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Financial debt contracting and managerial agency problems

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Abstract

This paper analyzes if lenders resolve managerial agency problems in loan contracts using sweep covenants. Sweeps require a (partial) prepayment when triggered and are included in many contracts. Exploiting exogenous reductions in analyst coverage due to brokerage house mergers and closures, we find that increased borrower opacity significantly increases sweep use. The effect is strongest for borrowers with higher levels of managerial entrenchment and if lenders hold both debt and equity in the firm. Overall, our results suggest that lenders implement sweep covenants to mitigate managerial agency problems by limiting contingencies of wealth expropriation.

KEYWORDS

agency problems, covenant, loan contract, sweep provision

1 | INTRODUCTION

The allocation of control rights between creditors and shareholders is the main object of interest in a large body of literature (see, e.g., Christensen et al., 2016 for an overview). In contrast, the potential conflict between creditors and the firm's management is often neglected. However, actions by entrenched managers can have adverse consequences for shareholders and lenders alike, often without triggering standard financial covenants. For instance, assets sales or the misuse of corporate cash reserves may affect firms' default risk or collateral value. In this paper, we document evidence that is consistent with sweep covenants—which are included in almost half of all loan contracts in our sample—being used to address potential adverse consequences of managerial actions on lenders.

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Sweep clauses do not oblige firms to maintain certain balance sheet or profit and loss statement ratios. Instead, sweep covenants require the borrower to immediately repay a given percentage of the loan when certain cash proceeds become available (e.g., from asset sales, debt or equity issuance, or insurance proceeds). By requiring payouts to creditors, sweep clauses may help discipline management. First, sweeps restrict managers' ability to accumulate excess cash flow and hence may reduce managers' flexibility. Second, sweeps may have incentive effects as they reduce the benefits from strategic asset sales or security issues (Lang et al., 1995), as part of the proceeds must be used to pay down debt. This may limit managers' incentives to engage in such behavior *ex ante*. Third, the misuse of cash windfalls, for example, from unexpected insurance proceeds, can be restricted (Blanchard et al., 1994; Glaser et al., 2013). Overall, the implementation of sweep covenants in loan contracts implies that managerial flexibility is curtailed *ex ante*, limiting potential adverse effects of managerial agency problems on debt holders.

We use *exogenous* variation in outside monitoring to examine the link between managerial agency problems and loan contract design. Specifically, we utilize changes in analyst coverage of borrowers induced by brokerage house mergers or closures (see, among others, Hong & Kacperczyk, 2010). Following the merger of two brokerage houses, due to overlapping coverage, a redundant analyst is typically let go (Wu & Zhang, 2009). This results in a decrease in analyst coverage and accordingly a lower degree of outside monitoring of the firm, which is independent of any firm and manager characteristics.¹ Given that managerial agency problems are more severe for firms with less outside monitoring (e.g., Jensen & Meckling, 1976),² this creates a natural setting to study the effects of agency problems on loan contract design.

We find that an exogenous decrease in outside monitoring increases the probability of including a sweep covenant in a loan contract by nine percentage points. This corresponds to a sizable increase of sweep use in treatment firms' loan contracts of 21% relative to the unconditional mean. We also observe that the number of sweep covenants in a contract increases. In contrast, we do *not* observe an effect of the change in outside monitoring on financial covenants, which are typically used to address conflicts of interest between shareholders and debtholders. Further tests show that the parallel trends assumption holds; that is, there is no pre-trend in sweep use for treated versus control firms prior to a brokerage house merger or closure, providing support for our identification strategy.

We provide a series of cross-sectional tests that corroborate our main result. First, we investigate the effect of outside monitoring for firms with different initial levels of analyst coverage. We hypothesize that effects should be more pronounced for more opaque firms, that is, firms with less initial coverage. Our results indicate that lenders implement sweep covenants following a reduction in analyst coverage, especially when firm transparency is low *ex-ante*.

Second, we investigate the role of other corporate governance mechanisms. External monitoring and internal monitoring (e.g., board oversight) might be either substitutes or complements (e.g., Irani & Oesch, 2013). We therefore subdivide firms by their pre-treatment governance quality. For example, managerial agency problems are more severe for companies with excess funds or long CEO tenure (Jensen, 1986; Kalcheva & Lins, 2007; Lie, 2000), while more institutional ownership reduces managerial entrenchment (Chava et al., 2010; Finkelstein & Hambrick, 1989). Our results show that the effect of a reduction of analyst coverage on sweep covenant use is particularly pronounced among firms with poor corporate governance. In contrast, the effect is limited for well-governed firms. Our results thereby complement the finding of Irani and Oesch (2013) that external monitoring by analysts and other governance mechanisms are substitutes.

One potential concern might be that even if managerial agency problems affect debtholders, actions taken by equity holders might be better suited to discipline managers. For instance, shareholders might incentivize firms to use excess cash for payouts (dividends or share repurchases) or interest payments, that is, to lever up (Easterbrook,

¹ It is also independent of the individual analyst. For instance, Hong and Kacperczyk (2010) and Irani and Oesch (2013) provide evidence that analyst coverage reductions are concentrated at the target brokerage house, that is, the reduction of analysts follows a rule which is not related to skill.

² For instance, Kelly and Ljungqvist (2012) document that a decrease in analyst coverage increases information asymmetry (see also Brennan & Subrahmanyam, 1995; Ellul & Panayides, 2018). Irani and Oesch (2013) provide evidence that financial reporting quality is lower following a reduction in coverage. Dyck et al. (2010) show that information intermediaries are often among the first to detect managerial misbehavior. Chen et al. (2015) present results, which are consistent with the conjecture that managerial agency problems increase following analyst coverage reductions.

1984; Jensen & Meckling, 1976). Such actions, however, might not be in the interest of debtholders. Increasing leverage increases default risk and may exacerbate risk-shifting incentives of shareholders (Chava et al., 2010; Maxwell & Stephens, 2003). Hence, debtholders may prefer other means of addressing manager misbehavior, such as sweep contracts.³

Furthermore, most firms operate well above their default barrier such that the contingencies that sweeps address are particularly relevant for equity holders. If sweeps are used by lenders to address managerial agency problems, we should therefore expect to observe a stronger increase in sweep use for lenders that simultaneously also hold equity of the borrowing firm (dual holding). In a third step, we therefore identify dual holders. Our results confirm that lenders increase sweep use more in loan contracts of firms in which they also hold equity.

The work by Huang (2010) is closest to this paper. He argues that sweep provisions can mitigate conflicts of interest between creditors and shareholders. Sweeps may shorten the effective maturity of loans by requiring firms with excess cash flow to prepay their debt. This may force firms to return to the capital market more frequently, giving lenders more control over firms' investment decisions in future contract negotiations. Thereby, the ability of firms to pursue investment strategies that benefit shareholders but hurt debtholders might be reduced. Consistent with this view, Huang (2010) observes a positive correlation between sweep covenants and firm leverage and institutional ownership.

We complement the work by Huang (2010) by documenting an increased use of sweep provisions in loan contracts following an *exogenous* decrease in borrower transparency. This effect is particularly pronounced for more opaque borrowers and borrowers with a higher level of managerial entrenchment. This evidence suggests that managerial agency problems are a first-order determinant of sweep provisions.⁴

Our work also contributes to several other strands of literature. First, we add to the literature on the impact of managerial agency problems on debt contracting. Chava et al. (2010) document that factors associated with managerial entrenchment positively correlate with investment restrictions in bond contracts and negatively correlate with subsequent financing and dividend restrictions. Their findings suggest that managerial agency problems are a factor in debt contract design. Begley and Feltham (1999) document that managerial share ownership has a significant effect on the inclusion of bond covenants that restricts additional borrowing or dividend payouts. This literature exclusively focuses on bonds. We contribute to this literature by documenting that lenders implement sweep covenants in loan contracts to address contingencies of wealth expropriation by firm management. Our identification strategy thereby allows us to establish a causal relationship between the degree of outside monitoring, used as a proxy for the degree of managerial agency problems, and the use of sweep clauses in loan contracts.

