# Short-termism Spillovers from the Financial Industry*

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**Abstract**

To meet short-term benchmarks, lenders may alter their monitoring behavior, providing a channel for short-termism to spill over to their borrowers. We find that short-termist lenders are significantly more likely to enforce covenant breaches. This behavior is pronounced when performance benchmarks are precise or salient, and when managers have high pay-performance sensitivity, but not when they face strong shareholder governance. Affected borrowers are more likely to switch lenders, pay higher spreads on renegotiated loans, and reduce investment. Our findings suggest that bank managers trade off relationship capital for income-boosting fees and term changes from covenant enforcement to meet earnings benchmarks.

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# 1 Introduction

The transmission of corporate shocks is an important friction affecting intrafirm and interfirm relationships. Capital and labor shocks are transmitted through firms' internal markets (Silva 2013; Giroud and Mueller 2015, 2017); stockholders transmit shocks to corporations through financing and governance channels;[1] customers and suppliers transmit shocks through production networks;[2] perhaps most importantly, financial intermediaries transmit shocks. Indeed, because the financial sector interacts with other sectors (through lending, equity issuance, bond underwriting, etc.), financial sector shocks and incentives could have far-reaching effects.[3] Given rising concerns about "quarterly capitalism" (Barton 2011), in this paper we focus on the consequences of myopic behavior in the financial sector. Specifically, we investigate how lender short-termism spills over to borrowers through covenant enforcement decisions.

The first challenge in answering this question is how to measure lender short-termism. We build on prior work on managerial myopia that focuses on the incentive to meet short-term earnings benchmarks (Stein 1989; Graham, Harvey, and Rajgopal 2005; Cheng and Warfield 2005; Bergstresser and Philippon 2006; Peng and Roell 2008). As this literature argues, short-term earnings benchmarks provide a natural context in which to study managerial myopia because firms close to the threshold stand to gain an immediate capital market benefit from increasing reported earnings (Bhojraj, Hribar, Picconi, and McInnis 2009; Bird, Karolyi, and Ruchti 2019). For our sample of public lenders, there are significant capital market benefits to just-beating the analyst consensus forecast (See Panel A of Figure 1). Panel B of Figure 1 shows a significant abnormal mass of banks that just manage to beat the forecast. The literature suggests that these

[1] See Cooper and Kaplanis (1994), Coval and Moskowitz (1999), Pedersen, Mitchell, and Pulvino (2007), Van Nieuwerburgh and Veldkamp (2009), Boyson, Stahel, and Stulz (2010), Shek, Shim, and Shin (2018), Dew-Becker and Giglio (2016), and Belo, Lin, and Yang (2019).

[2] See Gabaix (2011), Acemoglu, Carvalho, Ozdaglar, and Tahbaz-Salehi (2012), Breza and Liberman (2017), Murfin and Njoroge (2015), Wu (2016), and Kelly, Lustig, and Van Nieuwerburgh (2017).

[3] For example, financial markets regulation and bank supervision spills over to the corporate sector through financial intermediaries (Hirtle, Kovner, and Plosser 2016), and lenders transmit financial shocks to borrowers (Chava and Purnanandam 2011; Chodorow-Reich 2014; Bord, Ivashina, and Taliaferro 2015; Carvalho, Ferreira, and Matos 2015).

two facts are directly related—banks respond to capital market incentives by manipulating their earnings. We thus define short-termist lenders as those that end up just to the right of the analyst consensus forecast, as these lenders are both the most likely to have had short-termism incentives and to have acted on them.

We investigate the consequences of this short-termism in the context of covenant enforcement decisions. When borrowers breach a financial covenant threshold, the lender gains some control rights and then must decide how to use this potential leverage over the borrower. The lender could accelerate the loan, requiring the borrower to immediately post principal repayment, but more typically pursues a formal waiver, which can involve a waiver fee, or a loan amendment, which may involve an amendment fee and changes in loan terms. Financial covenant thresholds and the underlying financial ratios and amounts are observable, so we can calculate the distance to covenant thresholds and identify covenant breaches, which occur when the distance to covenant thresholds is below zero. We define enforcement using material covenant violations as disclosed by borrowers; that is, we say that the covenant breach is enforced when it has material consequences for the borrower.[4]

The lender faces a tradeoff in making the enforcement decision. Enforcing a covenant breach can increase the lender's earnings in the short term, for example through fees, but it may harm the lender's relationship with the borrower, reducing future earnings. Our hypothesis is that short-termist lenders tilt their calculus toward the short-term benefits. If so, we expect to see that relatively short-termist lenders increase enforcement rates. In making this comparison, we hold fixed the borrower's covenant slack and include a rich set of other control variables and fixed effects. This means that other important considerations in the enforcement decision, such as risk mitigation, should be held constant in our tests.

We find strong empirical support for both sides of the tradeoff. Short-termist lenders indeed enforce covenant breaches at a significantly higher rate; this finding is robust to a wide variety of alternative specifications and measurement choices. Further, incremental enforcement

[4] We are grateful to Greg Nini for providing us data on material covenant violations (Becher, Griffin, and Nini 2018).

by short-termist lenders is greater when the lender's top executives have higher pay-performance sensitivity, so that they likely have greater incentive to meet short-term earnings benchmarks, but enforcement is reduced when the lender has strong corporate governance. Not only does enforcement result in increased earnings through fees, but short-termist lenders also negotiate relatively larger increases in spreads and loan amounts following a breach. Moreover, for the subsample of loan amendments and waivers where we can observe fees, these are approximately twice as high for short-termist lenders. However, this shift toward short-term benefits is costly, as affected borrowers are significantly more likely to switch lenders for their next loan (Kang and Stulz 2000; Bharath, Dahiya, Saunders, and Srinivasan 2011; Gopalan, Udell, and Yerramilli 2011). Switching borrowers are not different from non-switchers, which is inconsistent with short-termist lenders efficiently pruning their portfolio of loans. Furthermore, we find that higher covenant enforcement rates are associated with lower loan growth and diminished reputation. Most importantly, short-termist lenders experience significantly lower returns over the following three years, suggesting that this behavior is costly to lenders in the long run.

To attribute these effects to lender short-termism, it is important to show that the differential choices of short-termist lenders actually result in a significant increase in earnings; that is, they plausibly help lenders meet their short-term performance benchmarks. To this end, we use our empirical estimates of both the incremental enforcement by short-termist lenders as well as the consequences for fees, spreads, and loan amounts to conduct two simulations. First, for quarterly observations of each lender in our sample, we simulate the earnings impact of enforcement on that lender's average loan in that particular year. We then use this quantity to calculate the number of such enforcements that would be necessary to increase the lender's reported earnings per share by one cent. We find that this would require two and four enforcements for the modal and median lender-quarter in our sample. Second, we quantify approximately how much of the apparent overall earnings manipulation by lenders suggested by Panel B of Figure 1 could be explained by our proposed covenant enforcement mechanism. When we recompute the distribution of earnings surprise by removing the effects of incremental enforcement, we obtain a discontinuity around the benchmark that is 41.1% smaller. While both

of these simulations require some assumptions, they both support the quantitative importance of the covenant enforcement channel.

Following our approach to these simulations, we construct an ex ante measure of short-termism incentives based on the latent (i.e., pre-enforcement) distribution of EPS. This measure captures lenders that have an incentive to manipulate earnings to help beat their benchmark and that could also plausibly do so via covenant enforcement, as our simulations demonstrate. Our estimates using this ex ante measure are consistent with our main estimates in terms of economic magnitude and statistical significance despite relying on a distinct source of identifying variation. Together with our simulations, these findings illustrate how our main estimates capture the ex ante short-termism incentives of lenders to increase covenant enforcement rates rather than other differences between lenders.

The incremental cost of funding renegotiated loans, combined with waiver fees and unobservable switching costs, may lead borrowers to alter their investment behavior. We provide evidence that lenders' short-termism has real effects on the investment policy of their borrowers. Relative to other borrowers that breach their covenant thresholds, affected borrowers cut capital investment by 3.9%. Additionally, borrowers' announcements of material covenant violations are met with 160 basis points lower market reactions when they have short-termist lenders. Together, these findings suggest that the incremental attention from lenders facing short-termism incentives is value-decreasing for borrowers' shareholders.

There is an extensive range of literature studying the role of myopia in finance. Asker, Farre-Mensa, and Ljungqvist (2015) study the consequences of short-termism for investment decisions by investigating the differences between public and private firms. They find that private firms invest more than public firms and are more responsive to investment opportunities. Edmans, Fang, and Lewellen (2016) develop a measure of managerial short-termism based on the impending vesting of CEO equity and tie this to reductions in investment. Salitskiy (2015) uses a similar measure to show that CEO compensation duration is positively associated with firm risk. Ladika and Sautner (2019) use the implementation of FAS 123R, which required option compensation to be expensed, to show that executives with more short-term incentives spend less on long-term

investment. Contrastingly, Laux (2012) shows that short-term incentives may provide benefits through earlier feedback regarding CEO talent. Relevant to this literature, we find that one of the key determinants of myopic behavior, the incentive to meet short-term earnings benchmarks, is transmitted from the financial sector to the corporate sector through financing relationships.

Theoretically, the incentives provided to lenders by analysts' consensus forecasts depend on the ex ante precision and salience of this benchmark. As a validation test of our measure, we analyze lender heterogeneity in the dispersion and coverage of analyst forecasts, and find evidence that lenders are more likely to act myopically when forecast dispersion is low and analyst coverage is high. Additionally, we corroborate our results with alternative measures of short-termism that are not dependent on analyst forecasts or realized lender performance.

Earnings management is an important and well-documented manifestation of managerial myopia that can occur through accruals manipulation (e.g., Burgstahler and Dichev 1997) or real activities manipulation (e.g., Roychowdhury 2006). We look specifically at this behavior for banks (Scholes, Wilson, and Wolfson 1990; Beatty, Ke, and Petroni 2002). Banks have been shown to be particularly likely to engage in end-of-quarter transactions management in an effort to meet their quarterly earnings benchmarks. For example, Dechow and Shakespeare (2009) study the increased use of loan securitization toward fiscal quarter ends, while Ertan (2019) shows that banks initiate more (and conditionally cheaper) loans toward the end of quarters in which this would help banks meet earnings benchmarks. Our findings suggest that lenders are willing to sacrifice relationship capital in order to meet earnings benchmarks. Because relationship capital is so valuable, our finding underscores the importance of benchmarks as a managerial objective to lenders, consistent with the survey evidence of Graham, Harvey, and Rajgopal (2005). However, the apparent costliness of covenant enforcement also suggests that lenders might seek less costly alternative tools before proceeding to covenant enforcement. Indeed, in cross-sectional tests, we find that short-termist lenders are more likely to increase covenant enforcement when they are already using these ostensibly cheaper tools, such as reducing loan loss reserves.

Our work is closely related to the literature on lender control and loan contracting. This literature finds that lenders exert either implicit or explicit control over borrower actions when

borrowers are in breach of covenants, including on governance and executive compensation, investment, employment, innovation, and capital structure (Chava and Roberts 2008; Roberts and Sufi 2009a; Nini, Smith, and Sufi 2009, 2012; Falato and Liang 2013; Gu, Mao, and Tian 2017; Chakraborty, Chava, and Ganduri 2016). Covenant enforcement also has direct and negative financing consequences for borrowers, such as covenant waiver fees, spread increases, renegotiation, refinancing, and potentially payment default (Roberts 2015, Freudenberg, Imbierowicz, Saunders, and Steffen 2017). Some of these negative consequences are persistent; renegotiation often involves increasingly strict covenants or collateral requirements, which make future default more likely. Our results suggest that lenders' incentives to meet short-term earnings benchmarks provide within-lender variation in their detection and enforcement of covenant breaches.

Bank financial health affects lending terms, and therefore borrowers (Hubbard, Kutner, and Palia 2002). In line with this evidence, shocks to lenders may spread to their respective borrowers. For example, Berger, Saunders, Scalise, and Udell (1998) find that bank mergers lead to a reduction in small business lending by the newly merged lender, though this is largely offset by the actions of other lenders. In particular, the reduction in lending is mitigated by increased lending by banks not affected by the shock (Bord, Ivashina, and Taliaferro 2015). Gan (2007) shows that borrowers whose lenders were exposed to the land market collapse in Japan experienced declines in market values and reduced investment. Further, Peek and Rosengren (1997) study the transmission of this shock to the lending activities of US branches of Japanese banks, finding similar negative outcomes. However, Ongena, Smith, and Michelsen (2003) study a crisis in the Norwegian banking system and find relatively small effects on borrowers. Chodorow-Reich (2014) finds that negative shocks to lenders lead to negative outcomes for borrowers, particularly a reduction in employment. This transmission is worse for bank-dependent borrowers (Chava and Purnanandam 2011), and this contagion is not mitigated by public debt markets (Carvalho, Ferreira, and Matos 2015). Murfin (2012) shows that, following negative shocks to their loan portfolios, banks impose stricter covenants on borrowers. However, Loutskina and Strahan (2009) find that securitization has reduced the effect of bank characteristics on borrowers. Our results

provide new evidence that lenders transmit to borrowers not only financial shocks, but also their own short-term capital markets incentives. These results suggest that, through financing relationships, the pressures of public markets can be transmitted across governance structures and potentially even to private firms.

## 2 Data and Measurement

We construct our main estimation sample using several sources: the Center for Research in Security Prices (CRSP), Standard & Poor's Compustat, I/B/E/S, and Thomson Reuters' DealScan. We obtain market data from CRSP, quarterly firm financials from Compustat, earnings-per-share forecasts and realizations from I/B/E/S, and loan details from DealScan. In addition to these sources, we rely on i) Michael Roberts' link table to match DealScan borrowers to Compustat/CRSP, ii) Aytekin Ertan's link table to match DealScan lead lenders to Compustat/CRSP and I/B/E/S, and iii) Greg Nini's data to identify material covenant violations (Becher, Griffin, and Nini 2018). We also exclude borrowers from the financial and utilities sectors from our analysis.[5] The intersection of these data spans 1990 to 2016, and we concentrate on this period in our main tests. However, limited DealScan coverage in the early years of this sample means that over 99% of our sample observations are from the 1996 to 2016 period. These considerations and matching procedures yield a total sample of 10,785 distinct loan packages issued by 3,647 borrowers and 81 lenders, which we measure at the parent level.

Since we want to understand lender behavior on a time-varying basis (rather than on a loan-initiation basis), we trace loans over time. To do so, we construct our main sample at the loan package-quarter level. We opt for loan packages rather than tranches because covenants are defined at the package level, and for loan packages rather than the borrowing entity since the same borrower may have multiple loans outstanding from different lenders in a given quarter. With these objectives in mind, we construct the sample as follows. We match each loan package

[5] Two-digit SIC codes between 60 and 69, and between 44 and 50, respectively.

to the underlying Compustat borrower, in order to compute the borrower's time-varying covenant slack and financial characteristics. We also match loans to the Compustat lead lender, which allows us to assign a time-varying measure of lender performance to each package-quarter. We convert packages to package-quarters using the stated start and end dates, which, after other data requirements, yields a total of 131,621 observations.[6]

As we explain in the next subsection, we are concerned with lender behavior around the zero EPS surprise threshold (i.e., where the lender's EPS realization is equal to the analysts' consensus EPS forecast). We thus focus on a narrow window within two cents of EPS around the analysts' consensus forecast in our main sample. Since borrowers that severely breach their covenant thresholds may be systematically different from other borrowers, we also exclude package-quarter observations with extreme values of covenant slack. Together, these restrictions reduce our main estimation sample to 38,217 observations. Appendix A includes variable definitions and calculations, and Panel A of Table 1 reports summary statistics of the borrower, loan, and lender characteristics for our main sample.

