Creditor Control Rights and Firm Investment Policy*

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Abstract

We provide novel empirical evidence of a direct contracting channel through which firm financial policy affects firm investment policy. We examine a large sample of private credit agreements between banks and publicly traded U.S. corporations and find that 32% of the agreements contain an explicit restriction on the firm's capital expenditures. Creditors are more likely to impose a restriction following negative borrower performance, and the effect of negative performance on the likelihood of facing a capital expenditure restriction is larger than the effect of negative performance on other loan terms such as the interest spread or pledging of collateral. We also demonstrate that the restrictions affect firm investment policy. For example, we show that most of the actual capital expenditures of borrowers with restrictions cluster just below their restricted amount, while in the year prior to the contract, the same borrowers' capital expenditures are distributed evenly above and below the restriction. Our results are consistent with control-based theories of financing in which creditors retain control rights over investment policy as a second-best solution to agency conflicts.

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One of the most important topics in corporate finance concerns the relationship between firm financial and investment policy. The majority of theoretical research on this topic argues that firm investment is constrained indirectly by external financial frictions such as information asymmetry, collateral constraints, or debt overhang. However, an alternative line of theoretical research posits a more direct, contractual link. These "control-based" theories of financial contracting argue that, in the presence of agency conflicts and contractual incompleteness, optimal contracts will give creditors direct control rights over firm investment policy in certain states (Jensen and Meckling (1976), Aghion and Bolton (1992), and Dewatripont and Tirole (1994)).

To date, existing empirical research on the investment policy of public firms has found little support for control-based theories of financial contracting, beyond cases in which the borrower has defaulted on a payment obligation. For example, in their classic examination of bond covenants, Smith and Warner (1979) state that "extensive direct restrictions on production/investment policy would be expensive to employ and are not observed." Similarly, Billett, King, and Mauer (2006) find that fewer than 5% of public bond indentures contain an explicit restriction on firm investments. Moreover, past studies that document an empirical relationship between financial and investment policy rely primarily on external financial frictions to explain their results (see Hubbard (1998), Stein (2003), Hennessy (2004), Hennessy and Whited (2006)).

In this paper, we provide novel empirical evidence on the micro-foundations of corporate financing and investment policy and find broad support for the control-based theories. In particular, we examine over 3,000 private credit agreements between banks and publicly-traded U.S. corporations and document that roughly 30% of these contracts include explicit restrictions on the borrower's capital expenditures. Moreover, we demonstrate that these restrictions are more likely to be put in place following poor firm performance, and that the restrictions impact actual firm investment policy. To our knowledge, this is the first paper to document both the widespread use and the effects of direct contractual restrictions on the investment policy of solvent public firms.

The novelty of our findings is due, in part, to the contracts we examine: private credit agreements represent an ideal – and largely overlooked – setting for examining the influence of financing on investment policy. Roughly 80% of all public firms maintain private credit agreements, typically as bank revolving credit facilities. By comparison, only 15 to 20% of public firms have public debt (Sufi (2006a), Faulkender and Petersen (2006)). Moreover, private credit agreements contain covenant restrictions that are more detailed, comprehensive, and tightly set than public bond indentures (see Dichev and Skinner (2001), Sweeney (1994), Kahan and Tuckman (1995), Chava and Roberts (2006), and Sufi (2006a) for empirical evidence and Park (2000) for a theoretical justification). In other words, private credit agreements contain the covenants that are most likely to influence firm behavior. Given that 95% of firms with public debt also have a private credit agreement in the form of a revolving credit facility (Sufi (2006a)), an analysis of these contracts is essential to understanding the impact of covenants on firm behavior.

We motivate our analysis of these contracts with the help of three control-based theories of financing. Jensen and Meckling (1976) contend that explicit creditor control of firm investment policy is part of second-best optimal debt contracts in the presence of managerial agency problems. Investment restrictions can lower the cost of debt by mitigating the chance that a manager, acting in the interest of shareholders, chooses riskier investments at the expense of creditors. Aghion and Bolton (1992) and Dewatripont and Tirole (1994) build on the intuition in Jensen and Meckling (1976) by examining the allocation of control rights when contracts are incomplete (Grossman and Hart (1986), Hart and Moore (1990)). They show that the division of control rights over firm investment policy should be contingent upon the outcome of a noisy performance signal that is correlated with either unobservable effort (Dewatripont and Tirole (1994)) or states of nature indicating that managerial actions are most likely to create a negative externality (Aghion and Bolton (1992)). Under standard parameterizations of the models, low values of the performance signal cause control rights to shift to creditors to minimize inefficient future investment decisions.

We empirically investigate two implications of these models. First, we examine whether the allocation of control rights over firm investment policy is an important part of observed contracts. Second, we examine whether creditors exert more control over firm investment policy in response to negative borrower performance.

Our first result documents that the allocation of control rights over firm investment policy is an important part of observed contracts, even among borrowers that have not defaulted on interest payments. More specifically, 32% of the private credit agreements in our panel contain an explicit restriction on the firm's capital expenditures, and 40% of the firms in our sample obtain at least one loan between 1996 and 2005 that contains a restriction. Moreover, capital expenditure restrictions are present in the contracts of borrowers across all industries and size categories. These findings stand in stark contrast to studies of bond indentures, and constitute prima facie evidence that the allocation of control rights over investment policy is a common element of debt contracting.

Second, we find that creditors obtain more control over firm investment policy in response to negative firm performance. More specifically, using firm fixed-effects regressions within a panel of loan contracts, we find that a capital expenditure restriction is more likely to be imposed following a decline in cash flow, a financial covenant violation, or a downgrade in the firm's credit rating. The effect of negative firm performance on the likelihood of having a capital expenditure restriction is both statistically robust and economically meaningful. For example, a firm that is downgraded from the lowest investment-grade S&P rating (BBB) to the highest speculative-grade rating (BB) experiences an 18 percentage point increase in the likelihood of facing a capital expenditure restriction, which translates to a 56% increase in the likelihood, evaluated at the mean.

We also show that control over investment policy is among the most important performance-contingent contractual features found in loan agreements. The elasticity of imposing a capital expenditure restriction with respect to a negative performance shock is larger than the elasticities associated with the interest spread, the pledging of collateral, and the restriction of dividend payments. For example, our estimates imply that a financial covenant violation leads to a 14 percentage point increase in the

likelihood that a capital expenditure restriction is put in place, which is a 44% percent change evaluated at the mean. By contrast, the same loan experiences an estimated increase in the contracted interest spread of 23 basis points, which is only a 14% increase evaluated at the mean.

In our third set of results, we document a direct contracting channel through which financing affects investment policy by showing compelling evidence that capital expenditure restrictions constrain firm investment. Using a sub-sample of 483 credit agreements from which we collect the actual value of the capital expenditure restriction, we show that firm capital expenditures cluster tightly at or below the contractual limit. Expenditures of nearly 30% of firms lie within one percentage point below the actual restriction, measured relative to lagged assets, and more than 80% of the firms have expenditures clustered within five percentage points below the restriction. In contrast, we only observe 10% of the firms with expenditures within one percentage point *above* the restriction, and only 15% with expenditures that lie within five percentage points above the restriction. Relative to an estimated smooth parametric distribution, we can easily reject the hypothesis that observations do not cluster just below the restriction; likewise, we are able to statistically reject the hypothesis that the number of observations above the restriction is not too low.

The results are even more dramatic when we focus on the credit agreements in which a capital expenditure restriction is imposed after being absent in the previous contract. Before the new credit agreement is signed, almost 50% of observations are above the yet to be imposed restriction amount. After the restriction is imposed, less than 10% are above the restriction amount and over 60% of the firms lie in the expenditure area two percentage points below the restriction. Such a dramatic shift in expenditures to the area just below the restriction amount is difficult to reconcile with the hypothesis that the restrictions do not affect investment policy. We buttress these findings by showing in a broader sample that firms obtaining a loan with a capital expenditure restriction exhibit a larger decline in their capital expenditures than firms obtaining a loan without such a restriction, even after controlling for changes in performance.

We view our paper as making a novel contribution to the literature along three dimensions. Our most important contribution is to provide evidence that creditors play a much more direct, contractual role in the investment policy of public firms than has previously been recognized in the literature. While there is a large body of empirical research that documents an indirect relationship between financing and investment (Hubbard (1998), Hennessy (2004), and Hennessy and Whited (2006)), the existing literature overlooks the more straightforward explanation that investment levels can be explicitly governed by contract. In addition, our paper departs from the existing literature that emphasizes how financial constraints can lead to inefficiently low investment. Instead, we find support for the optimal contracting framework of control-based theories of financing, in which constraints on investment are a second-best solution when management is likely to engage in potentially value-destroying investment.

In terms of how debt financing affects investment, the recent paper by Chava and Roberts (2006) is most closely related to the segment of our empirical findings related to financial covenant violations. Chava and Roberts (2006) use a regression discontinuity approach to show that violations of financial covenants cause a reduction in capital expenditures. They focus more on identifying the effect rather than documenting the channel through which the effect occurs and make no mention of the explicit restrictions on capital expenditures that are at the heart of our analysis. We view their findings as complementary. Indeed, our specific finding that creditors impose a capital expenditure restriction after a financial covenant violation is a possible explanation for their findings. Moreover, our finding that capital expenditures cluster just below the contractually restricted amount complements their regression discontinuity approach. Using two different empirical approaches, both papers find that covenants affect investment.

Our second contribution is to provide empirical support for the idea that shifts in control rights occur in a manner that is consistent with Aghion and Bolton (1992) and Dewatripont and Tirole (1994). Two other papers have documented related findings. Kaplan and Strömberg (2003) study 211 financial contracts between venture capitalists (VCs) and entrepreneurs and document that the division of control rights between VCs and entrepreneurs is often contingent on observable performance signals, with control

shifting to the VCs when signals are low (See also Kaplan and Strömberg (2004) and Kaplan, Martel, and Strömberg (2006)). Lerner, Shane, and Tsai (2004) examine 200 contracts between small biotechnology firms and major corporations that act as investors, and show that control rights shift to investors after negative industry-wide shocks, and shift back to the firms after positive industry shocks. Our paper complements these findings by demonstrating that debt-related control shifts are a common feature of private credit agreements among a wide set of large, public firms.

Our third contribution relates to identifying the large role that creditors can have in controlling day-to-day corporate decision-making. Our results fall in line with the more general thesis in Baird and Rasmussen (2006) that creditors have substantial, though traditionally unrecognized, influence over the governance of corporations. This paper provides large sample statistical corroboration of the mainly anecdotal evidence in Baird and Rasmussen (2006).

The rest of the paper proceeds as follows. The next section discusses the data and summary statistics. Section II presents the theoretical framework with which we motivate the empirical analysis. Sections III through V present the results, and Section VI concludes.