Second, we contribute to the literature on sweep covenants in loan contracts. Despite their frequent use, most of the literature does not specifically focus on sweep covenants. Instead, sweep covenants are often included in covenant intensity indices (Bradley & Roberts, 2015; Demiroglu & James, 2010) or investigated in relation to financial covenants (Christensen & Nikolaev, 2012). As discussed above, one notable exception is Huang (2010). We add to this literature by documenting an increased use of sweep provisions in loan contracts when borrower transparency decreases. Furthermore, we provide empirical evidence that this effect is particularly pronounced for more opaque borrowers and borrowers with a higher level of managerial entrenchment, suggesting that sweep provisions are used to address managerial agency problems.

Finally, we also add to the larger literature on lender rights and control through loan contract design. Most prior work focuses on the allocation of control rights between creditors and shareholders via financial covenants. Roberts (2015), Li et al. (2016), and Nikolaev (2018) investigate the allocation of control rights within a loan, while Chava and Roberts (2008), Roberts and Sufi (2009a, 2009b), Nini et al. (2009, 2012), Demerjian and Owens (2016), and Freudenberg et al. (2017) investigate the effects of shifts in control rights to creditors on the firm level, and Demerjian (2011)

³ Stulz (1988) argues that takeover threats can constrain managers, which, however, might also not be beneficial for debtholders. For instance, the financial risk of the target firm increases if the takeover is accompanied by a large increase in leverage (Chava et al., 2009).

⁴ Clearly, managerial agency problems and conflicts of interest between shareholders and debtholders are not mutually exclusive. That is, our evidence does not preclude that sweep provisions may also help mitigate creditor–shareholder conflicts of interest.

and Christensen and Nikolaev (2012) distinguish between different types of covenants. Murfin (2012) and Demerjian and Owens (2016) develop and investigate aggregate measures of financial covenant strictness. Hallman et al. (2022) also examine loan contract terms around changes in analyst coverage of non-financial firms. They provide evidence that an increase in information asymmetry as a result of a reduction in analyst coverage is related to higher loan spreads, a reduction in credit supply, and more restrictive covenants. We add to this literature by emphasizing the differing role of sweep covenants compared to other financial covenants and loan contract restrictions. Sweep covenants have the potential to mitigate managerial agency problems by limiting contingencies of wealth expropriation.

The remainder of this paper proceeds as follows. Section 2 describes our identification strategy and the data. Section 3 reports the results of the empirical analysis. Section 4 concludes.

2 | EMPIRICAL SETUP AND DATA

2.1 | Identification strategy

A simple empirical strategy to examine the link between managerial agency issues and loan contracting would be to regress a measure of sweep or financial covenant use on proxies for the degree of misuse of corporate resources by managers, such as measures of firm transparency. While intuitively appealing, the estimates from such regressions are hard to interpret due to endogeneity problems. For instance, it could be that firms with higher managerial agency problems *require* more outside control and hence may have higher transparency levels. That is, a simple regression of sweep use on transparency could indicate a misleading positive relationship, as both variables are endogenously determined by the (unobservable) degree of managerial agency problems.

To overcome this issue, we utilize *exogenous* changes in outside monitoring, which directly affect the firm's information environment. In particular, we follow Hong and Kacperczyk (2010) and Irani and Oesch (2013) and study brokerage house mergers and closures.⁵ Wu and Zhang (2009) find that subsequent to a merger of two brokerage houses with an active equity research department, the resulting entity lays off analysts to mitigate redundancies caused by overlapping coverage. As a result, companies that were previously covered by both brokerage houses experience a decline in analyst coverage. Importantly, the reduction in coverage is independent of unobservable firm and manager characteristics and is neither determined by the individual analyst nor by the firm for which the coverage is reduced. Hong and Kacperczyk (2010) and Irani and Oesch (2013) provide evidence that the analyst coverage reduction follows a rule unrelated to skill—in most cases, the analyst of the target brokerage house is let go. Similarly, the closure of a brokerage house leads to a decline in analyst coverage for affected firms. Further, particularly important in our setting, brokerage houses are generally not financial institutions granting loans; that is, it is unlikely that a brokerage house merger or closure has a direct effect on credit supply to the firms covered by the entities.⁶

Several studies provide evidence consistent with the first step required for our argument; that is, the idea that a loss in analyst coverage indeed increases information asymmetry as outside monitoring and information production is reduced. Kelly and Ljungqvist (2012), for instance, document that stock market-based measures for information asymmetry (e.g., probability of informed trading, bid-ask spreads) worsen following losses of analyst coverage (see also Brennan & Subrahmanyam, 1995; Ellul & Panayides, 2018). Irani and Oesch (2013) provide evidence that financial reporting quality worsens. The next step, that is, the link between (external) information production, managerial entrenchment, and managerial agency problems, goes back to Jensen and Meckling (1976) and has been examined in

⁵ While broker research reports primarily target equity investors, they are also an important source of information used by (prospective) lenders. For instance, Irani and Oesch (2013) show that a reduction in analyst coverage leads to an overall lower financial reporting quality and that reporting quality has been shown to affect loan terms (Graham et al., 2008).

⁶ Some brokerage houses are affiliated with financial institutions that also have lending business. However, as discussed in the next section, we do not find evidence that our results are driven by events that involve a brokerage house that is affiliated with an active lender in the syndicated loan market.

several studies since.⁷ Our main hypothesis is on the final step of this causal chain; that is, that sweep provisions can be used to limit managerial discretion if external oversight is (exogenously) reduced and managerial entrenchment increased.

We follow Hong and Kacperczyk (2010) and Irani and Oesch (2013) and screen the SDC Mergers and Acquisition database for mergers of two financial institutions and limit the sample to firms with SIC code 6211 (“Investment Company Firms, Dealers and Exchanges”). We require that both brokerage houses are disseminating estimates to the I/B/E/S database and that both brokerage houses have an overlapping coverage of at least two stocks. Given that I/B/E/S does not assign analysts to individual brokerage houses after 2006, we end up with the same 13 mergers as Hong and Kacperczyk (2010) and Irani and Oesch (2013). In addition to mergers, we further identify 11 brokerage house closure events in I/B/E/S following Kelly and Ljungqvist (2012).⁸ Finally, we merge the I/B/E/S information to LPC DealScan, which contains detailed loan-level information. Sweep and covenant information relates to a loan package which oftentimes includes several loan facilities. As is common in the literature (e.g., Nini et al., 2009, 2012), we analyze the data at the facility level to be able to account for factors varying at this level such as the size and the maturity of a facility. We also include fixed effects for the specific type of facility in our analyses.⁹

For each brokerage house merger, we identify all stocks that are covered by both merging parties in the year prior to the merger, that is, stocks with an “overlapping coverage.” Similarly, for closures, we identify all stocks that are covered by the brokerage house in the year prior to the closure. These firms are the focus of this paper and are in the following referred to as “treated.” We analyze all loan facilities contained in LPC DealScan in the symmetric 4-year window around each merger or closure, that is, a window consisting of 2 years before (720 days) the event and 2 years after the event.¹⁰ Note that in this setting, being treated is not a firm fixed effect; that is, each event affects a different set of firms. To construct symmetric windows and deal with overlapping events, we first construct separate samples for each event. These samples are then pooled. Accordingly, the same loan contract might be included in different windows when event windows are overlapping. We address this issue by including event (merger or closure) \times firm fixed effects in all our estimations, that is, focus on within-event variation across firms. Further, the use of a staggered design, that is, pooling event samples, addresses potential concerns otherwise associated with staggered difference-in-difference (DiD) frameworks (Goodman-Bacon, 2021).