### 2.1 Defining short-termist lenders

Our proxy for lender short-termism is *STLender*, an indicator variable that equals one for lender-quarters in which reported EPS either meets or beats the Institutional Brokers' Estimate System (I/B/E/S) consensus by less than two cents (Roychowdhury 2006; Cheong and Thomas 2011). In calculating these measures, we use unadjusted and unscaled one-quarter-ahead forecasts, following Diether, Malloy, and Scherbina (2002) and Cheong and Thomas (2011). Panel A of Table 1 shows that the mean (median) EPS surprise of lenders in our main sample is 0.4 (0), and 58.5% of the lender-quarter observations in this sample are short-termist (i.e., $STLender = 1$).

We follow the literature on earnings benchmarks by interpreting the realized EPS surprise as a measure of earnings management (Burgstahler and Dichev 1997). Relative to the group of

[6] We define package maturity as the stated maturity date of the largest tranche.

lenders further from the zero EPS surprise threshold, the group with zero or one cent of EPS surprise will include more lenders that were likely to have altered their behavior (Healy 1985; Roychowdhury 2006; Bird, Karolyi, and Ruchti 2019). The key idea is that these lenders are both the most likely to have had incentives for short-termism and to have acted on them. Importantly, this measure of lender short-termism is observable even though the ex ante circumstances of each bank's manipulation decision are not. Further, the fact that the capital market benefit of benchmark beating is discontinuous provides powerful variation for studying the banks' incentives.

We acknowledge several concerns and limitations with this empirical approach. It is possible that short-termist lenders change their behavior in the presence of benchmarks, but the change does not actually help lenders achieve the benchmark. We return to an investigation of the quantitative implications of covenant enforcement as a strategy to achieve EPS benchmarks in Section 4.4. Short-termist lenders may also change their behavior for reasons other than meeting their EPS benchmarks. If borrowers are affected by the lender's change in behavior, then regardless of the rationale for the change, our results are consistent with costly spillovers from the financial industry. However, because we document that covenant enforcement does help with benchmark beating and is costly, for example in terms of harming lending relationships, the most parsimonious explanation is that the enforcement is a purposeful strategy aimed at beating the benchmark.

To the extent that analysts are rational when making EPS forecasts and have access to information that is not observable to the econometrician, the influence of such unobservable lender characteristics on our tests could be mitigated. Despite this possibility, we cannot rule out that short-termist lenders, according to our definition, differ from other lenders on other unobservable dimensions that are relevant to enforcement choices. To help address this limitation, we include a broad set of fixed effects in our empirical specification, as detailed in Section 3.3 and investigate several specific dimensions of potentially relevant lender heterogeneity in Appendix B. Further, in Section 3.4 we explore a number of alternative measures of lender short-termism to corroborate our baseline findings.

## 2.2 Defining covenant slack and breaches

A key independent variable in our analysis is covenant slack, which serves two purposes. First, we use covenant slack to identify cases in which the borrower breaches a pre-set covenant threshold. Second, we control for covenant slack to focus identifying variation in our tests on borrowers close to their covenant thresholds. As mentioned in the previous section, we construct a dataset that includes covenant thresholds and the underlying financial metrics for a sample of loan package-quarter observations. Using these inputs, we calculate the slack of firm $i$'s $j^{\text{th}}$ covenant in quarter $t$ as:

$$Slack_{ijt}^{min} = \frac{u_{ijt} - \underline{u_{ijt}}}{\sigma_{ijt}} \tag{1}$$

for minimum covenants, such as minimum interest coverage ratio, and as:

$$Slack_{ijt}^{max} = \frac{\overline{u_{ijt}} - u_{ijt}}{\sigma_{ijt}} \tag{2}$$

for maximum covenants, such as maximum debt-to-EBITDA ratio. In these equations, $u$ represents the underlying financial ratio or amount, $\underline{u}$ ($\bar{u}$) the minimum (maximum) covenant threshold, and $\sigma$ the past eight-quarter volatility of the underlying ratio or amount.[7] Among these standardized values, we code the minimum as the firm-quarter *Slack*. Because borrowers with extreme values of *Slack* may be systematically different from others, we restrict our main sample to include package-quarter observations within a $10\sigma$ band of the covenant threshold. As shown in Panel A of Table 1, the average *Slack* in our main sample is 1.42. We also code an indicator variable, *Breach*, denoting firm-quarters with *Slack* less than zero. In our sample, 38.7% of package-quarter observations have slack less than zero, consistent with the prior literature (e.g., Chava and Roberts 2008).

An important consideration when calculating covenant slack is measurement error. Covenant definitions are not universally standardized, and interpreting modifications in loan agreements is a challenge given the lack of data and nonstandard references to covenant variables

[7] Covenant threshold calculations appear in Appendix A.2. These are broadly in line with Demerjian and Owens (2016).

(Zhang 2008; Demerjian and Owens 2016). Therefore, calculating covenant ratios and amounts in Compustat may generate measurement error in covenant slack because, unlike the observed covenant thresholds, the calculated covenant variables may not always reflect contract-specific definitions. Additionally, covenant thresholds may vary over time for reasons that are unobservable to the econometrician (e.g., dynamic covenant thresholds, renegotiation). A benefit of our setting is that these sources of measurement error should not influence our inferences; measurement error is unlikely to vary systematically with lender short-termism within and across lenders. We discuss further tests that explicitly address these various sources of measurement error in Sections 3.2 and 3.4.

### 2.3 Defining covenant enforcement

The main dependent variable of interest in our analysis of lender short-termism is *Enforcement*, an indicator for package-quarter observations with material covenant violations identified in the data sets collected and provided by Greg Nini. These material covenant violations are observable because SEC disclosure rules (17 CFR 210.4-08 "General Notes to Financial Statements") require borrowers to disclose both breaches of covenant thresholds that exist at the time of the filing as well as cured breaches, such as through covenant waivers or loan amendments, associated with material consequence, such as waiver or amendment fees, within four quarters. As Panel A of Table 1 shows, 11.1% of package-quarter observations in our sample have a material covenant violation, which, when combined with information about covenant breaches, implies an average enforcement rate of 28.7%. Although the data used in our study come from DealScan, these summary statistics are quantitatively consistent with those reported in related work using the Shared National Credit supervisory data from the Federal Reserve, FDIC, and Office of the Comptroller of the Currency (e.g., Chodorow-Reich and Falato 2019).

# 3 Lender Short-termism and Covenant Enforcement

From a theoretical standpoint, we characterize short-termism as behavior that, on the margin, prioritizes immediate payoffs versus future payoffs. A more restrictive characterization requires that the behavior has negative total welfare consequences. Politicians typically invoke this definition of short-termism when describing the ill-effects of performance benchmarks as reflecting "quarterly capitalism" (e.g., Hillary Clinton quoted in Luce 2015[8]). The setting of performance benchmarks is the canonical example of short-termism among academics (e.g., Stein 1989), regulators (e.g., Levitt 1998), and the popular press (e.g., Barton 2011). A related issue is how the welfare consequences of this behavior are shared among stakeholders. In the corporate sector, much of the empirical evidence suggests that short-termism incentives driven by performance benchmarks are harmful (Graham, Harvey, and Rajgopal 2005; Cheng and Warfield 2005; Benmelech, Kandel, and Veronesi 2010; Gopalan, Milbourn, Song, and Thakor 2014), but less is known about these effects in the financial sector.

## 3.1 Economic framework

Like other public corporations, public lenders are subject to capital market pressure to meet quarterly earnings benchmarks (Beatty, Ke, and Petroni 2002). Within our sample of public lenders, diagnostic tests suggest statistically significant benefits from beating the analysts' consensus forecast and the existence of economically significant manipulation around this benchmark. Panel A of Figure 1 presents a regression discontinuity plot of the three-day cumulative market-adjusted return around earnings announcements across the distribution of EPS surprise, or the distance between lenders' realized EPS and their equity analysts' consensus forecast. The figure shows a 0.69% discontinuity in these capital market benefits for lenders that just beat their analysts' consensus forecast relative to lenders that just miss their analysts' consensus forecast.

[8] https://www.ft.com/content/0cb6c12a-321a-11e5-8873-775ba7c2ea3d

Panel B of Figure 1 presents a histogram of the lender EPS surprise distribution in the [-10, 10] cent range. A McCrary (2008) test of this discontinuity rejects the null hypothesis of no discontinuity at the 1% significance level. In economic terms, the discontinuity estimates suggest that an abnormal mass of 5.0% of lender-quarters exists just above the zero earnings surprise cutoff relative to just below. Relative to the 28.0% of lender-quarters within two cents of the zero EPS surprise cutoff, this abnormal mass represents an economically meaningful set of lender-quarter observations. Overall, these tests provide evidence consistent with changes in lender behavior due to short-term pressure from capital markets. Our measure of short-termism incentives, *STLender*, is based on exactly this diagnostic evidence of short-termism around the zero EPS surprise performance benchmark. We infer that lenders that manipulated EPS to achieve their benchmark must have had stronger short-termism incentives (i.e., higher benefits or lower costs) than lenders that did not.

What specific actions might lenders take in response to these short-termism incentives? As the delegated monitor, the lead arranger of a loan syndicate is obligated to engage borrowers frequently on behalf of the syndicate participants. These interactions fall under the broad scope of monitoring, and include coordinating and observing payments and compliance with contractual terms. Among this set of interactions, one of the most common involves an assessment of the borrower's conformity to financial covenants.

Lead arrangers may observe borrowers to be in breach of covenant thresholds (e.g., maximum Debt/EBITDA), and may then relay this information to syndicate participants. Although the loan syndicate may vote to accelerate the loan—which would force the borrower to refinance, renegotiate, or incur a payment default—in practice, the syndicate would typically waive the covenant breach (Gopalakrishnan and Parkash 1995; Dichev and Skinner 2002). In this process, the lead arranger has discretion over the intensity of the syndicate's detection technology

and, to some extent, over the ex-post demands on the borrower given the individual syndicate voting procedure (Lee and Mullineaux 2004).[9]

Importantly, in exchange for the covenant waiver, borrowers typically pay a fee; for the lenders, this fee may be immediately recognized as income. Alternatively, lenders may agree to change the interest spread for the duration of the loan (Beneish and Press 1993, 1995; DeAngelo, DeAngelo, and Wruck 2002; Roberts and Sufi 2009b). Lenders, including the lead arranger, stand to increase contemporaneous income in the form of fees and increased spreads.[10] The syndicated loan market is dominated by a small subset of publicly-listed lenders. Members of this group typically serve as the lead arranger, particularly for larger loans, which enables them to conduct primary negotiations with the borrower, collect incremental fee income, and monitor the borrower. As a result, the pressure to beat short-term earnings benchmarks plausibly affects borrowers through a covenant enforcement channel.

Given the lead arrangers' unique discretion over the detection and outcomes of covenant breaches and the frequency with which publicly listed lenders act as lead arrangers, we investigate the effects of the capital market incentives of short-term earnings benchmarks on the propensity for these lenders to enforce covenant breaches. When making this enforcement decision, lenders face a dynamic tradeoff. Enforcing a covenant breach can increase the lender's earnings in the short-term, for example through fees, but it may harm the lender's relationship with the borrower, reducing future earnings. By definition, short-termist lenders prioritize contemporaneous earnings, shifting the calculus toward more covenant enforcement despite the long-run cost of lost relationship capital. If there were no long-run costs, then all lenders, whether short-termist or not, would enforce all covenant breaches.

[9] For covenant waivers, simple majority or super majority voting rules apply. As lead arrangers typically retain between one-quarter and one-half of the loan amount, they have a high voting stake in technical default proceedings.

[10] As per SFAS 91 (1986), lenders are entitled to immediately recognize amendment and waiver fees, leading to a contemporaneous increase in book earnings. Further, the lender is entitled to recognize previously deferred fees if the terms of the new agreement are at least as good from the lender's viewpoint, as they generally would be in cases of renegotiations following covenant violations. Changes in interest rates are generally amortized over the remaining life of the loan. See Ertan (2019) for a more detailed treatment of the relevant accounting issues.

### 3.2 Nonparametric evidence

We begin our analysis with motivational nonparametric evidence that lenders' short-term incentives affect borrowers in their loan portfolio. Specifically, Figure 2 presents evidence that, conditional on the distance to covenant thresholds, borrowers are significantly more likely to disclose a material covenant violation in quarters in which their lenders have short-termism incentives. Moreover, Figure 2 also shows that the difference in enforcement rates for short-termist and non-short-termist lenders increases with the severity of the borrower's covenant breach. That is, for more significant breaches of the threshold, short-termist lenders enforce covenant breaches at rates of up to 10 percentage points higher, which is almost double the average rate of enforcement. The motivating evidence from Figure 2 suggests that lenders alter their treatment of borrowers in their loan portfolio when they face short-term incentives.

However, it is also important to consider the unobservable characteristics of lenders or borrowers that might otherwise explain these observations. In Panel B of Table 1, we explore univariate differences between short-termist and other lenders for the key dependent and independent variables used in our analysis. We present the conditional mean, difference, and statistical significance of the difference using $p$-values based on standard errors that are double clustered at the borrower and lender levels. Across all characteristics, only two have statistically significant differences. One of these is *Enforcement*, the key dependent variable of interest, which suggests that short-termist lenders are unconditionally more likely to enforce covenant breaches. The other is loan amount. Short-termist lenders' loans are, on average, \$88.2 million smaller than other lenders' loans, and this difference is statistically significant at the 5% level. This difference is unlikely to explain the univariate difference in enforcement rate, both because the magnitude of this difference is small, and because any given lender switches between *STLender* states. Nevertheless, we control for all of these borrower and loan characteristics in our baseline tests.

### 3.3 Baseline results

A lender has a discontinuous increase in control at the covenant's thresholds because when the borrower's underlying financial ratio exceeds the maximum or falls below the minimum threshold, the borrower is in breach of the covenant and the lender has the right to accelerate the loan. Figure 2 shows that the probability of experiencing covenant enforcement increases significantly at the covenant threshold.[11] In our regression analysis, to focus on the region in which the lender has the discrete choice (i.e., to be precise, the loan syndicate votes) to accelerate the loan and extract some benefit from the borrower, we rely on $Breach_{it}$, an indicator variable that equals one if borrower $i$ has at least one covenant in quarter $t$ with negative slack and zero otherwise.

As defined in the previous section and in keeping with the literature on short-termism and earnings management, $STLender_{jt}$ is an indicator variable that equals one if lender $j$ has an earnings surprise of zero or one cent in quarter $t$ and zero otherwise (Roychowdhury 2006; Cheong and Thomas 2011). To avoid inference problems associated with behavioral differences among lenders at different places in the earnings surprise distribution, we control for lender EPS surprise using indicators for each cent of EPS surprise.

Although the graphical evidence in Figure 2 is consistent with our conjecture that lenders alter their behavior by enforcing covenant breaches (*Enforcement*) on a larger fraction of borrowers with *Breach*, one might be concerned that unobservable characteristics of lenders, or even unobservable trends in the syndicated loan market, may drive this finding. To that end, we incrementally add fixed effects to our baseline regression model to isolate variation that cannot be explained by these unobservable factors. Our baseline regression model is as follows:

[11] As Figure 2 shows, the probability of ex post enforcement falls between zero and one. This implies that lender forbearance is a key component of the loan contracting environment. Consistent with this notion, Bird, Ertan, Karolyi, and Ruchti (2019) show that the probability of ex post enforcement varies with coordination costs within the loan syndicate and substitutes for ex ante contract strictness.

$$Enforcement_{it} = a + b_1 STLender_{jt} + b_2 Breach_{it} + b_3 STLender_{jt} \times Breach_{it} + b_4 Slack_{it} \quad (3)$$
$$+ b_5 LenderEPSSurprise_{jt} + b_6 X_{ijt} + u_{j\times t} + u_{industry\times t} + e_{ijt}$$

in which $Enforcement_{it}$ is an indicator that equals one if borrower $i$ discloses a material covenant violation in quarter $t + s$ where $s = \{0, 1, 2, 3\}$ and zero otherwise.[12] *Breach* is an indicator that equals one in loan package-quarter observations for which the borrower has negative *Slack* and zero otherwise. *STLender* is an indicator that identifies lender-quarter observations in which the lender has zero or one cent of EPS surprise. $X_{ijt}$ represents control variables at the loan package-quarter level of observation, and $u_{j\times t}$ and $u_{industry\times t}$ represent lender-by-quarter and industry-by-quarter fixed effects, respectively. We present estimates of this model in Table 2.

