# Price Discipline for Non-Price Loan Terms

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**Abstract**

In the standard model of capital markets contracting, issuers select non-price terms a la carte, optimizing future flexibility in light of the price they imagine investors will charge for it. A recent development in restructuring practice—invention of the so-called non-pro rata uptier exchange—offers a rare opportunity to assess the model's explanatory power with respect to terms that are economically meaningful in expectation but only contingently applicable.

In an uptier, the borrower and a bare majority of its lenders agree to subordinate ostensibly *pari passu* debt, transferring wealth from minority lenders to the majority and especially to the borrower's equity investors. Not all loans are susceptible to an uptier, however. After Summer 2020, when three uptiers were executed in short order, newly originated loans became likely to include a provision blocking the transaction. The mechanism that caused contracts to change is, however, unclear.

To assess the notion that price discipline induced the contractual change, we study the returns to loans outstanding in Summer 2020 around events that disclosed the uptier's legality and commercial practicability. If investors price the relevant terms, then yields on susceptible and immune loans ought to have diverged. In a sample of all publicly available loans, however, we find only weak evidence of a relationship between susceptibility and abnormal returns.

Several possibilities could explain our results. The most likely, including mere insufficiency of statistical power, cast doubt on the notion that borrowers could observe the price of uptier flexibility—or, by extension, that they can observe the prices of similarly important contractual provisions—in a manner that would allow them to calibrate terms optimally. Instead, our results support a model of financial contracting in which "talk" sourced from non-price mediating institutions such as lawyers and trade associations play an important role in term selection.

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## 1 Introduction

Standard theories of optimal financial contracting depend on price discipline. Coase's (1960) observation that parties who face minimal bargaining frictions will agree to surplus-maximizing terms does not obviously follow in capital-markets contexts, where the one-to-many nature of transacting rules out or at least curtails explicit dickering. In a world in which (at first approximation) companies seeking to raise capital offer terms on a take-it-or-leave-it basis, one might suppose that financial contracts would tend to favor insiders relative to the social optimum. But according to the bonding notion first developed by Jensen and Meckling (1976), profit-maximizing companies propound optimal terms despite the absence of a bargaining framework because proponents internalize the social costs of the terms they select. Capital providers demand a return on their investments commensurate with the risks they foresee from investing, including moral hazard attributable to contractual terms. Consequently, issuers face a choice between supplying optimal, self-constraining terms and paying for socially excessive flexibility (Schwartz and Scott, 2003). Explicit negotiation of non-price terms is beside the point because the price mechanism encourages contract proponents to simulate a hypothetical Coasean bargain.

The logic of price discipline pervades the finance and law-and-economics literatures wherever unilaterally imposed terms are to be explained or justified in markets with symmetrically informed parties. Bonding was central to the corporate law revolution of the 1970s and 1980s, for example (e.g., Winter, 1977; Easterbrook and Fischel, 1989). Its application to bond indentures and loan agreements, with their elaborate articulations of state-contingent control rights, is perhaps even more apparent (Aghion and Bolton, 1992). And, indeed, price discipline is often invoked at least implicitly to explain creditor-friendly staples of

debt contracts such as collateral (e.g., Benmelech, Kumar & Rajan, 2022; Donaldson, Gromb & Piacentino, 2021) and borrower covenants (e.g., Bradley and Roberts, 2015; Smith and Warner, 1979). So, for example, Bradley and Roberts (2015, p. 29) reason confidently that a borrower's choice to include a covenant in its loan contract "amounts to weighing the costs stemming from the restrictions imposed … against the decrease in the promised yield (cost) of the loan."

Despite scholarly emphasis on the price-discipline mechanism, however, its ability to explain real-world contracting practices is hard to assess. The mechanism's presumptive efficacy depends on the sensitivity of prices to contractual variation. The degree of such sensitivity is, however, non-obvious (other than with respect to core financial terms, such as interest rate, maturity, and so forth) and challenging to verify empirically. When it comes to obscure provisions and to provisions governing remote contingencies, the standard bonding model depends either on a heroic conception of the investor, who is cognitively able to estimate the value of myriad rules that might apply in only a fraction of myriad possible futures, or else on a market structure that efficiently aggregates, through arbitrage or otherwise, the judgments of able albeit less-than-heroic individual investors. To find out if prices in a particular market are sensitive to variation in a particular non-price term, one ideally would compare the returns on otherwise-similar contracts that differ with respect to the variable of interest. But financial contracts have hundreds if not thousands of analytically separable dimensions. Empirical estimation of the impact of a single variable on observed prices is impossible for all but the biggest-impact items. Moreover, selection problems abound. In most instances, the prospect that contractual variation with respect to a term of interest is the product of variation in, say, borrower type frustrates efforts at causal inference. As a result, one is left to gauge the theory largely on first principles.

In this paper, we use the invention and proliferation of a transactional form known as the "non-pro rata uptier exchange," or "uptier," to study price sensitivity to non-core terms in the syndicated loan market. An uptier is a recapitalization transaction that, among other things, transfers wealth (compared to functionally similar alternatives) from the subject company's senior lenders to its equity investors. In an uptier, a financially troubled company and a bare majority of its lenders agree to do two things: (1) amend the relevant contracts to allow the company to borrow additional money on a super-senior basis and (2) roll the participating lenders' loans into some of the new paper. The infusion of new money postpones a reckoning that would wipe out equity investors, while the roll-up subordinates the claims of lenders excluded from the deal to the new-money as well as previously equal-priority lenders.

Two features of the uptier make it useful for an inquiry into contracting. First, lenders' susceptibility to the transaction is a function of contract: uptiers depend on the concurrence of several, seemingly unrelated provisions often but not always found in modern syndicated loans. Second, in light of the number of provisions and subtlety of interactions between them that cumulatively determine a borrower's ability to execute an uptier, which we elaborate below, there is reason to think that the susceptibility of loans originated before Summer 2020, when three companies deployed the transaction in quick succession and so revealed both its legal logic and commercial viability, was at most weakly correlated with borrower or lender characteristics.

We use this variation to study how contractual susceptibility affected the secondary-market prices of stressed and distressed loans. The price impact of susceptibility is useful for assessing the price-discipline mechanism because contract terms in fact changed markedly after Summer 2020. Buccola and Nini (2022) find that loans originated after the first uptiers became much more likely to block the transaction. In their sample, the frequency of immunity in new

loans increased from approximately 30–40 percent in periods before the uptier was deployed to almost 90 percent two years later. The speed and magnitude of change indicate both that the uptier came as a surprise (otherwise there would have been no reason for change) and that market participants viewed it unfavorably (otherwise contracts would have changed differently if at all). The price-discipline model of term selection holds that borrowers after Summer 2020 must have anticipated that they would have to pay higher yields in exchange for an option later to execute an uptier and concluded that the option was not worth the higher yield. Those beliefs would be hard to justify, though—at least with the precision assumed by optimality models—unless (at minimum) lenders in fact began to demand higher yields on the stock of existing, susceptible loans.[4]

Our approach is to study the impact on loan returns of three events during Summer 2020: (1) the announcement by the bedding company, Serta Simmons, of its plan to execute an uptier (revealing the transaction's commercial practicability as well as broadly applicable contractual logic), coupled with a judge's decision, twelve days later, to allow the transaction to close (suggesting the transaction's legality); and the announcements by (2) TriMark and (3) Boardriders of two more uptiers soon thereafter (normalizing the transaction). We obtain and read the contracts underlying 282 institutional term loans trading in the secondary market at stressed or distressed prices (bids quoted at 90 or below) in the months before the Serta

[4] The connection between secondary-market prices and primary-market term selection is straightforward. Most par buyers of institutional term loans do not expect to hold them through a restructuring cycle. Such buyers should be expected to care about a loan's non-price terms principally because and to the extent that the terms will determine the sale price a par investor will fetch in the secondary market conditional on the credit becoming stressed or distressed (Borowicz, 2021). Santos and Nigro (2009) find that the secondary-market liquidity of a borrower's loans impacts its subsequent borrowing costs and, in that sense, offer an empirical confirmation of feedback from the secondary market to the terms of loan origination.

transaction. As far as we can tell, the sample comprises all such contracts that are publicly available. We then compute the abnormal return on each loan around the event windows.[5]

We find only weak evidence of a relationship between susceptibility and abnormal returns and conclude that any relationship cannot be estimated precisely enough to be consistent with a price-calibrated theory of contractual change. In our primary specification, the mean difference between the cumulative abnormal returns on susceptible and immune loans is nearly 100 basis points and (barely) significant at the 95 percent level. But confidence internals are wide, and the relationship is not robust to alternative, plausible specifications of the event windows. Nor does restricting the sample to plausibly distinctive subsets of borrowers, such as sponsored or especially distressed borrowers, generate significant price differences. Moreover, on the basis of the variability in loan prices, we show that we would be unlikely to detect plausible effect sizes even if we had access to the contracts underlying every non-public as well as public loan in existence in 2020.

