Columbia Readers: I look forward to the opportunity to share this paper with you. Please feel free to skip the technical material in Sections 3 and 5 if it is not to your taste. Sections 1, 2, and 6 should provide an overview suitable for discussion and Section 4 describes the underlying data for those interested.

Price Discipline for Non-Price Loan Terms

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Abstract

A standard model of capital markets contracting argues that issuers select non-price contract terms to optimize their value relative to the price they imagine investors will charge for it. To assess this model's ability to explain term selection in the syndicated loan market, we study the secondary market reaction to events—the appearance and proliferation of a new type of restructuring transaction known as an uptier—that spurred quick changes in the contract terms that parties negotiated in the primary market. We find only weak (and fragile) evidence of a significant price effect for loans traded on the secondary market. The imprecision of our results reveals a challenge for scholars of contracting who might rely on an event-study research design. The fact that market participants cannot obtain a reliable indication of "price" when there is a substantial shock to the understanding of a meaningful non-price term also casts doubt on the descriptive power of the price-discipline mechanism for all but the most important features of a loan contract.

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1 Introduction

Standard models 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 apply straightforwardly to capital markets, where the one-to-many nature of contracting rules out or at least curtails explicit dickering. Nevertheless, according to the bonding notion first developed by Jensen and Meckling (1976), a profit-maximizing company propounds optimal terms despite the absence of a bargaining framework because the proponent internalizes the costs and benefits of the terms it selects. If capital providers demand a return on investment commensurate with the risks they expect from investing, then issuers face a choice between offering self-constraining terms and paying a higher price for additional flexibility (Schwartz & Scott, 2003). In short, explicit negotiation is unnecessary because the price mechanism allows contract proponents to mimic a hypothetical Coasean bargain.

This logic 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 that sought to explain how corporate institutions ameliorate the problem of agency costs. (e.g., Winter, 1977; Easterbrook & Fischel, 1989). Its relevance to bond indentures and loan agreements, with their elaborate articulations of state-contingent control rights, is perhaps even more apparent (Aghion & 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 & Roberts, 2015; Smith & Warner, 1979). Bradley and Roberts (2015, p. 29), for example, 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."

How well price discipline explains real-world contracting practices is a separate question. The mechanism depends on investors changing their price requirement in response to variation in contract terms. Contract proponents will weigh a term's prospective cost against its value only if investors will in fact price it. While investors are highly sensitive to core terms such as interest rate, maturity, and the presence of collateral, their responsiveness to more obscure non-price terms is an open question. When it comes 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 the judgments of able but less-than-heroic individuals. Perhaps not surprisingly, evidence for a strong version of the model has proved elusive in some areas of corporate contracting that have attracted scholarly interest (see, e.g. Klausner, 2018).

Efforts to evaluate the bonding model empirically face several challenges. To determine if prices are sensitive to variation in a particular non-price term, one ideally would compare the yields on similar contracts that differ only with respect to the term of interest. But financial contracts have hundreds of unique terms, so holding all-else-equal is practically impossible. More problematic is that contract terms may interact in complicated ways to create thousands of analytically separable dimensions, making it difficult to isolate the sensitivity of yields to a single contract provision. Finally, 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.

This paper reports our effort to test the pertinence of the price-discipline mechanism to the leveraged loan market by using a shock that should circumvent some of these issues. We conduct an event study of loans trading in the secondary market after the appearance and proliferation, in Summer 2020, of a transaction known as the "non-pro rata uptier exchange," or "uptier." An uptier is a recapitalization transaction that transfers value from the subject company's senior lenders to its stockholders. In the three prominent transactions that occurred in Summer 2020, the secondary market prices of the existing loans fell by at least 25% upon the announcements of the transactions. We use the event study to measure how the secondary market prices of other loans adjusted to the newly apparent prospect of a future uptier. This design allows us to assess the degree to which prospective borrowers looking to issue new loans can glean price information that they would need to choose terms optimally as the bonding model posits.

Three features of the uptier and its history make the episode useful for testing the price-discipline model. First, lenders' susceptibility to an uptier is a function of contract terms that vary substantially across syndicated loans originated before Summer 2020. In the sample of loans we study, roughly 30% of contracts prohibited the transaction, allowing us to compare the reaction of susceptible loans with the reaction of loans immune to the transaction. Second, since the loans were issued before the uptier's legal logic and commercial practicability were revealed, the contract terms (and interactions between terms) that determine a particular loan's susceptibility are at most weakly correlated with borrower or lender characteristics. Contracts that permitted the transaction likely did so incidentally, so any price reaction unique to susceptible loans during Summer 2020 can be attributed to investors' updating their views of the significance of previously determined contract terms rather than underlying differences between borrowers or lenders. Third, contracting behavior changed rapidly after Summer 2020, with newly originated loans becoming much more likely to rule out an uptier (Buccola and Nini, 2024). A price-discipline model of term selection would explain this contractual change as a function of prospective borrowers anticipating that they would have to pay a higher yield for an option to pursue an uptier and concluding that the incremental cost would not be worthwhile. Evidence about whether, and how much, loan investors in fact "charged" for susceptibility of loans trading in the secondary market presumably bears on the plausibility of such beliefs and therefore of the bonding model's capacity to explain observed change in contract term selection.[4]

To implement this logic, we estimate abnormal returns to susceptible and immune loans after events that revealed the uptier's practical viability. Our study considers three events during Summer 2020: (1) the announcement by the bedding company, Serta Simmons, of its plan to execute an uptier, which revealed the transaction's commercial practicability as well as its 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 of two additional transactions by (2) TriMark and (3) Boardriders that helped normalize the transaction and should have increased the transaction's salience and the probability that distressed borrowers with susceptible loan contracts would execute an uptier in the future. We obtain the contracts underlying 282 institutional term loans that traded in the secondary market at distressed prices (bids quoted at 90 or below) in the months before the Serta transaction and read the underlying

[4] The connection between secondary market prices and primary market term selection is straightforward. Most buyers of non-distressed 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 in the secondary market conditional on the credit becoming 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.

credit agreement to determine susceptibility to an uptier.[5] We then compute the abnormal return on each loan around the event windows.[6]

We find only weak and fragile evidence of a relationship between susceptibility and abnormal returns. In our primary specification, the mean difference between the cumulative abnormal returns on susceptible and immune loans is large—nearly 100 basis points—but not statistically significantly less than zero at the 95 percent level in a one-sided test. The estimated confidence internals are wide, and the point estimate is sensitive to alternative, plausible specifications of the event windows. Even when restricting the sample to plausibly distinctive subsets of borrowers, such as sponsored or especially distressed borrowers, we do not observe significant price differences. Prices generally are too variable for us confidently to infer a relationship, much less a precise relationship, between a loan's susceptibility to an uptier and the yield investors demand.[7]

To confirm that the noisy character of our results is not just a property of the experience surrounding the uptier transactions, we simulate shocks of comparable magnitude to a wider set of loans during different time periods and ask whether a hypothetical market participant could reliably detect a price effect. We find that, given prevailing price volatility, a shock must change the price of "treated" loans substantially to generate results that are reliably significant. For example, in a two-sided test against the null of zero difference, a shock of 25 basis points to the price of 70 percent of 250 randomly selected loans produces a statistically significant (at the

[5] We believe that the sample comprises all publicly available contracts underlying loans that are quoted in secondary market dealer quotations.

