].
Return on Equity	Trailing four quarters net income [niq] / lagged common equity [ceqq]
Cash flow	Trailing four quarters of income plus depreciation [ibq + dpq] / lagged assets [atq].
R&D	Research and development expenditures [xrd] / lagged total assets [atq].
SG&A	Selling, general, and administrative expenditures [xsga] / lagged total assets [atq].
Stock Return	Equity stock return over the past year.
Bond Yield	Yield to maturity of the bond at issuance. When multiple bonds are issued in the same quarter, this is computed as the dollar-weighted yield of the issuances.
Bond Amount	Amount of issuance in millions. When multiple bonds are issued in the same quarter, this is computed as the sum issuances.

APPENDIX B

Signaling, Financial Constraints, and Performance-Sensitive Debt

B.1 Variable Construction

Table B.1: Variable Definitions

This table defines the construction of borrower characteristics in terms of Compustat variables. All measures are taken from the quarter prior (or year prior when the data item is available only on an annual basis) to loan origination.

Variable	Definition
Compustat Data Items:	
Assets	total assets [atq].
Leverage	total debt [dlcq + dlttq] / assets [atq].
Market-to-Book	market value of firm [prccq * cshoq + dlcq + dlttq + pstkq + txditcq] / total assets [atq].
Profitability	previous four quarters of EBITDA [oibdpq] / previous four quarters of sales [atq].
Interest Coverage	previous four quarters of EBITDA [oibdpq] / previous four quarters of interest expense [xintq].
EBITDA Volatility	ratio of the standard deviation of the past eight earnings [oibdpq] changes to average book asset [atq] size over the past eight quarters.
Future Distress	dummy variable equal to one if (i) the borrower has two consecutive years where the interest coverage ratio falls below 1.0 or (ii) the borrower has one year where the interest coverage ratio is below 0.8.
Dealscan Data Items:	
Spread	loan spread in basis points [allindrawn].
Loan Size	facility amount in millions of dollars [facilityamt / 1000000].
Secured	dummy variable equal to one if the loan is secured by collateral.
Financial Covenants	total number of financial covenants in the loan.
Relationship	log(1 + number of loans with the current lender in the five years prior to the current loan).
New Interest Expense	annual interest expense from the loan [allindrawn/10000 * facilityamt] / total assets [atq].
PSD	Dummy variable equal to one if the loan has a performance pricing grid.
High Convexity	dummy variable equal to one if the loan has a performance pricing grid that is in the top tercile of the convexity measure (see Section 2.4 for details).
Low Convexity	dummy variable equal to one if the loan has a performance pricing grid that is not in the top tercile of the convexity measure.

APPENDIX C

Design of Financial Securities: Empirical Evidence From Private-Label RMBS Deals

C.1 Sample Construction and Data Collection

We use a stratified random sampling method to select private-label (i.e., non-agency backed) RMBS deals for inclusion in our study. We choose two time periods for our sample selection: an "early period" that covers deals from 2001-02 and a "late period" that covers deals from 2005. This stratification strategy allows us to separate out time-specific effects from our main cross-sectional results. It also allows us to investigate the time variation in the functioning of this market and exploit changes in anti-predatory-lending laws. Ashcraft and Schuermann (2008) report that the issuance of non-agency mortgage-backed securities increased eight-fold from $99 billion in 2001 to $797 billion in 2005 in the sub-prime and Alt-A segment. Thus our sample covers both an early/nascent period and a relatively matured period of RMBS market. We also stratify the sample along the prime-subprime dimension, slightly over-sampling the subprime pools to make sure that portion of the sample is large enough to make statistically meaningful inference. Our random sample begins with 196 deals. Due to variation in the data items included in the filings, our main regression specifications include 163 deals that have full data on all variables of interest.

We collect data on mortgage pools and their tranches from Form 424(b)(5) filings which are submitted to the SEC pursuant to SEC Rule 424(b)(5). While the detail of the information provided varies slightly from deal to deal, the form typically contains data on all the major participants in the deal (e.g., sponsor, originators), pool-level characteristics and tranche-level data. Among other items, these data specifically include the loan originators and the share of the deal they originated, weighted average loan-to-value (LTV) ratio, weighted average FICO score, and a breakdown of loan types, geography and loan documentation levels within the pool.

Form 424(b)(5) also provides a listing of each tranche in the pool along with its principal amount and credit rating. For our analysis, we aggregate the tranches into three bins: AAA-rated tranches, mezzanine tranches and equity tranches. We present a detailed discussion of the equity tranche in Section 3.4. The AAA tranche is self-explanatory and the mezzanine tranche is simply the subordinated tranche that lies between the AAA and equity tranches. The publicly offered tranches (AAA and mezzanine) include ratings from at least two major credit rating agencies. While disagreements in ratings among the ratings agencies are rare for the senior tranches, we use the lower of the ratings when conflicts occur.