Three generic stories might explain our results: (1) yields did not diverge because loan investors rely on low-dimension pricing models (computational complexity story); (2) yields did not diverge because the costs of learning about a given loan's susceptibility outweigh anticipated trading gains (contractual complexity story); or (3) yields diverged, but our statistical methods are not sensitive enough to detect the divergence (statistical power story). Although we take steps to disambiguate, we cannot say which of the three stories, or in what proportion each story, accounts for our results.

[5] In this draft, we use price change as a proxy for total return. Computing LIBOR and margin for each loan in each period does not meaningfully affect any result.

Whatever the explanation, though, our results fit uneasily with the conventional bonding model. The complexity stories directly challenge the premise that loan investors are sensitive to non-core or contingent terms. If investors price only a small number of loan dimensions, then borrowers needn't internalize the social costs of many, perhaps most, contract features. Ayotte and Bolton (2011) helpfully develop this line of thought: if it costly for lenders to understand or value contractual meaning, then, in a price-discipline model of term selection, supra-optimal borrower flexibility is to be expected in equilibrium. Complexity thus cannot explain changing loan terms within a price-discipline framework.

The implications of the statistical power story are more vexing. Our contention is that borrowers after Summer 2020 lacked an empirical grounding for the specific belief that a loan's susceptibility to an uptier increases the yield lenders demand to hold it. That should cast doubt on the notion that most borrowers in fact believed that lenders would demand incremental yield on susceptible loans in excess of the associated benefit to the borrower, much less that such a belief motivated contractual change. But the conclusion is not logically necessary. To the extent that borrowers harbored a well-grounded but more general belief that loan prices are highly sensitive to term variation and perceived that the social costs of uptiers exceed the benefits, they might sensibly have behaved as if the specific belief were also well grounded. Priors have power especially where empirical verification is elusive. That said, we are unaware of the kind of research that would seem to justify a local prior—that is, research about the sensitivity of loan yields to similarly important (obscure) contract provisions—and discount this explanation accordingly.

We thus interpret our results to suggest a wider scope for models of term selection that emphasize a role for non-price institutions and what might loosely be labeled "talk." One vein of

empirical contracts scholarship in recent years has focused on the significance of intermediaries such as lawyer networks and trade associations (e.g., Bartlett, 2023; Jennejohn, Nyarko & Talley, 2022; Nyarko, 2021; Scott, Choi & Gulati, 2020; Gulati and Kahan, 2018). In a world of price discipline they, by tautology, can affect term selection only with respect to trivialities. Selection of a term with an economic significance equal to, say, 25 basis points is not enormously important—it's not a year of maturity or the difference between a secured and unsecured loan—but nor is it trivial. Certainly loan-market intermediaries were active after Summer 2020. The Loan Syndication and Trading Association offered a series of webinars on uptier mechanics and proposed language that contract drafters could use to foreclose the transaction (LSTA, 2021). Leading finance lawyers and debt-market information services likewise offered a stream of commentary and issued memoranda outlining their own favored responses. Our results suggest although they do not prove a causal influence.

The extent to which loan (and perhaps by extension bond) contracting depends on rough verbal signals about term propriety matters both for predicting contract terms and for gauging optimality. Directionally one should expect non-price intermediation to push toward surplus-enhancing terms, just as price discipline does. A cacophony is, after all, the product of distributed but more or less interested parties. The loudest voices are apt to be those with a stake in the trajectory of leveraged finance. But the mechanism presumably matters. For example, one might expect less heterogeneity in terms than would be ideal given variation in borrower type. Standardization in financial contracts has been an interesting subject since Kahan and Klausner (1997), who explain how network effects can lead to contractual uniformity and thus account for standardization within a world of perfect price sensitivity. Price *insensitivity* might, however,

better explain in fact the standardization of many terms that are below a threshold of economic significance.

## 2 Institutional Setting

A grasp of the structure of the leveraged loan market and the uptier transaction is needed to make sense of our research design. To that end, this section (1) outlines key features of the loans we study and the markets in which they are originated and trade, (2) sketches the legal mechanics and economic significance of uptiers, and (3) describes the events we use to identify the impact of contractual susceptibility on loan returns.

### *2.1 Institutional Term Loans*

Among the most remarkable developments in leveraged finance over the last two decades has been the emergence and proliferation of institutional term loans (sometimes called "Term Loan B"). These are loans made to speculative-grade and unrated borrowers, arranged by a commercial bank with an eye toward syndicating the credit immediately upon closing to non-bank institutions such as collateralized loan obligations (CLOs), loan mutual funds, hedge funds, and alternative asset managers. They are floating-rate instruments secured typically by liens on substantially all of the borrower's assets. Most institutional term loans—approximately 90 percent (Nini and Smith, 2023)—are first-lien, meaning that they are the most senior form of term debt in the borrower's capital structure.[6] The product dates only to the early 2000s but has become a major factor in corporate credit since the 2008–2009 financial crisis. More than a trillion dollars of institutional term loans are outstanding, making up about half—and together

[6] Revolving lines of credit usually rank ahead of all term debt. In the case of asset-based revolvers, the lenders have lien priority with respect to current assets (the borrowing base). Otherwise, an intercreditor agreement typically gives the revolving lenders contractual priority through a turnover requirement.

with high-yield bonds, the vast majority—of the debt financing of large, risky companies (Nini and Smith, 2023).

Despite their branding and legal classification as non-securities, institutional term loans have as much in common with high-yield bonds as they do with traditional "bank" loans. Before the institutional market developed, banks originating corporate loans typically planned to hold the investment to maturity (Bord and Santos, 2012). A loan might be syndicated, but only to a handful of fellow banks or insurers (Roberts and Sufi, 2009), and the originating bank retained significant exposure and an informal leadership role in the syndicate. Tight maintenance covenants designed to facilitate the lead bank's bespoke monitoring of the borrower's business were important features of the contractual design (Baird and Rasmussen, 2006; Nini, Smith, and Sufi, 2009, 2012; Roberts and Sufi, 2009). Institutional loans, by contrast, are tailored to the needs of investors, such as CLO managers and loan mutual fund advisers, who pursue portfolio rather than focused investment strategies.[7] The loans are designed to be held by many investors in increments too small to justify active monitoring in any case. Indeed, Berlin, Nini, and Yu (2020) report that the average syndicate consists of some 200 lenders. Consistent with renegotiation being relatively difficult, institutional term loans feature looser restrictions on borrower activity than traditional loans do (Griffin, Nini, and Smith, 2021; Ivashina and Vallée, 2022) and, like high-yield bonds, often eschew maintenance covenants altogether.

The numerosity and variety of investors in a given loan implies governance challenges within the syndicate as well as between the syndicate and the borrower. Routine matters are coordinated by a bank—the "administrative agent"—who holds liens on behalf of the lenders,

[7] Cordell, Roberts, and Schwert (2023) find that a typical CLO holds loans to 150–250 borrowers.

administers payments, facilitates communication with the borrower, and, if necessary, prosecutes litigation. Larger questions are left to the lenders themselves. The key construct is the "required lenders." This is a fraction of lenders—usually a bare majority by amount of outstanding loan held—that can direct the agent bank in its application of discretion and, crucial for our purposes, consent to most types of amendment of the loan contract. The majority's power to amend is invariably qualified by a list of matters, known colloquially as the lenders' "sacred rights," that can be altered only with the unanimous consent of (affected) lenders.

Loan terms are set through an iterative origination process centered on consultation between the borrower (or, when the loan is being made to finance a LBO, the buyout sponsor) and an arranging bank or group of banks. The arranger is akin to a bond underwriter, and the same banks, including J.P. Morgan Chase, Bank of America, and Goldman Sachs dominate both businesses. The arranger advises the borrower with respect to market conditions and intermediates the flow of capital from loan investors to the borrower. After the arranger draws up a term sheet, bookrunners market the loan to prospective buyers, who have a brief window—as little as a few days—to indicate interest. If, as is common, the arranger's retention deal with the borrower allows it to "flex" the terms, the arranger may adjust the spread that the loan pays over the benchmark rate or the discount at which the loan is to be sold to ensure adequate demand when the transaction closes (Bruche, Malherbe, and Meisenzahl, 2020).

An active secondary market is an important ingredient in the institutional term loan's viability. The principal primary-market investors have a business model predicated on the manager's ability to deploy and redeploy capital as individual loans mature or are refinanced and

as macroeconomic conditions change (Kundu, 2023).[8] Compared to, say, the public equity market, however, trading is opaque and illiquid. The market is mediated by a small number of dealers (the same desks that trade high-yield bonds) who hold loans as inventory and can match buyers and sellers of relatively illiquid loans. Dealers provide daily, indicative bid/ask quotes, which investors use to mark their holdings; but, unlike with bonds, dealers do not report trades, and consequently analysts cannot observe price or volume directly (Nini and Smith, 2023).

### *2.2 The Uptier Transaction*

In recent years, distressed borrowers looking to forestall bankruptcy have begun to challenge the first-priority status of institutional term loans. When a company with negative cash flows and fully encumbered assets faces a liquidity crunch, raising additional funds is difficult. Debt overhang makes anything but a senior claim on the business unattractive to investors, while debt covenants and liens together preclude the company's selling anything but junior claims. Chapter 11 offers a conventional way out of the pickle, because it allows debtors, subject to statutory standards and judicial oversight, to issue senior claims irrespective of liens or contractual commitments not to do so (Triantis, 1993; Ayotte and Skeel, 2013). But bankruptcy means canceling the interests of out-of-the-money equity investors, and so is, from their perspective, an unattractive solution (Buccola, 2023a).