To account for systematic differences between treated and control firms, for each brokerage house merger or closure, we match untreated firms to each treated firm based on firm size (total assets).¹¹ These firms form the control group. Deryugina et al. (2020) provide empirical evidence that this matching estimator generates more precise estimates than the standard DiD estimator. We further control for other differences across treated and control firms by including standard firm-level and loan contract control variables in our regressions, defined in more detail in the following section. To empirically implement our natural experiment and test how the use of sweep clauses (and financial covenants) changes following a shock to analyst coverage of the firm, we estimate versions of the following pooled

⁷ Chen et al. (2015), for instance, provide evidence that following an exogenous decrease in outside monitoring, CEO compensation increases, the likelihood that management invests in value-destroying acquisitions increases, and managers are more likely to engage in earnings management activities. Irani and Oesch (2013) document that a reduction in outside monitoring reduces financial reporting quality; that is, management may strategically make financial statements opaquer to cover self-dealing.

⁸ We identify fewer closure events compared to Kelly and Ljungqvist (2012), as we require firms that are covered by a brokerage house to also be active borrowers in the LPC DealScan database.

⁹ One concern might be that results differ between term loans and revolvers. In unreported robustness tests, we also examine whether revolving loan facilities exhibit differential outcomes with respect to the implementation of sweeps in response to treatment and do not find this confirmed. Note that the trigger of a sweep typically implies a repayment of the loans included in a loan package in the following order: i. term loans, ii. cancellation of available revolving commitments, iii. prepayment and cancellation of used revolving commitments, and iv. repayment and cancellation of ancillary facilities.

¹⁰ Hong and Kacperczyk (2010) and Irani and Oesch (2013) analyze a 2-year window, whereas our window is 4 years. The reason is that their object of analysis is financial statement information, while we analyze loan issuances, which are in general less frequent to observe. That is, only a few firms issue a loan both in the year prior to the brokerage house merger as well as in the year afterward. The increase in time therefore allows for more statistical power.

¹¹ We only match on total assets to increase the probability that a suitable control firm can be found for each treated firm. However, we confirm in Table 1 that after matching on total assets treated and control, firms are very comparable across most observable dimensions.



panel (DiD) regression:

$$SWEEP_{m,j,i,t} = \alpha_{m,i} + \alpha_{m,t} + \beta POST \times TREATED_{m,i,t} + \theta' Y_{i,t} + \delta' Z_{m,j,i,t}, \quad (1)$$

where $SWEEP_{m,j,i,t}$ is an indicator variable that equals one if loan j by firm i at time t in the estimation window around event (brokerage house merger or closure) m includes a sweep clause, and zero otherwise. $POST_{m,t}$ is a dummy variable that equals one in the period after the event m , and zero otherwise. $TREATED_{m,i}$ is a dummy variable that equals one if firm i is part of the treatment sample for event m , and zero otherwise. $a_{m,i}$ is a set of firm \times event fixed effects, $a_{m,t}$ is a set of time \times event fixed effects, $Y_{i,t}$ is a set of firm characteristics, and $Z_{m,j,i,t}$ is a set of loan characteristics. Note that, following Irani and Oesch (2013), we do not include calendar year fixed effects as any period-specific effect will be captured by the merger (\times time) fixed effects. The coefficient of interest is β , which captures the treatment effect. It shows the effect of the brokerage house merger/closure, and the associated reduction in analyst coverage, on the use of sweep covenants (or other outcome variables) in loan contracts. In all regressions, we report standard errors clustered at the firm level as treatment variation is mainly across firms.¹²

2.2 | Sample selection and control variables

We obtain data on security analyst coverage from I/B/E/S. For each event, we obtain all loans issued by public U.S. non-financial companies in a 720-day window before and after the brokerage house merger/closure date from LPC DealScan. We merge this sample with borrower balance sheet and income statement information from Compustat.¹³

Throughout the analysis, we control for basic firm characteristics. We control for firm size (log of total assets), leverage, market-to-book ratio, profitability, tangibility, interest coverage, current ratio, and credit rating. The latter is based on S&P and included via indicator variables for each rating notch. Further, we control for basic loan characteristics. While loan characteristics are important factors that can explain the use of financial covenants and sweep provisions, most loan terms are simultaneously determined, that is, endogenous. For instance, a borrower may pay a lower spread *because* a covenant is included in the contract. We therefore restrict our loan level control variables to these with a high likelihood of being independent of the decision to include a sweep covenant in the loan contract. We include the (log) loan size, (log) maturity, loan type, and loan purpose. The rationale is that these factors are in general determined by the firm *prior* to applying for a loan.¹⁴ Table 1 shows descriptive statistics split by the treatment and control groups.

The table shows that a high fraction of loans include sweep covenants: 46% (43%) of all loans in the control (treatment) sample include at least one sweep covenant. Sweeps can be classified by the source of the cash proceeds: (i) asset sale, (ii) debt issuance, (iii) equity issuance, (iv) excess cash flow, and (v) insurance proceeds. Table 1 reports that on average, loans include 1.38 (1.27) sweep provisions. If we focus on the subset of loans that include at least one sweep, we find that on average, loans to treated (control) firms include 2.93 (3.02) sweep provisions (not tabulated). This indicates that usually a combination of sweep clauses is used. This should not be surprising given that firms are able to substitute between different sources of cash to some degree. For instance, if a loan includes an excess cash flow sweep but no security issuance sweeps, a manager could simply finance a project with debt or equity instead of using

¹² In unreported robustness tests, we re-estimate our regression and cluster standard errors at several other levels. The results show that clustering does not seem to be a factor which substantially influences our results. We also estimate a Poisson model for our dependent variables *number of sweeps* and *number of financial covenants* following Cohn et al. (2022). Results are very comparable to the OLS estimates we provide in our tables.

¹³ We use Michael Robert's Dealscan-Compustat Linking Database to merge Dealscan with Compustat (Chava & Roberts, 2008). We obtain borrower information from the last available fiscal year prior to the loan.

¹⁴ We acknowledge that these variables are not entirely independent of the loan contract design negotiations. For instance, a firm may require a "large" loan, but the exact size is determined by the design of the loan contract and an outcome of the negotiation with the lender. However, all our results remain virtually unchanged if we do not control for these factors.

TABLE 1 Summary statistics for the treatment and control samples.