I. Data and Summary Statistics

Our investigation centers on the information we gather from the covenants of private credit agreements between banks and public firms. As mentioned above, we focus on bank credit agreements because, relative to other debt agreements, the covenants contained in bank loans are tighter, more likely to be violated, and more relevant in terms of restrictions on the firm. Historically, information on bank loans has been difficult to gather directly because of the customary secrecy between banks and borrowers. As private agreements, the loans are not legal securities and are not subject to direct SEC regulation. However, SEC precedent has established a requirement that public companies include copies of all "material" contracts, including bank loan agreements, with relevant SEC disclosures.[1] The contracts typically appear as exhibits at the end of a 10-K or 10-Q report, or as an attachment to an 8-K filing. The

[1] The reporting requirements for loan contracts fall within item 601(b) of regulation S-K, which is the general provision that requires exhibits to be filed. Item 4 and item 10 under this regulation require disclosure of securities and the disclosure of all material contracts, respectively. Most loan contracts fall within one of these two categories.

SEC's *Edgar* electronic filing system now makes it possible to search, extract, and download these credit agreements. We use these agreements to construct our sample of contracts with and without capital expenditure restrictions.

To build our complete dataset, we begin with loan deals from Loan Pricing Corporations's *Dealscan* database that have already been linked to firms in Standard & Poor's *Compustat* database. To these deals, we match the credit agreements downloaded from *Edgar*. Our final data set includes 3,720 credit agreements to 1,931 public borrowers from 1996 through 2005. Below, we detail the data-collection process.

A. Data: Loan agreements from Edgar

We begin with a sample of loan deals from *Dealscan* that are matched with firm financial characteristics from *Compustat*. A loan deal may contain more than one loan "tranche". For instance, a typical deal may include a revolving line of credit tranche and two term-loan tranches. As Sufi (2006b) notes, the credit agreement governing each loan deal is drafted at the deal level. A deal-level analysis, as opposed to a tranche-level analysis, is therefore more appropriate when analyzing credit agreements. Our sample includes deals made to non-financial firms, and we require that each deal have information on the amount of the deal and the interest spread of all tranches in the deal. The sample is restricted to deals made from 1996 through 2005. We impose the latter restriction to merge *Dealscan* observations to their electronic contracts contained in *Edgar*. The SEC began requiring firms to file electronically only in 1996; electronic filings are only sparsely available before that date. Once these restrictions are in place, we are left with 9,580 deals (representing 13,715 loan tranches).

From *Compustat*, we construct financial statistics as the average of the four quarters prior to the loan agreement being signed. Cash flow is constructed using *item 21*, scaled by the book value of total assets (*item 44*). The book leverage ratio is long term debt (*item 51*) plus short term debt (*item 45*), scaled by book assets. The market to book ratio is total assets less the book value of equity plus the market value of equity, all scaled by total assets. The book value of equity is the book value of assets less the book value of liabilities (*item 54*) and preferred stock (*annual item 10*) plus deferred taxes (*item 52*). The

market value of equity is common shares outstanding (*item 14*) multiplied by the share price (*item 61*). We include only deals in which the borrower's lagged cash flow, lagged market to book, and the lagged leverage ratio are non-missing.

Dealscan contains no information on capital expenditure restrictions. To obtain this information, we use text-search programs to scan every 10-Q, 10-K, and 8-K filing in *Edgar* for loan contracts. More specifically, we match every firm in *Compustat* to its respective set of SEC filings, and then scan these filings for key search terms that allow us to extract loan agreements. Given SEC reporting requirements, the loan contracts examined in this paper are those deemed to be "material" by the borrowing company.

We extract the actual bank credit agreements from SEC filings by "tagging" each filing to see if it contains an agreement. Our specific tag is whether a line of the filing contains one of the following 10 terms in capital letters: "CREDIT AGREEMENT," "LOAN AGREEMENT," "CREDIT FACILITY," "LOAN AND SECURITY AGREEMENT," "LOAN & SECURITY AGREEMENT," "REVOLVING CREDIT," "FINANCING AND SECURITY AGREEMENT," "FINANCING & SECURITY AGREEMENT," "CREDIT AND GUARANTY AGREEMENT," and "CREDIT & GUARANTY AGREEMENT." If we find one of these 10 terms, we also require the document to contain the search term "TABLE OF CONTENTS" within 60 lines after the initial search terms. This process allows us to extract most original credit agreements, and many of the major amendments and restatements of credit agreements, that are contained in *Edgar*. We match the credit agreement to *Dealscan* based on the date of the loan agreement and the company identifier.[2]

Of the 9,580 deals in *Dealscan*, we are able to successfully match almost 40% to the actual credit agreement from *Edgar* to yield the final sample of 3,720 loan contracts for 1,931 borrowers. In order to understand why the match rate is only 40%, it is instructive to describe how LPC constructs its *Dealscan* data set. *Dealscan* obtains its most detailed observations from SEC filings. LPC follows the filing of SEC documents and continually extracts information from those filings that contain credit agreements.

[2] For less than 5% of deals in *Dealscan*, a borrower has more than one deal signed on the same date. We treat all deals signed on the same date by the same firm as one deal, and we append all credit agreements collected from *EDGAR* for the same firm and the same credit agreement date into one master credit agreement.

But LPC also creates additional *Dealscan* observations through information collected from financial institutions that report "deal flow" directly to LPC. The company uses this information to construct league tables of bank loan deals. Although LPC requires that the financial institutions provide enough information on the loans to verify the accuracy of the information, they do not typically obtain the level of detail available from a copy of the credit agreement. Thus, the level of detail in a *Dealscan* record will tend to depend on whether LPC could find the original credit agreement in an SEC filing.

[TABLE 1]

Table 1 examines the differences in *Dealscan* deals that are matched and unmatched to a loan contract from *Edgar*. The table indicates that the *Dealscan* data quality of unmatched deals is much poorer than matched deals. In particular, data describing collateral, whether a deal has a dividend restriction, and the percentage held by each lender are more likely to be missing for deals that we are unable to match to a contract. The results for collateral and the dividend restriction are particularly striking; these data are missing in *Dealscan* for 44% and 52% of the deals that we are unable to match, respectively, whereas these data are unavailable for only 16% and 7% of matched deals. In addition, financial covenant data are missing for over 42% of unmatched deals, but only 7% of matched deals.[3] Thus, our data set appears to overlap heavily with the set of deals in *Dealscan* for which the LPC is able to obtain the full contract.

Deals that we are unable to match to a contract are also more likely to be sole-lender deals (as opposed to syndicated deals) and revolving credit agreements with maturity of less than 1 year. This latter result may be due to the fact that short term revolving credit facilities are less likely to be considered material contracts, and are less likely to be filed with the SEC.

In terms of any broader sample selection issues, the bottom of Table 1 shows that deals that we are unable to match to a contract are to larger, less levered, and lower cash-flow firms. Only the size

[3] We code financial covenant data as missing if the field "Covenants: Financial Covenants" is listed as "No." Representatives from *Dealscan* maintain that virtually every loan has at least some form of a financial covenant, and that the financial covenant field being set to "No" more accurately reflects the data being unavailable rather than the loan not having a financial covenant. The evidence in Table 1 that almost 95% of matched contracts contain a financial covenant is consistent with this interpretation.

difference is statistically distinct from 0 at the 1 percent level. Moreover, there is no statistically significant difference between the average loan amount or interest spread for matched and unmatched loans. Overall, we do not believe we introduce any significant bias by focusing only on deals that we have matched to contracts.

We also collect data on whether firms violate a financial covenant in the year prior to the loan agreement. Financial covenants are accounting-based risk and performance hurdles that the borrower must meet to be in compliance with the credit agreement. Typical financial covenants include leverage ratios (measures of debt-to-assets or debt-to-cash flow), interest coverage and "fixed charge" ratios (measures of cash-to-interest-type expenses), and measures of net worth (assets – liabilities). The breach of a financial covenant means that the borrower is in technical default on the loan, and that the lender has the *right* to demand immediate repayment of the entire loan. Such a demand can easily trigger a bankruptcy filing because the borrower's other creditors will also typically have the right to demand "acceleration" of payments conditional on the bank debt default. Of course, the bank is not required to accelerate the loan, and can opt to waive the covenant – that is, grant the borrower temporary relief from the covenant -- or renegotiate the contract altogether. Thus, financial covenants serve as "tripwires" to renegotiation that put substantial bargaining power in the hands of the banks.[4]

The SEC requires "companies that are, or are reasonably likely to be, in breach of [financial] covenants [to] disclose material information about that breach and analyze the impact on the company if material (SEC (2003))." We exploit this requirement by searching all company 10-Ks in years immediately preceding a loan agreement to determine whether a company is in violation of a financial covenant. We use text-search algorithms to identify violations using the procedure described in Sufi (2006a).

B. Capital Expenditure Restrictions

[4] See Chava and Roberts (2006) and Sufi (2006a) for more information on financial covenants. Forced renegotiation following financial covenant violations is broadly consistent with hypotheses developed in the incomplete contracts literature. For example, Hart and Moore (1988) hypothesize that "parties can make up for … incompleteness … by building into the contract the mechanism for revising the terms of trade as they each receive information about benefits and costs."

Using the loan contracts that we match to *Dealscan* deals, we collect information on capital expenditure restrictions contained in the agreement. There are a number of interesting control-oriented covenants in private credit agreements, including restrictions on acquisitions, expenditures on non-capital items, and changes in company ownership (Baird and Rasmussen, 2006). However, we restrict our analysis to capital expenditure restrictions for three reasons. First, covenants restricting capital expenditures are relatively straightforward to identify using our search methodologies. Second, a limit on capital expenditures corresponds nicely with how control rights are modeled in the control-based theories of financial contracting, particularly in Dewatripont and Tirole (1994), where creditors retain the right to stop an investment from going forward. Third, the main component of a capital expenditure restriction is "cash" capital expenditures, (*Item 128* in Compustat), the measure most often used in papers on corporate investment policy.[5] Thus, the capital expenditure restriction pertains specifically to what is usually termed "investment" in the corporate finance literature.

Capital expenditure restrictions are usually documented in the section on negative covenants near the end of the credit agreement, and are commonly set as a nominal dollar amount for a given fiscal year. The capital expenditure restriction contained in the June 29th, 2001 loan agreement for Airborne Express, Inc is a typical example:

> Limitation on Capital Expenditures. Capital Expenditures for each Fiscal Year shall not exceed the maximum levels as set forth below opposite such Fiscal Year:
>
Fiscal Year Ended:	Maximum Level
December 31, 2001	$205,000,000
December 31, 2002	$255,000,000
December 31, 2003	$305,000,000

Alternatively, capital expenditure restrictions are sometimes enforced as percentages of performance variables. For example, the loan agreement between American Precision Industries, Inc. and Marine Midland Bank, dated August 31st, 1998 contains the following restriction:

> CAPITAL EXPENDITURES. For any one fiscal year, [the borrower shall not] make or incur aggregate Capital Expenditures in excess of seven and one-half percent (7-1/2%) of the

[5] Capital expenditure restrictions typically cover cash capital expenditures as reported in a company's Statement of Cash Flows plus the capitalized value of new capital leases.