In the late-2010s, clever financial advisers and lawyers developed two transactional forms that would allow distressed borrowers to create "super-senior" debt without invoking Chapter 11. One came to be known as a dropdown transaction. Made (in)famous by J. Crew, the

[8] CLO constitutive documents typically limit the portfolio's exposure to especially risky loans, or at least penalize the manager for excessive risk exposure, so that the ability to trade out of loans, especially those deteriorating in price, is a key piece of the system. Indeed, as loans lose value, they tend to concentrate in the hands of hedge funds and other asset managers who specialize in distress (Kundu, 2023).

dropdown exploits a provision in many loan contracts that causes liens to be released with respect to collateral that is validly transferred to an "unrestricted" subsidiary. Once an asset is unencumbered, the borrower—more precisely the subsidiary that has taken title to the erstwhile collateral—can re-pledge the asset to support new debt that is senior to the preexisting, ostensibly first-lien loan. Where feasible, a dropdown allows a borrower to raise new capital while extending the equity investors' implicit option. The transaction has two limitations, however: a dropdown is possible only if the loan permits the transfer of collateral to an unrestricted subsidiary, and even if it does, the amount of value a borrower can so transfer is inevitably limited.

The non-pro rata uptier exchange, or "uptier," is in some respects a bolder transaction. Like a traditional workout, an uptier is predicated on lender consent and need not involve transferring collateral or releasing liens. Its distinctive feature is the way it pits members of a syndicate against one another (Baird, 2023; Buccola, 2023b; Dick, 2023). An uptier has two essential steps. First, a bare majority of lenders agree to amend the loan contract to permit the borrower to incur new, super-senior debt. Second, the borrower agrees with the participating lenders to swap their loans—but not the loans of lenders left out of the deal—into some of the new debt, the rest of which is used to raise new money. In the end, the company is left with a more hierarchically differentiated capital structure: the new-money lenders typically have a first priority; the participating lenders come second; and the excluded lenders are third, with pre-existing junior debt claims and equity interests behind them.

As this sketch should suggest, the permissibility of an uptier is a matter of contract. Uptier proponents rely, in particular, on an interaction between two sets of provisions common to but not ubiquitous in institutional term loans. One has to do with the ability of the required

lenders to sign away their own and their fellow lenders' place in the pecking order. Until 2020, the vast majority of institutional term loans—approximately 90 percent of the sample examined by Buccola and Nini (2022)—did not include the maintenance of lien and payment priority among lenders' enumerated, sacred rights and, in that sense, allowed a bare majority to subordinate the loan to newly created debt. The other crucial set of provisions has to do with the borrower's ability to swap the participating lenders' loans, but only their loans, into newly created debt. This ability cannot be assumed. The traditional rule in syndicates is that a lender could not assign its loan back to the borrower or the borrower's affiliate.[9] Almost all loan contracts have a provision saying as much. A bare majority of lenders can lift the prohibition, but doing so would often be practically ineffectual: ratable-sharing provisions, which exist in every loan and which usually cannot be altered without unanimous lender consent, are frequently written so that a lender who assigns its loan to the borrower (whether the contract is amended to permit such assignment or not) must share the proceeds with fellow lenders. Consequently, loans with a traditional anti-assignment rule and a certain kind of ratable-sharing clause are immune to an uptier. In the years following the financial crisis, many loans began to carve out exceptions to the general anti-assignment rule (Bellucci and McCluskey 2017, pp. 640–643). For our purposes, the key exception allows the assignment of loans back to the borrower or its affiliates on a non-pro rata basis through what are called "open market" repurchases. Most uptiers executed to date have been premised on the idea that the borrowers could use the open-market exception to offer a swap to a subset of favored lenders in exchange for their consent to the recapitalization.[10]

[9] An ability to put one's loans presumably was understood to threaten unduly the collective-action norm underlying traditional syndicates.

[10] Until the uptier was invented, the potential interaction between contract provisions governing loan subordination, on one hand, and assignments, on the other, seems to have escaped notice. Functionally the provisions serve ends that can be justified without recourse to one another. One is about a minority lender's right to insist on the seniority of its credit; the other, about the incentives of individual lenders to defect from a collective-

Judged a priori, the direction of an uptier's economic effects on various investor groups are clear in some instances but ambiguous in others: (1) The most junior investors, the equity holders, benefit unambiguously. At first approximation, equity represents a call option on the company's assets with a strike price equal to the face amount of outstanding debt. For the kinds of distressed companies that might consider an uptier, that option is, in effect, expiring (Casey, 2011). An uptier extends it. (2) The lenders who are left out of an uptier are the biggest losers, since they are on the other side of the expiring option and are subordinated in the pecking order relative to their former peers. (3) The participating lenders fare better than the left-behind but not necessarily better than in a counterfactual world without uptiers (because the transaction has a structurally coercive design). (4) Junior creditors such as second- or third-lien lenders or unsecured bondholders face an ambiguous result. The more junior they are, the more they resemble equity in benefiting from a delay of a realization event such as bankruptcy.

### *2.3 The Events of Summer 2020*

There was little precedent for an uptier before 2020. To be sure, distressed companies had long sought creditor permission to incur priming debt while threatening to subordinate non-consenting creditors (e.g. Donaldson et al. 2020). In a common form of bond workout, for example, the company seeks holders' consent to strip the indenture's negative-pledge clause and issue new, secured instruments into which the consenting holders will swap. Traditionally, though, the offer to exchange was made ratably (at least to holders whose participation would not destroy a securities-offering exemption). Whether market participants understood ratable treatment to be a legal requirement is debatable, but ratability was at least an entrenched

action norm. Historically the exception to the borrower repurchase prohibition emerged as a response to the great financial crisis, when the prohibition appeared to prevent borrowers (and especially sponsors) from curing macro-inspired illiquidity in the secondary market. It was not about facilitating workouts. In this sense, the uptier qualifies as what Ayotte and Badawi (2022) call a contractual "loophole."

convention in workouts. This convention was perhaps especially strong in the loan context. An aborted 2017 refinancing proposed by the specialty clothier, NYDJ, is as far as we know a lone exception to the pro rata norm in loan workouts. As Dick (2021) explains, the NYDJ transaction would have been the first uptier but was abandoned after minority lenders grumbled and a judge expressed skepticism.[11]

Then, in June 2020, in the midst of the Covid pandemic's first wave, Serta Simmons announced that it had reached a non-pro rata recapitalization deal with a subset of its lenders. Serta had two term loan facilities outstanding. With first- as well as second-lien loans trading at distressed prices, the company had sought restructuring proposals from ad hoc creditor groups. On June 8, it announced that it had entered an agreement with a majority of lenders in each facility that would see the company incur more than a billion dollars of incremental, super-priority debt: $200 million to be issued for new money and $875 million to swap for the consenting lenders' existing loans. Overnight the quoted price of the first-lien loan dropped by approximately 2000 basis points.

**[Insert Figure 1 here]**

The lenders who had been left out of the deal immediately sought an order from the commercial division of New York's supreme court blocking the transaction's closing. The contractual features outlined above are necessary but not—or at least in 2020 *were* not—obviously sufficient conditions to the uptier's validity. Two unsettled questions loomed large: first, whether a thoroughly negotiated exchange could qualify as an "open market" transaction;

[11] NYDJ could have revealed a latent weakness in credit documents; but perhaps because it was a relatively small credit—the term loan at issue was at the small end of the institutional market ($144 million)—and the transaction did not close, the affair did not capture public attention.

and, second, whether, assuming that the literal text of the loan contract allows it, an uptier is nevertheless inconsistent with borrowers' and majority lenders' implied duties of good faith and fair dealing. Less than two weeks later, however, the court denied the lenders' application on the ground that their claims were unlikely to succeed on the merits and allowed the transaction to close.[12] In plain English, the judge concluded that the uptier appeared to be legally valid.

Two more uptiers followed soon after, confirming that Serta was not unique in its willingness to pursue a non-pro rata refinancing. TriMark announced its deal on September 15. Boardriders executed a similarly structured transaction that was dated August 31 but that appears to have become public knowledge only a few weeks later, on October 9, when excluded lenders filed a lawsuit. Figure 1 shows that quoted loan prices for each company fell dramatically on the relevant event date.[13]

### *2.4 Reactions and Subsequent Events*

The loan world reacted volubly to the transactions. In each instance, subordinated lenders brought litigation against the respective borrowers and participating lenders. More generally, uptiers became the topic du jour for discussions among lenders and advisors. Industry events were convened specifically to discuss the uptier and other priming transactions. The Loan Syndication and Trading Association convened a series of webinars on the topic and devoted a significant part of its first live post-pandemic meeting to fallout from the transactions.