	Treatment group					Control group					diff. (<i>p</i> -value)		
	N	Mean	Q1	Median	Q3	SD	N	Mean	Q1	Median		Q3	SD
Loan characteristics													
Facility amount (million USD)	1692	436.05	109.77	266.75	550.12	479.26	1849	454.47	115.00	274.42	575.00	516.49	18.42 (0.272)
Maturity (months)	1671	41.67	12.00	36.00	60.00	26.85	1827	43.47	12.00	38.00	60.00	28.12	1.80 (0.053)
SWEEP (0/1)	1692	0.43	0.00	0.00	1.00	0.50	1849	0.46	0.00	0.00	1.00	0.50	0.03 (0.227)
#SWEEP	1692	1.27	0.00	0.00	3.00	1.68	1849	1.38	0.00	0.00	3.00	1.78	0.11 (0.079)
FIN COV (0/1)	1692	0.94	1.00	1.00	1.00	0.23	1849	0.95	1.00	1.00	1.00	0.22	0.01 (0.684)
#FIN COV	1692	1.99	1.00	2.00	3.00	1.08	1849	2.04	1.00	2.00	3.00	1.05	0.05 (0.198)
#PERF COV	1692	1.56	1.00	1.00	2.00	1.18	1849	1.65	1.00	2.00	2.00	1.15	0.09 (0.018)
#CAP COV	1692	0.43	0.00	0.00	1.00	0.55	1849	0.39	0.00	0.00	1.00	0.55	0.04 (0.012)
Firm characteristics													
Total assets (million USD)	1690	5111.68	873.54	2031.54	5937.98	7394.65	1845	5062.0	822.67	2163.9	6104.0	6944.1	-49.62 (0.84)
Leverage	1686	0.37	0.20	0.36	0.49	0.22	1839	0.37	0.22	0.37	0.50	0.21	0.00 (0.593)
Market-to-book	1618	1.93	1.22	1.57	2.19	1.18	1735	1.74	1.13	1.45	1.95	1.00	-0.19 (0.00)
Profitability	1686	0.19	0.09	0.15	0.26	0.16	1812	0.18	0.10	0.17	0.26	0.17	-0.01 (0.822)
Tangibility	1681	0.40	0.17	0.37	0.59	0.25	1837	0.37	0.17	0.34	0.54	0.23	-0.03 (0.00)
Coverage	1648	10.71	2.62	4.90	10.56	19.80	1783	11.69	2.49	4.46	9.14	24.33	0.98 (0.197)
Current ratio	1618	1.74	1.03	1.44	2.12	1.18	1678	1.55	0.98	1.38	1.95	0.86	-0.19 (0.000)

Note: This table reports summary statistics for the sample of syndicated loans to non-financial North American borrowers. Statistics are reported separately for the treatment and control samples over the 4-year window surrounding brokerage house mergers. The treatment sample consists of loans obtained by firms that are covered by both merging brokerage houses in the year prior to the merger. The control sample consists of loans obtained by firms that are not affected by the event. Treatment and control firms are matched based on firm size (total assets). All firm data are measured in real terms with 2000 as base year and are winsorized at the 1% and 99% levels. All variables are defined in Supporting Information Appendix A.1. The last column shows the difference between control and treatment firms and the *p*-value for a *t*-test of its statistical significance.

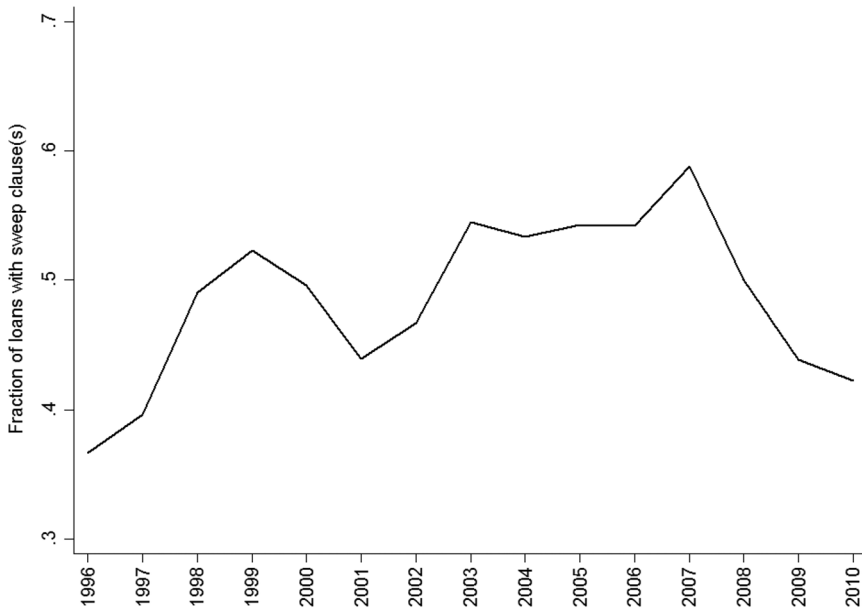


FIGURE 1 Use of sweep clauses over time. This figure shows the fraction of loan contracts that include at least one sweep clause. The sample comprises syndicated loans obtained by public U.S. non-financial firms over the 1996–2010 period.

excess cash flow. Almost all loans in our sample (94%) include at least one financial covenant, in line with prior studies (see, e.g., Roberts & Sufi, 2009a). The average number of financial covenants is slightly higher in the control sample compared to the treatment sample (2.04 vs. 1.99).

The average loan maturity is 42 months for the treatment sample and 43 months for the control sample. Loans by treated firms are slightly smaller than loans by control firms. The average loan size is 454 million USD for the control sample compared to 436 million USD for the treatment sample. The table shows that the average book value of assets is 5112 million USD in the treatment sample and 5062 million USD in the control sample.

Treated firms have on average the same leverage as control firms (37%), have higher market-to-book ratios (1.93 vs. 1.74), and have a higher fraction of tangible to total assets (40% vs. 37%). Further, treated firms, on average, have a higher return on assets than control firms (0.19 vs. 0.18), smaller interest coverage (10.7 vs. 11.7), and larger current ratios (1.74 vs. 1.55). The descriptive statistics show that—despite only matching based on firm size—our propensity score matching approach performs reasonably well; that is, the remaining differences between the treatment and the control sample are minor.

We depict in Figure 1 the use of sweep covenants in loan contracts over time. Interestingly, the use of sweep clauses appears to be pro-cyclical. This suggests that sweeps are used especially in periods when the possibility of wealth expropriation due to excess funds to a firm is higher.

3 | RESULTS

3.1 | Baseline results

We first investigate the impact of a change in analyst coverage on the use of sweep covenants in loan contracts in general. That is, we examine the average treatment effect. This includes an analysis of the parallel trends assumption

and is followed by an examination of the treatment effect by initial analyst coverage. Thereafter, we investigate sweep strictness as well as further action restrictions and collateral requirements.

3.1.1 | Average treatment effect

We begin our analyses by testing our main hypothesis: loans issued by treated firms are more likely to include sweep covenants following analyst reductions relative to loans issued by control firms. In addition, we use two more dependent variables. These are the (log of one plus the) number of sweep covenants in the loan contract and the ratio of the number of sweep covenants to the sum of sweep and financial covenants. For financial covenants, we estimate the same model and use very comparable dependent variables. These are a financial covenant indicator, the (log of one plus the) number of financial covenants, and the ratio of the number of performance-covenants divided by the total number of financial covenants, defined as in Christensen and Nikolaev (2012).

Prior to our main analyses, we test the assumption that brokerage house mergers and closures indeed reduce the number of analysts covering a treated firm. The results are presented in Table 2. Columns (1) and (2) show the reduction of the number of analysts covering a firm. Column (1) investigates the *change* (delta) in analyst coverage in the 2 years following an event relative to the pre-event year *at the firm-event level*. Column (2) shows corresponding evidence *at the firm-loan-event level*; that is, the dependent variable is the number of analysts covering firm i at the time it issues loan j around event m . Both specifications confirm that the coverage of the firm by analysts of brokerage houses reduces by roughly one.