> Company's Consolidated net sales as shown on the Company's audited financial statements for such fiscal year.

It is important to understand that these restrictions are not "boilerplate" covenants; instead, they are tailored specifically to borrowers, and can be very detailed. We provide examples of some of the more detailed restrictions in Section V.

For our full sample of loans, we collect information on whether a loan contract contains a capital expenditure restriction by coding as a restriction any limit on the capital expenditure activities of the firm or any of its subsidiaries. To find such restrictions, we use a text searching algorithm that scans all contracts for the term "capital expenditure." The search program tells us if the term is in the agreement, which we then further examine to confirm whether the agreement has a capital expenditure restriction.

For firms that have a capital expenditure restriction, a fiscal year ending in December, and a specific dollar restriction on aggregate capital expenditures defined over the fiscal year, we also collect the actual capital expenditure restriction amount for the first year reported in the loan agreement. We isolate this subset of firms in order to accurately measure the timing and amount of the restriction.[6] This subset includes 483 deals.

C. Summary statistics

[TABLE 2]

Table 2 contains the summary statistics for the sample of 3,720 private credit agreements signed by 1,931 borrowers. The first statistic is also one of our main results: 32% of the agreements contain an explicit restriction on capital expenditures. Across agreements with a capital expenditure restriction for which we gather the restricted amount, the average level of the restriction measured relative to lagged assets is 9.0%. The average capital expenditures in the year of the agreement for these firms, also measured relative to lagged assets, is 6.4%. The average loan deal amount is $450 million, which

[6] Actual restrictions often have rollover provisions that permit some portion of unused annual limits to be carried over to the following fiscal year. To avoid the effect of accumulating limits, we focus on the first year. We use only firms with fiscal year end in December for two reasons. First, the restrictions are sometimes given in terms of calendar years, which would make calculation difficult for firms with alternative fiscal year ends. Second, if a firm has a non-December fiscal year end, it is difficult to ascertain to which fiscal year the restriction applies.

represents 34% of book assets. While this may appear large, it is important to understand that over 94% of the deals contain revolving credit facilities, 2/3 of which typically remain unused (Sufi (2006a)). In terms of the borrower's performance in the year before the loan agreement is signed, cash flow averages 3.4% of total book assets, and 6% of firms have violated a financial covenant within the past year. Almost 50% of deals in the sample are made to rated firms, which confirms the fact that most firms with access to public debt markets also utilize bank loans. Conditional on having a credit rating, 2% of firms in our sample have a rating of CCC or below. In other words, very few of the borrowers in our sample are in, or very near bankruptcy.

II. Theoretical Framework

The results in Table 2 suggest that investment restrictions are a common component in the private credit agreements of publicly traded corporations. The restrictions imply that, as part of the agreement with borrowing firms, creditors often assign themselves explicit control rights over firm investment policy. In this section, we motivate our empirical analysis by examining why creditors should care about control rights, and why price and quantity mechanisms might, by themselves, be insufficient in optimal financial contracts.

Jensen and Meckling (1976) provide the basis for control-based theories of financial contracting by considering optimal mechanisms for mitigating agency costs of debt. They argue that explicit covenants against risk-increasing investments can lower financing costs as long as the cost of abiding by the restriction does not exceed the savings from lower interest payments. Aghion and Bolton (1992) and Dewatripont and Tirole (1994) extend this intuition, drawing on the idea that incomplete financial contracts require some rule for allocating control rights across events that are not covered in the contract. They demonstrate that optimal contracts within this framework may shift control rights from one party to another, conditional on the outcome of some observable signal.

In Aghion and Bolton (1992), a wealth-constrained owner-manager seeks capital to finance company projects that produce both cash profits and managerial private benefits. In their model, contracts are written so that control rights optimally shift from the manager to the investor when private

benefits are most likely to distort the manager into inefficient decisions.[7] The result relies on a verifiable signal that is correlated with the externality created by the manager's private benefits. Even though control by investors may itself be inefficient, there are states when investor control is more efficient than continued manager control. Their most prominent example is when one of the potential states involves low monetary benefits and high managerial private benefits. In this situation, managers have excess continuation bias. If a noisy contractible signal implies that it is more likely that such a state has been realized, creditors will optimally take control of firm investment policy.

Dewatripont and Tirole (1994) offer a theory in which shifts in control rights serve as a deterrent to discourage *ex ante* managerial misbehavior. Again assuming a manager with private benefits of control, the threat of losing control serves as a disciplining device, and the optimal contract shifts control away from managers when there is evidence of an inefficient effort choice. In order to encourage investors to implement the optimal change of control, control rights are correlated with cash flow rights. In fact, the optimal capital structure includes debt-like financing, where concave cash-flow rights encourage debt-holders to acquire control rights after signs of poor performance. Thus:

> Proper managerial incentives require outsiders to go against the managers' will only when it is likely that they have engaged in suboptimal courses of action. Poor performance is thus followed by a high probability of external interference [by creditors], while good performance is rewarded by a low probability of external interference (p. 1049, Dewatripont and Tirole (1994))

The model by Dewatripont and Tirole (1994) is related to our empirical analysis because "external interference" involves giving the creditor the right to decide whether or not an investment should proceed (See their Table I and description of the action space on page 1031).

A general point concerning these three models is that control shifts are triggered by the realization of a verifiable signal, which may or may not be restricted to simple payment defaults. In fact, the framework of Jensen and Meckling (1976) implies that creditors should exert control well before bankruptcy in order to mitigate managerial risk shifting. Likewise, in Aghion and Bolton (1992), managers receive monetary returns even after creditors obtain control rights, making their model more

[7] Wealth constraints are the common friction used to prevent the Coase theorem from applying in control-based theories of debt. Such a constraint is natural in a model of external finance.

applicable to debt covenant violations for solvent firms rather than liquidations following bankruptcy (see the discussion in their conclusion).

[TABLE 3]

Table 3 summarizes the main aspects of these three models, and how they apply to our empirical setting. As the table suggests, the agency problems used in the three models to motivate creditor control are distinct. Despite using different assumptions, these three models have two common hypotheses that we use to motivate our empirical analysis. First, the allocation of control rights between management and creditors should be an important part of observed financial contracts. Second, negative performance by the borrower should increase the likelihood that control rights shift to creditors. We use our sample of private credit agreements between public firms and banks to empirically examine these two hypotheses.

A further question regarding the second hypothesis is whether the models predict that contracts themselves must be literally performance-contingent to resolve the underlying agency problem, or whether it is sufficient for borrowers to understand that if they perform poorly, they must relinquish control when they sign future or renegotiated contracts. Part of the reason for the ambiguity is that the control-based theories abstract from maturity considerations; shorter maturity can be used by creditors to effectively make future contract terms contingent on observed future performance, even if the original contract terms are not explicitly performance-contingent. As the fifth line of Table 3 suggests, we believe that a literally performance-contingent contract is necessary only to resolve the *ex ante* managerial moral hazard problem in Dewatripont and Tirole (1994). In their framework, if creditors wait until after the negative performance is realized to threaten investment restrictions in a future contract, then the ability to renegotiate may mitigate the *ex ante* incentive effects of the punishment mechanism. One of our goals is to shed light on whether private credit agreements contain the type of contingent-control language that is hypothesized in Dewatripont and Tirole (1994), or whether creditors use alternative instruments to effectively make future contract terms contingent on future borrower performance.

III. The Widespread Use of Capital Expenditure Restrictions

The most direct implication of control-based theories of financial contracting is that the allocation of control rights over firm investment policy should be an important feature of observed financial contracts. In this section, we find support for this hypothesis by documenting the widespread use of capital expenditure restrictions in private credit agreements.

[TABLE 4]

Table 4 documents that 32% of the credit agreements in our sample contain an explicit restriction on capital expenditures. At the borrower level, this proportion translates to over 42% of the sample firms having a capital expenditure restriction at some point between 1996 and 2005. Over roughly the same period, Sufi (2006a) shows that over 80% of public firms in the *Compustat* universe utilize private credit agreements in the form of bank lines of credit. These two statistics imply that (0.42*0.80 =) 34% of *Compustat* firms are likely to face a capital expenditure restriction in a debt contract. Capital expenditure restrictions are common across industries, outside of agriculture. In particular, about 40% of credit agreements obtained by borrowers operating in the retail trade, wholesale trade, and services industries contain a restriction, while roughly one-third of credit agreements to manufacturing borrowers have the restriction. Across size cateogires, restrictions are more common for small firms, but a substantial fraction of firms with over $1 billion in book assets also have agreements containing capital expenditure restrictions.

Conditional on having an S&P corporate credit rating, only 6% of agreements obtained by investment grade borrowers have a capital expenditure restriction. Among junk borrowers, the fraction of agreements with a restriction is 44%. The large difference in the incidence of capital expenditure restrictions among loans to junk and investment grade borrowers is evidence that negative borrower performance shifts control of investment policy to creditors, something we explore further in Section IV.

Although capital expenditure restrictions are more common on loans to borrowers of lower credit quality, Table 4 shows that capital expenditure restrictions are not exclusively associated with bankrupt firms. For example, 39% of credit agreements obtained by firms with a BB rating have a capital expenditure restriction, and on average, less than 1 in 100 of these borrowers default over a one year

horizon, according to Moody's historical default probability tables. Capital expenditure restrictions are correlated with borrower performance, but they are not a restriction used exclusively on loans to borrowers that are in or near bankruptcy.

IV. Capital Expenditure Restrictions and Borrower Performance

A. Empirical methodology

In Section III, we document that investment restrictions are an important part of private credit agreements. In this section, we examine the second main implication of control-based theories of financial contract: namely, that the imposition of capital expenditure restrictions by creditors is more likely after negative borrower performance. We use three proxies for performance in order to empirically evaluate this hypothesis. First, we use the borrower's average cash flow scaled by assets in the four quarters prior to the loan origination. Second, we use whether the firm has experienced a financial covenant violation in the previous four quarters. As we discuss below, financial covenant violations are an especially close empirical analog to the performance measures in control-based theories of financial contracting. Finally, we use the borrower's S&P corporate credit rating as a measure of performance. The drawback of the credit rating measure is that it is only available for rated firms, which comprise only 49% of our sample.