[12] North Star Debt Holdings, L.P. v. Serta Simmons Bedding, LLC, 2020 WL 3411267, No. 652243/2020 (N.Y. Sup. Ct. June 19, 2020). The justice, Andrea Masley, appears to have formed a different view of uptiers some 18 months later, in the Boardriders litigation. Other judges have expressed differing views. But for a year-and-a-half after Serta announced its transaction, Justice Masley's opinion was the only judicial guidance on legality.

[13] We take October 9 to be the relevant event date for Boardriders both because we cannot find any record of the transaction dated between August 31 and October 8 and *because* the loan price shows a reaction only on the 9th. Since we are interested in the impact of widely disseminated information about the events, rather than the events themselves, on the prices of other companies' loans, the date on which investors react to the transaction is the relevant date.

Loan contracts also began to change. Buccola and Nini (2022) show that new loans increasingly included provisions that would prevent uptiers going forward. In the sample of loans they study, the frequency of immune originations hovered between 30-40 percent between 2016 and June 2020, and then rose sharply to nearly 90 percent by mid-2022. Before Serta, only approximately 10 percent of contracts required unanimous lender consent to subordinate lien and payment priorities; by mid-2022, nearly 70 percent did.

**[Insert Figure 2 here]**

For a time after Summer 2020, the uptier seemed to disappear. Easy monetary policy and fiscal support from Congress meant that there was little restructuring action in late-2020 and most of 2021. In 2022, however, a large stock of susceptible loans remained, and uptiers reemerged: Envision Healthcare, Diamond Sports Group, Mitel, West Marine, Yak Mat, Rodan + Fields, Robertshaw, and LifeScan are among the later cohort of companies to pursue an uptier.

## 3 Hypotheses and Research Design

Our basic hypothesis is that the prices of loans susceptible to an uptier would experience a negative response to news indicating that the transaction is more likely to happen. We use the three transactions during 2020 as events that cause lenders to update the probability of an uptier, as each transaction reveals or confirms the uptier's legal or commercial viability.

### *3.1 Event Study*

We conduct an event study using secondary-market loan prices around the dates when news was released related to the uptier transactions in 2020. We use weekly data on loan prices and to construct the percentage change in the loan price for each loan $i$ during week $t$, which we

denote as $R_{it}$ and refer to as the price return..[14] Since we are interested in the difference in returns between immune and susceptible loans, we compare the evolution of loan prices across the groups using the following model

$$R_{it} = \alpha_i + \delta_t + \beta I_i^{sus} I_t^{event} + \varepsilon_{it} \quad (1)$$

The variable $\alpha_i$ is a loan-specific fixed effect that captures the average return of each loan, and $\delta_t$ is a weekly fixed effect that captures market-wide changes in loan prices during week $t$. We estimate these parameters using the panel of loan returns. The variable $I_t^{event}$ is an indicator that week $t$ is in the event window, and the indicator variable $I_i^{sus}$ denotes a loan that is susceptible, as opposed to immune, to an uptier. The coefficient $\beta$ is an estimate the difference in returns for susceptible loans relative to immune loans during the event period.

We define the event windows in three alternative ways. In our baseline approach, we identify event weeks as those when initial significant news was released regarding the three transactions. For Serta, we use the week of the initial announcement, which was Monday June 8, and the two subsequent weeks. The two weeks after the initial announcement included court hearings that culminated in a decision tentatively endorsing the uptier's legality. The three-week period ends on Friday June 26. For TriMark and Boardriders, we set the event dates as the weeks of the announcement (September 15) and court case (October 9), respectively.

[14] We construct weekly returns using the last trading day of each week, which typically means returns are from Friday to Friday. The price return differs from total return because it omits the floating rate, typically LIBOR, and the contractual margin on the loan. The difference is negligible for our purpose, however, which is confined to assessing market perceptions over a handful of weeks, and consequently we use the measure that is easier to interpret.

Ederington, Guan, and Zongfei (2015) show that using standardized returns can help increase the power of standard statistical tests, so we modify the dependent variable in (1) to be $\frac{R_{it}}{\sigma_i}$, where $\sigma_i$ is the sample standard deviation of returns for loan $i$.

### *3.2 Null and Alternative Hypotheses*

This framework allows us to test the null hypothesis that $\beta = 0$ against the alternatives that $\beta \neq 0$ and $\beta < 0$. A negative coefficient is what we would expect if investors mark down the prices of susceptible loans relative to immune loans. In this section we provide some informed speculation about the expected size of the effect, which we believe is in the range of 10 to 25 basis points in total across the event weeks.

We start with the proposition that effect size is a product of the expected likelihood that a given borrower's ability to do an uptier will affect the terms of a restructuring and the expected effect on loan values if it does. A borrower's ability to do an uptier can impact loan values either directly through a realized uptier or indirectly by altering the terms of a negotiated, "consensual" restructuring.

Consider first the expected effect on loan values if an uptier occurs. We can use the experience of lenders in the Serta, TriMark, and Boardriders transactions to ballpark the impact. Panel A of Figure 1 captures the observation that the average effect on excluded lenders is a loss in value of approximately 2500 basis points. In expectation, of course, a lender is slightly more likely to participate in, than to be excluded from, an uptier. The structurally coercive nature of an uptier implies that, as a matter of theory, participating lenders need not fare better than in the status quo ante. As a matter of practice, though, participating lenders seem to improve their positions. If investors anticipated that participating lenders on average would fare comparably to

the participating lenders in Serta, TriMark, and Boardriders, then we calculate the net expected loss to lenders would come to approximately 500 basis points.

Consummated uptiers are uncommon. Through 2022, about a dozen uptiers have been executed, which is roughly 1 percent of the loans outstanding in 2020. To the extent that investors in 2020 expected the realized future, a naïve calculation is that the expected loss attributable to realized uptiers would be $500 \cdot .01 = 5$ basis points. Of course, the widely divergent outcomes associated with an uptier are themselves costly. A selling point of senior secured loans is their low volatility. Presumably the expected effect of a realized uptier is not the whole story. But the infrequency of consummated transactions suggests a direct effect of anticipated uptiers in the neighborhood of 5–10 basis points.

Because investors bargain in the shadow of contractual rights, the indirect effects of loan susceptibility may be as or more important than the direct effects. Indeed, we have heard from restructuring professionals that the terms of renegotiations do in fact depend on the borrower's ability to play hardball. For example, one anecdote is that borrowers with the contractual ability to subordinate holdout lenders can often bargain to an amend-and-extend transaction without equity investors having to contribute new capital, whereas an incremental equity contribution is typical for borrowers without a similar threat. It is challenging to quantify the potential impact of increased bargaining power, but presumably it is smaller than the 500 basis points impact of an actual uptier but happens more frequently. As a very rough approximation, we suppose that 5 percent of loans trading less than $90 conduct a restructuring and conjecture that the uptiering threat leads to an additional loss to lenders in the range of 100–300 basis points. This comes to an expected loss to lenders of 5–15 basis points. Adding together the direct and indirect effects yields a range of 10–25 basis points.

## 4 Data

### *4.1 Loan Pricing Data*

We examine secondary-market prices of loans using data provided by IHS Markit. Markit provides daily bid and offer prices for several thousand loan facilities and reference data on several dozen characteristics of each facility, including details of the borrower and features of the underlying loan. The pricing data is provided by market participants who actively trade loans and is used by a wide range of investors in the loan market. Since the data is price quotes and loans trade only infrequently (Keßler and Mählmann 2022), we create a weekly time series of prices using the last day of each week.

### *4.2 Loan Sample*

We begin by creating a sample of loans that have secondary-market prices during 2020. Among all U.S.-dollar loans in the Markit data, we choose only first-lien loans marketed to institutional investors. Although second- and other junior-lien loans can be, and have been (in Serta and Mitel), made subject to an uptier, the economic significance of subordination is less clear-cut for loans that are not first-lien. To the extent that second- or junior-lien lenders are out of the money, they resemble equity investors and thus may benefit, not suffer, from an uptier. We focus on institutional loans because institutional (unlike bank) syndicates are typically numerous and fragmented enough for borrowers to plausibly exploit collective-action difficulties among the lenders.

Limiting the sample to loans with at least 30 weeks of price data beginning well before the Serta uptier (2020 week 9) yields roughly 2,000 loans. Among these, we zoom in on loans with an offer price that was below $90 as of 2020 week 12, when the COVID pandemic caused loan prices to fall sharply. We limit the sample to stressed loans because there is no economic

reason for a borrower to uptier a loan outside of distress, so low-risk loans are not commercially susceptible even if they are legally susceptible.[15] Figure 2 shows that Serta, TriMark, and Boardriders loans were all quoted below $80 when they were subordinated. Restricting the sample in this way removes about 30% of the loans, leaving us with 1,384 loans to 1,130 unique borrowers. Borrowers sometimes have multiple first-lien term loan facilities that differ in spread and maturity but are otherwise governed by the same contract. Because the choice to issue one or multiple facilities is for our purposes arbitrary, we exclude all but the largest loan for any single borrower and thereby avoid mechanical correlation.

Markit does not provide information on the contract features necessary to know whether a loan is susceptible to an uptier or not. We therefore source the loan agreements themselves from EDGAR. Loan contracts are generally private documents, but most qualify as "material agreements" that SEC-reporting borrowers must file as an exhibit to an 8-K or 10-K). We find a contract for 282 unique borrowers.