[6] We use price change as a proxy for total return, but computing the return incorporating LIBOR and the contractual margin for each loan does not meaningfully affect any result.

[7] We also conducted an equity event study. The results were even noisier, and we do not report them.

five-percent level) difference in observed prices in only 6.6 percent of simulations. Statistical power increases with the size of the hypothetical effect and sample size, but even with a shock of 50 basis points and a sample of roughly 1000 loans, a one-sided test produces statistical power of only about 26%. The randomness in loan prices is simply too large to permit precise statistical inference in a sample of loans available to market participants. The power analysis thus suggests that analysts should struggle to identify a secondary market reaction to a change in contract terms unless the change produces a large effect on prices. Event studies based on secondary market data are not very useful for students of debt contract design. There's just not enough power.[8]

We also interpret our results to cast doubt on the price-discipline model's ability to explain term selection for all but the very most important terms. On this score, interpretation is more difficult, however, and requires some explanation, because two generic stories are consistent with our results. The simpler story is that secondary market prices did not diverge in Summer 2020 for reasons of complexity—either because loan investors use low-dimension pricing models (computational complexity) or because the costs of learning about a given loan's susceptibility outweighed anticipated trading gains (contractual complexity). The complexity story directly challenges the premise that loan investors are sensitive to less salient terms. If investors price only a small number of loan dimensions, then borrowers need not internalize the costs of many contract features. Ayotte and Bolton (2011) helpfully develop this line of thought

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[8] Choi et al. (2024) conduct an event study of bond prices to assess the impact of two events that made investors aware of obscure contract terms. Despite a very small sample, they find statistically significant effects. We attribute the difference to two unique features of their setting. First, they have secondary market prices for bonds from the same issuer that differ with respect to the examined contract term, which permits a very precise estimate of the counterfactual expected return and improves statistical precision relative to our setting that uses a broader cross-section of loans. Second, their estimated effect sizes are quite large—1.2% and 8.4%.

by showing that if it is costly for lenders to understand or value contract meaning, then, in a price-discipline model of term selection, supra-optimal borrower flexibility is to be expected in equilibrium. The changes that Buccola and Nini (2024) document are inconsistent with this logic, however, since terms evolved quickly to limit borrowers' ability to conduct an uptier transaction. If complexity prevented yields from diverging, then price discipline must not be the operative engine of term selection.

The alternative story is about statistical power. It could be that market prices in fact diverged but that the lack of power in our sample—and indeed in the universe of all loans—frustrates detection of the divergence with the level of confidence social scientists typically demand for truth claims. This story is not necessarily inconsistent with the notion that borrowers began to forswear the uptier because of concern about having to pay higher yields. Price discipline depends on the beliefs of prospective borrowers, not social scientists. Even before Summer 2020, borrowers might have believed that lenders are apt to price terms like uptier susceptibility at approximately their fundamental (dis)value. Depending on the strength of that belief, it could account for contractual change despite the weak statistical relationship between uptier susceptibility and secondary market prices. Indeed, as we show, a Bayesian prospective borrower having looked at our results would be modestly more confident than before looking that susceptibility would be priced. Although the event study would not persuade a social scientist that uptier susceptibility led to lower loan prices, it also would not dissuade a borrower from believing that the uptier option would require paying a higher yield.

Nevertheless, we see our results as a mark against the explanatory power of the price-discipline model for all but the very most important loan features. For the model to explain the selection of more obscure terms, contract proponents must believe, at a minimum, that there is some relationship between the term and the yield investors will demand. If analyses of the type we pursue were able to reveal such relationships for terms of similar salience and economic importance, then such a belief could make good sense. But if, as our results suggest, observable price data can generate only very noisy signals of a term-yield relationship, then the source of such supposed confidence is mysterious. More demandingly, for the price-discipline model to generate *efficient* contract terms, the expected incremental yield priced by investors should approximate the fundamental (dis)value of the term. Given our inability to reliably identify *any* relationship between a loan term and market prices, the empirical evidence seems too uncertain to generate the strength of belief on which the price-discipline mechanism depends.

What, then, accounts for the change in contract terms after Summer 2020? Or more generally for selection of non-price terms of similar importance and salience? Recent work in contracts scholarship emphasizes the role of intermediaries, including lawyers, trade associations, and news and data hubs, in determining a vast array of terms (e.g., Bartlett, 2023; Jennejohn, Nyarko & Talley, 2022; Nyarko, 2021; Scott, Choi & Gulati, 2020; Gulati & Kahan, 2018). Certainly, intermediaries were active in response to the Serta, Trimark, and Boardriders transactions. The Loan Syndication and Trading Association (LSTA) 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 show that the noisiness of market prices leaves space for these efforts to have a causal influence on subsequent contract language.

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 outlines key features of the loans we study and the markets in which they are originated and trade, sketches the legal mechanics and economic significance of uptiers, and 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 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 other asset managers. They are floating-rate instruments secured typically by liens on substantially all the borrower's assets. Most institutional term loans—approximately 90 percent (Nini & Smith, 2023)—are first lien, meaning that they are the most senior form of term debt in the borrower's capital structure.[9] 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 with high-yield bonds, the vast majority—of the debt financing of large, risky companies (Nini and Smith, 2023).

[9] Term loans are often contractually subordinated to revolving lines of credit even when both are "first lien." In the case of asset-based revolvers, the lenders have lien priority with respect to current assets (the borrowing base).

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, originating banks typically planned to hold corporate loans to maturity (Bord & Santos, 2012). A loan might be syndicated, but only to a handful of other banks or insurance companies (Roberts & Sufi, 2009), and the originating bank retained 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 & Rasmussen, 2006; Nini, Smith & Sufi, 2009, 2012; Roberts & 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.[10] The loans are designed to be held by many investors in increments too small to justify active monitoring. 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 & Smith, 2021; Ivashina & Vallée, 2022; Badawi, Dyreng, de Fontenay & Hills, 2023) and, like high-yield bonds, often eschew maintenance covenants altogether.[11]

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

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

[11] Berlin, Nini, and Yu (2020) show that borrowers who have a term loan with few or, even no, financial covenants are still often subject to financial discipline through split control rights. The holders of the revolving credit facility, which are typically banks, often have the exclusive right to renegotiate loan terms using the financial covenants contained in the revolving loan agreements.

facilitates communication with the borrower, and, if necessary, prosecutes litigation. Larger questions are left to the individual lenders themselves, who make collective decisions in a somewhat democratic process. The key construct is the “required lenders,” which is the 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 amendments to 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 all (affected) lenders.