Given that control-based theories of financial contracting focus on how within-firm changes in performance affect the allocation of control rights, our empirical strategy seeks to estimate how within-firm changes in these three performance measures affect the imposition of a capital expenditure restriction. Our preferred specification is a borrower fixed effects linear probability model, in which the borrower-specific, time-invariant error component is explicitly estimated. More formally, we estimate:

$$\Pr(capexrestriction_{it} = 1) = \alpha_i + \alpha_t + \beta * Perf_{i,t-1} + \Gamma X_{i,t-1} + \varepsilon_{it} \qquad (1)$$

Each observation represents a private credit agreement obtained by firm *i* in year *t*. The independent variables are measured as the average values of the 4 quarters prior to the loan origination. The coefficient of interest is β, which represents the linear estimate of the effect of within-firm changes in

performance on the probability of having a capital expenditure restriction in the loan agreement. The matrix *X* includes the following control variables: the market to book ratio, the leverage ratio, the natural logarithm of total assets, indicator variables for the type of loan tranches in the agreement {all lines, all term, or mix}, and indicator variables for the purpose of the loan {acquisitions, commercial paper back-up facility, refinancing, or general corporate purposes}. Reported standard errors are clustered by firm. The disadvantage of using borrower fixed effects to estimate (1) is that we are forced to limit the sample to borrowers that have more than 1 credit agreement in our sample. There are 978 borrowers with only 1 credit agreement in our full sample. We also estimate (1) using pooled regressions to obtain partial correlations for the full sample, but we rely on the fixed effects estimates when discussing magnitudes.

We acknowledge that there are well-documented drawbacks to using linear probability estimation. While non-linear maximum likelihood estimation such as a logit or probit specification is often preferable to linear probability models in cross-sectional analyses, we prefer the linear probability specification in a panel data setting. As is well known, probit estimation suffers the incidental parameters problem if one attempts to explicitly estimate the firm-specific time invariant error component (Bester and Hansen (2006)). While a conditional logit estimator allows for estimates of coefficients using a conditional density for each firm, it is difficult to assess the average partial effect of a covariate across the entire sample. Given the drawbacks of non-linear maximum likelihood techniques in a panel setting, we believe fixed effects linear probability estimation is the best methodology for obtaining average partial effects while exploiting within-firm changes in covariates. Recent work by Fernandez-Val (2005) suggests that any bias associated with fixed effects linear probability estimation may be minimal. In any case, our results are qualitatively similar when using a conditional logit model.

B. Results

[TABLE 5]

Table 5 presents the coefficient estimates. The fixed effects estimate in column 1 shows that a drop in cash flow leads to an increase in the probability of having a capital expenditure restriction in the agreement. The magnitude suggests that a drop from the 75th percentile to the 25th percentile of the cash

flow distribution leads to a (0.025*2.3 =) 6 percentage point increase in the probability of having a capital expenditure restriction, which is a (0.06/0.32 =) 19% increase, measured at the mean. The coefficient estimate is statistically distinct from zero at the 1% level. The coefficient estimate in column 2 implies that a financial covenant violation in the year preceding the loan origination leads to a 14 percentage point increase in the probability of having a restriction on capital expenditures in the loan agreement, which is a (0.14/0.32 =) 44% increase at the mean. Column 3 reports estimates from regressions performed on the sample of rated borrowers, and the estimates imply a strong effect of credit downgrades on the probability of having a capital expenditure restriction. For example, a firm that is downgraded from A or above to BB experiences a 23 percentage point increase in the probability of having a capital expenditure restriction, which is a (0.23/0.32 =) 72% increase at the mean.

Columns (4) through (6) report estimated coefficients from pooled regressions that allow us to utilize the entire sample, as opposed to only firms with 2 or more credit agreements. The results are similar: firms that perform poorly are more likely to have a capital expenditure restriction in their new credit agreement. The estimate of the linear effect of a financial covenant violation is almost identical, whereas the estimates on cash flow and ratings are slightly smaller.

Overall, the results in Table 5 provide support for one of the primary implications from control-based theories of financial contracting. In response to poor firm performance, creditors are more likely to place explicit restrictions on firm investment policy. As we further explore in Section V, this result suggests that the well-documented positive correlation between firm performance and investment may be explained in part by the fact that poorly performing borrowers are more likely to have investment restrictions in their private credit agreements.

C. The relative importance of capital expenditure restrictions

Table 5 suggests that the incidence of a capital expenditure restriction is highly responsive to negative firm performance. In this section, we explore how other contract terms respond to negative performance in order to understand the relative importance of shifts in control over investment. We focus

specifically on capital expenditure restrictions versus interest spreads, whether a loan is secured, and whether a loan contains a dividend restriction.[8]

[TABLE 6]

Table 6 reports the unconditional correlations of the four contract terms we examine, and documents that the terms are all positively correlated. In particular, a capital expenditure restriction is most highly correlated with the interest spread and whether the loan is collateralized. The interest spread is more highly correlated with whether the loan is secured than whether the loan has a capital expenditure restriction. Interestingly, dividend restrictions are least correlated with capital expenditure restrictions. Dividend restrictions are also relatively ubiquitous, appearing in 80% of the contracts in our sample. Taken together, these statistics indicate that creditor control rights over dividend policy, while important, likely restrict direct wealth transfers to shareholders rather than influence firm investment decisions.

[TABLE 7]

In Table 7, we report estimated coefficients from fixed effects regression specifications similar to (1), except we separately replace the capital expenditure restriction dependent variable with the natural logarithm of the interest spread (in basis points), an indicator of whether or not the loan is secured, and an indicator of whether or not the loan contains a dividend restriction. The estimated coefficients in columns 1 and 2 show that negative firm performance results in higher interest spreads, although the effect of a financial covenant violation is not statistically distinct from zero at a reasonable confidence level. Likewise, columns 3 and 4 show that negative performance results in a higher probability of a loan being secured, although again the effect of a financial covenant violation is not statistically distinct from zero at a reasonable confidence level. The effect of negative performance has a weaker effect on the probability of a dividend restriction, both in terms of the magnitude and statistical significance of the estimates. The latter result is consistent with the argument that blatant wealth transfers are always a threat to creditors, regardless of how the firm is performing.

[8] In unreported results, we also examine how negative performance affects maturity and the dollar loan amounts. We find weak evidence that loan amounts are positively correlated with cash flow, but generally find that performance has no effect on loan maturity or amount.

A main goal of this subsection is to document the *relative* importance of negative firm performance on various contract terms. It is not surprising that interest rates and the probability of a loan being secured both increase when the firm performs poorly. A standard risk-return framework generates this prediction. What is more informative is the fact that the effect of negative performance on the allocation of control rights is comparable or even larger than the effect of negative performance on interest spreads or the pledging of collateral.

[FIGURE 1]

Figure 1 presents the evidence. It is constructed from the coefficient estimates in Tables 5 and 7, and it shows how within-firm negative performance changes the contract term in question, where the effect is stated as the percent change at the mean of the left hand side variable.[9] The left panel examines how a drop in the cash flow of a firm from the 90th to the 10th percentile of the distribution affects the contract terms, whereas the right panel of Figure 1 examines how a financial covenant violation affects contract terms. For example, a drop in a borrower's cash flow from the 90th to the 10th percentile results in a 38% increase in the likelihood of having a capital expenditure restriction at the mean. Figure 1 demonstrates that the effect of a large drop in cash flow has a similar magnitude effect on the probability of a capital expenditure restriction or the interest spread, but a weaker effect on the probability that a loan is collateralized or contains a dividend restriction. Figure 1 also shows that the effect of the financial covenant violation has a substantially larger effect on the probability that a loan contains a capital expenditure restriction than the effect on other contract terms. In particular, a financial covenant violation increases the incidence of having a capital expenditure restriction by almost 45% at the mean; the effect on the interest spread is only 15% at the mean.

[FIGURE 2]

Figure 2 shows similar results using credit ratings. The omitted group is firms rated A or higher, and the graphs show the marginal effect of rating downgrades on various contract terms, where the effects

[9]There is one exception. Figures 1 and 2 reflect estimated coefficients from regressions using the interest spread in basis points as the left hand side variable, as opposed to the natural logarithm of the interest spread.

are stated as percent changes at the mean of the left hand side variable. The slope of the downgrade effect is steeper for capital expenditure restrictions than for collateral or dividend restrictions. A downgrade from A or above to BB leads to a 70% increase in the incidence of a capital expenditure restriction at the mean; a similar downgrade leads to a 45% increase in the interest spread. The differences are particularly sharp when firms are rated CCC or worse, which is consistent with the notion that control shifts to creditors as firms approach bankruptcy.

D. Contingency

In this sub-section, we explore the degree to which creditor control over firm investment policy is explicitly contingent on borrower performance in credit agreements, as is hypothesized by Dewatripont and Tirole (1994).

Our first finding is that the vast majority of contracts do not specify an explicit contingency. That is, we rarely find contract provisions that specify that a capital expenditure will be imposed if the borrower's performance deteriorates.[10] When we do find such a contingency, it is actually the mirror-image of that discussed in theory. That is, the capital expenditure is imposed at the outset of the contract, but the contract promises to remove the restriction conditional upon *good* borrower performance. Thus, while such contingencies appear feasible, lenders do not specify that control will shift conditional on *bad* borrower performance. Instead, observed agreements tend to either restrict or not restrict capital expenditures.

The lack of explicit contractual contingencies does not imply that contingent control shifts do not occur. One contingency arises from the fact that observed debt contracts always specify an exact maturity date. Given that outstanding debt must be repaid at a specified date, creditors can use the maturity of the loan to revise contract terms in response to negative borrower performance.[11] Indeed, the results in Table

[10] This stands in contrast to VC contracts; Kaplan and Strömberg (2003) show that is common for these contracts to specify explicit control-oriented contingencies that occur in reaction to bad performance.

[11] Berglöf and von Thadden (1994) study the benefits from issuing short-term versus long-term debt in the presence of incomplete contracts. However, they otherwise assume that creditors can only gain control from payment defaults (see also Flannery (1986) and Diamond (1991)). We are not aware of any theoretical research that explores

5 demonstrate that capital expenditure restrictions are more likely to be imposed in response to negative borrower performance. Given that all of our sample borrowers are public firms with at least four quarters of history before the loan begins, many of our sample contracts represent refinancing of prior loans.

A contingency more in line with the spirit of Dewatripont and Tirole (1994) is creditors' use of financial covenants. As mentioned above, financial covenants are performance-based limits contained in almost all private credit agreements. A borrower that violates a financial covenant is in "technical default" of the credit agreement, and the agreements give the lender the right to accelerate the outstanding loan in response to a technical default. Technical defaults typically lead to a renegotiation of the loan agreement in which creditors use their acceleration right to extract amendment fees and to impose harsher terms in the new renegotiated agreement. It is important to note that financial covenant violations are not uncommon incidents: Chava and Roberts (2006) report that 30 to 40% of firms in their sample violate a net worth or current ratio covenant. Sufi (2006a) reports that 1 in 3 firms with a bank line of credit violate a financial covenant at some point between 1996 and 2003.