**[Insert Table 1 here]**

The loans for which we find a contract differ in predictable ways, given our reliance on EDGAR, from loans for which we do not find a contract. Table 1 reports summary statistics for five loan terms split by whether we find a contract or not. Since SEC-reporting borrowers tend to be bigger and safer than non-reporting firms, Table 1 reports that the loans for which we find contracts are, on average, larger in size, have a lower interest rate, and are more highly rated at origination.[16] The most striking difference is the frequency with which borrowers are owned by a

[15] Loans are quoted at a percentage of face value. Since the coupon on a leveraged loan floats with a benchmark rate (Libor or SOFR), loan prices (unlike bond prices) are largely insensitive to economy-wide changes in interest rates. A loan's price is thus a good indicator of borrower's financial condition.

[16] Data on loan amount, maturity, and spread is missing for some observations.

private equity sponsor. Over 70 percent of the loans for which we do not find a contract are sponsor owned, whereas the sponsor-owned share of loans for which we find a contract is only about 25 percent.

### *4.3 Coding for Susceptibility*

Following the methodology outlined by Buccola and Nini (2022), we read and code each contract for susceptibility to an uptier. Specifically, we code a loan as susceptible if the contract *both* (1) allows the lenders' lien or payment rights to be subordinated without the consent of at least all "affected lenders" *and* (2) (a) expressly allows the borrower to repurchase an unlimited amount of loans on a non-pro rata basis or (b) can be amended by a bare majority of lenders to allow non-pro rata repurchases (and, where relevant, to insulate the proceeds of any such repurchases from lenders' ratable-sharing obligation). If a contract prohibits either subordination or non-pro rata repurchases, we code the loan as immune to an uptier.

**[Insert Table 2 here]**

Approximately 70% (199 out of 282) of the loans we analyze were susceptible to an uptier during the summer of 2020. Susceptible and immune loans are similar along the dimensions we can observe. As Table 2 reports, the distributions of loan size, maturity, loan spreads and credit ratings are quite similar across the groups. One notable difference is that susceptible loans are significantly more likely to be made to sponsor-owned borrowers, which Buccola and Nini (2022) show is due to sponsored loans more often permitting the borrower to make "open market" loan repurchases.[17] We infer from the similarity of the groups that evolution

[17] Immune loans have slightly higher loan spreads, on average, but the difference is not statistically significant and reflects the lower fraction of BBB ratings and higher fraction of unrated borrowers.

of loan prices should be similar and attribute differences to the difference in susceptibility to an uptier.

## 5 Results

We begin by exploring the evolution of loan prices for the two sets of loans during the weeks surrounding the three uptiering transactions. Figure 3 plots the cumulative change in the average loan prices for susceptible and immune loans relative to their prices on January 1, 2020. After falling sharply with the onset of the COVID pandemic, loan prices rebounded sharply during the second and third quarters of 2020. By the end of 2020, the average loan price in each portfolio increased by over 10 percent relative to the price in April. The grey bars denote the event weeks in our baseline estimation of (1): the three weeks following the Serta transaction and the single weeks of the TriMark and Boardriders transactions. Around the events, there is little visual evidence of a gap happening during the event periods. We formally test whether the changes in loan prices are significantly different using the event study outlined in Section 3.1.

**[Insert Figure 3 here]**

### *5.1 Event Study Results*

Table 3 reports the results from estimating equation (1) for two different dependent variables. In the left side of the table, the dependent variable is the weekly change in the price of the loan, measured in basis points. In the right side, the dependent variable is the change in the price of the loan divided by the loan's sample standard deviation. The table reports estimates of $\beta$ in the columns labeled "Coefficient," along with the estimated standard error of the estimate. The $\beta$ coefficient can be interpreted as the average number of basis points that susceptible loans underperform immune loans during a single event week. We estimate the cumulative abnormal

return over all the event weeks in the column labeled "CAR," which we generate as the coefficient estimate multiplied by the number of weeks in the event period. The last two columns report p-values for the tests of the hypothesis that the coefficient (and CAR) are equal to zero. In the two-sided test, the alternative hypothesis is that the coefficient is not equal to zero, and in the one-sided test, the alternative hypothesis is that the coefficient is less than zero. We offer a one-sided test because our hypothesis is that susceptible loans *under*perform immune loans. The table reports the coefficient estimates and associated statistics for three different event windows. For the baseline estimation, we use an event window of five weeks comprising the three weeks following the Serta transaction and the individual weeks of the TriMark and Boardriders announcements. For the alternative estimations, we use event windows of three weeks (the week of each announcement) and six weeks (the week of each announcement plus the following week), respectively.

**[Insert Table 3 here]**

The top portion of Table 3 shows that susceptible loans underperformed immune loans during the five-week event window. On average across the weeks, the change in the prices of susceptible loans was about 19.8 basis points lower than the percentage change in the prices of immune loans. Aggregating across the events weeks yields an estimate of nearly 100 basis points. However, the statistical precision of the estimates is fairly low, so we cannot reject the hypothesis that the difference in price changes is statistically significant at conventional levels. The statistical precision improves when using standardized returns. The p-value for a one-sided test is 4.7%, which provides some evidence that the difference is less than zero. In our sample, the average weekly standard deviation of price changes is 270 basis points, so the estimate of -0.07 implies an average weekly underperformance of 18.9 basis points. Across the five event

weeks, this sums to 94.5 basis points, similar to the point estimate using the Price Return. We can confirm that using standardized returns improves the statistical precision of the estimates, and we find marginally statistically significant results for our baseline event window.

The bottom two panels show that the estimates are quite sensitive to the definition of the event window. Using only the weeks of the transaction announcements, the size of the estimated effect falls by about 75 percent to about 25 basis points across the event window, and the effect falls even more using the two weeks following each transaction. Removing the third week following the Serta transaction is the important difference with the baseline; immune loans outperform susceptible loans by about 50 basis points during that particular week. Given the imprecision of the estimates, none of the smaller coefficient estimates for the alternative windows are close to statistically significant, even for one-sided tests using standardized returns.

Given the substantial statistical uncertainty and sensitivity to the choice of the event window, we are reluctant to draw the conclusion that the prices of susceptible loans fell relative to those of immune loans. Although the one-sided test using standardized returns produces a p-value less than 5%, the conclusion appears quite fragile. Using alternative, yet reasonable, event windows, the evidence that susceptible loans traded down relative to immune loans becomes markedly weaker. In none of the alternative specifications do we find a p-value near 5%, leading us to conclude that there is no evidence to reject the null hypothesis that the returns of susceptible loans were lower than those of immune loans.

### *5.2 Heterogeneity*

Given the lack of evidence that susceptible loans generally underperform immune loans, we zoom in on two subsets of loans that seem to face the greatest risk of an uptier. First, it is a realized fact that sponsor-owned borrowers have accounted for every uptier to date (for a table

through 2022, see Buccola 2023a). Although investors in 2020 cannot be expected to have anticipated such a strong correlation, we hypothesize that investors may have updated more with respect to loans to sponsor-owned borrowers. Second, since the probability of an uptier increases with financial distress, we hypothesize that investors may have updated more with respect to lower-priced loans.

We modify the regression in (1) to include an additional interaction:

$$R_{it} = \alpha_i + \delta_t + \beta_1 I_i^{sus} I_t^{event} + \beta_2 I_i^{group} I_t^{event} + \beta_3 I_i^{group} I_i^{sus} I_t^{event} + \varepsilon_{it} \quad (2)$$

where $I_i^{group}$ is an indicator variable denoting that loan $i$ is either sponsored or had a price below 80 before the Serta transaction.[18] In this specification, the triple interaction $I_i^{group} I_i^{sus} I_t^{event}$ indicates that the return is during the event window for a loan that is a member of our target group and susceptible to being uptiered. We include the double interaction $I_i^{group} I_t^{event}$ to control for any general differences in sponsored or low-priced loans during the event window. The coefficient $\beta_1$ continues to capture the difference in returns for susceptible loans relative to immune loans during the event period, and $\beta_3$ provides an estimate of the difference in the effect for loans that are member of the target group. For this exercise, we use the 5-week event window that includes the three weeks of the transaction announcements plus the two weeks following the Serta transaction.

**[Insert Table 4 here]**

[18] The price cutoff is to some extent arbitrary. Multiple market participants have told us, however, that 80 marks a discontinuity in investor base. The terms of many CLOs penalize the manager for holding loans marked below 80. At the same time, a loan priced below 80 implies a potential yield high enough to attract distress-focused investors.

The results are provided in Table 4, which shows the coefficient estimates for $\beta_1$ and $\beta_3$ along with 1-sided and 2-sided p-values for the test that the coefficient is equal to zero. The weekly price return and the standardized return are again the dependent variables. The top part of the table indicates that separately analyzing sponsored loans has very little effect on our estimate of relative performance. The point estimate changes modestly, from -19.8 to -23.0, and neither is statistically different from zero. The small change reflects the fact that sponsored susceptible loans performed very similarly to other susceptible loans during the event period; the coefficient estimate on the triple interaction is only +9.2. Splitting the sample by loan price produces a larger change in the point estimate. In the bottom portion of Table 4, the estimate of the underperformance of susceptible loans shrinks from -19.8 to -6.7. The triple interaction term, however, is -26.6, meaning that the fall in price of susceptible loans during the event window is concentrated in more distressed loans. However, the uncertainty of the estimate again prevents us from drawing any strong conclusions.