The estimates in Table 2 correspond to an increasingly restrictive set of fixed effects. These fixed effects are meant to isolate and eliminate various confounding explanations for the baseline result. In column (1), we present our baseline estimates, which include only quarter fixed effects. The inclusion of quarter fixed effects alleviates the concern that just-meet-or-beat quarters are more likely during certain times, such as recessions, that are associated with greater incidence of covenant enforcement. This specification suggests that short-termist lenders are 3.3 percentage points more likely to enforce a covenant breach than other lenders. This effect is statistically and economically significant, corresponding to a 36.3% marginal effect given the average enforcement rate is 9.1%.

Columns (2) through (4) add successively more fixed effects to the specification.[13] Column (2) adds interactive fixed effects for the borrower's industry (two-digit SIC) and quarter. These ensure that differences in lender portfolios, which may change over time, do not interact with the

[12] Because the detection of covenant breaches typically depends on quarterly accounting information, we measure *STLender* and *Enforcement* using lender EPS and material covenant violation disclosures, respectively, from the quarter following the borrower's fiscal quarter used to calculate *NegativeSlack* and *Slack*. A limitation of our analysis is that we observe *Enforcement* at the firm-quarter level. This may create measurement error in our loan package-quarter level regressions because loan package-quarters that are not in violation may be assigned to *Enforcement* even though they are not subject to lender influence. However, this measurement error is unlikely to affect our analysis because its source is unrelated to *STLender* given the construction of *STLender*.

[13] Changes in the sample size reported are a function of observations that are perfectly identified by the incrementally restrictive fixed effects or control variables.

business cycle (or other time series variation) to produce spurious results. In column (3) we add lender fixed effects to isolate variation in enforcement behavior by the same lender at times with and without short-termism incentives. This is particularly important given that, for example, some lenders may persistently be close to their earnings benchmarks, perhaps because they are more easily understood by analysts than others, while also exhibiting consistent covenant enforcement behavior. In column (4) we include interactive fixed effects at the lender-quarter level. These fixed effects control for observable and unobservable time-varying differences across lenders in enforcement behavior and short-termism incentives. For example, they control for changes in the composition in the lender's loan portfolio over time (i.e., time-varying matching between lenders and borrowers (Schwert 2018).

Finally, in column (5) we add observable borrower and loan characteristics as control variables. These loan characteristics are especially valuable controls because they capture information about the borrower that is observable to the lender at the time of issuance but never observable to the econometrician. Across the columns in this table, we observe quantitatively similar estimates of the incremental covenant enforcement induced by short-termism incentives. That is, we find robust evidence that borrowers with negative slack are significantly more likely to disclose a material covenant violation if their lender has short-termism incentives. While the results are quantitatively stable across specifications, the most restrictive specification delivers the largest marginal effect of 51.6%, given the incremental enforcement rate for short-termist lenders of 3.2 percentage points and the baseline enforcement rate of 6.2 percentage points.

A potential concern with the interpretation of these tests is the well-known fact that borrowers appear to manipulate the underlying financial metrics to avoid covenant breaches. This phenomenon may reduce the comparability of borrowers just-above and just-below their covenant thresholds. Indeed, we explore this issue empirically in Appendix B. We find economically and statistically significant evidence of a density break in the distribution of covenant slack at covenant thresholds, consistent with manipulation. However, as shown in Figure B1 and Table B1, we find no significant differences in the magnitude across short-termist lenders and other lenders. Additionally, the distinction between manipulating and non-manipulating borrowers has

little bearing on the interpretation of our baseline tests because our objective is to model the lender's decision to enforce covenant breaches as a function of its incentives from performance benchmarks.

The distinction is more relevant for our subsequent tests that examine the real effects of lenders' short-termist enforcement behavior. In the prior literature that studies the real effects of covenant enforcement (e.g., Chava and Roberts 2008), a potential identification concern is that the decision to enforce is endogenous to the borrower's subsequent investment. If successfully manipulating to avoid a covenant breach signals some unobservable quality, then subsequent investment growth for manipulating borrowers may not be a useful counterfactual for breaching borrowers' subsequent investment growth.

The identification concern in our tests is one step removed. Specifically, for borrower manipulation to bias our estimates of the incremental enforcement by short-termist lenders, it must be the case that either (i) lenders have more opportunities to enforce covenant breaches when they have short-termism incentives, or (ii) borrowers manipulate differently when their lenders have short-termism incentives. Table B2 presents evidence that lender short-termism is not correlated with either borrowers' covenant slack or the propensity for borrowers to have a covenant breach, suggesting that short-termist lenders have no greater opportunities to enforce covenant breaches (i.e., their borrowers are of no worse quality with respect to covenant thresholds during these quarters). Figure B1 and Table B1 show evidence based on McCrary (2008) density tests that borrowers are not statistically or economically more likely to manipulate when their lenders have short-termism incentives.

Another potential concern involves omitted lender characteristics. Since our measure of short-termism relies on analyst forecasts, the effects of such unobservables could be mitigated to the extent that they are observable to analysts, even if they are not observable to the econometrician. However, the possibility remains that short-termist lenders, defined according to this measure, differ from other lenders in ways that are unobservable to analysts and so are not accounted for in our empirical design. An important type of omitted lender factor relates to lender health. We explore this specific concern in greater detail in Appendix B. In Table B3, we present

fixed effects regression evidence that *STLender* is uncorrelated with lender health, which we measure using lenders' capital ratios and liquidity ratios. In Table B4, we add interactions between *Breach* and each measure of lender health to our baseline and most restrictive specification from Table 2 and find that the inclusion of these controls has insignificant effects on the magnitude of our estimates. These tables suggest that, to the extent that lender health affects covenant enforcement, it operates through a separate mechanism.

A related specific concern could be that lenders and borrowers experience short-termism incentives at the same time. This might bias our estimates if short-termist lenders are especially likely to enforce covenant violations for short-termist borrowers. In Tables B5 and B6 of Appendix B, we explore this issue in more detail. In Table B5, we estimate fixed effects regressions of borrower's EPS surprise and *STBorrower*, which we define analogously to *STLender*, on *STLender*. In these tests, we find no evidence that lenders' short-termism incentives are correlated with their borrowers' short-termism incentives. In Table B6, we add interactions between *Breach* and each measure of borrower short-termism incentives to our baseline specification from Table 2 and find that the inclusion of these interactive controls has no quantitative impact on our baseline estimates.

### 3.4 Alternative measurement and robustness

In this section, we explore the robustness of our main findings in Table 2 to alternative measures of our key independent variables of interest, *Slack* and *STLender*, and also investigate a measure of ex ante short-termism incentives based on the latent (pre-enforcement) distribution of EPS surprise (following the logic of Almeida, Fos, and Kronlund 2016). In Table 3, we address the potential sources of measurement error in covenant slack described in Section 2.2 by estimating our baseline specification in subsamples of the data where measurement error related to the calculation of covenant slack is likely to be small. Every specification presented in Table 3 corresponds to the most restrictive specification presented in Table 2 with the full set of control variables as well as industry-by-quarter and lender-by-quarter fixed effects.

In Panel A of Table 3, we construct four alternative measures of *Slack* based on subsets of covenants for which the definitions are relatively standardized and unambiguous. Specifically, following Chava and Roberts (2008) and Demiroglu and James (2010), in columns (1) through (3), we construct covenant slack using only the distance to the maximum Debt/EBITDA threshold, the minimum current ratio threshold, or the minimum net worth (including tangible net worth) threshold. In these tests, we identify covenant breaches only when they are associated with the specific covenant used to construct *Slack*. For example, in column (1), *Breach* equals one only if the maximum Debt/EBITDA threshold is breached and zero otherwise. In column (4), we construct a combined measure based on the minimum covenant slack across only these three covenant types.

Finally, in column (5), we estimate our baseline specification as in Table 2, but we restrict the sample to loan packages with at least one covenant written on Debt/EBITDA, current ratio, or net worth. These five measures mitigate the potential influence of measurement error due to nonstandard covenant definitions. We find statistically significant evidence of higher enforcement rates for short-termist lenders in every column of this table. Moreover, the coefficient estimates are bounded from below by our baseline estimates, consistent with a benign measurement error interpretation. Because they use only data from the subsample of three standardized covenants, the column (4) estimates in this table are our preferred estimates, and they suggest a 4.8 percentage point higher enforcement rate for short-termist lenders (compared to 3.2 percentage points in our baseline specification).

In Panel B of Table 3, we explore alternative sources of measurement error in covenant thresholds, including time-varying thresholds and renegotiation. In column (1), we restrict the sample to loans without dynamic thresholds as specified in the DealScan database. Although the sample size decreases and the power of our tests drops, we find that short-termist lenders enforce at 3.5 percentage point higher rates ($p = 0.023$), suggesting that any measurement error introduced by unobserved, yet contractual changes in thresholds is attenuating. In column (2), we exclude loans that DealScan flags as being renegotiated prior to maturity. Because most loans are amended before maturity, this restricts our sample to about one-fifth of our main sample. In

this subsample, we estimate that short-termist lenders enforce at 6.5 percentage point higher rates ($p < 0.01$). The third column of Panel B restricts the sample to package-quarter observations that are within four quarters of the first measured covenant breach. Although DealScan's "dealamendment" and "facilityamendment" files measure covenant amendments, they may not capture all such amendments. Since prior literature has found that loans are renegotiated in anticipation of a covenant breach (Denis and Wang 2014), one might be concerned that unobserved amendments to covenant definitions or thresholds may strengthen or loosen covenants in anticipation of a breach, leading to measurement error in our measure of *Slack* around the first breach of a covenant threshold. Our estimate in column (3), which restricts the sample to observations that are unlikely to be subject to unobserved changes in covenant thresholds, is also quantitatively consistent with our baseline estimates from Table 2: short-termist lenders enforce at a 3.3 percentage point higher rate ($p < 0.01$).

Finally, in column (4), we exclude all observations associated with loans that never breach a covenant threshold. This effectively alters the control group to mitigate a different source of measurement error, namely that *Slack* underestimates the loan's distance to a breach. After excluding these loans and comparing loans that breach their covenant thresholds at least once before maturity when they have and have not breached a threshold, we estimate that short-termist lenders enforce at a 3.2 percentage point ($p < 0.01$) higher rate. Overall, the estimates in Panel B of Table 3 suggest that measurement error induced by renegotiation or unobserved, time-varying thresholds is, if anything, attenuating our baseline estimates.

In Table 4, we explore the robustness of our baseline estimates to alternative definitions of *STLender* and alternative sample restrictions based on the distance to the zero EPS surprise cutoff. In columns (1) and (2), relative to our baseline specification, we expand the sample window to include lenders with EPS surprise in five and ten cent bands around zero, respectively. Our estimated incremental enforcement rates for short-termist lenders in these tests are 2.6 and 4.0 percentage points, and they are statistically significant at the 5% and 1% levels, respectively. Neither deviates significantly from our baseline estimate of 3.2 percentage points.

In column (3), we restrict the definition of *STLender* so that it equals one only for lenders with an EPS surprise of zero cents, and we estimate an incremental enforcement rate of 3.8 percentage points. In columns (4) and (5), we expand the definition of *STLender* so that it equals one for lenders with an EPS surprise of zero to two cents and zero to three cents, respectively. The estimates in these columns are also consistent with our baseline estimates and are statistically significant at the 5% level. Finally, because lenders that just miss their benchmark may have unsuccessfully manipulated their earnings (Bird, Karolyi, and Ruchti 2019), we define *STLender* so that it equals one for lenders with an EPS surprise in the range of negative one to three cents. Our column (6) estimate is statistically significant at the 1% level and suggests that short-termist lenders (including a subset that may have unsuccessfully manipulated their actions to achieve the benchmark) are 2.7 percentage points more likely to enforce covenant breaches.

Our measure of short-termism relies on analysts' consensus EPS forecasts. Across lenders, these forecasts are provided by different numbers of analysts with varying forecast accuracy. As a test of the internal validity of this construct, we evaluate whether cross-sectional features of consensus forecasts mediate the baseline effects identified in Table 2. Specifically, if the analysts covering the lender have disparate forecasts, then either (i) the lender may have less precise information about the amount of earnings needed to achieve its benchmark, or (ii) capital markets may rely less on the consensus forecast, providing the lender with less incentive to target the analysts' benchmark.

In columns (1) and (2) of Table 5, we explore whether short-termist lenders are more likely to enforce when forecast dispersion is low. Column (1) presents estimates from a specification that is identical to our baseline specification from Table 2, but includes additional interaction terms between *Breach*, *STLender*, and *Disp*, which is the standardized standard deviation of analyst EPS forecasts. Relative to column (1), column (2) replaces *Disp* with an indicator, 1[*Low Disp*], that equals one if *Disp* is in the bottom quartile of the forecast dispersion distribution. The estimates in both of these columns indicate that forecast dispersion significantly moderates lender behavior. For example, in column (2), we find that short-termist lenders with more precise

information about their benchmark are 7.3 percentage points more likely to enforce ($p < 0.01$), which is more than twice the incremental enforcement rate of other short-termist lenders.

Similarly, the analysts' consensus forecast may be a particularly salient earnings benchmark for lenders with a high level of analyst coverage, so we investigate whether lenders with high analyst coverage are more likely to enforce to meet their benchmarks. The specifications in columns (3) and (4) mimic those of columns (1) and (2); column (3) presents interactions with a continuous measure of analyst coverage, *Cov*, and column (4) presents interactions with an indicator, 1[*High Cov*], that equals one if the lender is in the top quartile of analyst coverage in a given quarter. The estimate in column (4) suggests that short-termist lenders with high analyst coverage are 8.0 percentage points more likely to enforce ($p < 0.05$). Together, the results in Table 5 validate the *STLender* construct and suggest that lenders are more likely to act in a short-termist way when more attention is paid to their earnings benchmarks and when there is more agreement about the relevant level of their earnings benchmarks.

Despite the supporting evidence of the robustness and internal validity of *STLender*, one might be concerned that it is based on realized EPS surprises, not the ex ante expectations of the lender. After all, not all lenders with a realized EPS surprise of one cent necessarily manipulated their earnings, and even if they do, there may be omitted unobservables correlated with realized EPS surprise that our previous analysis does not address. Nevertheless, in Table 6, we investigate alternative measures of short-termism that have different economic motivations from *STLender*. Like our main measure of short-termism, *STLender*, each of these alternative measures of short-termism has individual strengths and weaknesses. To the extent that their weaknesses are non-overlapping, it is reassuring that we consistently find that short-termism is associated with higher enforcement rates.

In column (1), we interact *Breach* with *%manip*, which is a measure of the ex ante likelihood of manipulation conditional on EPS surprise based on the structural model of Bird, Karolyi, and Ruchti (2019). The coefficient estimate suggests that a 10 percentage point increase in the likelihood of manipulation is associated with a 3.7 percentage point higher enforcement

rate. This estimate suggests that correcting for lender expectations does not have a significant impact on our findings.