[TABLE 8]

Table 8 documents how creditors use financial covenants to make capital expenditure restrictions contingent on borrower performance. We isolate the sample to "contingent-contract" pairs, where each pair represents two credit agreements between the same borrower and the same lender, the borrower has violated a financial covenant before the new credit agreement, and the origination date of the new credit agreement is before the maturity date of the old credit agreement. The new credit agreement therefore likely represents a renegotiated loan agreement following a technical default by the borrower. Column 1 of Table 8 reports the fraction of original and renegotiated agreements that contain capital expenditure restrictions. Just over 37% of credit agreements obtained before the financial covenant violation contain a capital expenditure restriction, whereas over 60% of the renegotiated agreements following the violation contain a capital expenditure restriction. That is, the renegotiated agreement after the covenant violation

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the trade-off between creditors' use of explicit contingencies versus the use of maturity when contracts are incomplete. Perhaps the closest model is Hart and Moore (1998).

is almost twice as likely to contain a restriction on capital expenditures. Table 8 also shows that the renegotiated loan agreement has an interest spread that is 30% higher than the original loan agreement. Creditors are also more likely to impose collateral and dividend restrictions in the renegotiated agreements, but the difference is not statistically distinct from 0 at a reasonable confidence level. This is partly due to the fact that over 80% of agreements prior to the covenant violation are already secured and contain a dividend restriction.

The evidence in Table 8 suggests that credit agreements are very much contingent on future performance through the creditors' use of financial covenants. In response to a financial covenant violation, the creditors are more likely to impose an explicit restriction on capital expenditures. This contingency is a close empirical analog to the contingent contracts envisioned in Dewatripont and Tirole (1994).

V. The Effect of Restrictions on Firm Investment

We now turn to the question of whether capital expenditure restrictions actually matter for firm investment policy. Before moving to the formal analysis of the relationship between investment and capital expenditure restrictions, we emphasize two facts that suggest that the restrictions are relevant for firm investment policy. First, the results in Section IV suggest that the introduction of capital expenditure restrictions is not random—it is systematically correlated with negative firm performance. While control-based theories of financial contracting provide a strong theoretical framework for explaining why binding investment restrictions would be imposed on firms that exhibit negative performance, there is no framework, to our knowledge, that would predict banks putting irrelevant capital expenditure restrictions on firms in response to negative performance. Simply stated, theory strongly suggests that the restrictions are imposed with the intent of actually limiting investment.

Second, we can infer the relevance of the restrictions by simply noting the level of detail associated with the restrictions in the contracts. As an example, the April 19, 2002 credit agreement for The Chalone Wine Group, Ltd. contains a capital expenditure restriction that specifically limits the amount that Chalone could spend on the purchase of wine barrels during the 2002 fiscal year. Non wine-

barrel capital expenditures were restricted separately. Similarly, the March 27, 1997 revolving loan agreement for casino operator Hollywood Park, Inc. (now Pinnacle Entertainment, Inc.) contains the following set of detailed restrictions:

> Capital Expenditures. [Borrower shall not] Make, or become legally obligated to make, any Capital Expenditure except:
>
> (a) Maintenance Capital Expenditures not in excess of (i) $15,000,000 for the Fiscal Year ending December 31, 1997, (ii) $15,000,000 for the Fiscal Year ending December 31, 1998 and (iii) $20,000,000 for any subsequent Fiscal Year;
>
> (b) Capital Expenditures to the extent financed by Indebtedness permitted under Section 6.9(h);
>
> (c) Capital Expenditures for the construction of approximately 200 additional hotel rooms, a restaurant, an entertainment lounge, meeting rooms, retail space and parking facilities at the Reno Property not in excess of $25,000,000;
>
> (d) Capital Expenditures for the construction of buffet and restaurant facilities at the New Orleans Property not in excess of $10,000,000;
>
> (e) Capital Expenditures for the purchase of capital assets which, as of the Closing Date, are leased by Borrower or any Restricted Subsidiary from other Persons pursuant to operating leases not in excess of $8,000,000; and
>
> (f) Capital Expenditures not otherwise permitted above which, when added to all other Basket Expenditures theretofore made, do not exceed $40,000,000.

Imposing such meticulous restrictions requires time and expense; it is difficult to see why banks would include such a covenant unless it provides a real constraint that also adds significant value to the contracting parties. Such examples are common in credit agreements and suggest that creditors often play an important role in the investment choices of their borrowers.

We now take a more formal look at whether capital expenditure restrictions influence investment policy. Documenting a causal effect of a capital expenditure restriction on actual investments is a difficult task for three reasons. First, observed contracts are the outcome of bilateral negotiations. Borrowers have the choice of accepting or rejecting a contract with such restrictions in place, and may choose to accept only those contracts in which restrictions are set at planned capital expenditure levels. Under this interpretation, firms that choose freely to stay at or below their planned expenditures would be difficult to distinguish from firms that are constrained to remain below their restricted amount. Second, the restrictions can be waived or renegotiated at the discretion of the lender. Indeed, creditor control

implies that the lender has the option to reject or accept a borrower request to invest in capital expenditures beyond the restriction amount; the restrictions do not necessitate that the lender must reject all requests. Thus, unobservable waivers and amendments to restrictions make it more difficult to ascertain the "bite" in the restrictions. Third, because our results show that capital expenditure restrictions are imposed on firms that exhibit negative performance, and it is well documented that negative performance is correlated with reductions in capital expenditures, we would likely expect a reduction in capital expenditures even in the absence of the restriction. More formally, identifying the causal effect of the restriction involves isolating an exogenous source of variation in the introduction of a capital expenditure restriction. The primary source of variation we identify in Section IV is firm performance, which is clearly not exogenous to investment.

We use two sets of empirical techniques to overcome these identification problems. First, we exploit the actual restriction amount, and show that borrowers tend to cluster just below the restriction amount in the year after the loan origination. Second, we conduct difference-in-differences tests comparing the changes in expenditures through time of firms obtaining contracts with and without capital expenditures.

Our most compelling evidence comes from examining the distribution of actual capital expenditures relative the amounts restricted by contract. As mentioned in Section I, we collect the exact capital expenditure limit in the loan agreement for a sub-sample of 483 loans in our sample. We use these data to compare the borrower's actual capital expenditures (annual *item 128* in Compustat) to the contractual limit. Specifically, we compute the difference between actual capital expenditures and the contractual limit, and scale the result by lagged total assets (lagged annual *item 44* in Compustat). Values below zero indicate actual expenditures were below the limit, and values above zero indicate that actual expenditures exceeded the limit.[12]

[12] There are several reasons why actual expenditures may be above the limit. First, as mentioned above, firms can obtain waivers to most contractual provisions, including the capital expenditure restriction. Second, most contracts contain rollover provisions that permit some portion of 'unused' expenditures to be spent the following year. While we attempt to identify new contracts that apply to a specific fiscal year, we likely include outstanding contracts

[FIGURE 3]

Figure 3 plots a histogram of this difference along with a continuous normal distribution with identical mean and standard deviation (the smooth black line). The noticeable kink in the empirical distribution around zero suggests that exceeding the limit is costly. Although roughly half of the firms fall within 2% of assets *below* the limit, only 12% of firms are within 2% of assets *above* the limit. Even if a value of zero corresponded to the modal planned expenditures among these borrowers, we would expect that, without the restriction, deviations from this mode would lie equally above and below the restriction. Using the normal distribution distribution, we can test the hypothesis that the spike in observations just below zero is statistically significant. Specifically, under the null hypothesis that the data are drawn from the estimated normal distribution, the number of observations in the bin (-1%, 0%] is distributed as a binomial random variable with success probability given by the difference in the estimated cumulative distribution function evaluated at 0% and at -1%. While 30% of the observations are in this bin, the expected number under the null hypothesis is only 16%, and the probability of observing so many observations in that bin is less than 0.1% under the null hypothesis.[13] Similarly, the probability of observing only 12% of the observations in the bin (0%, 2%] is 0.2%. Combined, as compared with the estimated parametric distribution, we have statistical evidence that a significant portion of firms seem to move their actual capital expenditures from above the limit to just below the limit. We view this as evidence that exceeding the contractual limit is costly.

[FIGURE 4]

Rather than considering a parametric distribution as the counterfactual measure of the difference between actual capital expenditures and the contractual limit, Figure 4 provides a comparison with capital expenditures from the prior year to the loan contract. Here we limit the sample to the subset of 81 loans

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where the firm has some rollover capacity. Finally, contractually defined capital expenditures may differ from the accounting definition used in financial statements. For example, capitalized leases are often included in the contractual limit but accounted for separately from other capital expenditures. While we try to exclude unique definitions of capital expenditures, our measures undoubtedly contain some noise.

[13] While we can find a slightly better fitting parametric distribution, the conclusions are qualitatively robust to most standard two parameter distributions.

where we can identify that the immediate prior sample loan did not contain a restriction, so actual capital expenditures from the prior year are more likely unrestricted by a contractual limit. Two results are relevant. First, in the year of the agreement in which the restriction applies, the clustering of observations just below the limit is more pronounced for this sample than for the broader sample underlying Figure 3. Here, over 60% of the observations are within 2% of assets just below the threshold, suggesting that the limit is most binding immediately after it is first introduced. Second, actual expenditures in the prior year appear to have little relation with the contractual limit imposed subsequently. Most striking is that 41% of the firms actually exceeded the limit in the prior year, and we cannot reject the hypothesis that firms are equally likely to be above or below the limit in the prior year. Conversely, in the year of the agreement, only 7% exceed the limit, and we can strongly reject the hypothesis that current year expenditures exceed the limit with 50% probability.

[TABLE 9]

Finally, in Table 9, we examine the broader sample and compare annual changes in capital expenditures for firms with and without a capital expenditure limit using difference-in-differences methods. Panel A in Table 9 shows unconditional means of capital expenditures divided by lagged assets around the year in which the credit agreement is signed. We compare agreements that do not contain a restriction, agreements with a restriction, and agreements with a restriction where the previous agreement does not contain a restriction. The latter group is analogous to the sample covered in Figure 4 and provides a rough indicator of a new restriction. In all three groups, capital expenditures decrease monotonically over the four-year period, beginning two years prior to the agreement and ending one year after, primarily reflecting the calendar time period covered in our sample. However, the groups with contractual restrictions experience significantly larger declines. Comparing the year before with the year after, expenditures fall by an extra 0.4% of lagged assets when the credit agreement contains a restriction and by an extra 1.4% of lagged assets when the agreement introduces a new restriction. Both estimates are statistically significantly different from zero at the 10% level, and significantly different from each other. The evidence is similar when comparing two-year averages before and after the contract.