Loan terms at origination are set through an iterative process centered on consultation between the borrower (or, when the loan is being made to finance an 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 & 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).[12] Compared to, say, the public equity market, however, trading is more opaque and more 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 & 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 secured 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 mean that the company can sell only 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 & Skeel, 2013). But bankruptcy means canceling the interests of stockholders, 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.

[12] 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).

One came to be known as a dropdown transaction. Made infamous by J. Crew, the 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 collateral—can re-pledge the asset to support new debt that is senior to the preexisting, 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). The transaction 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 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 suggests, 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 universal 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 (2024)—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.[13] 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 & 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

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

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.[14]

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, lenders may choose to participate in a deal that destroys value overall. (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-

[14] 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-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."

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 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.[15]

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-lien and 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. During the week of the transaction, the quoted price of the first-lien loan dropped by approximately 25 percent.

[15] 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.

[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; 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.[16] In plain English, the judge concluded that the uptier appeared to be legally valid. The price of Serta's outstanding loan dropped a bit further during the week of the announcement.

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

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[16] 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.

filed a lawsuit. Figure 1 shows that quoted loan prices for each company fell dramatically on the relevant event date.[17]

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.

Loan contracts also began to change. Buccola and Nini (2024) 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 near 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

[17] 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.

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 a later cohort of companies to pursue an uptier.

3 Hypotheses and Research Design

Our central 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

Using available price data, we construct a variable corresponding to the percentage change in the price of each loan i during week t, which we denote as R_{it} and refer to as the price return.[18] 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:

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

The variable α_i is a loan-specific fixed effect that captures the average return of each loan, and δ_t is a weekly fixed effect that captures market-wide changes in loan prices during week t. We estimate these parameters using a 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,

[18] 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.

as opposed to immune, to an uptier. The coefficient β is an estimate of the difference in returns for susceptible loans relative to immune loans during the event period. The regression specified in (1) is comparable to a difference-in-difference design in which the immune loans serve as the control group for susceptible loans. In alternative versions of (1), we modify the dependent variable to be $\frac{R_{it}}{\sigma_i}$, where σ_i is the sample standard deviation of returns for loan i. Ederington, Guan, and Zongfei (2015) show that using standardized returns can help increase the power of standard statistical tests in event studies.

We define the event windows in three alternative ways. In our baseline approach, we identify event weeks as those when 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, which spans the related court decision. 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. With two forms of the dependent variable and three options for the event window, we perform six separate regressions.

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 $\beta < 0$. We expect a negative coefficient if investors discount the value of susceptible loans relative to immune loans, so we perform both 2-sided and 1-sided tests of the null hypothesis. In the remainder 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. Since investors do not know whether they would eventually be invited to participate in a future uptier, their expectation of an uptier's effect is a weighted average of the expected consequences for participating and excluded lenders, respectively. If investors in Summer 2020 anticipated that lenders on average would fare comparably to the 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, however. Through 2022, about a dozen uptiers were executed, which is roughly 2 percent of the loans outstanding that traded at a distressed level 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 $500 \cdot .02 = 10$ 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 10-15 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 15–30 basis points. In our simulation analysis, we examine effect sizes of 10, 25, and 50 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 & Mählmann, 2022), we create a weekly time series of prices using the last day of each week. We compute the price return as the percentage change in the bid price during the week.

4.2 Loan Sample

We begin by creating a sample of loans that have secondary market prices during 2020.[19] 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

[19] 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.

susceptible even if they are legally susceptible. 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 40% of the loans, leaving us with 1,224 loans to 1,092 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 across loans in the sample. The final sample includes 1,092 loans.

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 242 of the loans.

[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 shows 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.[20] The most striking difference is the frequency with which borrowers are owned by a

[20] 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 (2024), 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% (167 out of 242) of the loans we analyze were susceptible to an uptier. 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 (2024) show is due to sponsored loans more often permitting the borrower to make "open market" loan repurchases.[21] We infer

[21] 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.

from the similarity of the groups that evolution of loan prices should be similar and attribute any differences to the difference in susceptibility to an uptier.

Figure 3 plots the cumulative change in the average price of each type of loan relative to its average 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 stood only about 5 percent below the price at the beginning of the year and had increased by over 15 percent relative to the trough. The average prices of the two portfolios track quite closely, confirming the similarity of the loans in each group. The grey vertical bars in Figure 3 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, a proposition that we formally test in the next section.

[Insert Figure 3 here]

5 Results

Our empirical question is whether the secondary market prices of immune and susceptible loans diverged when news was released that uptier transactions had become more commercially acceptable and legally viable. There are at least two relevant perspectives from which to address the question. One is the perspective of a conventional social scientist, who asks, in effect, whether the null result—i.e., that prices did not diverge—can be rejected with sufficient statistical confidence, typically using a 95-percent threshold. Thus, we report in conventional terms the results of the event study outlined in Section 3.1 and the results of a simulation designed to test the ability of an analyst with access to the entire universe of loan contracts to detect a price signal.

Another important perspective is that of a would-be borrower after Summer 2020. Market participants are not, of course, subject to the same standards as a social scientist. Even a relatively weak signal might influence a borrower's behavior. And price discipline depends ultimately on whether—and to what extent—those who propound contracts believe that terms will be priced, not on whether social scientists are sufficiently satisfied that they are. We thus report results from the perspective of market participant who has a prior belief about the extent of the price impact of an uptier and Bayesian updates based on the observed data.

5.1 The Social Scientific Perspective

5.1.1 The Generic Event Study

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 β in the columns labeled "Coefficient," along with the estimated standard error of the estimate. The β 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.

As a check on the inferences from the event study results in Table 3, we conduct a randomization inference test inspired by Klick & Sitkoff (2008). During the two-year sample period including all weeks in 2019 and 2020, we randomly choose five weeks and estimate the regression (1) using the Price Return as the dependent variable. We repeat this process 1,000 times to generate an empirical distribution of the estimated β under the null hypothesis that the event dates provide no news about the impact of uptier susceptibility. Figure 4 plots a histogram of the estimated coefficients. The distribution is centered on zero—the mean estimate is 0.5 basis points—and fairly symmetric, as would be expected if immune and susceptible loans have similar return distributions but are subject to large idiosyncratic shocks. There is considerable uncertainty in any particular β estimate, as evidenced by the large fraction of estimates that are far from zero. The sample standard deviation of the estimates is 17.5 basis points, not far from the standard error reported in Table 3. The vertical line denotes the -19.8 estimate that we get when using the actual event weeks. The estimate falls at the 12th percentile of the empirical distribution, similar to the 1-sided p-value reported in the top row of Table 3. Obviously, given the much smaller coefficient estimates using the 3- and 6-week windows, a similar exercise will confirm their lack of statistical significance. This exercise helps confirm that our inferences are not distorted by any parametric assumptions underlying the regression results.