We next explore measures of the CEO's decision horizon. In columns (2) and (3), we follow the literature that argues that retirement age and relatively long-tenured CEOs have shorter decision horizons and are more likely to act myopically (Cheng 2004; Antia, Pantzalis, and Park 2010). We interact *Breach* with *RetirementAge*, an indicator that equals one if the CEO is at least 64 years old, and *LongTenure*, an indicator that equals one if the CEO is in the top quartile of the lenders' distribution of CEO tenure. In both columns, we find that lenders with CEOs that have shorter decision horizons are significantly more likely to enforce covenant breaches. CEOs near retirement are 18.7 percentage points more likely to enforce breaches, and long-tenured CEOs are 9.7 percentage points more likely to enforce breaches. Both of these estimates are significant at the 5% level.

In columns (4) and (5), we explore two regulatory motives to improve contemporaneous earnings performance by enforcing covenant breaches. In column (4), we interact *Breach* with *LowCapital*, an indicator of whether the lender's capital ratio is below 8%, and we find that poorly capitalized lenders are 10.0 percentage points more likely to enforce covenant breaches ($p < 0.01$). In column (5), we interact *Breach* with *LowLiquidity*, an indicator of whether the lender's liquidity ratio is below 6%, and we find that lenders with low liquidity are 4.7 percentage points more likely to enforce covenant breaches ($p < 0.10$). These findings provide a link with contemporaneous work on cross-sectional differences in covenant enforcement for healthy and unhealthy lenders during the financial crisis (Chodorow-Reich and Falato 2019) and the time series cyclicality of enforcement rates, particularly during the financial crisis (Bird, Ertan, Karolyi, and Ruchti 2019). Overall, the findings in Table 6 show that a variety of stimuli could encourage lenders to act more or less myopically with respect to covenant enforcement. They corroborate our primary finding that performance benchmarking incentives can induce lenders to enforce covenant breaches at higher rates.

Finally, we construct an ex ante measure of short-termism incentives based on a latent distribution of EPS surprise for lenders in which we remove the direct effects of enforcement on

earnings for all lenders. We restrict our attention to a bank-level panel to address the simultaneity of decisions to enforce individual covenant breaches. Based on this distribution, we define *ExAnteSTLender* as an indicator that equals one for lenders with a latent EPS surprise in the [-2, 0) window of this distribution.[14] Using the logic of Almeida, Fos, and Kronlund (2016), which applies this notion of a latent EPS surprise distribution to the share repurchase decision of non-financial firms, lenders in this window are those that have both the ex ante incentive to manipulate earnings to hit their benchmark, and, as demonstrated by the simulations we present in Section 4.4, could also plausibly do so via covenant enforcement.

In Table 7, we compare bank-level enforcement rates, which we calculate as a fraction of the number of loans in columns (1)-(4) or the number of loans in breach in columns (5)-(8), for lenders with ex ante short-termism incentives to those without in the same narrow [-2, 2] cent window of the latent EPS surprise distribution. In our most restrictive specifications with lender characteristics, lender fixed effects, and quarter fixed effects, we find that ex ante short-termism incentives increase enforcement rates by 3.7%, or 69% relative to the mean enforcement rate of 5.4%. This directly compares to our baseline loan-level estimates in Table 2 of 3.2%, which suggests that the lenders that are moved across the zero EPS surprise threshold in the latent distribution are marginal enforcers. Therefore, these findings support and extend our main findings using a different source of identifying variation. In particular, whereas our main results use banks that just-missed their benchmarks as the control group, these new results change the control group to consist of banks that would have met their benchmarks even with no enforcement. Together, our simulations and tests based on a latent (pre-enforcement) distribution of EPS surprise provide evidence that lenders with ex ante short-termism incentives pursue higher enforcement rates to help them achieve their EPS targets.

[14] For this calculation, we remove only the effects of waiver and amendment fees as these do not depend on estimates obtained from other tests. This precludes any circularity from driving results obtained using measures based on this latent distribution of EPS surprise. We provide further detail on the construction of this latent distribution in Section 4.4.

### 3.5 Cross-sectional heterogeneity in lender short-termism

In this section, we investigate two sources of cross-sectional heterogeneity in the effect of lender short-termism on covenant enforcement rates documented in Section 3.1.[15] The first concerns the availability of alternative earnings management tools. In an attempt to meet short-term earnings benchmarks, banks take a portfolio approach to different methods of earnings management (Cohen, Dey, and Lys 2008; Zang 2011). As such, one would expect banks to use the covenant enforcement strategy when they are relatively constrained in their use of other strategies, such as accounting-based manipulation. In theory, lenders should opportunistically use covenant enforcement as a tool to meet performance benchmarks if alternative methods (i) have a finite limit, or (ii) have convex costs that at some point exceed the marginal cost of enforcement. Therefore, if covenant enforcement is a particularly costly means of generating contemporaneous earnings, we should expect lenders to use it especially when they are also using other earnings management tools (Barton and Simko 2002). We also note that the design of loan contracts to include financial covenants and observed enforcement rates, even in the absence of incentives to act myopically, suggests that enforcement is not so costly that it is never used.

To test our prediction, we construct four measures of alternative earnings management tools available to lenders and interact each of them with *Breach*, *STLender*, and *Breach* × *STLender*. Lenders can use a variety of tools to manage earnings to meet EPS benchmarks, some of which are specific to the banking industry. Prior work has also argued that firms repurchase shares or shift the classification of gains and/or losses to boost EPS (e.g., Almeida, Fos, and Kronlund 2016). In column (1) of Table 8, we first investigate whether short-termist lenders that repurchased stock are also more likely to enforce covenant breaches at higher rates. We construct *Repurchase*, an indicator that equals one if the lender had a net repurchase of common shares during the quarter and zero otherwise, and interact it with *Breach*, *STLender*, and *Breach* ×

[15] We do not explore heterogeneity in enforcement by borrower characteristics, such as those based on the borrower's outside financing options, because of ambiguous theoretical predictions. For example, although more held-up borrowers are better to enforce on now, because lenders can extract more rents, these borrower are also likely to generate the most surplus in the future.

*STLender*. We find that repurchasing short-termist lenders are 6.4 percentage points ($p < 0.01$) more likely to enforce covenant breaches, consistent with our prediction.

The literature on earnings management tools that are specific to banks has focused on the strategic use of loan loss reserves, the largest accruals account for banks (Beatty and Liao 2014). Banks can alter loan loss reserves to delay recognizing losses on outstanding loans, or avoid providing for loan losses when issuing new loans. We next investigate the lender's change in loan loss reserves as a complementary earnings management tool. To recognize greater contemporaneous income, the lender would prefer to decrease loan loss reserves. The triple interaction coefficient in column (2) shows that a one standard deviation *decrease* in the change in loan loss reserves is associated with a 6.4 percentage point higher enforcement rate, consistent with our prediction. Next, we investigate whether one component of this change in loan loss reserves over which banks may have greater discretion, the provision for loan losses, is driving this finding (e.g., Beaver, Eger, Ryan, and Wolfson 1989; Ahmed, Takeda, and Thomas 1999; Beatty, Ke, and Petroni 2002; Leventis, Dimitropoulos, and Anandarajan 2011; Bushman and Williams 2012; Acharya and Ryan 2016). In column (3), we focus on the level of loan loss provisions (which is already a flow variable from the income statement), and we estimate that lenders with one standard deviation higher loan loss provisions (and therefore lower earnings than if they had made a smaller provision) are 4.6 percentage points less likely to enforce covenant breaches. These two findings suggest that when lenders' contemporaneous earnings benefit from their treatment of loan loss reserves, they are also more likely to enforce covenant breaches.

Lastly, we construct $1[LargeUnrealizedLoss]$, an indicator that identifies lender-quarter observations in which the lender reported an above average level of unrealized losses. This measures how aggressively the lender avoids recognizing losses in contemporaneous income. In column (4), we find evidence that lenders that avoid recognizing losses also enforce covenant breaches at higher rates, and the coefficient estimate is statistically significant at the 5% level. Collectively, the evidence in Table 8 suggests that covenant enforcement is part of a portfolio of earnings management tools that lenders use to meet earnings benchmarks.

We next study cross-sectional heterogeneity in the effects of lender short-termism based on aspects of the lender's governance and executive compensation scheme. Lenders with high or concentrated institutional ownership or experienced board members may have better governance and, therefore, the ability to prevent myopic enforcement behavior by managers. Similarly, the value of meeting performance benchmarks is likely to be greater for executives who have high variable compensation. Because capital markets reward lenders for meeting performance benchmarks, we would expect bank executives with more performance-sensitive compensation (i.e., stock- and option-based compensation) to be more sensitive to performance benchmarks. The idea is that the triple interaction term in each case captures differences in enforcement behavior by banks with high versus low contractual incentives conditional on meeting their benchmark. This distinguishes banks that were likely to have achieved their benchmark due to these incentives and those that did so organically.

In column (1) of Table 9, we test whether lenders whose top managers have high pay-performance sensitivity are more likely to enforce covenant breaches. We do so by measuring *Delta*, standardized pay-performance sensitivity, which is calculated as in Coles, Daniel, and Naveen (2006). The estimate in column (1) indicates that lenders whose top executives have one standard deviation more *Delta* are 3.6 percentage points ($p < 0.05$) more likely to enforce covenant breaches than other short-termist lenders. Second, we test whether lenders whose CEOs have a high fraction of variable compensation, which we operationalize using *VariableCompensation*, the standardized fraction of stock- and option-based compensation, are more likely to enforce to meet performance benchmarks. The results in column (2) indicate that short-termist lenders whose executives have one standard deviation more variable compensation are 2.1 percentage points ($p < 0.01$) more likely to enforce covenant breaches than other short-termist lenders.

In column (3), we investigate whether short-termist lenders with a high share of institutional ownership tend to have lower enforcement rates. Because institutional owners may be more likely to monitor management than individuals, institutional ownership may be interpreted as a measure of governance quality. If strong governance limits myopic behavior by bank executives, then we would expect institutional owners to curb incremental enforcement that

is driven by performance benchmark incentives. This is indeed what we find. Specifically, we find that a 10 percentage point increase in institutional ownership is associated with a 2.0 percentage point decrease in enforcement by short-termist lenders. If diffuse institutional ownership creates opportunities to free ride in monitoring, then we would expect lenders with concentrated ownership to also restrict myopic decisions. In column (4), we find evidence that lenders are less sensitive to performance benchmarks when they have more concentrated ownership, which we measure using the standardized Herfindahl-Hirschman Index of institutional ownership shares. Lastly, we investigate whether the relative experience of members on the lender's board of directors plays a role in mitigating myopic enforcement behavior. Indeed, in column (5), we find that lenders whose board members have above average experience are 3.8 percentage points ($p < 0.05$) less likely to enforce covenant breaches.

Overall, the results in Table 9 provide evidence that lenders with strong governance are less susceptible to myopic decisions driven by performance-benchmarking incentives, but that bank executives with more contractual sensitivity to benchmark-beating are more likely to adjust their behavior in response to performance benchmarks. These cross-sectional tests also help mitigate measurement error concerns by showing that our baseline estimates are driven by the subset of *STLender* banks with strong incentives to meet their benchmarks, rather than banks that would have ended up in the *STLender* group without manipulating earnings.

## 4 Consequences of Lender Short-termism

In this section, we investigate the consequences of the incremental covenant enforcement documented in Section 3. Of primary importance to both borrowers and lenders is the consequence for their relationship capital (Bharath, Dahiya, Saunders, and Srinivasan 2007). Covenant breaches also have direct and negative financing consequences for borrowers, including covenant waiver fees, spread increases, renegotiation, refinancing, and potentially payment default (Roberts 2015; Freudenberg, Imbierowicz, Saunders, and Steffen 2017). We examine the outcomes of

covenant breaches in the loan market, including borrowers' propensity to switch lenders for their next loan and renegotiated terms, as well as the consequences of covenant breaches for borrower investment and equity value.

### 4.1 Relationship dynamics and renegotiation

We first examine the effect of short-termist lender enforcement on the borrower's propensity to switch lenders and on renegotiated spreads and amounts. Because lead arrangers may exercise discretion over their monitoring intensity, borrowers may update their expectations regarding future treatment based on lead arranger behavior when the borrower breaches a covenant threshold. That is, borrowers may prefer to switch lenders altogether if they believe they will receive harsher treatment from their current lender during instances of financial distress. However, despite facing this potential consequence, lenders may be willing to exercise discretion to achieve short-term objectives.

Typical renegotiation outcomes like waiver fees and spread increases improve lenders' contemporaneous (and future) income. Another common outcome of renegotiations is a reduction in loan amount (Roberts and Sufi 2009b). However, because the dollar contribution to income from waiver fees and spread increases is proportional to the loan amount, lenders facing short-termism incentives should, in theory, prefer to avoid decreasing loan amounts in renegotiation. Therefore, lenders who face short-termism incentives should not only enforce covenant breaches more frequently, but also increase waiver fees and spreads as opposed to reducing loan amounts. Our tests on changes in loan amount therefore help distinguish strategic short-termism from simply harsher detection or enforcement of covenant breaches.

We examine renegotiation outcomes and the propensity of borrowers to switch lenders in subsequent borrowing using the following regression models:

$$\begin{aligned} Y_{it} = a + b_1 STLender_{jt} + b_2 Breach_{it} + b_3 STLender_{jt} \times Breach_{it} + b_4 Slack_{it} \\ + b_5 LenderEPSSurprise_{jt} + b_6 X_{ijt} + u_{j\times t} + u_{industry\times t} + e_{ijt} \end{aligned} \tag{4}$$

where $Y$ is one of $\{Switch, \Delta\ln Spread, \Delta\ln Amount\}$. *Switch* is an indicator that equals one if borrower $i$ chooses a different lead arranger in its next loan and zero otherwise. In tests that investigate *Switch*, we restrict the sample to include package-quarter observations for which we observe at least one of the borrower's subsequent loans in our sample within eight quarters of the breach. $\Delta\ln Spread$ ($\Delta\ln Amount$) is the difference in the natural logs of the spread (amount) on borrower $i$'s current loan and the spread on borrower $i$'s renegotiated loan spread (amount). We do not use *Spread* or *Amount* as controls in the $\Delta\ln Spread$ and $\Delta\ln Amount$ specifications, respectively. We identify renegotiated loans using filters based on the identity of the lender on the subsequent loan and the timing of the subsequent loan. If the breach is not cured, lenders may have the ability to enforce covenant breaches within four quarters, so we re-define *Slack* and *Breach* using the minimum *Slack* from the prior four quarters. This definition is less subject to false negatives in measuring enforcement, and if the breach is cured during the interim period, this definition will attenuate our estimates toward zero.

We present estimates of the effects of lender short-termism on *Switch* in Table 10 and on renegotiation outcomes in Table 11. Estimates with $\Delta\ln Spread$ and $\Delta\ln Amount$ are presented in Panel A and Panel B of Table 11, respectively. As in Table 2, the estimates in these tables correspond to specifications with increasingly restrictive sets of fixed effects.

#### 4.1.1 Relationship dynamics

In Panel A of Table 10, column (1) presents the baseline estimates, which include quarter fixed effects, of the propensity of borrowers in breach of a financial covenant to switch lenders conditional on whether their lender has short-termism incentives or not. The coefficient on the interaction of *Breach* with *STLender* shows that lender short-termism incentives increase the likelihood that the borrower will switch lenders for its next loan by 2.6 percentage points ($p < 0.05$). This coefficient estimate is quantitatively similar across specifications; in our most restrictive specification, which includes industry-by-quarter and lender-by-quarter fixed effects as well as loan and borrower controls, we estimate an incremental switch rate of 4.6 percentage

points ($p < 0.05$). Relative to the 33.0% base switching rate, our estimates imply a marginal effect on the order of 13.9%.