The first two columns of Panel B replicate the above results in a regression framework. The slight differences in the magnitude of the coefficients reflect the inclusion of calendar year dummy variables. Columns 3 and 4 incorporate changes in firm cash-flow and Q into the first-difference regression. Both performance measures are significantly related to capital expenditures and slightly reduce the magnitude and statistical significance of the capital expenditure variables. This is consistent with the results in Table 5 that the introduction of a capital expenditure restriction is highly correlated with cash flow. Even after controlling for these measures of performance, the point estimates in column 3 imply that capital expenditures drop by 0.1% of lagged assets more when firms have a capital expenditure restriction imposed in a loan agreement and by an additional 0.8% of lagged assets when the restriction is first introduced. The latter estimate is statistically distinct from zero at the 10% confidence level.

In sum, we find compelling evidence that capital expenditure restrictions affect firm investment policy. Our results suggest a direct contracting channel through which financing affects investment. To our knowledge, this is the first study to document such a channel.

VI. Conclusion

This paper provides novel empirical evidence of a direct contracting channel through which firm financial policy affects firm investment policy. We focus on private credit agreements between banks and publicly-traded U.S. corporations, and we document that creditors regularly impose explicit restrictions on capital expenditures. These restrictions are more likely to be put in place after negative borrower performance, and the elasticity of a capital expenditure restriction with respect to borrower performance is larger than the elasticities of other contract terms, such as the interest spread or a collateral requirement. Taken together, these results fall closely in line with control-based theories of financing, which imply that creditors retain control rights over investment policy as a second-best solution to agency conflicts (Jensen and Meckling (1976), Aghion and Bolton (1992), and Dewatripont and Tirole (1994)).

In addition, we provide compelling evidence that restrictions on capital expenditures contained in private credit agreements constrain firm investment policy. This result suggests that there is a direct contractual link between financing and investment which goes beyond indirect financing frictions such as

collateral constraints or debt overhang. It also suggests that the common interpretation in the literature that external financing constraints lead to inefficiently low investment may be incorrect. Instead, our findings suggest that restrictions on investment may be a second-best solution that constrains otherwise inefficient investment.

We motivate future research by recognizing the main limitation of our analysis: we do not take an empirical stand on the *ex post* efficiency of investment outcomes in the presence of capital expenditure restrictions. That is, we do not attempt to measure whether restrictions bring firms closer to the first-best level of investment in which incentives are perfectly aligned. This is a fundamentally important question since understanding the efficiency of investment outcomes in the presence of creditor restrictions on capital expenditures would allow us to quantify the broader implications of our findings on aggregate investment.

We believe that there are two methodologies for addressing the *ex post* efficiency of investment in the presence of contractual restrictions. First, one can examine the degree to which borrowers are able to credibly convey information on profitable investments in order to renegotiate capital expenditure limits. That is, how well can information on the quality of investment be transmitted and how easily can contract terms be renegotiated *ex post*? This type of analysis would require detailed information on the process by which banks and borrowers amend credit agreement covenants.

Second, an analysis of how external equity-holders value credit agreements with and without capital expenditure restrictions would shed some light on whether the effect of restrictions on overall firm value is positive or negative. The theories of Aghion and Bolton (1992) and Dewatripont and Tirole (1994) suggest that external equity-holders benefit from the limitation of managerial agency problems, whereas Jensen and Meckling (1976) suggest that investment restrictions could reduce the value of external equity claims by limiting managers' ability to shift value from creditors to equity-holders. Identifying the independent effect of a capital expenditure restriction is a difficult task since credit agreements likely create additional news that might affect share prices. Nonetheless, preliminary evidence on share price effects of credit agreement announcements suggests that equity-holders *benefit*

from capital expenditure restrictions. More specifically, using the same sample employed in this paper, we find that cumulative abnormal returns from one month before to one month after the announcement of a credit agreement are 2 to 3% higher when the agreement contains a capital expenditure restriction. A more rigorous analysis of this result is material for future research, but the unconditional correlation suggests that equity-holders benefit from creditor-imposed investment restrictions.

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Table 1
Comparison of Deals Matched and Not Matched to a Credit Agreement

This table compares loan deals in LPC's *Dealscan* that we are able to match to a credit agreement from the SEC's Edgar database of company filings to those that we are unable to match. Tests for the difference in means allow for errors to be correlated for the same borrower.

	(1) Not matched to a contract	(2) Matched to a contract
Number of deals	5,860	3,720
Dealscan data quality		
Secured data not available {0,1}	0.439	0.162*
Financial covenant data not available {0,1}	0.421	0.069*
Percentage held by lenders not available {0,1}	0.718	0.567*
Dividend restriction data not available {0,1}	0.523	0.074*
Type of loan		
Sole-lender loan {0,1}	0.308	0.181*
Revolver with maturity of < 1 year {0,1}	0.234	0.096*
Borrower characteristics		
Total assets ($M)	2,005	1,622*
Book debt/total assets	0.295	0.301
Cash flow/total assets	0.030	0.034
Loan characteristics		
Loan amount ($M)	407	450
Interest rate spread (basis points)	177	170

*Statistically distinct from "not matched" category at 1% level

Table 2
Summary Statistics

This table presents summary statistics for the sample of 3,720 credit agreements to 1,931 borrowers.

	Mean	Median	St. Dev.	N
Capital expenditure restrictions				
Capital expenditure restriction$_t$ {0,1}	0.319	0.000	0.466	3,720
Restriction, stated as capital expenditures$_t$ /assets$_{t-1}$	0.090	0.057	0.143	483
Capital expenditures$_t$ /assets$_{t-1}$	0.064	0.037	0.087	483
Other loan characteristics				
Agreement amount$_t$ ($M)	450	200	985	3,720
Agreement amount$_t$ / assets$_t$	0.338	0.245	0.308	3,720
Intrerest rate spread$_t$ (basis points above LIBOR)	170	150	119	3,720
Agreement is secured {0,1}	0.647	1.000	0.478	3,117
Agreement contains dividend restriction {0,1}	0.813	1.000	0.390	3,446
Agreement contains a line of credit/revolver {0,1}	0.938	1.000	0.241	3,720
Borrower characteristics				
Cash flow$_{t-1}$ / assets$_{t-1}$	0.034	0.034	0.026	3,720
Financial covenant violation within past year$_{t-1}$	0.063	0.000	0.242	3,720
Total assets $_{t-1}$ ($M)	1,622	674	1,974	3,720
Market to book ratio $_{t-1}$	1.768	1.426	1.136	3,720
Book leverage ratio$_{t-1}$	0.301	0.288	0.193	3,720
Firm has a corporate credit rating $_{t-1}$ {0,1}	0.490	0.000	0.500	3,720
Conditional on borrower having credit rating				
Credit rating (1 = AAA or AA, 2 = A, 3 = BBB …)	3.502	3.000	1.066	1,822
Junk rated {0,1}	0.482	0.000	0.500	1,822
AAA, AA rated {0,1}	0.016	0.000	0.127	1,822
A rated {0,1}	0.160	0.000	0.367	1,822
BBB rated {0,1}	0.341	0.000	0.474	1,822
BB rated {0,1}	0.289	0.000	0.454	1,822
B rated {0,1}	0.172	0.000	0.378	1,822
CCC rated or worse {0,1}	0.020	0.000	0.141	1,822

Table 3
Theoretical Setting and Hypotheses Compared to Our Empirical Setting

	Jensen & Meckling (1976)	Aghion & Bolton (1992)	Dewatripont & Tirole (1994)	Our Empirical Setting
Setting Players?	Managers, external shareholders, creditors	Managers, creditors	Managers, external shareholders, creditors	Managers, external shareholders, creditors on private bank loan agreements.
Managerial agency problem?	Ex-post risk shifting at expense of creditors; managers pursue risky investment	Ex-post private benefit leads to inefficient investment decisions. Loosely, "excess continuation bias" in low monetary return states	Ex-ante private benefit from shirking	
Managerial – shareholder conflict?	No	No role for outside shareholders	Yes. Shirking by management hurts outside shareholders as well as creditors	
Hypotheses When does control over investment policy shift to creditors?	After negative performance, or when firms near bankruptcy kink point.	After signal implies that "bad" state is realized, where bad state (loosely) signifies low monetary returns and high managerial private benefit	After negative performance, as it is evidence that manager shirked	Three measures of performance: (1) low profitability, (2) financial covenant violation, (3) credit downgrades
Control rights contingent in original contract?	Not necessarily	Not necessarily	Yes. For ex-ante incentive purposes, manager must understand punishment mechanism in original contract	Financial covenant violations represent parallel to contingent contracts.
The effect of creditor control on ex-post gains to outside shareholders?	Bad for shareholders	No role for outside shareholders	Good for shareholders	

Table 4
Capital Expenditure Restrictions, Across Types of Firms

This table presents the fraction of credit agreements that have a capital expenditure restriction by industry, size, and credit rating.

	Fraction with capital expenditure restriction
Totals	
Fraction of credit agreements with restriction	0.319
Fraction of firms that ever have credit agreement with restriction	0.424
Estimated fraction of all Compustat firms with restriction, using Sufi (2006)	0.336
Fraction of credit agreements with restriction:	
By industry	
Agriculture, minerals, construction	0.154
Manufacturing	0.324
Transportation, communication, and utilities	0.230
Trade—wholesale	0.360
Trade—retail	0.433
Services	0.399
By size (book assets)	
Less than $100M	0.468
$100M to $250M	0.469
$250M to $500M	0.443
$500M to $1,000M	0.381
$1,000M to $2,500M	0.243
$2,500M to $5,000M	0.133
Greater than $5,000M	0.086
Borrower does not have credit rating	0.392
Borrower has credit rating	0.242
Conditional on firm having rating	
Investment grade	0.060
Junk rated {0,1}	0.437
AAA, AA rated {0,1}	0.000
A rated {0,1}	0.031
BBB rated {0,1}	0.077
BB rated {0,1}	0.393
B rated {0,1}	0.490
CCC rated or worse {0,1}	0.622

Table 5
Negative Firm Performance and Capital Expenditure Restrictions

This table presents estimated coefficients from linear probability regressions that relate the probability of having a capital expenditure restriction in a credit agreement to borrower performance in the 4 quarters preceding the loan origination. Columns 1 through 3 present borrower fixed effects regression estimates using only borrowers with at least 2 credit agreements and columns 4 through 6 present pooled regression estimates using the full sample. The sample for columns 3 and 6 includes rated firms only, and the omitted group is borrowers rated A or better. All regressions include year indicator variables, loan purpose indicator variables, and loan type indicator variables. Standard errors are clustered for each borrower.