[Insert Figure 4 here]

5.1.2 Potential Heterogeneity

Despite the weak signal available in our price data as a whole, market participants might have gleaned useful information if price reactions were heterogeneous in predictable ways. We thus amend the regression (1) to test whether prices reveal a stronger signal within two subsets of loans that might have faced an unusually high 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 could not have anticipated such a strong correlation, we hypothesize that investors may have anticipated a higher probability of an uptier for 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.[22] 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 β_1 continues to capture the difference in returns for susceptible loans relative to immune loans during the event period, and β_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]

The results are provided in Table 4, which shows the coefficient estimates for β_1 and β_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 of susceptible and immune loans. The point estimate changes modestly, from -19.8 to -23.0, and the associated standard errors and p-values lead to similar inferences.

[22] 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 coefficient on the standardized returns becomes slightly more negative and the p-vale slightly smaller. 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 +9.2, meaning that sponsored susceptible loans had slightly higher, though statistically indistinguishable from zero, returns during the event weeks. We find no evidence to support the hypothesis that prices of sponsored, susceptible loans fell during the event weeks. One possible explanation is that investors failed to realize the subsequently realized strong correlation between sponsored status and probability of an uptier. Under this explanation, it is possible that market participants understood the general price of uptiering flexibility but failed to incorporate important heterogeneity into the price. Alternatively, participants may have understood that sponsored loans faced higher risk, but secondary market prices failed to update, at least to a statistically detectable degree, to provide that signal to borrowers.

Splitting the sample by loan price produces a larger change in the point estimate for the general effect of being a susceptible loan. 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. The movement in the point estimate towards zero is again troubling for the price discipline model of term selection. Loans in the primary market are issued at prices very close to 100, so the most relevant evidence to inform the choice in the primary market is the price change of loans with prices well above 80. There is very little evidence suggesting that secondary market prices provided any useful information for these loans.

5.1.3 Incremental Statistical Power from Unavailable Contracts

Studies of publicly available loan contracts are doomed to a small sample compared to the total universe of traded loans. Our study is no exception, as we locate 242 contracts among the 1,092 traded loans that meet our filtering criteria. The contracts in our sample represent nearly, if not precisely, all of the publicly available contracts meeting our criteria, but in theory the sample size could increase by over 4 times with access to non-public contracts. Firmer conclusions about price impact might be possible with the benefit of a larger sample of contracts.

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

We proceed in five steps. First, we draw a random sample of loans from our full set of loans. We experiment with sample sizes of 250 loans and 1,000 loans. 250 is close to the sample size of public firms for which we can find a contract, and 1,000 loans is near the universe of loans and so can be taken to represent a world in which investors have access to nearly all possible loan contracts. Second, we choose a random set of 70 percent of loans to be deemed "susceptible," with the remainder being classified as "immune." We choose 70 percent to mimic our public-contract sample. Third, we randomly choose five weeks 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 susceptible loans during the event weeks by decreasing 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 statistical power. 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 5,000 times and estimate the power of the test as the fraction of the simulations for which we find a statistically significant coefficient.

[Insert Table 5 here]

Table 5 reports the results using the price return as the dependent variable in (1). The top part of the table reports estimates of statistical power of a standard two-sided test, and the bottom part reports estimates of a one-sided test against the alternative that the coefficient is non-negative. 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 13.4%, 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 is well below 50% in all cases. With nearly the entire universe of loan contracts and a true effect of 50 basis points, the power estimate increases to 26.2%, but for a more reasonable alternative of 25 bps, power is below 15%. In short, the variability of loan prices in general makes it challenging to detect changes of a magnitude that would correspond to all but the very most important contract features.

5.2 Bayesian Analysis

A different way to use the evidence from secondary markets is through the lens of a market participant accustomed to exploiting noisy price signals. Acting as Bayesian updaters, contract proponents may combine a prior belief about the price of a contract provision with evidence available from the secondary market. Even if the available evidence is noisy and based on a limited sample, it may still permit borrowers to update their beliefs of the market's price.

We assess the degree of updating induced by the event study evidence as a standard Bayesian updating exercise. In equation (1), we treat the parameter β, which captures the abnormal return for susceptible loans during the event window, as a random variable. By assuming a subjective prior distribution over β, Bayes theorem informs us how to update the parameters of the distribution based on the observed data. The posterior distribution is a function of the prior distribution for β and the likelihood of observing the data given an assumed process for generating the data. We make the standard assumption that the residual in (1) is drawn from a normal distribution with mean zero and variance σ^2, which fixes the likelihood of observing the data, conditional on parameter choices. We assume an uninformative Jeffreys prior distribution for the value of σ^2 and experiment with several alternative prior distributions for β.[23]

We estimate the posterior distribution of β using the Metropolis-Hastings (MH) algorithm, a simulation-based Markov chain Monte Carlo method. The MH algorithm is useful for sampling from an arbitrary posterior distribution without completely specifying the distribution analytically. Through a clever iterative simulation process, the resulting Markov chain produces a stationary distribution that closely approximates the true posterior distribution. With enough simulations, the empirical distribution can be used to summarize relevant quantities of the

[23] We account for the loan and week fixed effects in (1) by first regressing the returns, R_{it}, and the explanatory variable $I_i^{sus} I_t^{event}$ on the full set of loan and week fixed effects. We then implement the Bayesian analysis using the residuals from these regressions, which leaves only three unknown parameters: the variance of the residual (σ^2), our variable of interest (β), and the constant in the regression. We assume the constant is drawn from an uninformative flat prior.

posterior distribution of the unknown parameters. Moreover, the method is straightforward to implement.[24]

[Insert Table 6 here]

Table 6 summarizes how a prospective borrower's beliefs about the effective "price" of uptier susceptibility would have changed based on various options for the expected price and the degree of confidence in the belief. In panel A, we assume that the true effect is drawn from a normal distribution where the mean is the expected price of susceptibility, and the standard deviation summarizes the degree of uncertainty. In the top three rows of panel A, we assume a very uninformative prior by choosing a very large standard deviation. We increase the confidence in the prior in the subsequent two blocks of three rows. In each of the blocks, we use expected prices of -10 bps, -25 bps, and -50 bps, which are reasonable beliefs for the price. With a very uninformative prior, a Bayesian analyst will update the posterior distribution rather substantially based on the data. For all three values of the mean of the prior distribution, the mean of the posterior distribution is roughly -95 bps, which not surprisingly is very similar to the point estimate in Table 3. Similarly, the posterior standard deviations and 95% credible intervals are insensitive to the choice of prior mean and quite similar to the standard error and confidence interval in Table 3. With an uninformative prior, the analyst will update based on the data and must confront the noisiness of the estimates.