In Panel B of Table 10, we present estimates of our most restrictive specification from Panel A, but we use alternative horizon restrictions for the definition of *Switch*. Whereas in Panel A we restrict the definition of switching to be based on loans in the two years following the breach, we allow *Switch* to be defined using loans initiated in the one year, two years, three years, four years, or five years following the breach in Panel B. Our estimates in Panel B are consistent in magnitude and significance with those presented in Panel A. Notably, however, our estimates for shorter horizons are economically larger. Whereas borrowers switch with a 3.0 percentage point higher rate over a five year horizon, they switch at an 8.3 percentage point higher rate over the first year. This finding is consistent with borrowers switching when the enforcement action is arguably most salient. Given the importance of lending relationships (Petersen and Rajan 1994; Berger and Udell 1995; Bharath, Dahiya, Saunders, and Srinivasan 2007; Gopalan, Udell, and Yerramilli 2011) to both lenders and borrowers, this increase in switching is likely to have negative long-term effects for both parties to the loan.

Although relationship capital is critical to lenders, there may be countervailing benefits of incremental enforcement that occur through changes in the composition of the loan portfolio. For example, benchmark incentives may induce lenders to attend to their loan portfolios and more efficiently prune negative NPV relationships.[16] In Table C1 of Appendix C, we analyze borrower and loan characteristics among breaches that short-termist lenders selected to enforce. Specifically, we study whether these characteristics are different between breaching borrowers that switched lenders on their subsequent loan and those that did not. Across specifications with incrementally restrictive fixed effects, we find no systematic evidence that switching and non-switching

[16] Efficient pruning would rely on differences in the amount of information that enforcing and forbearing lenders would have about breaching borrowers. In our setting, it is not obvious why lenders would have different information sets whether they choose to enforce the breach or not, precisely because lenders gather information to inform that enforcement decision.

borrowers are different. These findings are not consistent with a story in which short-termist lenders efficiently prune low quality borrowers.

The findings in Tables 9 and C1 suggest that the relationships directly affected by enforcement are harmed. There may also be broader indirect effects of enforcement on lenders. To investigate these indirect effects, we study future loan growth and reputation. In Table D1 of Appendix D, we examine the changes in the total amount and number of loans granted by the lender, as well as changes in the lender's league table rank. We model each of these as a function of enforcement rate and find that when banks have higher enforcement rates, they tend to have lower subsequent loan growth and reputation (rank). These lend support to the argument that incremental enforcement driven by benchmark incentives is indeed costly, consistent with more restrictive definitions of short-termism in which the short-run benefits that lenders get from enforcement are met by significant long-run costs.

#### 4.1.2 Renegotiation

We next investigate the consequences of lender short-termism for renegotiated spreads and amounts. Column (1) of Panels A and B of Table 11 shows that short-termist lenders increase spreads *and* amounts when renegotiating loans after a covenant breach more than other lenders do. In our most restrictive specifications, we find that renegotiated loans by short-termist lenders have 6.8% higher spreads and 15.5% higher amounts than renegotiated loans by other lenders (or by the same lenders at other times). Although increasing the loan amount also increases the short-termist lenders' exposure to borrowers that have covenant breaches, this is just what we would expect if the lender's primary goal in renegotiating debt contracts following default is to extract fees, since both higher spreads and increased loan amounts contribute to contemporaneous income. Note that the actual cash-flow effect for borrowers is uncertain, since they get to borrow additional funds but at the cost of higher interest expense (in addition to the payment of waiver/amendment fees). The higher spread paid by affected borrowers is likely a key component in their subsequent decision to switch lenders.

Table 11 suggests that the direct effects of covenant breaches, on average, are to increase loan spreads and amounts. These estimates do not incorporate the typical waiver and amendment fees associated with covenant breaches and subsequent renegotiations. Osborn (2014) collects a sample of waiver fees and finds the average waiver fee to be 63 basis points. To validate this estimate in our data, we collect waiver and amendment fees disclosed in borrowers' 8-K filings that correspond to material covenant violations. The average waiver or amendment fee in our sample is 54.8 basis points, and we present evidence in Appendix Table B7 on the difference in fee amounts charged by short-termist and other lenders. We find that short-termist lenders charge higher fees in all specifications. In our preferred specification, which controls for borrower and loan characteristics as well as industry-by-year and lender-by-year fixed effects, we estimate that short-termist lenders charge 51.8 basis point higher fees, on average. These upfront fees contribute significantly to short-termist lenders' contemporaneous earnings, and represent an economically meaningful cost to borrowers. For the average loan in our sample with an amount of $539M, the fees, taking into account the renegotiated amount, imply a $7.3M transfer.

### 4.2 Real effects

The change in control rights associated with a material covenant violation provides lenders with a means of influencing corporate decisions. For example, borrowers tend to reduce investment following covenant violations, consistent with lenders' incentive to reduce risk-taking (Chava and Roberts 2008; Nini, Smith, and Sufi 2009; Gu, Mao, and Tian 2016). However, what remains unclear is the role of lenders' short-termism incentives. Short-termist lenders may prefer to intervene less with their borrowers' investment decisions than other lenders if incremental investment cuts require additional monitoring or reduce expected future lending to borrowers with relatively strong investment opportunities. However, short-termist lenders may continue to exercise their control rights if borrowers' investment policies are, from their perspective, excessively risky. Of course, because short-termist lenders charge higher waiver and amendment

fees and increase spreads more in renegotiations, short-termist lenders' covenant enforcement may affect investment through an increase in the cost of borrowing.

To investigate the incremental propensity of borrowers in violation of a financial covenant to cut investment due to the short-termism incentives of their lenders, we estimate the same model as presented in equation (4), but we allow $Y$ to represent either $\Delta Investment(\%)$ or $\Pr(\Delta Investment(\%) < -20\%)$, where we define investment as the sum of capital expenditures, R&D expense, and acquisition expenditures.[17] In Panel A of Table 12, we investigate the effects of short-termist lender enforcement on investment growth, and in Panel B of Table 12, we investigate the effects of short-termist lender enforcement on significant investment cuts. In both panels, we present increasingly restrictive specifications that eventually include a set of borrower and loan controls as well as industry-by-quarter and lender-by-quarter fixed effects. These fixed effects allow us to compare investment growth for covenant breaching and non-breaching borrowers from the same industry that are matched to the same lender in the same quarter.

The estimates in column (1) of Panel A of Table 12 suggest that covenant enforcement by short-termist lenders is associated with 5.6% lower investment than covenant enforcement by other lenders, and the estimate in column (1) of Panel B suggests that this is driven by extreme investment cuts. Specifically, the estimate in Panel B suggests that enforcement by short-termist lenders is associated with a 3.1 percentage point higher probability of a significant investment cut than enforcement by other lenders. These estimates are statistically significant at the 5% and 1% levels, respectively, and together they suggest that investment is lower at both the mean of the distribution and the left tail. Table 12 shows quantitatively stable estimates across specifications, suggesting that the incremental effect of short-termism incentives is not confined to specific time periods, industries, or lenders. Our most restrictive specifications suggest that covenant enforcement by short-termist lenders is associated with 7.8% ($p < 0.01$) lower investment and a

[17] The results that follow are robust to various alternative definitions of investment growth, including changes in the investment rate (i.e., change in total investment scaled by lagged total assets) and asset growth rate (change in total assets scaled by lagged total assets), and alternative thresholds for significant investment cuts, including indicators that identify the bottom quartile or decile of the investment growth distribution.

4.1 ($p < 0.01$) percentage point higher probability of a significant investment cut than covenant enforcement by other lenders.

### 4.3 Market reaction to violation announcements

The incremental attention from lenders with short-termism incentives impacts the financing and real investment of their borrowers. Although we have argued in previous sections that these effects are likely to be value-decreasing, they could, in principle, be value-increasing. Incremental lender monitoring may improve borrower performance, which could benefit shareholders as well as debtholders. It may be optimal for incremental lender attention to curtail investment for affected borrowers. For example, affected borrowers may have unobservably poor investment opportunities that are uncorrelated with the observable characteristics we study in earlier sections. To address the optimality of incremental lender monitoring, we focus on the market reaction to announcements of material covenant violations.

If the market reaction to these announcements by borrowers whose lenders have short-termism incentives is more positive than that of borrowers whose lenders do not have such incentives, then the incremental attention due to short-termism incentives is value-increasing for shareholders. This result would be consistent with the literature suggesting that banks are special and that monitoring by banks is valuable for borrowers (e.g., Schwert 2020). However, if the market reactions are insignificantly different or more negative for borrowers whose lenders have short-termism incentives, then we would infer that the net effect of incremental attention from these lenders is value-decreasing. To investigate differences in the market reactions for borrowers with lenders with and without short-termism incentives, we estimate the following regression model:

$$\begin{aligned} Ret_{it} = a + b_1 STLender_{jt} + b_2 Enforcement_{it} + b_3 STLender_{jt} \times Enforcement_{it} + b_4 Slack_{it} \\ + b_5 LenderEPSSurprise_{jt} + e_{ijt} \end{aligned} \quad (5)$$

where *Ret* is the cumulative abnormal return over the three-day period around the announcement of a covenant breach. We present specifications in which *Ret* is defined using five alternative measures: raw returns, market-adjusted returns, market model abnormal returns, Fama-French three-factor model abnormal returns, and Fama-French three factor plus momentum abnormal returns. In these tests, we restrict *Enforcement* to correspond to material covenant violation disclosures made by borrowers within four quarters of the covenant breach and the lender's EPS surprise. This restriction reduces measurement error from false negative identification of material covenant violations. Covenant breaches are publicly observable on the borrower's quarterly earnings announcement date, and material covenant violations are observable in public SEC filings, including 8-K, 10-Q, and 10-K forms. We present estimates of equation (5) for specifications that alternatively use two and ten cent windows around the zero EPS surprise cutoff. Columns (1) and (3) present estimates for the two-cent window, and columns (2) and (4) present estimates for the ten-cent window. Table 13 presents estimates of the market reaction to covenant violation announcements conditional on lender short-termism. For all estimates in Table 13, we present standard errors and significance indicators for both robust standard errors and robust standard errors that are clustered at the borrower and lender levels.

Columns (1) and (2) of Table 13 present estimates of the market response to covenant violation announcements for lenders without short-termism incentives. Across the five models of cumulative abnormal returns, we estimate the market response to be between -1.7% and -2.2% in the two-cent window and between -2.2% and -3.0% in the ten-cent window. When we evaluate statistical significance using the more conservative, clustered standard errors, our estimates are statistically significant at the 1% level in five of the ten specifications. With one exception, the specifications are statistically significant at the 5% level. Columns (3) and (4) present estimates for the incremental market response to covenant violation announcements made by borrowers with short-termist lenders. Across both samples, we estimate this incremental market response to be between -0.9% and -1.7%. It is statistically significant at the 5% level in eight specifications and at the 10% level in two specifications. In all specifications, we find that material covenant

violations are met with more negative market reactions for borrowers whose lenders face short-termism incentives.

### 4.4 Consequences for lender performance

In order to attribute the differential behavior we have documented to short-termism incentives, an important step is to verify that this enforcement behavior actually results in a significant increase in earnings. To this end, we use our estimates to conduct two simulation exercises.

In the first simulation, we calculate, for each lender-quarter in our sample, how many enforcements would be necessary to achieve an increase in reported EPS of one cent. In calculating the earnings impact of an enforcement, we incorporate all of our earlier findings, as earnings can increase due to increases in spreads (see Panel A of Table 11) and fees (see Table B7), in concert with increases in loan amounts. Our calculation assumes that the lender will receive one interest payment in the relevant quarter, for which the enforcement is credited for the estimated increase in the spread, since the interest payment at the original spread would have been received even with no enforcement. We also adjust the earnings effect for the proportion of the loan held by the lender, using the average retained share for that lender's outstanding loans in that quarter. Finally, the calculation assumes that the enforced loans are the size of the average loan in the portfolio, which should cause some downward bias in our findings. This is because earnings-seeking lenders would logically prioritize enforcement of loans where the available rents are larger than average.

The histogram in Figure 3 shows the results of this simulation. Notably, the modal lender-quarter requires only two enforcements to achieve the reported increase, while the median lender-quarter would require four enforcements. In interpreting these findings, it is important to note that covenant enforcement is used in concert with other earnings management strategies, as Table 8 shows. Thus in practice, some of the earnings needed to achieve a particular benchmark would come from these alternative strategies.

Our second simulation uses similar inputs to aggregate up the effect of the marginal enforcement that we identify to the lender-quarter level. That is, rather than calculate the earnings impact of one enforcement, we calculate the aggregate impact of the additional 3.2 percentage points of enforcement (as estimated in column 5 of Table 2). This requires two additional quantities: the size of the commercial loan portfolio for that lender-quarter, which we collect from bank holding company reports (i.e., FR-Y-9C), and the fraction of loans in breach, which we calculate directly from our data. We present these aggregate earnings impacts in Figure 4, where the empirical distribution of EPS surprise is depicted by the solid gray bars. The bars outlined in black are calculated by subtracting the earnings impact of the incremental enforcement, and they imply what the EPS surprise would have been in the absence of short-termism driven enforcement. The distribution becomes much more symmetric; that is, it suggests significantly less manipulation. A simple measure of this reduction comes from calculating the relative frequency difference in the two bins on each side of the threshold. This measure of the discontinuity in the pre-enforcement distribution is 41.1% smaller. Relative to the plausible baseline of a symmetric distribution, this implies that incremental covenant enforcement can explain about a third of observed manipulation by lenders.

### 4.5 Long-run costs for lenders

Our findings suggest that lenders systematically enforce breaches of financial covenant thresholds at higher rates when facing pressure to meet short-term earnings benchmarks. Common definitions of short-termism involve a tradeoff between short-run benefits and long-run costs. In this section, we close the loop and verify that banks indeed face long-run costs from their decisions to alter their behavior to meet short-term benchmarks. To do so, we follow Bhojraj et al. (2009), which argues that long-term cumulative abnormal returns at a three-year horizon are an appropriate measure of these long-run costs. Specifically, to investigate differences in the long-term reaction to earnings announcements for lenders with and without short-termism incentives, we estimate the following regression model:

$$Ret_{j,[t-1,t+3years]} = a_t + b_1 STLender_{j,t} + e_{j,t} \quad (6)$$

where $Ret_{i,[t-1,t+3years]}$ is the cumulative abnormal return of bank $i$ over the period spanning one day before bank $i$'s earnings announcement to three years following the earnings announcement, and $a_t$ are calendar-quarter fixed effects. We present specifications in which $Ret$ is defined using five alternative measures: raw returns, market-adjusted returns, market model abnormal returns, Fama-French three-factor model abnormal returns, and Fama-French three factor plus momentum abnormal returns. In all specifications, we restrict the sample to the [-2, 2] cent window of EPS surprise as in our preferred specifications from earlier tables. We also restrict our tests to bank-quarter observations with non-missing returns and year-over-year changes in enforcement rates, and exclude observations with nonsensical returns (e.g., below -100%).

In these tests, we interpret the coefficient $b_1$ as the incremental long-run return earned by banks that just-meet-or-beat their earnings benchmark in quarter $t$. Following Bhojraj et al. (2009), if $b_1 < 0$, then we infer that the actions taken by just-meeting-or-beating banks are costly in the long run. Column (1) of Table 14 presents estimates of $b_1$ for each of the five measures of long-run returns along with both heteroskedasticity-robust and clustered standard errors (at the bank level). Overall, these estimates are universally negative and almost all statistically different from zero.[18] Moreover, they are economically large when compared to the incremental short-term return of 1% for banks that just-meet-or-beat their benchmarks in quarter $t$ (e.g., see Figure 1). Overall, the estimates from equation (6) provide robust evidence that banks that were most likely to have manipulated to meet their earnings benchmarks subsequently earn between 3.9% and 8.2% lower returns over the following three years. These findings show that strategies employed by banks to achieve earnings benchmarks, including incremental covenant enforcement, are, on average, costly in the long run.