### *5.3 Statistical Power*

The relatively small ratio of the number of loans in our sample to the universe of all loans is a challenge to our ability to draw conclusions about the role of price discipline in contract term selection. The 282 contracts in our sample represent nearly if not precisely all of the publicly available contracts meeting our loan criteria. But that is only about one-quarter of the relevant loan contracts outstanding in 2020. Most market participants would have had access to at least some of non-public contracts. A concern with imputing to borrowers and investors the inherent uncertainty associated with our analysis is that, with the benefit of a larger sample, they might have been able to reach firmer conclusions about the price impact of uptier susceptibility.

To weigh the seriousness of this concern, we use our price data (which includes prices of non-public loans) to simulate loan returns under plausible alternative hypotheses of the true effect size of uptier susceptibility. This allows us to determine how often regression (1) produces a statistically significant estimate at various sample sizes.

We proceed in five steps. First, we draw a random sample of loans from our set of 1,130 loans. We experiment with sample sizes of 250 loans, 500 loans, and 1,000 loans. 250 is close to the sample size of public firms for which we can find a contract; 1,000 loans is near the universe of loans and so can be taken to represent a world in which investors have access to every non-public loan contract. Second, we choose a random set of 70 percent to be deemed "susceptible," with the remainder being classified as "immune" (the ratio being chosen to mimic our public-contract sample). Third, we randomly choose five dates during 2019 and 2020 to be considered event dates. We choose five dates to be consistent with our baseline approach that news became public primarily during the three weeks following the Serta announcement and the weeks of the TriMark and Boardriders announcements. Fourth, we impose an effect on the immune loans during the event weeks by increasing their price returns by either 10, 25, or 50 basis points. 10 and 25 basis points are close to our estimate of the true effect; 50 basis points is larger than we consider likely, so it provides a conservative estimate of the true statistical power. If the event study is unable to reliably detect an effect size of 50 basis points, it is very unlikely to detect a 10-25 basis point effect size. In each case, we split the true effect size evenly across the event weeks. Finally, we run the regression in (1) and record whether the estimated coefficient is different from zero at a 5% significance level. We repeat the procedure 100 times and estimate the power of the test as the fraction of the 100 simulations for which we find a statistically significant coefficient.

**[Insert Table 5 here]**

Table 5 reports the results using standardized returns as the dependent variable in (1). The top part of the table reports estimates of statistical power of a standard two-sided test. The bottom part reports estimates of a one-sided test. In all cases, the estimates of power are quite low. For a sample size of 250 loans, the power of a two-sided test is only slightly larger than the 5% significance level. Even with a true effect size of 50 bps, the power of a one-sided test is only 13%, meaning that an insignificant result is the most common outcome even if there is a large true effect. The power of the test increases with a larger sample size but still remains below 50% in all cases. With a sample size of 1,000 loans and a true effect of 50 basis points, the power estimate increased to 39%, but for a more reasonable alternative of 25 bps, power is below 20%. The variability of loan returns just makes it quite challenging to detect small changes in loan returns.

## 6 Discussion

Our results bear on two kinds of questions. One question relates to the loan secondary market's informational efficiency (Borowicz, 2021; cf. Awrey, 2016; Gilson and Kraakman, 1984). On this matter our study is inconclusive. At least three possibilities could explain our null result, and they point in different directions with respect to the market's price sensitivity to contractual variation. One possibility is that the difficulty investors faced in assessing the implications of uptier susceptibility caused prices not to reflect fundamental value. The intuition is that loan analysts use low-dimension models to compute value, at least until a restructuring appears to be in the cards. Analysts cover lots of loans. It is not worth their time to estimate the expected value of each of the hundreds of dimensions a contract has, and as a consequence contract features with limited and perhaps contingent relevance to value drop out of pricing

models altogether. This explanation would be consistent with Murfin and Pratt (2019), who find evidence that loan comparability, defined by similarity of just a few rough variables (industry, loan-type, tenor, and S&P and Moody's ratings), determines pricing to a large extent. A corollary is that many other features of loan contracts—for argument's sake, let's say any feature worth 25 basis points or less—are underweighted or ignored.

A second, related possibility is that learning costs caused prices not to reflect fundamental value attributable to uptier susceptibility. The intuition is that investors can and do price the expected effect even of remote terms but that the costs of discovering what exactly a given loan contract *means* can exceed the anticipated trading benefits of reading. Loan contracts consist of hundreds of pages of dense prose, and the legal mechanics of uptiers, in particular, are not self-evident. The leveraged finance industry supports multiple businesses whose product offerings include the distillation of contracts into plain English. In that sense, market practice reinforces the common-sense idea that parsing legal documents requires both domain-specific knowledge and time. Given the positive costs of comprehension, some contract terms with economic significance must not be worth worrying about.

After the Summer 2020 uptiers, Reorg Research and Covenant Review, competitors whose clients include many leveraged loan investors, began on an ad hoc basis to publish plain-English summaries of companies' abilities to undertake "liability management" transactions such as an uptier. In an effort to assess the importance of learning costs, we looked for price impact upon the publication of these reports. There were not enough reports to make a systematic study—we identified only 11 reports suitable for our purposes—but to the extent that prices move upon publication, it could indicate that the costs of learning about contractual meaning impose an important limit on the price-discipline mechanism. In nine instances, we saw no price

movement. In two instances, loan prices moved after publication of a report, but both reports included confounding information about the company, and prices moved in the "wrong" direction. That the publication of research could move markets at all is interesting. But the exercise does not allow us draw even a tentative conclusion about the cost of learning about uptier susceptibility.

The third possibility is that loan prices in fact adjusted to reflect fundamental value but that our statistical methods lack the power that needed to detect the reality. Any plausible effect size that could be attributed to uptier susceptibility is well within our confidence intervals. Thus, we cannot rule out the possibility that the loan secondary market incorporates information about contract terms in an extremely efficient manner. Ultimately, then, our results fail to teach anything useful about the loan market's informational efficiency.

Our results do, however, have implications for questions about contract term selection in the loan origination. The crux of price discipline is epistemological. Contract proponents have reason to select a surplus-maximizing term only to the extent that they believe it will be priced. Whatever the cause of our results, they indicate a weak empirical basis for the specific belief that loan investors priced contractual susceptibility after the revelation of the uptier's viability and an even weaker basis for any given borrower to conclude that the incremental yield it would have to pay for the option to uptier exceeded the option's benefit to equity investors.

The sources of belief are manifold, to be sure, and our results do not directly show anything about what borrowers in the institutional loan market believed in fact. It is possible that borrowers had strong priors that prices would be sensitive to uptier susceptibility. Absence of evidence to that effect would not necessarily cause rational borrowers to change their minds. At

the same time, we are unaware of other research finding price effects for loan terms of similarly contingent significance and economic magnitude.

We thus interpret our results to suggest that models of term selection emphasizing non-price intermediation have application even with respect to terms having substantial implications for a financial contract's fundamental value. One branch of empirical scholarship in recent years has explored lawyers' and other intermediaries' role in the production of contractual text. An open question in that line of research is the extent to which non-price intermediation matters for the selection of terms that have inarguable economic significance. An implication of the price-discipline model is that they intermediaries should not help to select terms except perhaps insofar as they specialize in assessing the price consequences of various terms. That provisions determining uptier susceptibility changed without a clear price signal being available suggests that non-price intermediation, which one can think of as a kind of "talk" about term propriety, can in fact influence term selection on economically significant dimensions.

The sources of term selection in turn matter for predictions about, and normative assessments of, the outputs of financial contracting. To the extent that non-price intermediation can be understood as a necessary evil when market prices either are insensitive to term variation or are too noisy for contract proponents to glean useful information about the costs of a term, a rough conclusion is that the importance of lawyers is inversely related to the optimality of contract terms. One way this effect should be expected to manifest is in relation to term standardization. An attractive feature of price discipline is that it allows for term variation in the presence of heterogeneity. Sometimes the benefits of standardization will outweigh the benefits of tailoring even in a perfectly price-world (Kahan and Klausner, 1997). But even when the benefits of tailoring notionally exceed those of standardization, market participants' resort to

rough indicators of propriety may prevent the realization of those benefits. More standardization than an optimality ideal would predict should thus be expected.

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**Figure 1. The Impact of an Uptier Transaction on Outstanding Loan Prices**

Panel A plots weekly loan prices of outstanding loans to Serta, TriMark, and Boardriders for the 25 weeks around the uptier event, which happens in event week 0. Panel B plots the weekly price relative to the price 12 weeks prior to the uptier event.