In the second grouping, we reduce the standard deviation in the prior distribution to 100 bps. With this lower level of uncertainty, a Bayesian analyst will have more confidence in the

[24] See Gelman et al. (2021) for a discussion of Bayesian methods and the MH algorithm. We implement the algorithm using the suite of Bayesian commands available in Stata 18.

posterior distribution; the standard deviation of β falls to around 70 bps, reflecting the combined influence of the prior and the data. Along with the reduced uncertainty, however, the mean of the posterior distribution moves closer to the mean of the prior distribution, which is closer to zero than the point estimate in Table 3.[25] With a prior expectation of -10 bps, the posterior mean is -51 bps, so even with the reduced uncertainty, there is insufficient evidence to confidently conclude that the mean is less than zero; the 95% credible interval extends to +87.3 bps. Reducing the mean of the prior distribution yields a more negative posterior, but even by assuming -50 bps, the credible interval comfortably spans 0. Although adding more certainty to the prior belief results in a more certain posterior, the degree of updating is sufficiently small to prevent analysts from having much confidence in their posterior beliefs.

In the bottom grouping, we further shrink the standard deviation of the prior distribution to examine the updating of a very analyst. In these cases, the posterior distribution resembles very closely the prior distribution. The two right-most columns report a 95% credible interval for the prior distribution, and for each assumption on the prior mean, the degree of updating is very small. With a prior expectation of -25 bps or -50 bps, the posterior credible intervals are entirely

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[25] We can provide a rough justification of the estimates in Table 6 using the well-known Bayesian updating formula for a random sample drawn from a normal distribution with mean θ, known standard deviation σ, and prior distribution for θ that is normal with mean μ and variance τ. Given a sample with mean m and standard error s, the posterior distribution for θ is normal with mean and variance that are intuitive functions of the prior parameters and the sample statistics. The posterior mean is a weighted average of the prior mean and sample mean $\mu^* = \frac{\tau^2}{\tau^2+s^2} \cdot \mu + \frac{s^2}{\tau^2+s^2} \cdot m$, where the weights depend on the prior and sample variances. The posterior variance is one-half of the harmonic mean of the prior and sample variances, $\tau^* = \frac{\tau^2 s^2}{\tau^2+s^2}$. Using the estimates in Table 3 as reasonable estimates of the sample mean and standard deviation, we have $m = -98.9$ and $s = 97.4$. Using a prior distribution with a mean of -25 bps and a standard deviation of 100 bps, the posterior distribution has a mean of $\frac{100^2}{100^2+97.4^2} \cdot (-25) + \frac{97.4^2}{100^2+97.4^2} \cdot (-98.9)$=-61.0 and a standard deviation of $\sqrt{\frac{100^2 97.4^2}{100^2+97.4^2}} = 69.8$ bps. Each of these is very close to the estimates in Table 6 using a normal prior with mean and standard deviation of (-25,100). Analysts using this back-of-the-envelope calculation would generate very similar estimates to our full Bayesian regression estimates.

below zero, but the prior credible intervals are also. The impact of the data on the analyst's beliefs is very small.

A downside of the assumption that the β parameter is drawn from a normal distribution is that the distribution permits positive values. A more reasonable belief is that β is negative but there is uncertainty on the exact value of β. We implement such a belief using a uniform distribution defined on nonpositive values of β, which we parameterize by a lower and upper bound, (u, l). In the top row of panel B, we set a very wide degree of uncertainty by assuming only that β is between -500 and 0. As with the normal prior, the updating is most significant with an uninformative prior; the posterior mean is -124 bps, which is considerably less than the prior mean and much closer to the point estimate in Table 3. With more precise priors, the degree of updating is very small, again reflecting the uncertainty in the data. Although the posterior distributions all confidently point to a negative value for β, this entirely rests on the distributional assumption that precludes positive values.

The takeaway from the Bayesian exercise is twofold. First, given the point estimate of nearly -100 bps in Table 3, the evidence provides modest confirmation to a Bayesian carrying a prior belief that the market charges for a term like uptier susceptibility. Of course, using an event window of 5 weeks provides the largest degree of confirmation; the point estimates are considerably smaller using the other event windows and would lead to less updating. Second, the amount of updating is sufficiently small that the secondary market evidence does not add much to the certainty of the market's expected price of uptier susceptibility. Highly confident posteriors only follow from highly confident priors, rendering the secondary market evidence of little value in developing expectations for the price of the contract term.

6 Discussion

Our combined results bear on two kinds of questions. One question relates to the informational efficiency of the secondary market for loans (Borowicz, 2021; cf. Awrey, 2016; Gilson & 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 it was costly for investors to assess the implications of uptier susceptibility, so prices did not adjust to reflect the true fundamental value. A likely reason is that analysts use low-dimension models to assess value, at least until a restructuring appears to be in the cards. Since analysts cover lots of loans, it may not be worth their time to estimate the value implications of each of the hundreds of dimensions a contract has. As a consequence, contract features with limited and perhaps contingent relevance to value may 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), largely determines pricing. A corollary is that other features of loans that do not meet some minimum threshold of impact are underweighted or ignored.

A second, related possibility is that the costs of learning the details of contracts caused prices to decouple from their fundamental value. Analysts may determine that the costs of deeply reading contracts exceed the anticipated trading benefits of understanding exactly what each provision *means*. 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 time and domain-specific knowledge. The cost of comprehension implies that some economically significant terms may nevertheless be rationally ignored.

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. To assess the importance of learning costs, we examined secondary market prices for changes upon the publication of these reports. 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. There are only 11 reports suitable for our purposes, so not enough reports to make a systematic study, but the anecdotes are telling. In nine instances, we observe no notable change in prices. In two instances, loan prices decreased after publication of a report, but both reports included confounding information about the subject companies. Although the exercise does not allow us draw a strong conclusion about the cost of learning about uptier susceptibility, the evidence does not suggest that markets would have updated more strongly if only investors were aware of which contracts were susceptible.

The third possibility is that loan prices in fact adjusted to reflect fundamental value but that our statistical methods lack the power needed to detect that 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 secondary market incorporates information about contract terms in an efficient way. Ultimately, then, our results fail to teach anything useful about the loan market's informational efficiency.

Our results do, however, bear on questions about the secondary market's utility as a source of information about the value of contract terms and other legal rules. The core conclusion is cautionary in nature. The event study, power analysis, and Bayesian approach together suggest that analysts are unlikely to find reliable evidence of price adjustments to legal or contractual shocks that should, and perhaps do, affect loan values, even for shocks that are highly salient and carry nontrivial economic implications. Event studies are among the most popular tools with which corporate finance researchers seek to weigh market reaction to legal change. Researchers should tread cautiously in the loan context.