Columns (3–5) show the results for our measures of the use of sweep covenants in loan contracts. All columns show that the DiD effect is positive and statistically significant. Column (3) displays that a reduction in analyst coverage and a corresponding decrease in firm transparency for treated firms increases the likelihood that at least one sweep clause is included in a loan contract by about nine percentage points. This effect is economically significant given that on average, 43% of loans issued by treated firms include sweep provisions. A similar effect can be observed in column (4) for the number of sweep clauses included in the contract. In column (5), the ratio of sweep covenants to the total number of sweep covenants and financial covenants is used as the dependent variable. The estimated coefficient indicates that the degree of sweep use increases post brokerage house mergers for treated firms also *relative* to the use of financial covenants.¹⁵

Columns (6–8) show the results for measures of the use of financial covenants in loan contracts. Both columns (6) and (7) indicate that the use of financial covenants in loan contracts, which are commonly used to address conflicts of interest between debtholders and *shareholders*, is *not* affected by a decrease in firm transparency. We also test for differences between financial covenants. Christensen and Nikolaev (2012) distinguish capital-based covenants from profitability-based covenants. Capital-based covenants require firms to maintain sufficient equity capital (e.g., net worth requirements or leverage restrictions), while profitability covenants require firms to meet profitability-related targets (e.g., interest coverage or debt-to-EBITDA requirements). Both types can be used to address conflicts of interest between shareholders and debtholders. Profitability-based covenants, however, act as tripwires because they *timely* indicate adverse performance. In column (8), we use the ratio of profitability-based to total financial covenants as the dependent variable. The coefficient is positive and significant; that is, while the use of financial covenants overall does not change around the events, *relatively* more performance-based covenants are used at the expense of capital-based covenants. This is in line with Aghion and Bolton (1992), who show that it is optimal for lenders to use action

¹⁵ One concern is that broker house mergers and closures have been used in several existing studies raising. Heath et al. (2023) provide adjusted critical values depending on how many previous outcomes have been examined using the same setting. At the time of writing, about 35 published papers in finance and accounting journals could be identified that examine firm-level outcomes around broker house mergers or closures (a related strand of literature uses similar experiments but focusses not on the covered firms but on analysts and analyst competition). t -values for our baseline results depend on the specification but are large (up to a value of 3.45 in Table 2, column 5). Such a t -value would be considered significant up to 108 outcome variables that have been previously studied in the same setting (see Heath et al., 2023, tab. A1, Panel B). We further focus on brokerage house mergers *and* closures, while several of the (earlier) papers we identify use merger events only.

TABLE 2 Brokerage house mergers—Baseline results.

	Change in coverage		Sweep covenants			Financial covenants		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Δ COVERAGE	COVERAGE	SWEEP (0/1)	LN (#SWEEP)	SWEEP RATIO	FIN COV (0/1)	LN(#FIN COV)	PERF COV RATIO
TREATED	−1.376*** (0.006)							
POST × TREATED		−1.227** (0.010)	0.087*** (0.007)	0.132*** (0.003)	0.074*** (0.001)	−0.011 (0.537)	−0.019 (0.465)	0.054** (0.025)
Firm controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Merger × POST FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Merger × firm FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	788	2966	2966	2966	2919	2966	2966	2788
Adj. R ²	0.011	0.916	0.688	0.735	0.649	0.314	0.593	0.807

Note: This table reports results from the estimation of a pooled panel regression analyzing the use of sweep and financial covenants around brokerage house mergers. For each merger, we consider a 2-year window prior to the merger (pre-merger window) and a 2-year window after the merger (post-merger window). We construct an indicator variable (TREATED) for each merger, which is equal to one for each firm covered by both merging brokerage houses in the pre-merger window (treatment sample), and zero otherwise. For each merger, POST is a variable that is equal to one for the post-merger period and zero for the pre-merger period. Both variables are included as base effects in each regression. Δ COVERAGE is the change in the number of analysts covering a firm from the pre-event year to 2 years after the event. COVERAGE is the number of analysts covering a firm. SWEEP (0/1) is a dummy variable, which equals one if the loan contract includes at least one sweep covenant, and zero otherwise. LN(#SWEEP) is the log of one plus the number of sweep covenants included in the loan contract. SWEEP RATIO is defined as the number of sweep covenants divided by the total number of sweep and financial covenants included in the loan contract. FIN COV (0/1) is a dummy variable, which equals one if the loan contract includes at least one financial covenant, and zero otherwise. LN(#FIN COV) is the log of one plus the number of financial covenants included in the loan contract. PERF COV RATIO is the number of performance-covenants divided by the total number of financial covenants (performance-covenants plus capital-covenants) in the loan contract. Financial covenants are divided into performance-covenants and capital-covenants following Christensen and Nikolaev (2012). Column (1) includes the aggregated difference at the merger × firm level. All other regressions include merger × firm as well as merger × POST fixed effects. Further, the regressions include firm characteristics (log total assets, leverage, market-to-book, profitability, tangibility, coverage, current ratio, and rating fixed effects [notch level]) used with their previous year-end value and loan characteristics (log loan size, log maturity, and indicator variables for loan purpose and loan types). *p*-values (in parentheses) are determined using standard errors robust to clustering at the firm level. ***, **, and * denote 1%, 5%, and 10% statistical significance. All variables are defined in Supporting Information Appendix A1.

restrictions in conjunction with control rights. That is, lenders may implement sweeps to disincentivize managers for misusing corporate resources, while at the same time including more profitability-based covenants to intervene ex post, if necessary.

One potential concern is that brokerage houses are affiliated with financial institutions that are active lenders in the syndicated loan market. That is, a brokerage house merger or closure might not only change analyst coverage but also credit supply conditions for firms (and hence loan contract terms). Any general change in credit conditions that does not differentially affect treated and control firms will be accounted for in the DiD design. However, it might be the case that brokerage houses are more likely to cover firms that have a lending relationship with their affiliated financial institution. If this is the case, treated and control group firms are differentially exposed to a change in credit supply conditions around a brokerage house merger or closure event. To address this problem, we exclude all events

TABLE 3 Brokerage house mergers—Baseline results: Dynamics.

	Sweep covenants		
	(1)	(2)	(3)
	SWEEP (0/1)	LN(#SWEEP)	SWEEP RATIO
T – 2 × TREATED	0.006 (0.865)	–0.034 (0.471)	–0.015 (0.584)
T – 1 × TREATED	(Omitted)	(Omitted)	(Omitted)
T + 1 × TREATED	0.074* (0.067)	0.100* (0.080)	0.056** (0.046)
T + 2 × TREATED	0.109** (0.012)	0.138** (0.011)	0.081*** (0.006)
Firm controls	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes
Merger × POST FE	Yes	Yes	Yes
Merger × firm FE	Yes	Yes	Yes
Number of observations	2966	2966	2919
Adj. R ²	0.688	0.735	0.649

Note: This table reports results from the estimation of a pooled panel regression analyzing the use of sweep and financial covenants around brokerage house mergers. For each merger, we consider a 2-year window prior to the merger (pre-merger window) in year T and a 2-year window after the merger (post-merger window). We construct an indicator variable (TREATED) for each merger, which is equal to one for each firm covered by both merging brokerage houses in the pre-merger window (treatment sample), and zero otherwise. This variable is interacted with indicator variables for the individual year in the pre- and post-merger windows. SWEEP (0/1) is a dummy variable, which equals one if the loan contract includes at least one sweep covenant, and zero otherwise. LN(#SWEEP) is the log of one plus the number of sweep covenants included in the loan contract. SWEEP RATIO is defined as the number of sweep covenants divided by the total number of sweep and financial covenants included in the loan contract. All regressions include merger × firm as well as merger × POST fixed effects. Further, the regressions include firm characteristics (log total assets, leverage, market-to-book, profitability, tangibility, coverage, current ratio, and rating fixed effects [notch level]) used with their previous year-end value and loan characteristics (log loan size, log maturity, and indicator variables for loan purpose and loan types). *p*-values (in parentheses) are determined using standard errors robust to clustering at the firm level. ***, **, and * denote 1%, 5%, and 10% statistical significance. All variables are defined in Supporting Information Appendix A1.

that involve a brokerage house that is affiliated with a financial institution that is an active lender in the syndicated loan market. The baseline results remain virtually unchanged (not tabulated), suggesting that not a change in credit supply conditions but the reduction in analyst coverage explains the increase in sweep use following brokerage house mergers and closures.