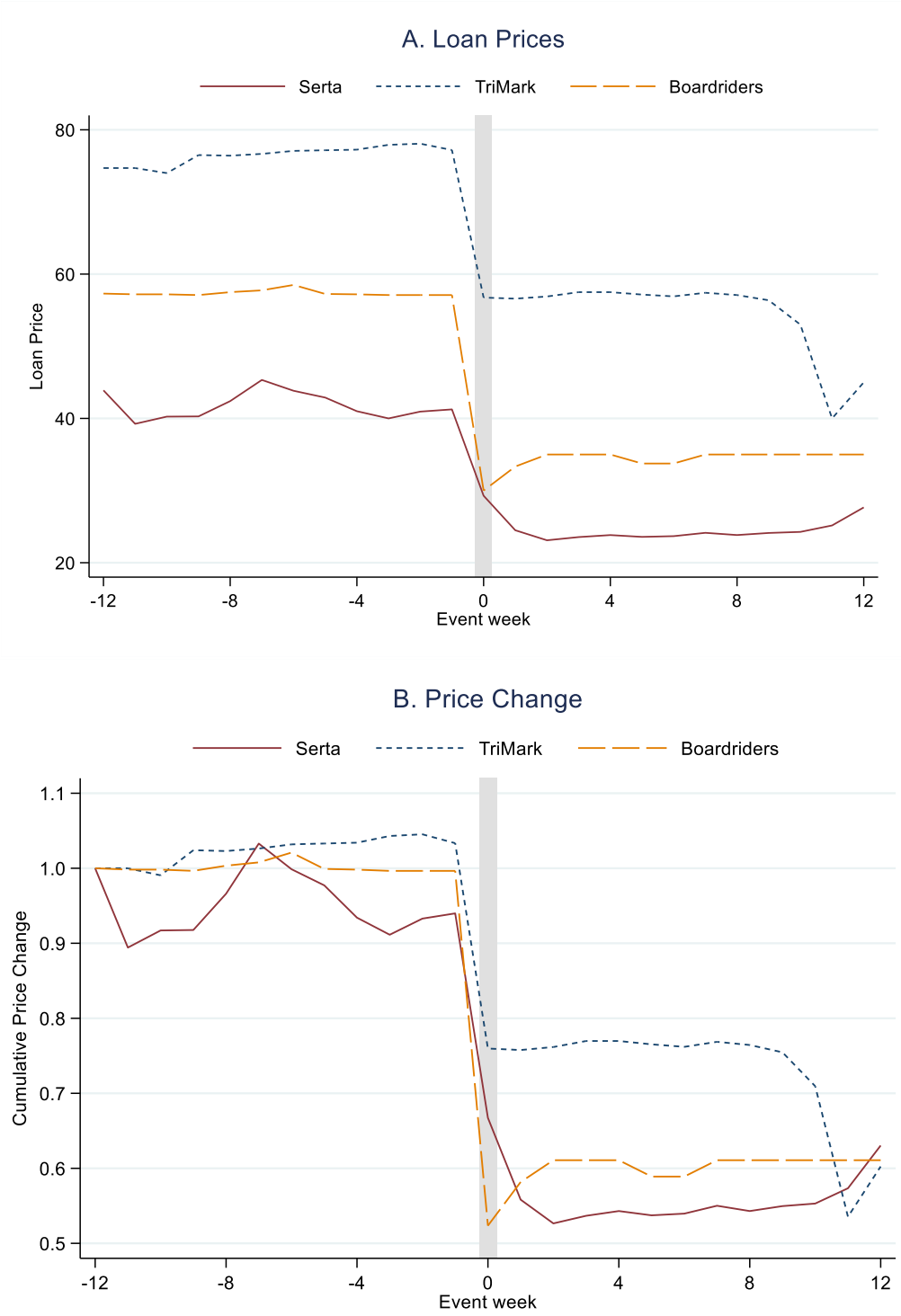


**Figure 2. The Frequency of Uptier Blockers**

The figure plots the frequency, in half-year intervals, of new contracts that block an uptier transaction using the data from Buccola and Nini (2022). The vertical capped lines are 95% confidence intervals. The vertical line at 2020h1 denotes the half-year during which the Serta uptier transaction happened.

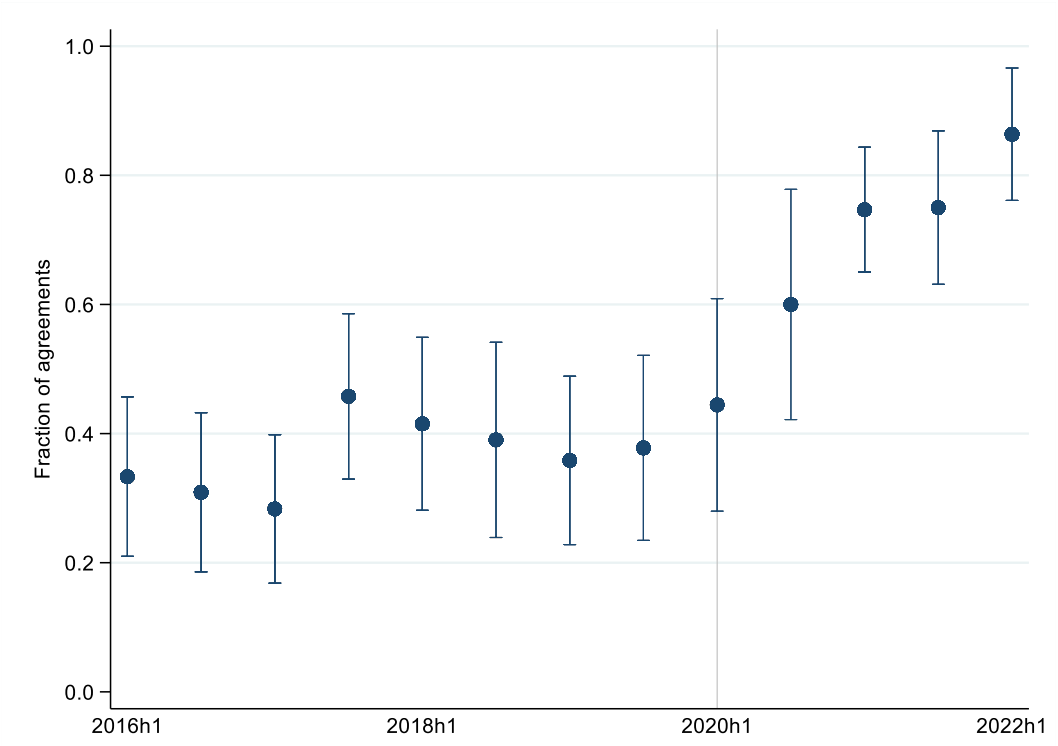
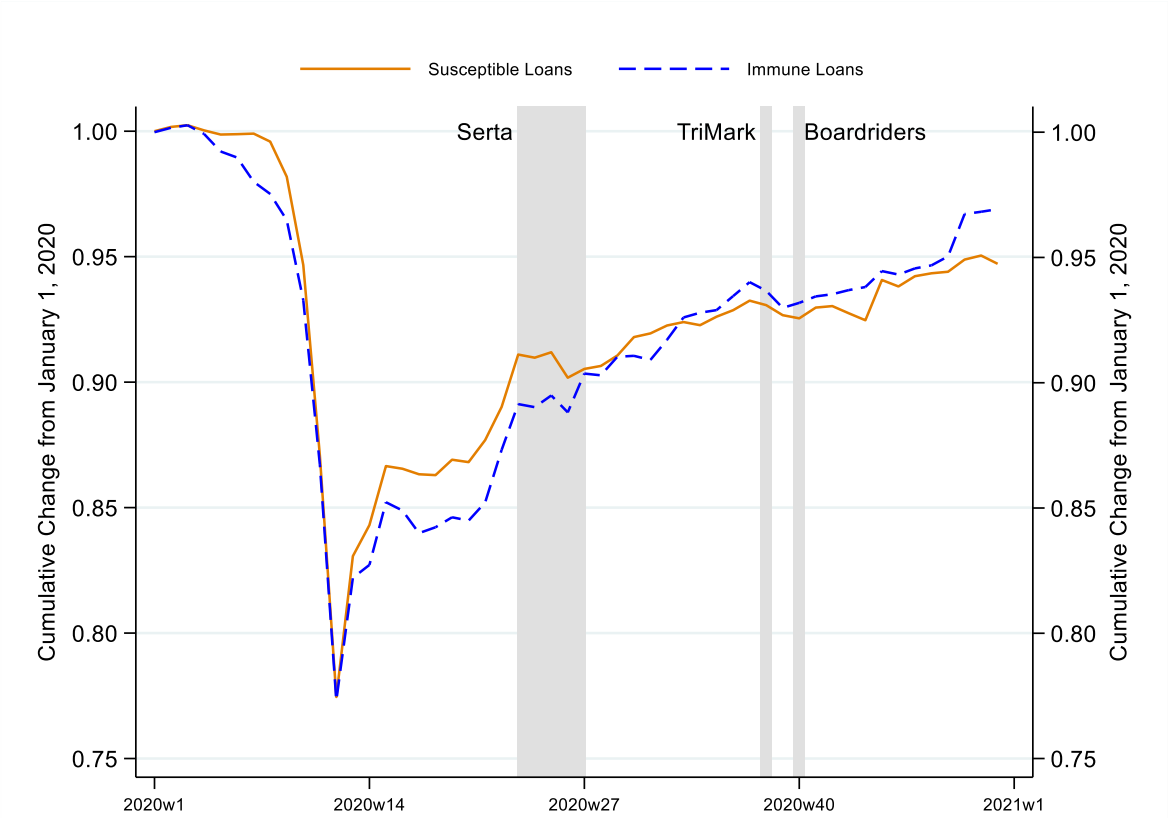


**Figure 3. The Evolution of Loan Prices during 2020**

The figure plots the cumulative change in the average loan price relative to the price on January 1, 2020. Susceptible loans are shown as the orange, solid line, and immune loans are the blue, dashed line. The grey bars denote the three weeks following the Serta transaction and the single weeks of the TriMark and Boardriders transactions.