The secondary market's limited capacity to inform analysts about the value of contract terms also teaches us something about term selection in the primary market. In a bonding model, contract proponents have reason to select surplus-maximizing terms only to the extent that they believe the terms will be priced to reflect their fundamental (dis)utility to investors. Whatever explains our event study results, the evidence indicates a weak empirical basis for the belief that loan investors price contract terms comparable to those that determine uptier susceptibility. 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, for example, that prospective borrowers after Summer 2020 held strong priors that yields on new loans would be sensitive to uptier susceptibility. Weak evidence that yields on the stock of existing, susceptible and immune loans had diverged would not necessarily change anyone's mind. At the same time, the weakness of the loan price evidence casts doubt on the plausibility of the kind of belief that could explain the rapid and extensive changes in contracts after Summer 2020. The evidence suggests that secondary markets are an unlikely source to generate estimates of the expected incremental yield associated with non-core terms and so anchor a price-discipline model of term selection more generally.

We thus interpret our results to suggest that models of term selection emphasizing non-price intermediation have application even with respect to terms that weigh substantially on a financial contract's fundamental value. A core concept in these models is the notion of standard terms on which intermediaries, such as lawyers, industry groups, and information providers focus with intensity and around which they coordinate negotiations (Choi & Triantis, 2013; Choi, Gulati & Scott, 2021). The content of such terms may reflect a broad consensus of commercial reasonableness but needn't indicate strict optimality. Among other things, agreement about what is market might protect lawyers from punishment when a term they have countenanced turns out to have adverse effects for a client (cf. Gao, Kleiner & Pacelli, 2020). This means that, after a shock, prevailing contract terms can lurch from one standard to another as intermediaries renegotiate the market. Such a process could explain the curtailing of uptier options after 2020.

More broadly, one might imagine three rough categories of loan contract terms. The first are the headline items that are likely to appear in the term sheets that get in front of principals. Existing evidence suggests that these features, such as the contractual interest rate, credit rating, collateral, and the covenant package get priced in primary markets (Murfin & Pratt, 2019). The second category includes terms on which intermediaries focus but which may not be subject to much, if any, pricing pressure in primary and secondary markets. The mechanisms that push these terms towards optimality, or at least perceived optimality, include the forces of standardization that have been documented in the contracting literature (Kahan & Klausner, 1997). The third category are "encrusted" terms and other boilerplate that is not part of active negotiation either by principals or intermediaries.

These categories are, of course, fluid. The uptier example that we explore shows how the interaction between what is arguably a term that is dickered among intermediaries and a term that is likely a piece of encrusted boilerplate led to a significant shift in loan contract terms. Once the issue materialized, intermediaries publicized the issue and the market terms changed to shut of the availability of uptiers in most cases (Buccola & Nini, 2024). To the degree this is the process that generates contract terms, it is a substantial deviation from the price discipline model. Understanding why prices are insensitive to certain contract terms and investigating the second-best desirability of non-price intermediation are important questions that we reserve for future work.

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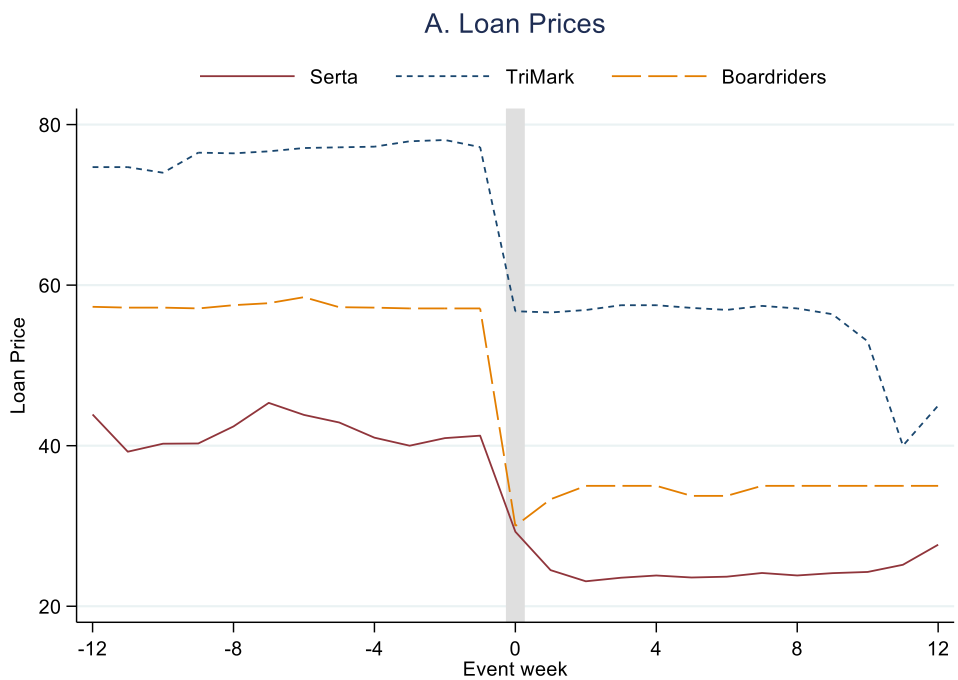
Figure 1. The Impact of an Uptier Transaction on Outstanding Loan Prices

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.



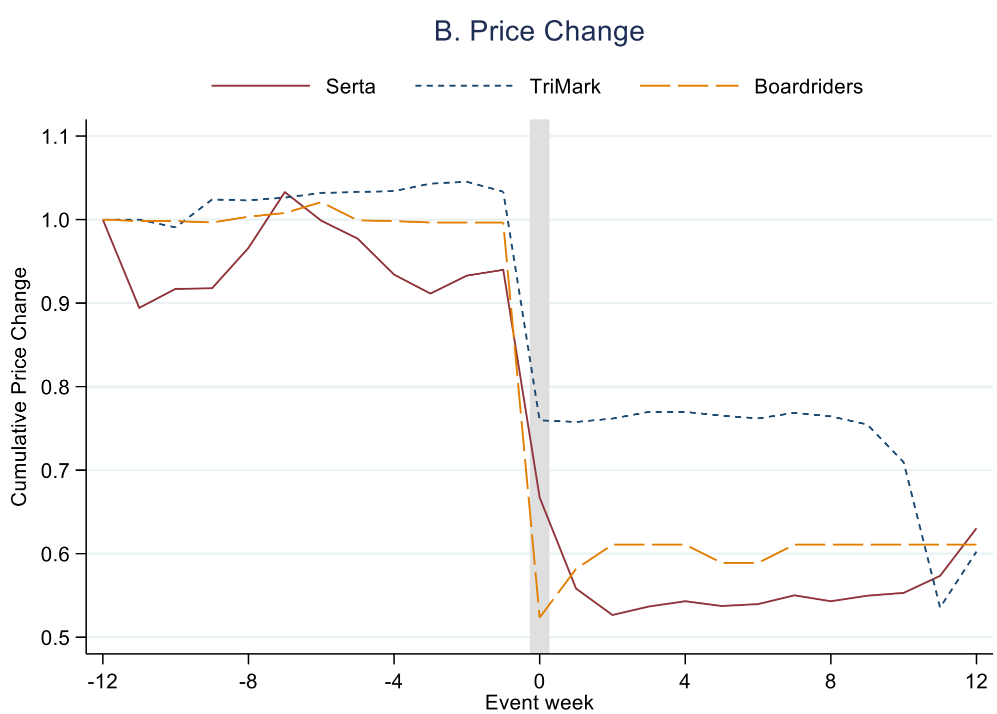


Figure 2. The Frequency of Uptier Blockers

The figure plots the frequency, in half-year intervals, of new contracts that block an uptier transaction using the data from Buccola and Nini (2024). The vertical capped lines are 95% confidence intervals. The vertical line at 2020h1 denotes the half-year during which the Serta uptier transaction happened.