Finally, we investigate the parallel trends assumption underlying our identification strategy. We split the 4-year window around each merger into the individual years and use the year prior to treatment as baseline. The results shown in Table 3 support that the parallel trends assumption holds. All coefficients in year t-2 are insignificant and close to zero, that is, there is no pre-event effect from year t-2 to t-1. The table furthermore shows that the use of sweeps seems to increase over time following the event.

3.1.2 | Treatment effect by initial coverage

In this section, we test whether the impact of analyst coverage reduction on the use of sweep provisions is more pronounced for firms with lower initial analyst coverage. We would expect that firms with already low coverage are

more affected by an additional reduction in analyst coverage compared to firms with a high ex ante coverage. We split the treatment sample based on the analyst coverage level pre-event. In particular, we define four groups: firms with low coverage (1–10 analysts), medium coverage (11–20 analysts), medium-high coverage (21–30 analysts), and high coverage (>30 analysts). The results are reported in Table 4.

Column (1) indicates that the effect of analyst coverage reduction on the use of sweep provisions is decreasing in initial coverage. The DiD estimate is 14.4% and is statistically significant for firms with low initial coverage compared to an insignificant effect of 0.4% for firms with high coverage. The difference between both groups is statistically significant. The same pattern of a decreasing coefficient by increasing initial coverage is also observable in column (2) for the number of sweep covenants and in column (3) for the ratio of sweep covenants.

Columns (4) and (5) show that the likelihood and the number of financial covenants are not affected by exogenous changes in analyst coverage, irrespective of the ex-ante level of analyst coverage. In column (6), we include the ratio of profitability-based covenants to total financial covenants as the dependent variable. We again observe that the effect of analyst coverage reduction on the use of profitability-covenants is decreasing in initial coverage.¹⁶

3.2 | Corporate governance

Our results reveal a causal impact of analyst coverage on the use of sweep covenants in loan contracts. In this section, we examine the cross-sectional variation in more detail. Chava et al. (2010) argue that managerial agency problems are a decreasing function of corporate governance; that is, good corporate governance helps constrain managers. A natural follow-up question to our baseline analysis is accordingly whether the impact of reductions in analyst coverage on the use of sweep provisions varies with other corporate governance characteristics of the firm.

As argued by Irani and Oesch (2013), external monitoring (i.e., analyst coverage) and other corporate governance mechanisms can be substitutes or complements. If other corporate governance mechanisms and analyst coverage are substitutes—as the evidence in Irani and Oesch (2013) suggests—we would expect to find a stronger effect of analyst coverage reductions on sweep use for poorly governed firms. In contrast, well-governed firms should have other means in place that constrain managers. We therefore distinguish firms by their characteristics and by indicators of managerial entrenchment. Each of our corporate governance variables is measured at the firm level *prior* to the respective brokerage house merger or closure, as any post-event governance changes might be the *result* of the analyst coverage reduction.

We use the five following standard proxy variables to measure corporate governance quality: (i) We identify firms without a credit rating by an external rating agency. Similar to analyst coverage, the lack of coverage by a credit rating agency implies lower outside monitoring. (ii) We split firms by their cash holdings, as managerial agency problems are more severe for companies with excess funds (Jensen, 1986; Kalcheva & Lins, 2007; Lie, 2000). (iii) We identify CEOs with long tenure, who can affect organizational structure over time to enhance entrenchment (e.g., Finkelstein & Hambrick, 1989). Information on CEO tenure is obtained from ExecuComp and firms with CEOs in the top decile in terms of tenure are classified as “long tenure.” (iv) We categorize managers by their compensation. Cash salary and bonuses provide relatively low-powered incentives (Jensen & Murphy, 1990), and a high fraction of cash salary and bonuses relative to total compensation has been shown to positively relate with managerial entrenchment. Information on

¹⁶ We provide several additional tests in the Supporting Information Appendix. Supporting Information Appendix A2 shows the distribution of sweep levels (i.e., how much of the proceeds must be used for prepayment) by sweep type. Supporting Information Appendix A3 provides regression results for each sweep type individually, confirming that results are not driven by a particular type of sweep. In Appendix A4, we investigate whether a reduction of analyst coverage has an impact on the strictness of sweeps in a loan contract. We calculate sweep strictness as the average percentage of the cash proceeds which must be used for repayment (across all sweep types if more than one sweep is included in the contract). Contracts without any sweep provision are coded as “zero percent” strictness. The average sweep level following this definition is 23.6%; that is, 23.6% of proceeds must be used for prepayment (52.7% conditional on at least one sweep being included in the contract). The results show that loan contracts receive stricter sweeps after events. In addition, we also analyze if a loan contract is more likely to include a dividend restriction, a capital expenditure restriction, and if the loan is secured by collateral. In contrast to sweeps, we observe that lenders neither change their use of dividend or capital expenditure restrictions nor secure loans more often with collateral.

TABLE 4 Brokerage house mergers—Effect by initial coverage.

	Sweep covenants			Financial covenants		
	(1)	(2)	(3)	(4)	(5)	(6)
	SWEEP (0/1)	LN(#SWEEP)	SWEEP RATIO	FIN COV (0/1)	LN (#FIN COV)	PERF COV RATIO
POST × TREATED × COVERAGE (1–10)	0.144** (0.013)	0.198** (0.013)	0.109*** (0.004)	−0.044 (0.151)	−0.070 (0.155)	0.062* (0.074)
POST × TREATED × COVERAGE (11–20)	0.101** (0.030)	0.136** (0.037)	0.064** (0.036)	−0.003 (0.898)	0.011 (0.721)	0.058** (0.032)
POST × TREATED × COVERAGE (21–30)	0.074 (0.190)	0.128* (0.054)	0.075** (0.037)	0.009 (0.787)	−0.012 (0.771)	0.042 (0.180)
POST × TREATED × COVERAGE (>30)	0.004 (0.944)	0.045 (0.531)	0.046 (0.250)	−0.009 (0.784)	−0.027 (0.669)	0.049 (0.566)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes	Yes	Yes	Yes
Merger × POST FE	Yes	Yes	Yes	Yes	Yes	Yes
Merger × firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	2966	2966	2919	2966	2966	2788
Adj. R ²	0.689	0.735	0.649	0.314	0.593	0.806
Statistical difference between coefficients						
POST × TREATED × COVERAGE (1–10) = POST × TREATED × COVERAGE (>30)						
Difference	0.140* (0.078)	0.152 (0.133)	0.063 (0.227)	−0.035 (0.430)	−0.042 (0.588)	0.013 (0.884)
Difference p-value						