## Table 1. The Sample of Loans

The table reports summary statistics for loans and credit ratings split by whether we find a contract or not. The sample includes one loan for 1,130 borrowers with sufficient loan pricing information during 2020. We find a loan contract in Edgar for 282 loans. Panel A reports summary statistics for the loan amount, maturity, spread, and fraction sponsored split by whether we find a contract or not. Panel B reports the fraction of loans with each credit rating.

A. Loan Characteristics

| | Mean | 25th Percentile | 50th Percentile | 75th Percentile | N |
|---|---|---|---|---|---|
| Loan amount ($ millions) | | | | | |
| Contract | 1030 | 400 | 750 | 1468 | 282 |
| No Contract | 761 | 322 | 510 | 913 | 848 |
| Maturity (years) | | | | | |
| Contract | 6.7 | 6.5 | 7.0 | 7.0 | 249 |
| No Contract | 6.8 | 7.0 | 7.0 | 7.0 | 748 |
| Loan Spread (bps) | | | | | |
| Contract | 326 | 225 | 300 | 375 | 282 |
| No Contract | 426 | 350 | 400 | 500 | 846 |
| Covenant-lite | | | | | |
| Contract | 70% | | | | 282 |
| No Contract | 68% | | | | 850 |
| Sponsored (%) | | | | | |
| Contract | 24% | | | | 282 |
| No Contract | 73% | | | | 850 |

B. Credit Ratings

| Rating | Contract | No Contract |
|---|---|---|
| BBB | 9.5% | 0.7% |
| BB | 31.8% | 11.8% |
| B | 29.8% | 59.6% |
| CCC | 0.0% | 0.2% |
| Unrated | 28.9% | 27.6% |

## Table 2. Susceptible and Immune Loans

The table reports summary statistics for loans and credit ratings split by whether a loan is susceptible to an uptier or not.

A. Loan Characteristics

| | Mean | 25th Percentile | 50th Percentile | 75th Percentile | N |
|---|---|---|---|---|---|
| Loan amount ($ millions) | | | | | |
| Susceptible | 1099 | 423 | 800 | 1500 | 199 |
| Immune | 1059 | 418 | 720 | 1500 | 83 |
| Maturity (years) | | | | | |
| Susceptible | 6.6 | 6.0 | 7.0 | 7.0 | 179 |
| Immune | 6.6 | 6.0 | 7.0 | 7.0 | 66 |
| Loan Spread (bps) | | | | | |
| Susceptible | 312 | 213 | 275 | 350 | 199 |
| Immune | 345 | 200 | 300 | 450 | 83 |
| Covenant-lite | | | | | |
| Susceptible | 76% | | | | 199 |
| Immune | 60% | | | | 83 |
| Sponsored (%) | | | | | |
| Susceptible | 30% | | | | 199 |
| Immune | 11% | | | | 83 |

B. Credit Ratings

| Rating | Susceptible | Immune |
|---|---|---|
| BBB | 11.6% | 7.2% |
| BB | 32.7% | 30.1% |
| B | 29.6% | 24.1% |
| Unrated | 26.1% | 38.6% |

## Table 3. Event Study Results

The table reports results from the regression $R_{it} = \alpha_i + \delta_t + \beta I_i^{sus} I_t^{event} + \varepsilon_{it}$ where $R_{i,t}$ is the weekly price return or the standardized return on a loan. The price return is the percentage change in the price, measured in basis points, and the standardized return is the price return divided by the sample standard deviation of the loan's returns. $\alpha_i$ is a loan-specific fixed effect, $\delta_t$ is a weekly fixed effect, $I_t^{event}$ is an indicator that week $t$ is in the event window, and $I_i^{sus}$ denotes a loan that is susceptible, as opposed to immune, to an uptier. The column Coefficient reports the estimate of $\beta$, and the column CAR is the coefficient estimate multiplied by the number of weeks in the event period. Standard errors of the estimates are reported in parentheses. The P-value columns report p-values for the tests of the hypothesis that the coefficient is equal to zero, compared with the alternative hypothesis that the coefficient is not equal to zero or less than zero.

| | Price Return (basis points) | | | | Standardized Price Return | | | |
|---|---|---|---|---|---|---|---|---|
| Event Window | Coefficient | CAR | P-value for 2-sided test | P-value for 1-sided test | Coefficient | CAR | P-value for 2-sided test | P-value for 1-sided test |
| 5 event weeks | -19.8 | -98.9 | 0.310 | 0.155 | -0.07 | -0.35 | 0.095 | 0.047 |
| | (19.5) | (97.4) | | | (0.04) | (0.22) | | |
| 3 event weeks | -8.4 | -25.3 | 0.734 | 0.365 | -0.03 | -0.09 | 0.578 | 0.289 |
| | (24.9) | (74.6) | | | (0.05) | (0.16) | | |
| 6 event weeks | -2.2 | -13.3 | 0.902 | 0.444 | -0.03 | -0.18 | 0.470 | 0.235 |
| | (17.9) | (107.4) | | | (0.04) | (0.23) | | |

## Table 4. Heterogeneity

The table reports results from the regression $R_{it} = \alpha_i + \delta_t + \beta_1 I_i^{sus} I_t^{event} + \beta_2 I_i^{group} I_t^{event} + \beta_3 I_i^{group} I_i^{sus} I_t^{event} + \varepsilon_{it}$, where $I_i^{group}$ is an indicator that loan $i$ is a member of a particular group, either a borrower owned by a private equity sponsor ("Sponsored") or a loan with a pre-event price less than 80 ("Price < 80"). Compared with the specification in Table 3, this specification includes the additional interaction terms $I_i^{group} I_t^{event}$ and $I_i^{group} I_i^{sus} I_t^{event}$. The table reports the coefficient estimates for $I_i^{sus} I_t^{event}$ ($\beta_1$) and $I_i^{group} I_i^{sus} I_t^{event}$ ($\beta_3$). All regressions use the 5-week event window.

| | Price Return (basis points) | | | | Standardized Price Return | | | |
|---|---|---|---|---|---|---|---|---|
| | Coefficient | CAR | P-value for 2-sided test | P-value for 1-sided test | Coefficient | CAR | P-value for 2-sided test | P-value for 1-sided test |
| Sponsored status | | | | | | | | |
| Event window | -23.0 | -114.9 | 0.286 | 0.143 | -0.1 | -0.45 | 0.063 | 0.032 |
| | (21.5) | (107.7) | | | (0.0) | (0.24) | | |
| Event window * Sponsored | 9.2 | 45.9 | 0.921 | 0.540 | 0.0 | -0.05 | 0.595 | 0.702 |
| | (49.7) | (248.6) | | | (0.1) | (0.55) | | |
| Distress status | | | | | | | | |
| Event window | -6.7 | -33.3 | 0.807 | 0.403 | 0.0 | -0.15 | 0.633 | 0.316 |
| | (27.3) | (136.6) | | | (0.1) | (0.30) | | |
| Event window * Price < 80 | -26.6 | -132.8 | 0.496 | 0.249 | -0.1 | -0.45 | 0.313 | 0.156 |
| | (39.0) | (194.8) | | | (0.1) | (0.43) | | |

## Table 5. Statistical Power

The table reports estimates of the statistical power of the test of the hypothesis that the coefficient $\beta$ is zero in the regression $R_{it} = \alpha_i + \delta_t + \beta I_i^{sus} I_t^{event} + \varepsilon_{it}$, at a significance level of 5%. The dependent variable is the standardized return. In the top part of the table, the alternative hypothesis is that $\beta \neq 0$, and in the bottom part of the table, the alternative hypothesis is $\beta \geq 0$. The table provides estimates for three different sample sizes and three different values for the true effect size.

| | True Effect Size | | |
|---|---|---|---|
| Sample Size | 10 bps | 25 bps | 50 bps |
| Two-sided test | | | |
| 250 | 0.05 | 0.03 | 0.08 |
| 500 | 0.07 | 0.07 | 0.13 |
| 1000 | 0.05 | 0.11 | 0.29 |
| | | | |
| One-sided test | | | |
| 250 | 0.08 | 0.07 | 0.13 |
| 500 | 0.11 | 0.08 | 0.20 |
| 1000 | 0.13 | 0.19 | 0.39 |