[18] For only one measure of long run returns, the Fama-French three-factor model of abnormal returns, the results are not statistically significant at the 10% level. However, even in this case, the coefficient estimate is similar to those estimated using the other four measures and has a $p$-value below 0.2.

Prior work has documented that banks use multiple tools to achieve this objective, and we documented evidence of banks' complementary use of incremental covenant enforcement and alternative tools in Table 8. To further investigate whether excess covenant enforcement is an especially costly strategy for banks to achieve their short-term earnings benchmarks, we estimate the following regression model:

$$Ret_{j,[t-1,t+3years]} = a_t + b_1 STLender_{j,t} + b_2 \Delta EnforcementRate_{j,t} + b_3 STLender_{j,t} \times \Delta EnforcementRate_{j,t} + e_{j,t} \quad (7)$$

where $\Delta EnforcementRate_{j,t}$ is defined to be the year-over-year change in covenant enforcement rate for bank $j$ in quarter $t$. Enforcement rate is alternatively defined using loan counts or amounts as in Table 7 and Section 3.4. Therefore, this specification allows us to test whether banks that increase covenant enforcement when they face short-termism incentives are subject to more long-run costs than banks that use other means to hit their earnings benchmarks.

We present two sets of estimates of $b_1$ and $b_3$. Columns (2) and (3) of Table 14 present estimates from a model in which enforcement rates are defined using the number of loans in violation divided by the number of loans in breach of at least one covenant threshold. Columns (4) and (5) instead define enforcement rates value-weighted by the amount of loans in breach and violation. Columns (2) and (4) corroborate the estimates of $b_1$ of equation (5) in column (1). The estimates of $b_3$, which we interpret as the incremental long-term cumulative abnormal return for an *STLender* bank that achieves its target by increasing its enforcement rate by 100%, are negative in all cases, but statistically significant in a subset. A negative $b_3$ coefficient suggests that increasing enforcement rates is an especially costly strategy for banks in the long run relative to the average alternative strategy. Therefore, together, the estimates in Table 14 suggest that covenant enforcement is as least as costly, if not more costly, than the average tool that banks use to meet earnings benchmarks. This evidence corroborates the micro-level evidence from Table 10 that borrowers are more likely to switch lenders following covenant enforcement by banks with

short-termism incentives, and is consistent with the market recognizing value in these lost relationships.

# 5 Conclusion

In this paper, we provide novel evidence that short-term capital market incentives are transmitted through financing relationships. Specifically, we find that lenders facing pressure to meet short-term earnings benchmarks are more likely to extract material income-increasing benefits from borrowers. Lenders are especially likely to do so when their performance benchmarks are precise or salient, and when their managers have high pay-performance sensitivity, but not when they are likely to face strong governance from shareholders.

Incremental enforcement of covenant breaches by short-termist lenders leads borrowers to incur not only the direct costs of waiver fees and loan renegotiations, but also the indirect costs of switching lenders for subsequent loans. We find that affected borrowers incrementally reduce investment and that the market response to the announcement of their material covenant violations is especially negative. Overall, our findings suggest that bank managers are willing to trade off relationship capital for income-boosting fees and term changes from stricter enforcement of covenant breaches in order to meet quarterly earnings benchmarks.

# Appendix A

## Table A1. Variable Definitions and Data Sources

| Variable | Definition | Data Source(s) |
|---|---|---|
| ***Main variables of interest:*** | | |
| *Enforcement* | Indicator that equals 1 if the borrower reports a material covenant violation in any of the subsequent four quarters and zero otherwise. | Greg Nini |
| *Slack* | The minimum standardized distance to the pre-set covenant threshold in the loan contract. See Section 2 for details. | Compustat, DealScan |
| *Breach* | Indicator that equals 1 if *Slack* is less than zero and zero otherwise. | Compustat, DealScan |
| *Lender EPS Surprise* | Realized earnings-per-share (EPS) minus the median analyst EPS forecast. | I/B/E/S |
| *STLender* | Indicator that equals 1 if the *Lender EPS Surprise* equals zero or one cent and zero otherwise. | I/B/E/S |
| *ExAnteSTLender* | Indicator that equals 1 if the lender's latent EPS surprise equals negative two or negative one cent and zero otherwise. | Authors' calculations |
| ***Lender characteristics:*** | | |
| *Dispersion* | The standard deviation of the lender's analyst forecasts. | I/B/E/S |
| *Coverage* | The number of unique analysts providing EPS forecasts for the lender. | I/B/E/S |
| *Capital ratio* | The lender's capital ratio (bhck3210/bhck2170). | FR Y-9C |
| *Liquidity ratio* | The lender's liquidity ratio ((bhck0010+bhck0383)/bhck2170). | FR Y-9C |
| *RetirementAge* | Indicator that equals one if the lender's CEO is at least 64 years old and zero otherwise (Cheng 2004). | Execucomp |
| *LongTenure* | Indicator that equals one if the lender's CEO is in the top quartile of tenure among lender CEOs and zero otherwise. | Execucomp |
| *IO(%)* | The percentage of institutional ownership in the lender. | Thomson Reuters 13-F Filings |
| *OwnConcentration* | The Herfindahl-Hirschman Index applied to the share of the lender's equity held by institutional owners. | Thomson Reuters 13-F Filings |
| *ExperiencedBoard* | Indicator that equals one if the members of the lender's board of directors have longer than average board experience. | RiskMetrics |
| *Variable compensation* | The ratio of stock plus option awards to total compensation for the lender's CEO. | Execucomp |
| *Delta* | The average pay-for-performance sensitivity of the lender's CEO and CFO | Lalitha Naveen |

| | | |
|---|---|---|
| $\Delta LLR$ | The quarter-over-quarter change in the ratio of loan loss reserves to total loans ((bhck3123 × 100)/bhck2122). | FR Y-9C |
| *LLP* | The ratio of provision for loan losses to total loans ((bhck4230 × 100)/bhck2122). | FR Y-9C |
| *Repurchase* | Indicator that equals one if the lender had a quarter-over-quarter decrease in shares outstanding and zero otherwise. | CRSP |
| 1[*LargeUnrealizedLoss*] | Indicator that equals one if the lender had an above average unrealized loss and zero otherwise. | FR Y-9C |
| ***Borrower characteristics:*** | | |
| *Whited-Wu* | Whited and Wu (2006) Index | Compustat |
| *M/B* | The ratio of market capitalization divided by book equity ((PRCCQ × CSHO)/CEQQ). | Compustat |
| *Leverage* | The ratio of the sum of debt in current liabilities and long-term debt to total assets ((DLCQ + DLTTQ)/ATQ). | Compustat |
| *ROA* | Return on assets (IBQ/ATQ). | Compustat |
| *Borrower EPS Surprise* | Realized earnings-per-share (EPS) minus the median analyst EPS forecast. | I/B/E/S |
| *STBorrower* | Indicator that equals 1 if the *Borrower EPS Surprise* equals zero or one cent and zero otherwise. | I/B/E/S |
| ***Loan characteristics:*** | | |
| *Spread (bps)* | The weighted average all-in-drawn spread for the loan package. | DealScan |
| *Amount ($M)* | The total loan package amount. | DealScan |
| *Maturity (months)* | The weighted average maturity (months) for the loan package. | DealScan |
| *Collateral* | Indicator that equals one if the loan is secured and zero otherwise. | DealScan |
| *TermLoan* | Indicator that equals one if the loan package includes a term loan and zero otherwise. | DealScan |
| *TermLoanB* | Indicator that equals one if the loan package includes a term loan B facility and zero otherwise. | DealScan |
| *#Tranches* | The number of facilities in the loan package. | DealScan |
| *Working capital purpose* | Indicator that equals one if the loan has a "working capital" primary purpose. | DealScan |
| *Investment purpose* | Indicator that equals one if the loan has an "investment" primary purpose. | DealScan |
| *Financing purpose* | Indicator that equals one if the loan has a "financing" primary purpose. | DealScan |
| *Corporate purpose* | Indicator that equals one if the loan has a "corporate purposes" primary purpose. | DealScan |

## Table A2. Covenant Calculations

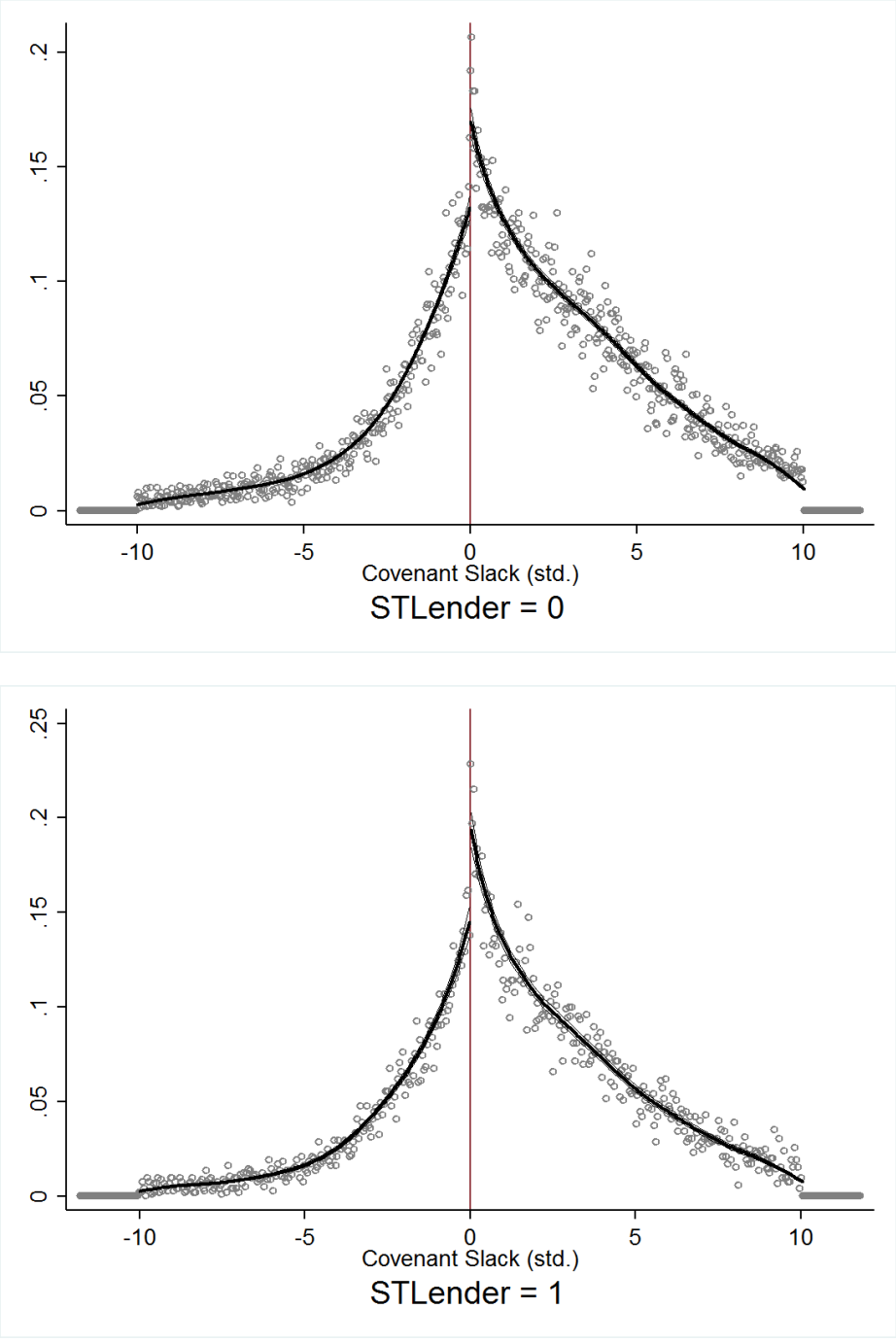
| Covenant Name | Calculation (Compustat codes) |
|---|---|
| *Debt-to-EBITDA* | (DLCQ + DLTTQ) / Rolling EBITDA |
| *Debt-to-Equity* | (DLCQ + DLTTQ) / SEQQ |
| *Debt-to-Tangible NW* | (DLCQ + DLTTQ) / (ATQ – INTANQ – LTQ) |
| *Leverage* | (DLCQ + DLTTQ) / ATQ |
| *Current ratio* | ACTQ/LCTQ |
| *Quick ratio* | (RECTQ + CHEQ) / LCTQ |
| *Cash interest coverage* | Rolling EBITDA/Rolling interest paid |
| *Interest coverage* | Rolling EBITDA/Rolling interest expense |
| *Debt service coverage* | Rolling EBITDA/(Rolling interest expense and principal payment) |
| *Fixed charge coverage* | Rolling EBITDA/(Rolling interest expense, principal payment, and rent payment) |
| *Net worth* | ATQ – LTQ |
| *Tangible net worth* | ATQ – INTANQ – LTQ |
| *EBITDA* | Rolling EBITDA |

*Rolling* EBITDA*, interest expense, interest paid, principal paid* are the sum of the firm's past four quarters.

# Appendix B

## Figure B1. Borrower Slack Manipulation and Lender Short-termism

This figure presents McCrary (2008) density test statistics around borrowers' pre-set covenant thresholds for clients of lenders with (i.e., $STLender = 1$) and without (i.e., $STLender = 0$) short-termism incentives.