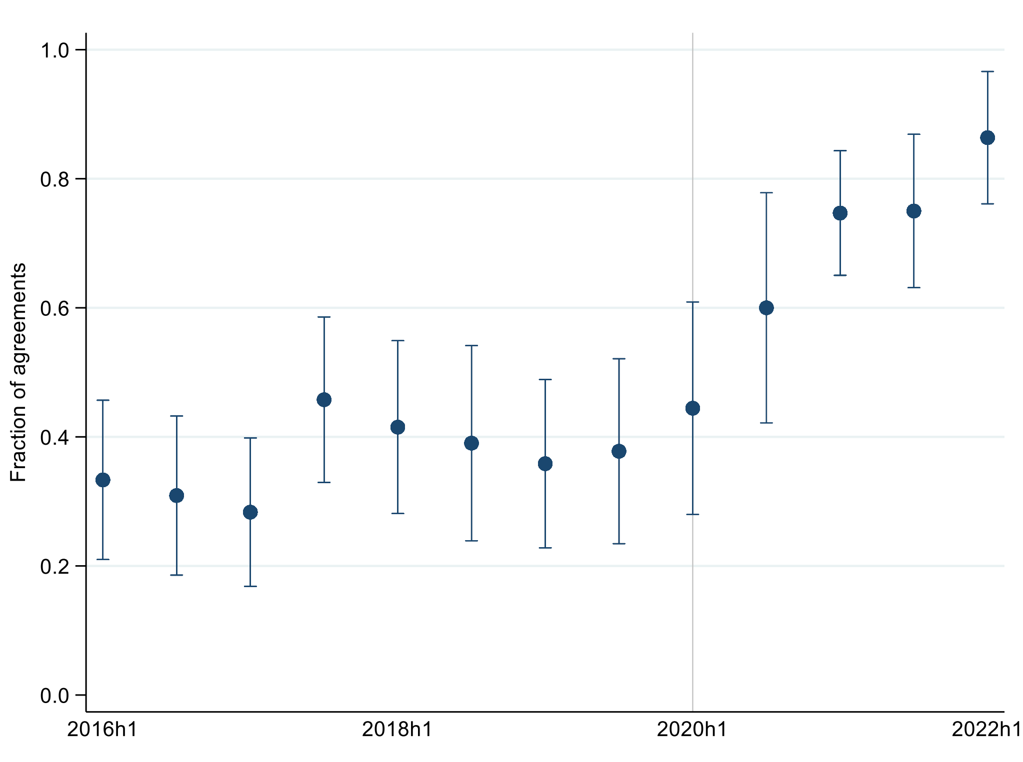


Figure 3. The Evolution of Loan Prices during 2020

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.

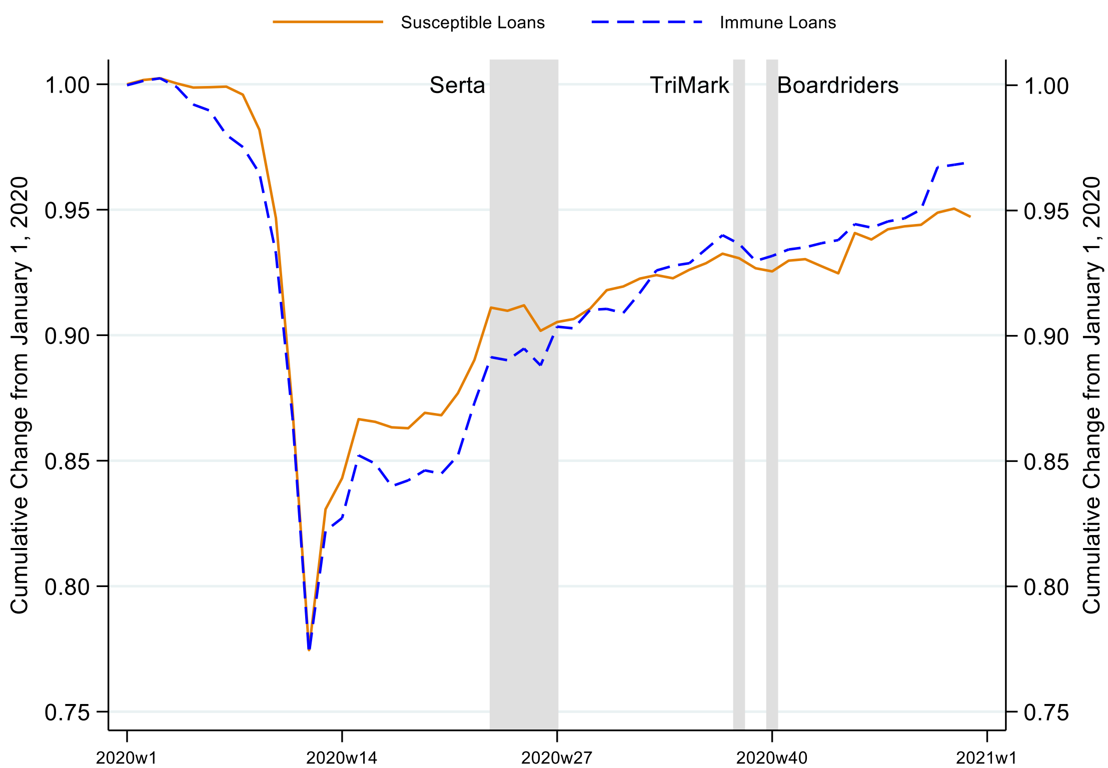


Figure 4. The Evolution of Loan Prices during 2020

The figure plots a histogram of the estimated β values from 1,000 versions of 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 on a loan, measured in basis points. α_i is a loan-specific fixed effect, δ_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. Each version of the regression is based on a randomly chosen set of 5 event weeks. The black vertical line labeled "Actual Events" is the coefficient estimate based on the 5 event weeks around the Serta Simmons, TriMark, and Boardriders announcements.