	Borrower fixed effects regressions			Pooled regressions		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	CapEx Restriction {0,1}	CapEx Restriction {0,1}	CapEx Restriction {0,1}	CapEx Restriction {0,1}	CapEx Restriction {0,1}	CapEx Restriction {0,1}
BBB rated $_{t-1}$ {0,1}			0.050			-0.018
			(0.037)			(0.020)
BB rated $_{t-1}$ {0,1}			0.234**			0.176**
			(0.076)			(0.035)
B rated $_{t-1}$ {0,1}			0.266*			0.216**
			(0.118)			(0.047)
CCC rated or worse $_{t-1}$ {0,1}			0.562**			0.283**
			(0.183)			(0.099)
Financial covenant violation$_{t-1}$		0.140**	0.134		0.134**	0.082
		(0.059)	(0.079)		(0.037)	(0.067)
Cash flow$_{t-1}$ / assets$_{t-1}$	-2.267**	-1.998**	-1.761	-1.141**	-0.979**	-1.853**
	(0.737)	(0.738)	(1.160)	(0.367)	(0.365)	(0.677)
Ln(total assets $_{t-1}$ ($M))	-0.053	-0.052	0.013	-0.064**	-0.062**	-0.043**
	(0.034)	(0.034)	(0.054)	(0.005)	(0.005)	(0.011)
Market to book ratio $_{t-1}$	-0.013	-0.012	-0.029	-0.021**	-0.020**	-0.015
	(0.016)	(0.015)	(0.024)	(0.008)	(0.008)	(0.013)
Book leverage ratio$_{t-1}$	0.100	0.071	-0.129	0.187**	0.178**	0.053
	(0.111)	(0.111)	(0.152)	(0.049)	(0.049)	(0.074)
Number of credit agreements	2,742	2,742	1,453	3,720	3,720	1,822
Number of firms	961	961	475	1,939	1,939	844
R^2	0.19	0.19	0.22	0.18	0.18	0.29

*,** statistically distinct from 0 at the 5 and 1 percent, respectively

Table 6
Correlations between Capital Expenditure Restrictions and Other Loan Contract Terms

This table presents unconditional correlations between the probability of a loan containing a capital expenditure restriction and other loan contract terms. All correlations are statistically distinct from 0 at the 1 percent level.

	Capital expenditure restriction {0,1}	Dividend restriction{0,1}	Secured {0,1}
Dividend restriction {0,1}	0.26		
Secured {0,1}	0.42	0.38	
Interest rate spread	0.41	0.33	0.59

Table 7
Negative Firm Performance and Other Loan Contract Terms

This table presents estimated coefficients from borrower fixed effects linear probability regressions that relate loan contract terms to borrower performance in the 4 quarters preceding the loan origination. The sample for columns 1, 3, and 5 includes borrowers with at least 2 credit agreements in the sample. The sample for columns 2, 4, and 6, includes rated borrowers with at least 2 credit agreements in the sample, and the omitted rating group is the set of borrowers that are rated A or better. All regressions include year indicator variables, loan purpose indicator variables, and loan type indicator variables. Standard errors are clustered for each borrower.

Dependent variable	(1) Ln(Interest rate spread)	(2) Ln(Interest rate spread)	(3) Secured {0,1}	(4) Secured {0,1}	(5) Dividend restriction {0,1}	(6) Dividend restriction {0,1}
BBB rated $_{t-1}$ {0,1}		0.345**		0.043		0.026
		(0.086)		(0.063)		(0.084)
BB rated $_{t-1}$ {0,1}		0.688**		0.321**		0.186
		(0.117)		(0.101)		(0.099)
B rated $_{t-1}$ {0,1}		0.855**		0.304**		0.132
		(0.154)		(0.117)		(0.109)
CCC rated or worse $_{t-1}$ {0,1}		0.895**		0.319*		0.146
		(0.162)		(0.150)		(0.126)
Financial covenant violation$_{t-1}$	0.072	0.049	0.039	-0.001	0.062	0.180*
	(0.060)	(0.099)	(0.053)	(0.068)	(0.049)	(0.084)
Cash flow$_{t-1}$ / assets$_{t-1}$	-5.691**	-5.478**	-1.395*	-1.537	-0.691	-0.922
	(0.896)	(1.492)	(0.694)	(1.174)	(0.606)	(1.054)
Ln(total assets $_{t-1}$ ($M))	-0.188**	-0.094	-0.116**	-0.100*	-0.070**	-0.106*
	(0.037)	(0.058)	(0.027)	(0.042)	(0.027)	(0.046)
Market to book ratio $_{t-1}$	-0.063**	-0.099**	-0.019	-0.067*	-0.012	-0.025
	(0.023)	(0.036)	(0.024)	(0.034)	(0.022)	(0.035)
Book leverage ratio$_{t-1}$	0.718**	0.655**	0.251**	0.260*	0.002	-0.024
	(0.116)	(0.145)	(0.097)	(0.115)	(0.091)	(0.127)
Number of credit agreements	2,742	1,453	2,272	1,194	2,559	1,386
Number of firms	961	475	942	466	957	475
R^2	0.53	0.74	0.34	0.56	0.15	0.22

*,** statistically distinct from 0 at the 5 and 1 percent, respectively

Table 8
Contingent Contract Analysis

This table presents mean differences in loan contract terms for contingent contract pairs. A contingent contract pair contains two credit agreements between the same borrower and the same bank where the origination date of the later contract is earlier than the maturity date of the earlier contract, and the later contract is preceded by a financial covenant violation. The later contract represents a renegotiated contract, where the renegotiation is caused by a financial covenant violation. Tests for differences in the means cluster standard errors for each borrower.

Contract term	(1) CapEx Restriction {0,1}	(2) Interest spread (basis points)	(3) Secured {0,1}	(4) Dividend restriction {0,1}
Original agreement	0.372	188	0.800	0.848
Renegotiated agreement that follows financial covenant violation	0.607*	250**	0.923	0.947
Number of credit agreements	112	112	97	103
Number of firms	51	51	49	51

*,** statistically distinct from 0 at the 5 and 1 percent, respectively

Table 9
Capital Expenditures Before and After a Capital Expenditure Restriction

This table examines the capital expenditure patterns of firms before and after signing a loan contract, conditional on whether the loan contract contains a capital expenditure restriction. Panel A presents the unconditional means, and it presents tests for differences in the differences in capital expenditures for firms that have or do not have a capital expenditures restriction in their loan agreement. Panel B reports regressions relating the difference in capital expenditures to an indicator variable of whether the loan contract contains a restriction and changes in control variables. The regressions in Panel B contain year indicator variables.

PANEL A

	2 years before contract	1 year before contract	Year of contract	Year after contract
Loan:				
does not contain restriction	0.089	0.083	0.080	0.072
contains restriction	0.094	0.072	0.065	0.056
contains restriction & previous loan does not contain restriction	0.082	0.072	0.055	0.047

Difference in difference	Year after – year before	Average 2 years after – Average 2 years before
contains restriction - does not contain restriction	-0.004^{+}	-0.007^{+}
contains restriction & previous loan does not contain restriction - does not contain restriction	-0.014^{+}	-0.016^{+}

PANEL B

	(1)	(2)	(3)	(4)
Dependent variable:	Year after – year before	Average 2 years after – average 2 years before	Year after – year before	Average 2 years after – average 2 years before
Capital expenditure restriction {0,1}	-0.001	-0.004^{+}	-0.001	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Capital expenditure restriction {0,1}*	-0.011$^{+,\#}$	-0.010$^{+,\#}$	-0.008$^{+,\#}$	-0.005$^{\#}$
Previous loan does not have restriction{0,1}	(0.005)	(0.004)	(0.004)	(0.004)
Difference in cash flow$_t$ / assets$_{t-1}$			0.073^{+}	0.103^{+}
			(0.022)	(0.024)
Difference Q_{t-1}			0.023^{+}	0.024^{+}
			(0.002)	(0.002)
N	2,671	2,671	2,671	2,671
R^2	0.02	0.04	0.14	0.21

+statistically distinct from 0 at the 10 percent level; # statistically significant from omitted group (no capital expenditure restriction) at 10 percent level

Figure 1: How Does a Negative Performance Shock Affect Loan Contract Terms?

This figure presents the marginal effect of negative performance on loan contract terms for a given borrower. It presents the marginal effect on contract terms for a drop in cash flow from the 90th percentile of the distribution to 10th percentile of the distribution (left) and the marginal effect of a financial covenant violation (right). The marginal effect is stated as the percent change relative to the mean. For example, a financial covenant violation in the past year results in a 44% increase in the incidence of a capital expenditure restriction at the mean, and a 15% increase in the interest rate spread at the mean. The estimated marginal effects come from fixed effects regressions reported in Tables 5 and 7.