Note: This table reports results from the estimation of a pooled panel regression analyzing the use of sweep and financial covenants around brokerage house mergers. For each merger, we consider a 2-year window prior to the merger (pre-merger window) and a 2-year window after the merger (post-merger window). We construct an indicator variable (TREATED) for each merger, which is equal to one for each firm covered by both merging brokerage houses in the pre-merger window (treatment sample), and zero otherwise. For each merger, POST is a variable that is equal to one for the post-merger period and zero for the pre-merger period. Both variables are included as base effects in each regression. COVERAGE (a-b) is a dummy variable that equals one if the number of analysts covering the firm is within the interval a-b, and zero otherwise. SWEEP (0/1) is a dummy variable, which equals one if the loan contract includes at least one sweep covenant, and zero otherwise. LN(#SWEEP) is the log of one plus the number of sweep covenants included in the loan contract. SWEEP RATIO is defined as the number of sweep covenants divided by the total number of sweep and financial covenants included in the loan contract. FIN COV (0/1) is a dummy variable, which equals one if the loan contract includes at least one financial covenant, and zero otherwise. LN(#FIN COV) is the log of one plus the number of financial covenants included in the loan contract. PERF COV RATIO is the number of performance-covenants divided by the total number of financial covenants (performance-covenants plus capital-covenants) in the loan contract. Financial covenants are divided into performance-covenants and capital-covenants following Christensen and Nikolaev (2012). All regressions include merger × firm as well as merger × POST fixed effects. Further, the regressions include firm characteristics (log total assets, leverage, market-to-book, profitability, tangibility, coverage, current ratio, and rating fixed effects [notch level]) used with their previous year-end value and loan characteristics (log loan size, log maturity, and indicator variables for loan purpose and loan types). *p*-values (in parentheses) are determined using standard errors robust to clustering at the firm level. ***, **, and * denote 1%, 5%, and 10% statistical significance. All variables are defined in Supporting Information Appendix A1.

manager compensation is obtained from ExecuComp, and above median cash compensation ratios are defined as “high.” (v) Finally, we identify firms that have a low number of institutional shareholders. There is ample evidence that institutional shareholders improve monitoring and hence reduce entrenchment (e.g., Del Guercio & Hawkins, 1999; Gillan & Starks, 2000; Hartzell & Starks, 2003). We define firms with few institutional owners to be firms that have a below median number of institutional shareholders.¹⁷ Institutional shareholders are identified based on Thomson Reuters 13-F institutions.

The results are shown in Table 5. They indicate that in general, analyst coverage and other corporate governance characteristics are substitutes. In column (1), we differentiate between firms with and without credit ratings. The effect is much larger in magnitude for non-rated firms (0.189) compared with rated firms (0.041) and the difference between both coefficients is statistically highly significant. This supports the conjecture that rated firms, on average, have better outside monitoring.

In column (2), we split firms by their cash holdings. The results confirm that effects are stronger for firms with larger cash holdings. This is consistent with firms with excess funds having a higher degree of managerial agency problems. The coefficient for firms with high cash holdings is 0.141 and statistically significant compared to statistically insignificant 0.051 for firms with low cash and the difference between both coefficients close to being statistically significant.

In column (3), we use CEO tenure as proxy for corporate governance. The effect for poorly governed firms is positive and statistically significant (0.152). The effect for firms with shorter CEO tenure is significantly smaller and not statistically significant (0.053). This is consistent with the effects being stronger for firms with more entrenched managers. However, plausibly due to a reduced sample of firms for which information on CEO tenure is available, the null hypothesis that the estimate for well-governed firms is equal to the estimate for poorly governed firms cannot be rejected at conventional levels (p -value = 0.18).

In column (4), we split firms into subgroups based on the CEO cash compensation ratio. We find that the effect of analyst coverage on sweep use is particularly pronounced for treated firms, whose CEOs have a high fraction of cash salary in their compensation package (0.116). The effect for well-governed firms, that is, firms with CEOs with compensation packages that offer more high-powered incentives, is close to zero and statistically insignificant. However, the null hypothesis that the estimate for well-governed firms is equal to the estimate for poorly governed firms cannot be rejected at conventional levels (p -value = 0.13).

Finally, in column (5), we use institutional ownership as proxy for corporate governance. The estimated marginal effect of analyst coverage on sweep use for firms with a low number of institutional investors is positive and significant (0.241). For firms with a high number of institutional owners, the estimated treatment effect is close to zero and statistically insignificant. We reject the null hypothesis that the estimates are equal.

This evidence also helps to discriminate between creditor–shareholder conflicts and managerial agency problems in our setting. As argued in Huang (2010), institutional block holders might be a proxy for a higher degree of creditor–shareholder conflicts, as block holders can exercise greater influence on management to adopt policies in favor of shareholders. Our results indicate that sweep provisions are particularly helpful in situations with *weaker* influence from prominent shareholders, which is consistent with sweeps being particularly valuable in situations with a high degree of managerial entrenchment.

Overall, our results in Table 5 support the substitution hypothesis. External monitoring and other corporate governance mechanisms are substitutes. The results for the effect of analyst reduction on sweep use in loan contracts are larger and only significant for poorly governed firms. The difference between the coefficients for well-governed firms and those with poor corporate governance is in most cases (close to) statistically significant. This provides further support that sweep covenants are used by lenders to address managerial agency problems.

¹⁷ Results based on the fraction of institutional ownership are virtually identical.

TABLE 5 Brokerage house mergers—Effect by corporate governance.

	SWEEP (0/1)				
	(1)	(2)	(3)	(4)	(5)
POST × TREATED × NOT RATED	0.189*** (0.000)				
POST × TREATED × RATED	0.041 (0.267)				
POST × TREATED × HIGH CASH		0.141*** (0.004)			
POST × TREATED × LOW CASH		0.051 (0.180)			
POST × TREATED × LONG TENURE			0.152** (0.035)		
POST × TREATED × SHORT TENURE			0.053 (0.163)		
POST × TREATED × HIGH CASH COMP				0.116** (0.039)	
POST × TREATED × LOW CASH COMP				0.021 (0.585)	
POST × TREATED × LOW INST INV					0.241** (0.028)
POST × TREATED × HIGH INST INV					0.041 (0.340)
Firm controls	Yes	Yes	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes	Yes	Yes
Merger × Post FE	Yes	Yes	Yes	Yes	Yes
Merger × firm FE	Yes	Yes	Yes	Yes	Yes
Observations	2966	2966	1893	2966	2941
Adjusted R ²	0.690	0.689	0.693	0.688	0.686
Statistical difference between coefficients					
Difference	0.148***	0.090	0.099	0.095	0.199*
Difference p-value	0.010	0.103	0.180	0.132	0.076

Note: This table reports results from the estimation of a pooled panel regression analyzing the use of sweep and financial covenants around brokerage house mergers. For each merger, we consider a 2-year window prior to the merger (pre-merger window) and a 2-year window after the merger (post-merger window). We construct an indicator variable (TREATED) for each merger, which is equal to one for each firm covered by both merging brokerage houses in the pre-merger window (treatment sample), and zero otherwise. For each merger, POST is a variable that is equal to one for the post-merger period and zero for the pre-merger period. Both variables are included as base effects in each regression. SWEEP (0/1) is a dummy variable, which equals one if the loan contract includes at least one sweep covenant, and zero otherwise. LN(#SWEEP) is the log of one plus the number of sweep covenants included in the loan contract. SWEEP RATIO is defined as the number of sweep covenants divided by the total number of sweep and financial covenants included in the loan contract. FIN COV (0/1) is a dummy variable, which equals one if the loan contract includes at least one financial covenant, and zero otherwise. LN(#FIN COV) is the log of one plus the number of financial covenants included in the loan contract. PERF COV RATIO is the number of performance-covenants divided by the total number of financial covenants (performance-covenants plus capital-covenants) in the loan contract. Financial covenants are divided into performance-covenants and capital-covenants following Christensen

(Continues)