We match these deals with detailed loan-level data obtained from CoreLogic. Pools in our sample cover over 500,000 individual mortgages. We obtain key information for each loan in a given pool from CoreLogic such as the loan amount, FICO score, LTV ratio, and loan type along with location of the property and various other characteristics. Finally, we obtain the ex-post performance of these loans from CoreLogic as well. We obtain information on the incidence of foreclosure anytime from the origination of the deal through December 2011. This information allows us to conduct our test relating tranche structure to ex-post loan performance. Our sample size drops slightly to 151 deals for which we are able to match our pool level data with CoreLogic database.

C.2 Example of Documentation Description from a Deal Prospectus

Series Name: ABFC Mortgage Loan Asset-Backed Certificate, Series 2002-WF2

> The Originator's subprime mortgage loan programs include a full documentation program, a "stated income, stated asset" program and a "lite" documentation program. Under the full documentation program, loans to borrowers who are salaried employees must be supported by current employment information in the form of one current pay-stub with year-to-date information and W-2 tax forms for the last two years (a complete verification of employment may be substituted for W-2 forms). The Originator also performs a telephone verification of employment for salaried employees prior to funding. In some cases, employment histories may be obtained through V.I.E., Inc., an entity jointly owned by the Originator and an affiliated third party, that obtains employment data from state unemployment insurance departments or other state agencies. Under the full documentation program, borrowers who are self-employed must provide signed individual federal tax returns and, if applicable, signed year-to-date income statements and/or business federal tax returns. Evidence must be provided that the business has been in existence for at least one year. If the business has been in existence less than two years, evidence must be provided that the applicant had previously been in the same line of work for at least one year. Under the full documentation program, at certain loan-to-value ratio levels and under certain circumstances not all sources of funds for closing are verified as the borrowers.
>
> Under the Originator's "Stated Income, Stated Asset" program, the applicant's employment, income sources and assets must be stated on the initial signed application. The applicant's income as stated must be reasonable for the applicant's occupation as determined in the discretion of the loan underwriter; however, such income is not independently verified. Similarly the applicant's assets as stated must be reasonable for the applicant's occupation as determined in the discretion of the loan underwriter; however, such assets are not independently verified. Except under the Stated Asset

Program, verification of funds sufficient to close the mortgage loan is performed. Under the "LITE" Documentation program, the Originator reviews the deposit activity reflected in the most recent six or twenty-four consecutive months of the applicant's bank statements as an alternative method of establishing income. Maximum loan-to-value ratios within each credit level are lower under the stated income, stated asset program than under the full documentation program.

C.3 Matched Pool Construction

We construct a hypothetical pool of loans that look observationally similar to loans in actual pools. As described in Section 3.5.2.2, our goal is to create a random pool of loans that is likely to have similar foreclosure performance as the actual pool in terms of observable loan and property characteristics, macroeconomic shocks, and correlation structure of loans with the pool. For every loan i in pool p, we start with all other loans in our sample, excluding the pool where loan i resides, and follow the following matching algorithm:

1. Drop potential matches that were not originated in the same (early or late) as loan i.
2. Drop potential matches that are not sufficiently close to loan i in terms of two most important observable characteristics of this market: FICO scores and LTV ratio. We ensure that potential control loans are within one-tenth of the standard deviation of FICO and LTV of the loan being matched. This criteria ensures that LTV ratio of matched firms fall within 1.4 percentage points and FICO score within 11.2 points of loan i.
3. Drop potential matches that are not located in the same state as loan i.
4. We break all loans into three groups based on the nature of interest rate: fixed rate loans, ARM, and Balloons. Drop potential matches that do not have the same interest rate type as loan i.
5. Drop potential matches that are not within 25% of the principal loan amount of loan i.
6. Drop potential matches that whose origination date is not within ± 90 days of loan i.
7. From the remaining set of potential matches, assign the loan with LTV ratio closest to loan i as the matched loan.

We repeat this exercise for all loans in a pool. We are able to obtain matches for 401,228 loans based on this criteria. This leaves us with approximately 100,000 loans that remains unmatched after the first iteration. For loans without a match, we continue as follows:

8. Return to Step (2) above, but drop the requirement that the matched loan be within 1.4 percentage points of loan i in terms of LTV ratio.

This iteration yields another 101,963 matches and almost completes the matching. For a very small number of loans (19,079) that remain unmatched, we continue as follows:

9. Return to Step (2), dropping the LTV caliper requirement as in Step (8), and widen the range of FICO scores to be within one-fifth of the standard deviation and allow the loan origination date to be within ± 180 days of that of loan i.