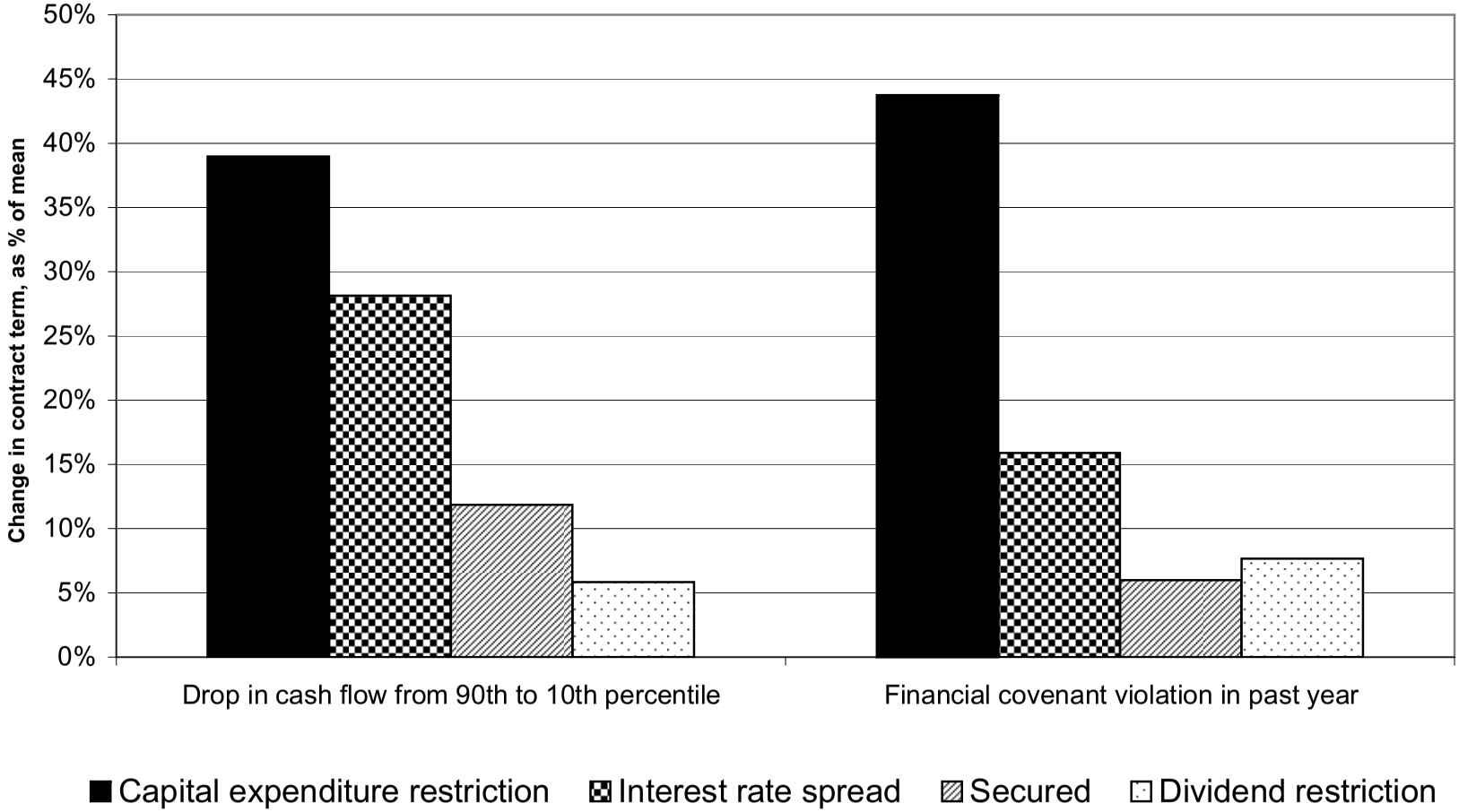

Figure 2: Changes in Loan Contract Terms in Response to Credit Downgrades

This figure presents the marginal effect of credit downgrades from A or above on loan contract terms for a given borrower. The marginal effect is stated as the percent change relative to the mean. For example, a firm that is downgraded from A to B expreriences a 60% increase in the likelihood of having a capital expenditure restriction at the mean. The estimated marginal effects come from fixed effects regressions reported in Tables 5 and 7.

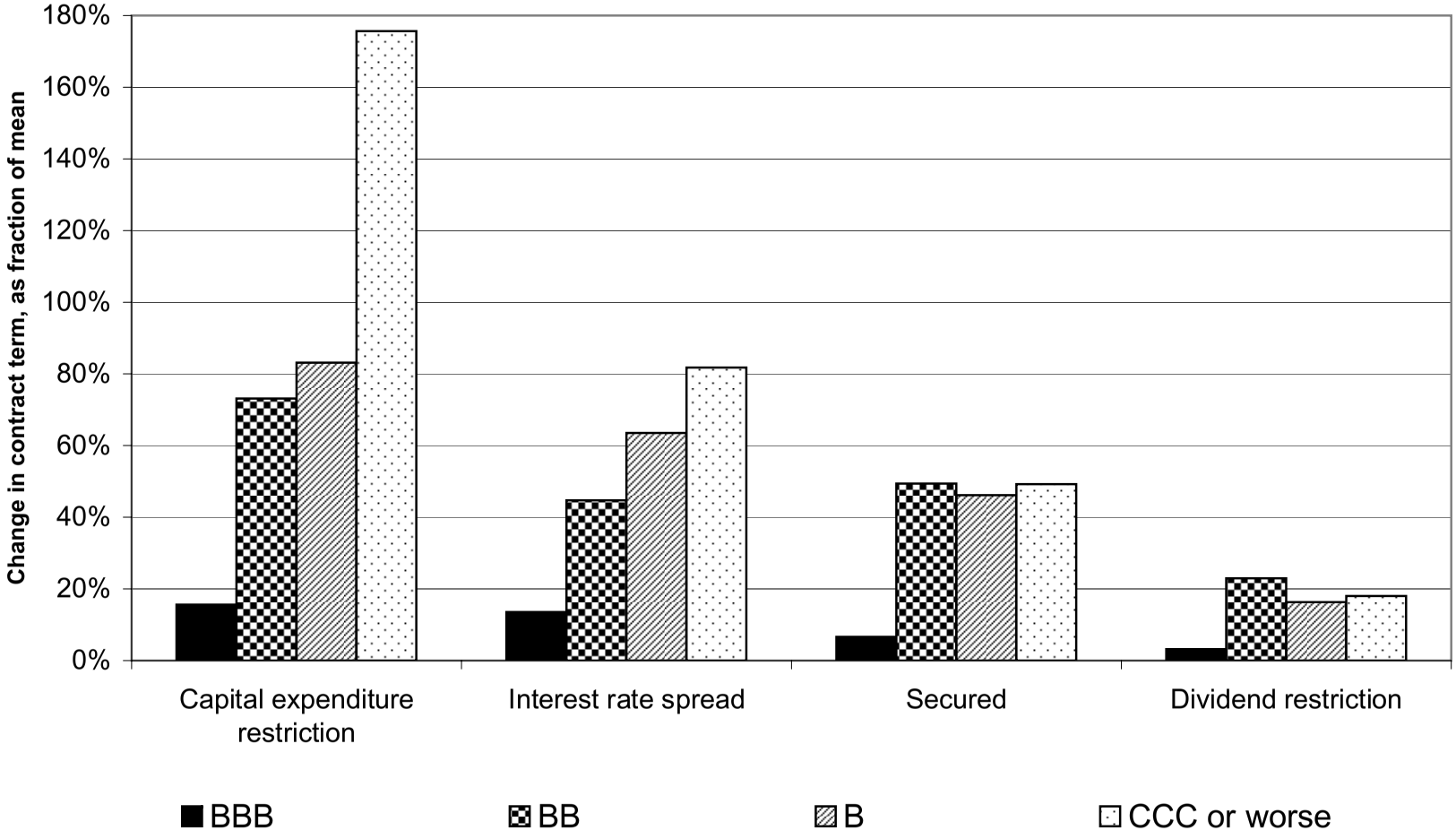

Figure 3: Capital Expenditures - Capital Expenditure Limit

This figure presents a histogram of the difference between actual capital expenditures and the capital expenditure limit, expressed as a ratio to lagged assets. Actual capital expenditures are for the first fiscal year after the loan contract is signed. Values below -10% and above 5% are excluded, and the sample includes the remaining 457 loans for which we have the actual capital expenditure restriction. The solid black line is a normal distribution with identical mean and variance.

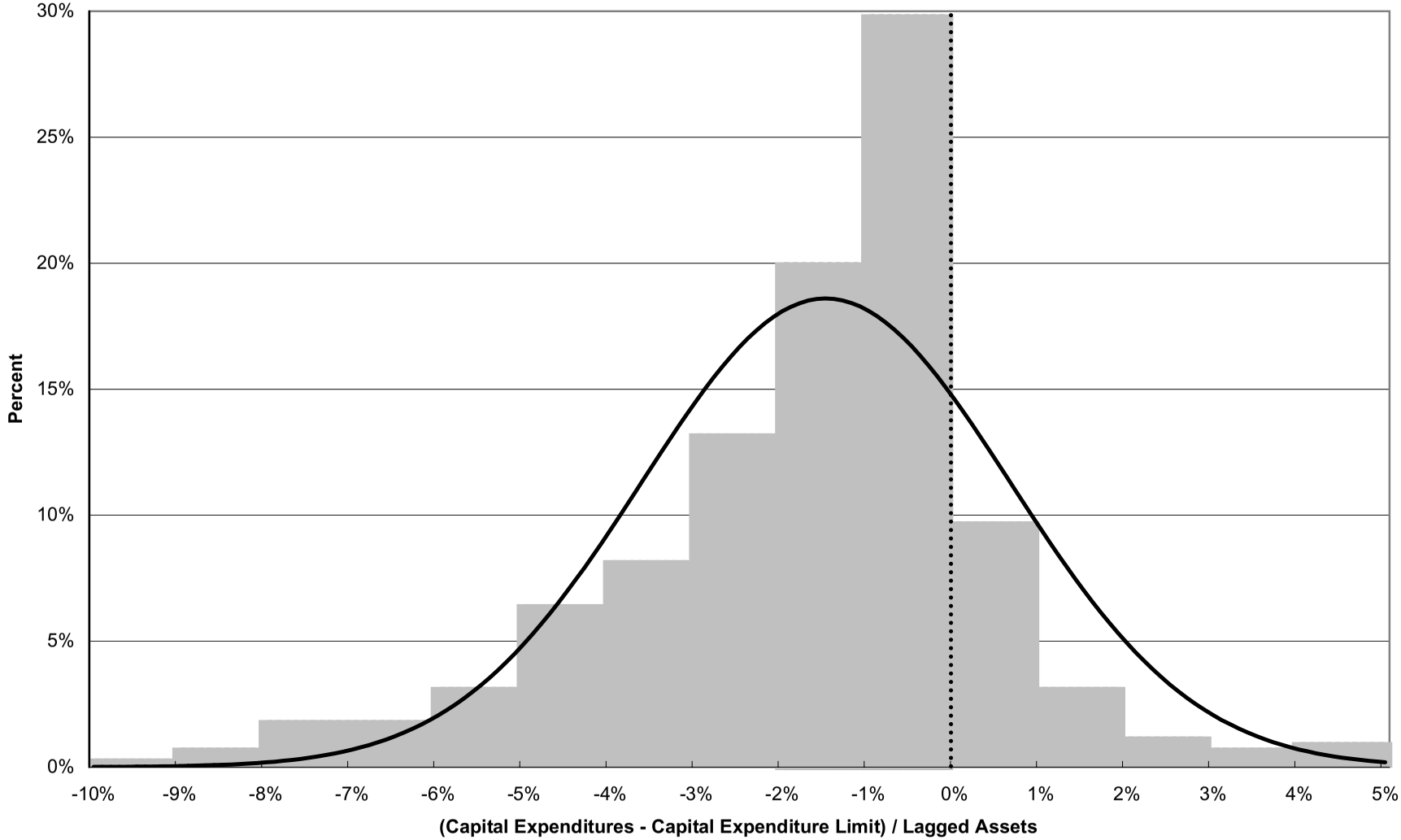

Figure 4: Capital Expenditures - Contractual Limit, Year Before and Year of Loan

This figure presents a histogram of the difference between actual capital expenditures and the capital expenditure limit, where actual capital expenditures are for the first fiscal year after the loan contract is signed (solid black) and for the fiscal year immediately preceding the year in which the contract is signed (striped). Differences are scaled by lagged assets. The sample includes only the 81 loans where the prior sample loan did not include a capital expenditure restriction.