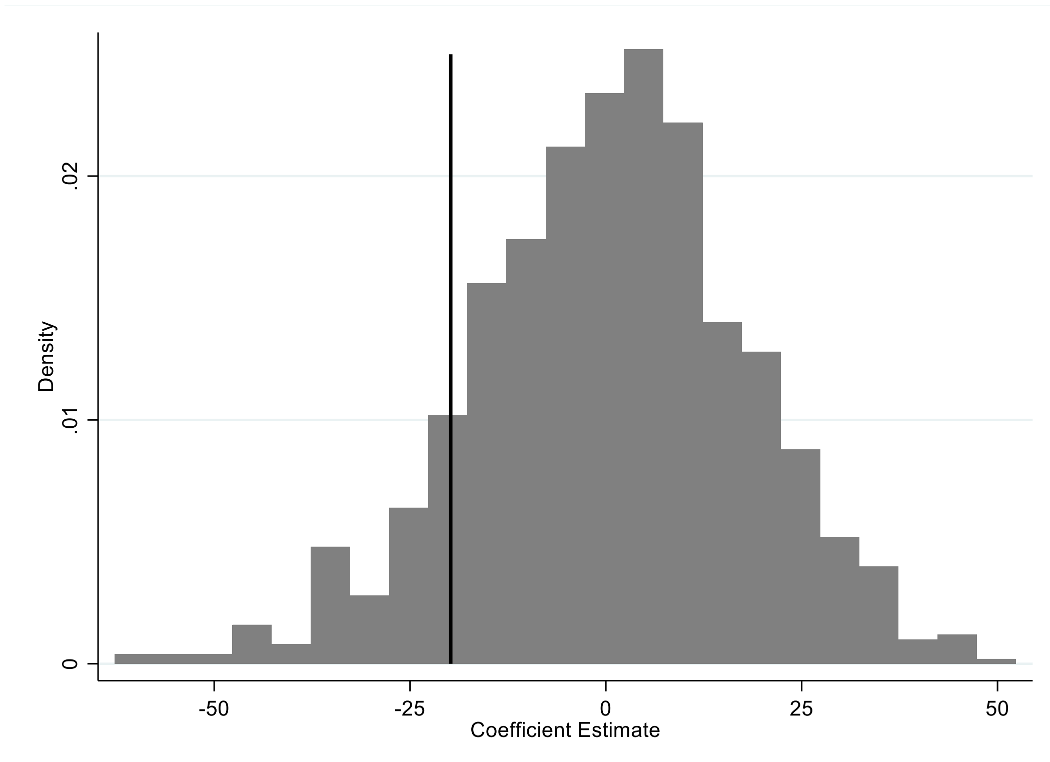


Table 1. The Sample of Loans

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 1,092 borrowers with sufficient loan pricing information during 2020. We find a loan contract in Edgar for 242 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
Loan amount ($ millions)				
Contract	1030	400	750	1468
No Contract	761	322	510	913
Maturity (years)				
Contract	6.7	6.5	7.0	7.0
No Contract	6.8	7.0	7.0	7.0
Loan Spread (bps)				
Contract	326	225	300	375
No Contract	426	350	400	500
Covenant-lite				
Contract	70%			
No Contract	68%			
Sponsored (%)				
Contract	24%			
No Contract	73%			

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. The sample includes one loan for 242 borrowers with sufficient loan pricing information during 2020 for which we find a loan contract in Edgar. Of the 242 loan contracts, 167 are susceptible to an uptier and 75 are immune.

A. Loan Characteristics

	Mean	25th Percentile	50th Percentile	75th Percentile
Loan amount ($ millions)				
Susceptible	1061	400	760	1485
Immune	960	400	637	1280
Maturity (years)				
Susceptible	6.7	6.5	7.0	7.0
Immune	6.6	6.0	7.0	7.0
Loan Spread (bps)				
Susceptible	312	225	275	350
Immune	358	225	325	450
Covenant-lite				
Susceptible	75%			
Immune	59%			
Sponsored (%)				
Susceptible	29%			
Immune	12%			

B. Credit Ratings

Rating	Susceptible	Immune
BBB	11.4%	5.3%
BB	32.3%	30.7%
B	31.7%	25.3%
Unrated	24.6%	38.7%

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. α_i is a loan-specific fixed effect, δ_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 β, 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 $I_i^{sus} I_t^{event}$ (β_1) and $I_i^{group} I_i^{sus} I_t^{event}$ (β_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	-0.09	-0.45	0.063	0.032
	(21.5)	(107.7)			(0.05)	(0.24)		
Event window * Sponsored	9.2	45.9	0.921	0.540	-0.01	-0.05	0.595	0.702
	(49.7)	(248.6)			(0.11)	(0.55)		
Distress status								
Event window	-6.7	-33.3	0.807	0.403	-0.03	-0.15	0.633	0.316
	(27.3)	(136.6)			(0.06)	(0.30)		
Event window * Price < 80	-26.6	-132.8	0.496	0.249	-0.09	-0.45	0.313	0.156
	(39.0)	(194.8)			(0.09)	(0.43)		

Table 5. Statistical Power

The table reports estimates of the statistical power of the test of the hypothesis that the coefficient β 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 price 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 two 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	0.058	0.066	0.084
1000	0.062	0.081	0.172
One-sided test			
250	0.094	0.102	0.134
1000	0.105	0.132	0.262

Table 6. Bayesian Analysis

The table reports characteristics of the posterior distribution of β in the regression $R_{it} = \alpha_i + \delta_t + \beta I_i^{sus} I_t^{event} + \varepsilon_{it}$. The dependent variable is the price return. In panel A, the prior distribution for β is assumed to be normal with mean and standard deviation given in the Parameters column. In panel B, the prior distribution for β is assumed to be uniform on the interval given in the Parameters column. The right two columns report a 95% credible interval for the prior distribution of β.

	Posterior Distribution				Prior Distribution	
			95% Credible Interval		95% Credible Interval	
Parameters	Mean	SD	Lower	Upper	Lower	Upper
A. Normal Distribution						
(-10, 2000)	-95.6	98.7	-286.7	95.5	-1970.0	1950.0
(-25, 2000)	-96.3	96.4	-274.5	98.6	-1985.0	1935.0
(-50, 2000)	-93.3	97.9	-280.8	102.2	-2010.0	1910.0
(-10, 100)	-51.0	69.9	-192.6	87.3	-206.0	186.0
(-25, 100)	-62.6	70.8	-199.5	77.1	-221.0	171.0
(-50, 100)	-70.2	68.0	-204.6	60.5	-246.0	146.0
(-10, 10)	-10.7	10.2	-30.3	8.6	-29.6	9.6
(-25, 10)	-25.7	10.0	-45.7	-6.9	-44.6	-5.4
(-50, 10)	-50.3	10.0	-70.2	-30.8	-69.6	-30.4
B. Uniform Distribution						
(-500, 0)	-124.2	77.7	-299.1	-5.3	-487.5	-12.5
(-20, 0)	-10.3	5.8	-19.5	-0.6	-19.5	-0.5
(-50, 0)	-26.6	14.2	-48.8	-1.6	-48.8	-1.3
(-35, 15)	-24.9	5.7	-34.4	-15.7	-34.5	-15.5