With less than 4% of loans matched based on the looser criteria of Step (9), our results do not change if we drop these loans altogether from the sample. Based on this matching procedure, we are able to create a hypothetical pool that has loans with extremely similar characteristics on observable dimensions (with exact matches for state, loan type, and early/late period).

C.4 Tables

Table C.1: Institutions and their Various Roles
This table presents the most common institutions in the sample and the frequency in which they participated in various roles.

Institution	Seller	Top Originator	Type
Ace	5	0	Mortgage Lender
Ameriquest	14	15	Mortgage Lender
Bear Stearns	17	0	Investment Bank
Bank of America	28	23	Commercial Bank
Citi	8	4	Commercial Bank
Credit Suisse	16	10	Investment Bank
Countrywide	6	10	Savings and Loan
Deutsche Bank	5	0	Commercial Bank
Goldman Sachs	16	0	Investment Bank
HSBC	3	0	Commercial Bank
IndyMac	10	11	Savings and Loan
JP Morgan	9	5	Commercial Bank
Lehman Brothers	6	4	Investment Bank
Merrill Lynch	8	1	Investment Bank
Option One	8	13	Mortgage Lender
Stanwich	3	0	Mortgage Lender
UBS	6	0	Commercial Bank
Washington Mutual	11	14	Savings and Loan
Wells Fargo	12	24	Commercial Bank
Other	5	62	

Table C.2: The Equity Tranche
This table presents examples of two common tranche structures used for RMBS and how the equity tranche is computed for each case.

Panel A: Six-pack

Offered	Class	Principal ($)	Rate	Rating S&P	Rating Moody's
Y	A	399,181,000	LIBOR+0.34	AAA	Aaa
Y	M-1	35,789,000	LIBOR+0.65	AA+	Aa2
Y	M-2	27,530,000	LIBOR+1.20	A+	A2
Y	M-3	23,776,000	LIBOR+2.00	BBB	Baa2
Y	M-4	6,757,000	LIBOR+2.30	BBB-	Baa3
N	CE	7,508,765		NR	NR
	Sum	500,541,765			
Pool:	Mortgages	3,737			
	Principal	500,541,765			
	Equity Tranche =	7,508,765/500,541,765 = 1.50%			

Panel B: Overcollateralization

Offered	Class	Principal ($)	Rate	Rating S&P	Rating Moody's
Y	A	154,414,000	5.50	AAA	Aaa
Y	M-1	27,440,000	LIBOR+0.50	AA	Aa2
Y	M-2	12,267,000	LIBOR+0.75	A	A2
Y	M-3	4,196,000	LIBOR+0.80	A-	A3
Y	B-1	5,058,000	LIBOR+1.25	BBB+	Baa1
Y	B-2	3,336,000	LIBOR+1.30	BBB	Baa2
Y	B-3	6,564,000	LIBOR+2.15	BBB-	Baa3
	Sum	213,275,000			
Pool:	Mortgages	1,039			
	Principal	215,212,063			
	Equity Tranche =	(215,212,063–213,275,000)/215,212,063 = 0.90%			

Table C.3: Default Model

This table presents the results of the default model. We use the estimated coefficients of this model to predict the loan-by-loan probability of foreclosure to construct our measure of *Abnormal Default* used for the estimates in Table 3.4. Following prior literature (e.g., see Demyanyk and Van Hemert, 2011), we include the borrower's FICO score, the loan-to-value ratio, loan purpose (e.g., Refinancing with Cash-Out), loan type, (e.g., 5-year Interest Only), state fixed effects, and year fixed effects. The results below show the key drivers of default risk, with the point estimates on the other variables in the estimation omitted in the interest of space.

	Prob(Foreclosure)	
	b	se
FICO	-0.0059***	(0.000)
LTV	0.0180***	(0.000)
Refinancing with Cash-Out	-0.2455***	(0.000)
Refinancing w/o Cash-out	-0.3458***	(0.000)
5-year Interest Only	0.8339***	(0.000)
10-year Interest Only	0.8071***	(0.000)
Adjustable Rate Mortgage	0.2906***	(0.000)
5-year I.O. ARM	0.6705***	(0.000)
10-year I.O. ARM	0.6598***	(0.000)
7-year I.O.ARM	0.0436	(0.441)
2-year I.O. ARM	0.8834***	(0.000)
7-year Balloon	1.9788	(0.185)
15-year Balloon	-1.0840***	(0.000)
ARM Balloon	0.8550***	(0.000)
Balloon-Other	1.0936***	(0.000)
Arizona	0.3898**	(0.019)
California	0.4543***	(0.006)
Florida	0.8219***	(0.000)
Georgia	1.0356***	(0.000)
Nevada	1.2092***	(0.000)
State Fixed Effects	Yes	
Year Fixed Effects	Yes	
Other Controls	Yes	
Observations	497367	

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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