TABLE 5 (Continued)

and Nikolaev (2012). NOT RATED (RATED) is a dummy variable that equals one if the firm has no credit rating pre-merger, and zero otherwise. LOW (HIGH) CASH is a dummy variable that equals one if the firm is in the bottom (top) half of the cash ratio to total assets distribution pre-merger, and zero otherwise. LONG TENURE (SHORT TENURE) is a dummy variable that equals one if the CEO is (not) in the top decile in terms of tenure pre-merger, and zero otherwise. HIGH CASH COMP (LOW CASH COMP) is a dummy variable that equals one if the proportion of total compensation of the CEO paid through cash salary and bonuses is above (below) median pre-merger, and zero otherwise. LOW INST INV (HIGH INST INV) is a dummy variable that equals one if the number of institutional owners is below (above) median pre-merger, and zero otherwise. All regressions include merger \times firm as well as merger \times POST fixed effects. Further, the regressions include firm characteristics (log total assets, leverage, market-to-book, profitability, tangibility, coverage, current ratio, and rating fixed effects [notch level]) used with their previous year-end value and loan characteristics (log loan size, log maturity, and indicator variables for loan purpose and loan types). *p*-values (in parentheses) are determined using standard errors robust to clustering at the firm level. ***, **, and * denote 1%, 5%, and 10% statistical significance. All variables are defined in Supporting Information Appendix A1.

3.3 | Dual holdings

We argue that sweep provisions are used by lenders to address managerial agency problems. However, one concern might be that actions taken by equity holders might be better suited to discipline managers, which might not be in the interest of debtholders. Furthermore, most firms operate well above their default barrier. In this case, the contingencies that sweeps address might be especially relevant for equity holders. This suggests that sweep use increases more for lenders who simultaneously also hold equity of the borrowing firm (dual holding).

Ferreira and Matos (2012) show that the benefit of dual holding mainly accrues to the bank. Chava et al. (2019) show that banks as dual holders are less likely to include a capital expenditure restriction in their loan contract. Peyravan (2020) investigates the impact of financial reporting quality on institutional investors' dual holdings and finds that institutional investors are more likely to become dual holders in firms with low reporting quality. This suggests that dual holdings might also often be related to firms with a potentially higher degree of managerial entrenchment.

We identify lenders who also hold equity of their borrowing firm using 13f filings. We investigate the use of sweep provisions separately for firms with and without a lender as dual holder. Table 6 reports the results. In Panel A, we interact the treatment \times post indicator with a dual holding indicator that is equal to one if the (lead) banks in the loan syndicate hold at least 0.5% of the equity of the borrowing firm in the pre-merger period. In Panel B, we split the sample into dual holding and non-dual holding banks. The results indicate that banks implement sweep provisions after reductions in analyst coverage in particular when they hold both equity and debt of the same firm. In terms of economic magnitudes, the results indicate that it is about three times more likely that sweep provisions are included post-treatment in the presence of dual holders compared to situations where banks do not have equity holdings in the borrowing firms. Note that while the effect is stronger in the presence of dual holders, also non-dual holders are significantly more likely to include sweep provisions following a reduction in analyst coverage. Overall, the results indicate that lenders increase sweep use in particular when they are most exposed to managerial wealth expropriation.

4 | CONCLUSION

We investigate how changes in analyst coverage affect the implementation of sweep covenants in loan contracts. For this purpose, we utilize exogenous changes in analyst coverage resulting from brokerage house mergers or closures. We observe that an exogenous decrease in coverage results in a more intense use of sweep provisions in a loan contract. This effect is stronger for firms with poor corporate governance and for firms with lenders as dual holders. Overall, our results are consistent with lenders implementing sweep covenants in loan contracts to address managerial agency problems.

TABLE 6 Brokerage house mergers—Effect by dual holdings.

Panel A: Effect by dual holdings—interaction term						
	Total sample		Total sample		Total sample	
	(1)	(2)	(3)	(4)	(5)	(6)
	SWEEP (0/1)	LN(#SWEEP)	SWEEP RATIO	SWEEP (0/1)	LN(#SWEEP)	SWEEP RATIO
POST × TREATED	0.065*	0.098**	0.058***			
	(0.052)	(0.033)	(0.008)			
POST × TREATED × DUAL	0.186*	0.274*	0.118			
	(0.095)	(0.056)	(0.118)			
Base effects	Yes	Yes	Yes			
Firm controls	Yes	Yes	Yes			
Loan controls	Yes	Yes	Yes			
Merger × Post FE	Yes	Yes	Yes			
Merger × firm FE	Yes	Yes	Yes			
Observations	2966	2966	2919			
Adj. R ²	0.689	0.737	0.652			
Panel B: Effect by dual holdings—sample split						
	No dual	No dual	No dual	Dual	Dual	Dual
	(1)	(2)	(3)	(4)	(5)	(6)
	SWEEP (0/1)	LN(#SWEEP)	SWEEP RATIO	SWEEP (0/1)	LN(#SWEEP)	SWEEP RATIO
POST × TREATED	0.063*	0.094**	0.060***	0.225**	0.337**	0.151**
	(0.059)	(0.041)	(0.007)	(0.047)	(0.031)	(0.021)
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes	Yes	Yes	Yes
Merger × Post FE	Yes	Yes	Yes	Yes	Yes	Yes
Merger × firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2638	2638	2592	326	326	326
Adj. R ²	0.684	0.736	0.644	0.790	0.804	0.803

Note: This table reports results from the estimation of a pooled panel regression analyzing the use of sweep covenants around brokerage house mergers. For each merger, we consider a 2-year window prior to the merger (pre-merger window) and a 2-year window after the merger (post-merger window). We construct an indicator variable (TREATED) for each merger, which is equal to one for each firm covered by both merging brokerage houses in the pre-merger window (treatment sample), and zero otherwise. For each merger, POST is a variable that is equal to one for the post-merger period and zero for the pre-merger period. Both variables are included as base effects in each regression. DUAL is defined as loans of lenders which simultaneously hold at least 0.5% of equity of the borrowing firm in the pre-merger period. POST × DUAL is included as base effect in each regression in Panel A. SWEEP (0/1) is a dummy variable, which equals one if the loan contract includes at least one sweep covenant, and zero otherwise. LN(#SWEEP) is the log of one plus the number of sweep covenants included in the loan contract. SWEEP RATIO is defined as the number of sweep covenants divided by the total number of sweep and financial covenants included in the loan contract. All regressions include merger × firm as well as merger × POST fixed effects. Further, the regressions include firm characteristics (log total assets, leverage, market-to-book, profitability, tangibility, coverage, current ratio, and rating fixed effects [notch level]) used with their previous year-end value and loan characteristics (log loan size, log maturity, and indicator variables for loan purpose and loan types). *p*-values (in parentheses) are determined using standard errors robust to clustering at the firm level. ***, **, and * denote 1%, 5%, and 10% statistical significance. All variables are defined in Supporting Information Appendix A1.

Our findings are important for loan contract design. Most of the existing literature focuses on financial covenants and, if at all, includes sweep covenants only as a by-product in the analyses. Our results indicate that sweep covenants serve an important role in addressing managerial agency problems. Financial covenants focus on borrower performance and leverage ratios and trigger a shift of control rights to lenders when the financial condition of the firm deteriorates. However, contingencies of lender wealth expropriation by management exist *especially* in situations with high managerial flexibility and hence need to be addressed through other means, such as sweep covenants. Contracting can increase the value of the firm by addressing potential agency conflicts. Accordingly, sweep covenants deserve much more attention than is given today. An analysis of sweep covenants and firm value might be a promising area for future research.

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Additional supporting information can be found online in the Supporting Information section at the end of this article.

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