## Table B1. Borrower Slack Manipulation and Lender Short-termism

This table presents McCrary (2008) tests of whether borrowers manipulate financial metrics around pre-set covenant thresholds when lenders have short-termism incentives (i.e., $STLender = 1$) and when they do not (i.e., $STLender = 0$). The density break test uses the *DCdensity.ado* function in Stata, which optimally selects the bin size and bandwidth and is available here. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

| | $STLender = 0$ | $STLender = 1$ |
|---|---|---|
| | (1) | (2) |
| *Breach* | 0.250*** | 0.289*** |
| | (0.027) | (0.038) |
| *Optimal bin size* | 0.032 | 0.047 |
| *Optimal bandwidth* | 1.694 | 1.745 |

## Table B2. Are Lender Short-termism and Borrower *Slack* Related?

This table presents fixed effects regression estimates of measures of *Slack* and *Breach* on *STLender* and control variables in a loan package-quarter panel. These tests investigate whether borrowers are more likely to have poor covenant performance or breach covenants when lenders have short-termism incentives. We include lender and quarter fixed effects to control for time-invariant lender characteristics and secular trends that may be related to lender benchmark incentives and borrower covenant slack. Other controls include loan characteristics (i.e., amount, spread, maturity, collateral indicator, term loan indicator, tranche B indicator, number of tranches, and loan purpose indicators) and borrower characteristics (i.e., Whited-Wu index, leverage, M/B, and a ROA). Heteroskedasticity-robust standard errors are double clustered by lender and borrower, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

| **Dependent variable:** | *Slack* | *Breach* |
|---|---|---|
| | (1) | (2) |
| *STLender* | 0.026 | -0.007 |
| | (0.049) | (0.007) |
| *Controls* | Yes | Yes |
| Fixed Effects: | | |
| *Lender* | Yes | Yes |
| *Quarter* | Yes | Yes |
| Adjusted $R^2$ | 0.2016 | 0.1779 |
| Obs. | 34,690 | 34,690 |

## Table B3. Are Lender Short-termism, Capital, and Liquidity Related?

This table presents fixed effects regression estimates of measures of lender health on *STLender* and control variables in a lender-quarter panel. The measures of lender health are *CapitalRatio*, *LowCapital*, *LiquidityRatio*, and *LowLiquidity*, each of which is relied upon by bank regulators. *CapitalRatio* is the ratio of total equity capital to total assets, and *LowCapital* is an indicator that equals one if *CapitalRatio* is less than 8%. *LiquidityRatio* is the ratio of cash and balances from depository institutions and securities maturing within one year to total assets, and LowLiquidity is an indicator that equals one if *LiquidityRatio* is less than 6%. We include lender and quarter fixed effects to control for unobservable time-invariant lender characteristics and secular trends in *STLender* and lender health. Heteroskedasticity-robust standard errors are clustered by lender, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

| **Dependent variable:** | *CapitalRatio* | *LowCapital* | *LiquidityRatio* | *LowLiquidity* |
|---|---|---|---|---|
| | (1) | (2) | (1) | (2) |
| *STLender* | 0.0004 | -0.037 | -0.002 | 0.020 |
| | (0.0011) | (0.024) | (0.004) | (0.025) |
| Fixed Effects: | | | | |
| *Lender* | Yes | Yes | Yes | Yes |
| *Quarter* | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.7524 | 0.5339 | 0.7441 | 0.5397 |
| Obs. | 2,416 | 2,416 | 1,285 | 1,285 |

## Table B4. Lender Short-termism and Covenant Enforcement: Controlling for Bank Capital and Liquidity Effects

This table presents fixed effects regression estimates of *Enforcement* on *Breach*, *STLender*, *Breach* × *STLender*, and control variables in a loan package-quarter panel. These tests are identical to those presented in column (5) of Table 2 except that they allow for additional interactive controls for lender health and *Breach* to test whether the short-termism induced enforcement of covenant breaches identified in the paper is driven by lender health (i.e., as opposed to lender performance relative to time-varying benchmarks). In columns (1) and (2), we explore the potential for lender capital to interact with *Breach*, and in columns (3) and (4), we allow lender liquidity to interact with *Breach*. Heteroskedasticity-robust standard errors are double clustered by lender and borrower, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

| **Dependent variable:** *Enforcement* | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Breach* × *STLender* | 0.034*** | 0.037*** | 0.032*** | 0.031*** |
| | (0.011) | (0.009) | (0.008) | (0.007) |
| *Breach* × *CapitalRatio* | -0.038*** | | | |
| | (0.008) | | | |
| *Breach* × *LowCapital* | | 0.102*** | | |
| | | (0.030) | | |
| *Breach* × *LiquidityRatio* | | | -0.003 | |
| | | | (0.004) | |
| *Breach* × *LowLiquidity* | | | | 0.046 |
| | | | | (0.028) |
| *Controls* | Yes | Yes | Yes | Yes |
| Fixed Effects: | | | | |
| *Quarter* | Yes | Yes | Yes | Yes |
| *Industry* × *Quarter* | Yes | Yes | Yes | Yes |
| *Lender* | Yes | Yes | Yes | Yes |
| *Lender* × *Quarter* | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.3214 | 0.3315 | 0.3316 | 0.3322 |
| Obs. | 33,818 | 33,818 | 32,521 | 32,521 |

## Table B5. Are Lender Short-termism and Borrower Short-termism Related?

This table presents fixed effects regression estimates of measures of borrower short-termism incentives on *STLender* and control variables in a loan package-quarter panel. The measures of borrower short-termism incentives are *EPSSurprise* and *STBorrower*. *EPSSurprise* is defined as the difference between the borrower's realized EPS and its analysts' consensus EPS forecast, and *STBorrower* is an indicator that equals one if *EPSSurprise* equals zero or one. For consistency, we define these variables like their lender counterparts. We include lender and quarter fixed effects to control for time-invariant lender characteristics and secular trends that may be related to borrowers' EPS surprises. Other controls include loan characteristics (i.e., amount, spread, maturity, collateral indicator, term loan indicator, tranche B indicator, number of tranches, and loan purpose indicators) and borrower characteristics (i.e., Whited-Wu index, leverage, M/B, and ROA). Heteroskedasticity-robust standard errors are double clustered by lender and borrower, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

| **Dependent variable:** | *EPSSurprise* | *STBorrower* |
|---|---|---|
| | (1) | (2) |
| *STLender* | 0.057 | -0.004 |
| | (0.295) | (0.005) |
| *Controls* | Yes | Yes |
| Fixed Effects: | | |
| *Lender* | Yes | Yes |
| *Quarter* | Yes | Yes |
| Adjusted $R^2$ | 0.0308 | 0.0396 |
| Obs. | 30,093 | 30,093 |

## Table B6. Lender Short-termism and Covenant Enforcement: Controlling for Borrower Short-termism Effects

This table presents fixed effects regression estimates of *Enforcement* on *Breach*, *STLender*, *Breach* × *STLender*, and control variables in a loan package-quarter panel. These tests are identical to those presented in column (5) of Table 2 except that they allow for additional interactive controls for borrower short-termism incentives and *Breach* to test whether the short-termism induced enforcement of covenant breaches identified in the paper is driven by borrower short-termism (i.e., as opposed to lender short-termism). In column (1), we use *EPSSurprise*, the difference between the borrower's realized EPS and its analysts' consensus EPS forecast, and in column (2) we use *STBorrower*, which is defined the same way as *STLender* for consistency. Heteroskedasticity-robust standard errors are double clustered by lender and borrower, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

| **Dependent variable:** *Enforcement* | | |
|---|---|---|
| | (1) | (2) |
| *Breach* × *STLender* | 0.025*** | 0.026*** |
| | (0.009) | (0.009) |
| *Breach* × *EPSSurprise* | -0.0004 | |
| | (0.0003) | |
| *Breach* × *STBorrower* | | 0.002 |
| | | (0.014) |
| *Controls* | Yes | Yes |
| Fixed Effects: | | |
| *Quarter* | Yes | Yes |
| *Industry* × *Quarter* | Yes | Yes |
| *Lender* | Yes | Yes |
| *Lender* × *Quarter* | Yes | Yes |
| Adjusted $R^2$ | 0.2840 | 0.2819 |
| Obs. | 29,216 | 29,216 |

## Table B7. Waiver Fee Analysis

This table presents fixed effects regression estimates of *Fee* (bps) on *STLender*, and control variables to test whether lenders with short-termism incentives extract more contemporaneous concessions from borrowers when they enforce covenant breaches. The sample includes loan package-quarter observations in which the borrower disclosed a material covenant violation, and the borrower was in breach of at least one of the loan package's covenant thresholds in the prior four quarters. We manually collect data on waiver and amendment fees from borrower 8-K filings. The mean waiver fee in this table's estimation sample is 54.81 bps, which is similar to what has been reported in other studies (Osborn 2014). We incrementally include year fixed effects, industry-by-year fixed effects, lender fixed effects, and lender-by-year fixed effects to eliminate industry dynamics and lender-specific enforcement behavior, which adjusts for characteristics that lead borrowers to match with a specific lender in a specific year. Other controls include loan characteristics (i.e., amount, spread, maturity, collateral indicator, and number of tranches) and borrower characteristics (i.e., Whited-Wu index, leverage, M/B, and ROA). Heteroskedasticity-robust standard errors are presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

| **Dependent variable:** *Fee (bps)* | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| *STLender* | 18.29 | 37.93** | 48.23** | 53.18** | 51.75* |
| | (14.22)$^{+}$ | (15.92) | (20.99) | (21.90) | (27.06) |
| *Controls* | No | No | No | No | Yes |
| Fixed Effects: | | | | | |
| *Year* | Yes | Yes | Yes | Yes | Yes |
| *Industry* $\times$ *Year* | No | Yes | Yes | Yes | Yes |
| *Lender* | No | No | Yes | Yes | Yes |
| *Lender* $\times$ *Year* | No | No | No | Yes | Yes |
| Adjusted $R^2$ | 0.0116 | 0.6936 | 0.7466 | 0.5070 | 0.5267 |
| Obs. | 135 | 73 | 69 | 52 | 52 |

# Appendix C

## Table C1. Does Enforcement Lead to Negative Sorting on Switchers?

This table presents regressions of *Switch*, an indicator that equals one if the borrower's next loan is issued by a new lead arranger and zero otherwise, on borrower and loan characteristics. We incrementally include quarter, lender, and borrower fixed effects to isolate within-bank and within-borrower variation in lender switching, holding fixed secular trends in economic conditions and the syndicated loan market. The sample includes the subset of loan package-quarter observations in which *STLender* = 1, *Breach* = 1, *Enforcement* = 1, and the bank's EPS surprise is in the [-2, 2] window. Heteroskedasticity-robust standard errors are double clustered at the borrower and lender levels and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

| **Dependent variable:** *Switch* | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| ***Borrower characteristics:*** | | | | |
| ln*Assets* | -0.074 | -0.070 | 0.003 | 0.161*** |
| | (0.054) | (0.061) | (0.062) | (0.057) |
| *Whited-Wu* | -0.284 | -0.408 | 0.670 | 1.185** |
| | (0.776) | (0.756) | (0.684) | (0.557) |
| *M/B* | -0.004 | -0.001 | 0.0008 | -0.008 |
| | (0.009) | (0.009) | (0.0075) | (0.006) |
| *Leverage* | 0.181 | 0.088 | 0.078 | -0.043 |
| | (0.119) | (0.147) | (0.162) | (0.083) |
| *ROA* | -0.244 | -0.358 | -0.394 | 0.289 |
| | (0.593) | (0.636) | (0.632) | (0.190) |
| ***Loan characteristics:*** | | | | |
| *Spread (bps)* | 0.117* | 0.122 | 0.094 | -0.056 |
| | (0.066) | (0.078) | (0.086) | (0.159) |
| *Amount ($M)* | -0.028 | -0.029 | -0.045 | -0.001 |
| | (0.042) | (0.046) | (0.047) | (0.068) |
| *Maturity (months)* | -0.0004 | -0.0009 | -0.002 | -0.006 |
| | (0.0015) | (0.0017) | (0.002) | (0.005) |
| *Collateral* | -0.044 | -0.021 | -0.033 | 0.186 |
| | (0.081) | (0.102) | (0.090) | (0.224) |
| *TermLoan* | -0.067 | -0.058 | -0.015 | -0.098 |
| | (0.091) | (0.082) | (0.091) | (0.112) |
| *TermLoanB* | -0.098 | -0.089 | -0.034 | 0.100 |
| | (0.091) | (0.094) | (0.093) | (0.158) |
| *#Tranches* | -0.018 | -0.021 | -0.022 | -0.010 |
| | (0.031) | (0.033) | (0.031) | (0.040) |
| Fixed Effects: | | | | |
| *Loan purpose* | Yes | Yes | Yes | Yes |
| *Quarter* | No | Yes | Yes | Yes |
| *Lender* | No | No | Yes | Yes |
| *Borrower* | No | No | No | Yes |
| Adjusted $R^2$ | 0.1043 | 0.0875 | 0.2153 | 0.7536 |
| Obs. | 858 | 856 | 850 | 723 |

# Appendix D

## Table D1. Enforcement Rates and Bank-level Outcomes

This table presents fixed effects regression estimates of measures of bank outcomes on *EnforcementRate*, the fraction of loans with a material covenant violation, and control variables in a bank-quarter panel. These tests investigate whether banks with high enforcement rates are more likely to have poor subsequent outcomes in the syndicated loan market, including growth in the total loan amounts ($\Delta\ln Amount_{t+1}$) and total number of loans ($\Delta\ln NumLoans_{t+1}$), and changes in the bank's league table rank based on market share ($\Delta Rank_{t+1}$). Panel A presents estimates using a measure of enforcement rate that is calculated with the number of loans in breach of a covenant as the denominator, whereas the estimates in Panel B use a measure calculated with the total number of loans in the bank's portfolio as the denominator. We include lender and quarter fixed effects to control for time-invariant lender characteristics and secular trends that may be related to lender benchmark incentives and borrower covenant slack. Other controls include bank characteristics (i.e., fraction of loans in breach, amount of total loans, percentage of commercial loans). Heteroskedasticity-robust standard errors are clustered by lender, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

**Panel A.** Enforcement Rate Calculated with Breaches as Denominator

| | $\Delta\ln Amount_{t+1}$ | | $\Delta\ln NumLoans_{t+1}$ | | $\Delta Rank_{t+1}$ | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *EnforcementRate* | -0.134*** | -0.152** | -0.092*** | -0.097*** | 1.424*** | 1.544*** |
| | (0.041) | (0.044) | (0.021) | (0.023) | (0.516) | (0.593) |
| *Controls* | Yes | Yes | Yes | Yes | Yes | Yes |
| Fixed Effects: | | | | | | |
| *Lender* | No | Yes | No | Yes | No | Yes |
| *Quarter* | Yes | Yes | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0386 | 0.1012 | 0.0923 | 0.1467 | 0.1923 | 0.2408 |
| Obs. | 4,650 | 4,623 | 4,650 | 4,623 | 4,650 | 4,623 |

**Panel B.** Enforcement Rate Calculated with Loans as Denominator

| | $\Delta\ln Amount_{t+1}$ | | $\Delta\ln NumLoans_{t+1}$ | | $\Delta Rank_{t+1}$ | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *EnforcementRate* | -0.285*** | -0.324*** | -0.161*** | -0.182*** | 2.067* | 2.279 |
| | (0.082) | (0.091) | (0.038) | (0.045) | (1.104) | (1.405) |
| *Controls* | Yes | Yes | Yes | Yes | Yes | Yes |
| Fixed Effects: | | | | | | |
| *Lender* | No | Yes | No | Yes | No | Yes |
| *Quarter* | Yes | Yes | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0397 | 0.1022 | 0.0922 | 0.1470 | 0.1919 | 0.2405 |
| Obs. | 4,650 | 4,623 | 4,650 | 4,623 | 4,650 | 4,623 |

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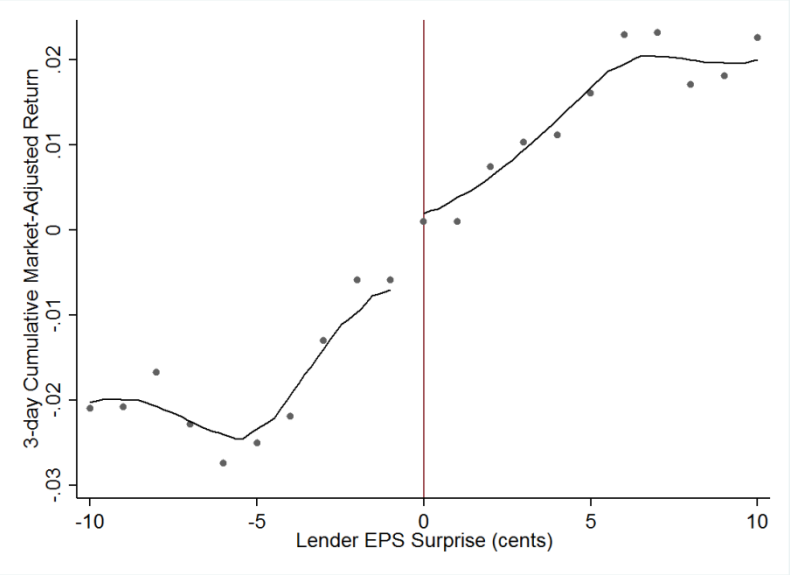
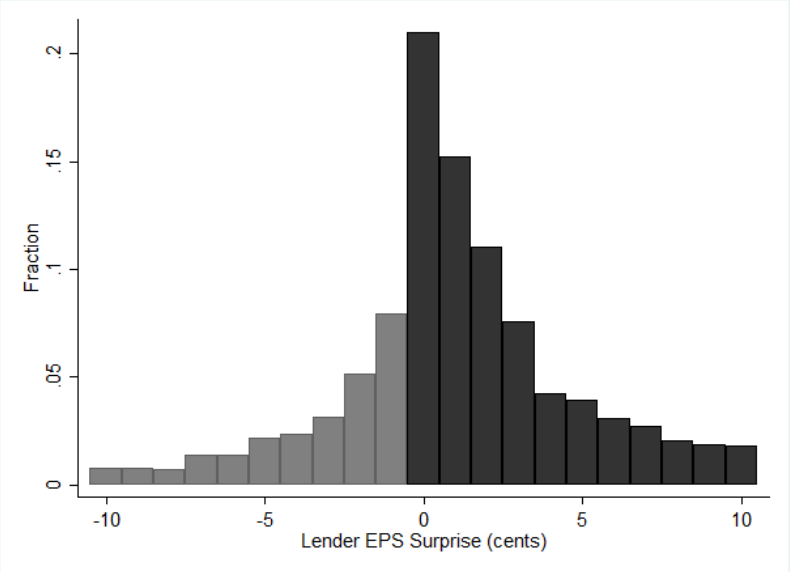



**Panel A. Announcement Returns and Lender EPS Surprise**




**Panel B. Distribution of Lender EPS Surprise**

## Figure 1. Lender Short-termism

Panel A of this figure presents a regression discontinuity plot of the three-day cumulative market-adjusted return around lenders' earnings announcements conditional on earnings-per-share (EPS) surprise. In any given quarter, an EPS surprise of zero indicates that the lender's realized EPS is equal to the analysts' consensus EPS forecast. The difference in cumulative market-adjusted returns between lenders that just-meet the zero EPS surprise cutoff (i.e., have EPS surprise of zero or one cent per share) and those that just-miss the zero EPS surprise cutoff (i.e., have EPS surprise of negative one or two cents per share) is 0.69%. Panel B of this figure presents a histogram of the distribution of lender EPS surprise. The bars in gray correspond to lender-quarter observations that missed their analysts' consensus EPS forecast, and the bars in black correspond to lender-quarter observations that beat their analysts' consensus EPS forecast.

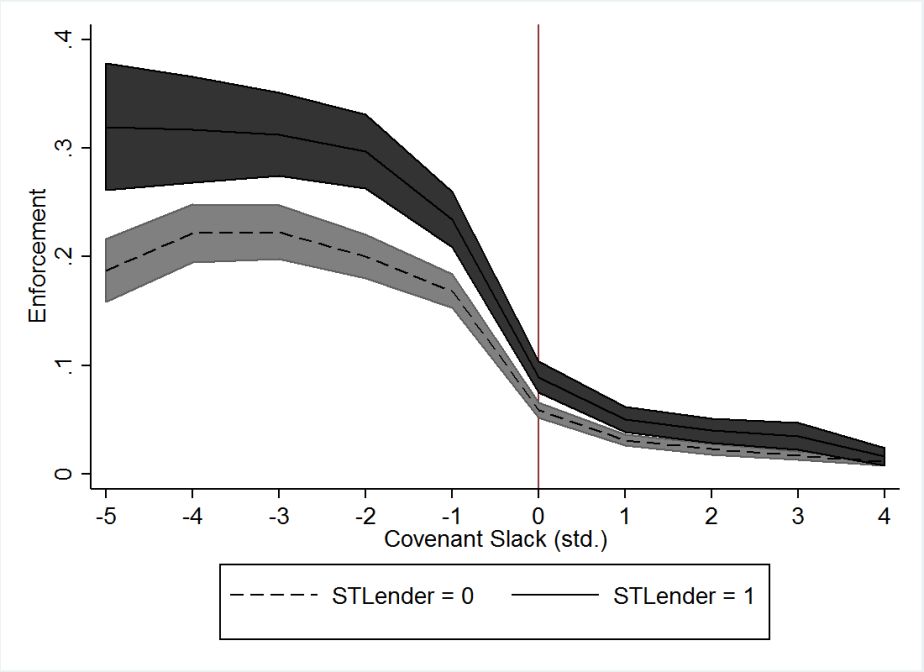



**Figure 2. Lender Short-termism and Covenant Enforcement**

This figure presents regression estimates of *Enforcement* on *STLender* and *STLender* interacted with indicators for bins of *Slack*. *Slack* is the standardized distance between the pre-set covenant threshold and the borrower's underlying financial metric. Negative values of *Slack* correspond to breaches of the contract, which shift control rights to lenders. The dashed line corresponds to the estimated enforcement rate when lenders do not have short-termism incentives (i.e., when $STLender = 0$). The light gray area represents the 95% confidence interval for these coefficient estimates. The solid line corresponds to the estimated enforcement rate when lenders have short-termism incentives (i.e., when $STLender = 1$), and the surrounding dark gray area represents the 95% confidence interval for these coefficient estimates. The difference between the two lines is the incremental enforcement rate at each level of *Slack*. For example, lenders with short-termism incentives enforce covenant breaches for borrowers with severe breaches (e.g., Slack $< -3$ ) at a rate of about 40% whereas lenders without short-termism incentives enforce at a rate of about 25% for borrowers with the same level of breach severity. The confidence intervals are based on standard errors that are double clustered by lender and borrower.

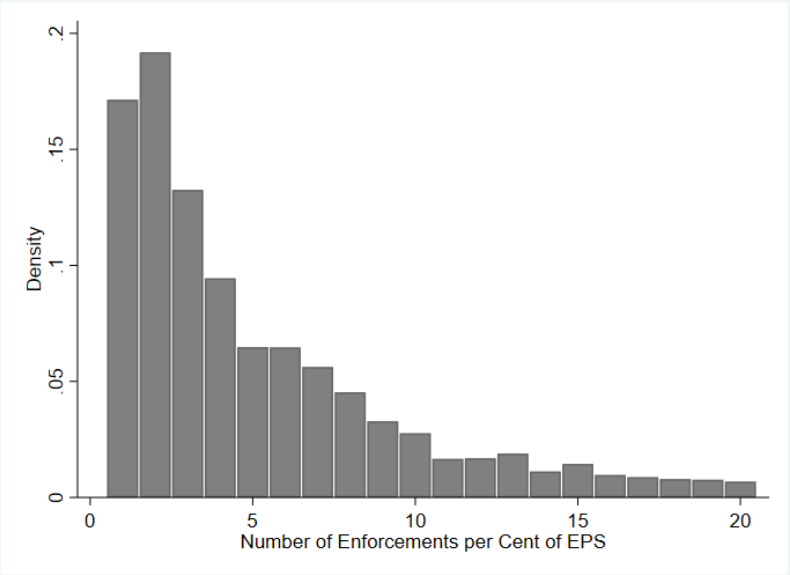



**Figure 3. Simulated Number of Enforcements to Increase EPS by One Cent**

This figure presents simulation evidence of the number of enforcements needed to increase reported EPS by one cent. To do so, we incorporate all of our relevant findings on fees and term changes as well as enforcement propensity documented in Tables 2, 10, and Appendix Table B7. Further details of the simulation are explained in Section 4.4.

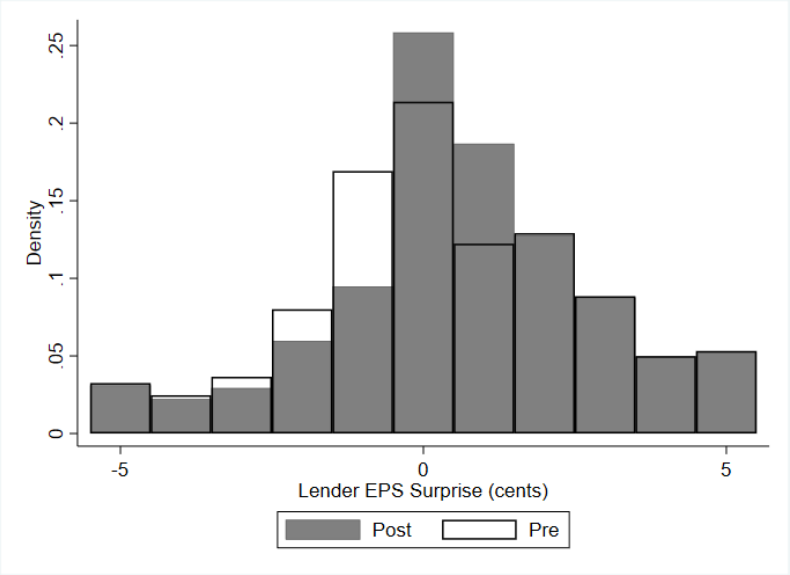



**Figure 4. Simulated Effect of Enforcement on Lender EPS Surprise**

This figure presents simulation evidence of the effect of incremental covenant enforcement on the distribution of EPS surprise, which is defined as the difference between reported EPS and analysts' consensus forecast. The empirical distribution of EPS surprise is depicted by the solid gray bars. The bars outlined in black are calculated by subtracting the earnings impact of the incremental enforcement, and they imply what the EPS surprise would have been in the absence of short-termism driven enforcement. Further details of this simulation are explained in Section 4.4.

## Table 1. Summary Statistics

This table presents summary statistics for the regression variables of interest. Panel A presents summary statistics of the main variables of interest. Panel B presents univariate differences in these variables across subsamples with and without *STLender*, which is an indicator that identifies observations with lenders that meet or just beat their earnings benchmark. In our tests, we isolate variation in lender short-termism by focusing on a two-cent window around the zero earnings-per-share surprise cutoff, so restrict the Panel B comparison to this sample.

**Panel A.** Summary statistics

| | Mean | SD | P25 | Median | P75 |
|---|---|---|---|---|---|
| ***Main variables of interest:*** | | | | | |
| *Enforcement* | 11.14% | | | | |
| *Slack* | 1.42 | 3.57 | -0.63 | 1.20 | 3.71 |
| *Breach* | 38.68% | | | | |
| *Lender EPS Surprise* | 0.37 | 1.18 | 0 | 0 | 1 |
| *STLender* | 58.45% | | | | |
| ***Lender characteristics:*** | | | | | |
| *Dispersion* | 0.03 | 0.05 | 0.02 | 0.02 | 0.03 |
| *Coverage* | 22.72 | 5.78 | 18 | 24 | 28 |
| *Capital ratio* | 9.30 | 1.82 | 7.90 | 9.14 | 10.91 |
| *Liquidity ratio* | 7.72 | 4.84 | 4.25 | 6.40 | 9.04 |
| *RetirementAge* | 21.62% | | | | |
| *LongTenure* | 22.48% | | | | |
| *IO(%)* | 64.20% | 9.20% | 58.19% | 62.43% | 73.82% |
| *OwnConcentration* | 3.07 | 1.15 | 2.49 | 2.83 | 3.25 |
| *ExperiencedBoard* | 49.14% | | | | |
| *Variable compensation* | 49.23% | 17.07% | 39.76% | 55.26% | 62.70% |
| *Delta* | 1,034.03 | 791.06 | 393.79 | 947.32 | 1,457.74 |
| $\Delta LLR$ | -0.04 | 0.11 | -0.08 | -0.04 | -0.00 |
| *LLP* | 0.52 | 0.67 | 0.17 | 0.33 | 0.63 |
| *Repurchase* | 49.93% | | | | |
| $1[LargeUnrealizedLoss]$ | 7.29% | | | | |
| ***Borrower characteristics:*** | | | | | |
| *Whited-Wu* | -0.33 | 0.31 | -0.40 | -0.34 | -0.27 |
| *M/B* | 2.53 | 1.73 | 1.32 | 2.08 | 3.24 |
| *Leverage* | 0.34 | 0.24 | 0.19 | 0.31 | 0.44 |
| *ROA* | 0.69% | 2.72% | 0.15% | 1.08% | 1.93% |
| *Borrower EPS Surprise* | 1.37 | 6.03 | -1 | 1 | 4 |
| *STBorrower* | 26.20% | | | | |
| ***Loan characteristics:*** | | | | | |
| *Spread (bps)* | 186.27 | 109.29 | 112.5 | 175 | 250 |
| *Amount ($M)* | 539.49 | 872.29 | 100 | 275 | 675 |
| *Maturity (months)* | 53.94 | 15.82 | 43 | 60 | 60 |

| | | | | | |
|---|---|---|---|---|---|
| *Collateral* | 61.45% | | | | |
| *TermLoan* | 38.23% | | | | |
| *TermLoanB* | 13.37% | | | | |
| *#Tranches* | 1.58 | 0.92 | 1 | 1 | 2 |
| *Working capital purpose* | 23.21% | | | | |
| *Investment purpose* | 19.01% | | | | |
| *Financing purpose* | 22.96% | | | | |
| *Corporate purpose* | 38.29% | | | | |

**Panel B.** Univariate differences on short-termism incentives

| | *STLender=1* | *STLender=0* | | |
|---|---|---|---|---|
| | Mean | Mean | Difference | *p-value* |
| ***Main variables of interest:*** | | | | |
| *Enforcement* | 12.09% | 9.80% | 2.29% | 0.012** |
| *Breach* | 39.32% | 37.79% | 1.53% | 0.417 |
| *Slack* | 1.35 | 1.51 | -0.16 | 0.225 |
| ***Borrower characteristics:*** | | | | |
| *Whited-Wu* | -0.33 | -0.34 | 0.01 | 0.109 |
| *M/B* | 2.50 | 2.56 | -0.06 | 0.230 |
| *Leverage* | 33.54 | 34.09 | -0.54 | 0.251 |
| *ROA* | 0.65% | 0.74% | -0.09% | 0.272 |
| ***Loan characteristics:*** | | | | |
| *Spread (bps)* | 187.37 | 184.73 | 2.64 | 0.615 |
| *Amount ($M)* | 502.58 | 591.40 | -88.82 | 0.031** |
| *Maturity (months)* | 53.66 | 54.34 | -0.67 | 0.160 |
| *Collateral* | 62.50% | 60.00% | 2.51% | 0.123 |
| *TermLoan* | 38.39% | 38.01% | 0.38% | 0.671 |
| *TermLoanB* | 13.36% | 13.38% | -0.03% | 0.964 |
| *#Tranches* | 1.58 | 1.59 | -0.01 | 0.392 |
| *Working capital purpose* | 23.37% | 22.98% | 0.38% | 0.843 |
| *Investment purpose* | 19.78% | 17.93% | 1.85% | 0.131 |
| *Financing purpose* | 23.90% | 21.64% | 2.26% | 0.170 |
| *Corporate purpose* | 36.70% | 40.51% | -3.81% | 0.284 |

## Table 2. Lender Short-termism and Covenant Enforcement

This table presents fixed effects regression estimates of *Enforcement* on *Breach*, *STLender*, *Breach* × *STLender*, and control variables in a loan package-quarter panel. In all specifications, we control for the distance to covenant breaches with *Slack* and for the lender's earnings surprise using indicator functions at each cent of earnings-per-share. We incrementally include quarter, industry-by-quarter, lender, and lender-by-quarter fixed effects to eliminate industry dynamics and lender-specific enforcement behavior, which adjusts for characteristics that lead borrowers to match with a specific lender in a specific quarter. Other controls include loan characteristics (i.e., amount, spread, maturity, collateral indicator, term loan indicator, tranche B indicator, number of tranches, and loan purpose indicators) and borrower characteristics (i.e., Whited-Wu index, leverage, M/B, and ROA). We isolate variation in lender short-termism by focusing on a two-cent window around the zero earnings-per-share surprise cutoff. Heteroskedasticity-robust standard errors are double clustered by lender and borrower, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

| **Dependent variable:** *Enforcement* | | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Breach* × *STLender* | 0.033** | 0.029*** | 0.031*** | 0.032*** | 0.032*** |
| | (0.010) | (0.008) | (0.007) | (0.007) | (0.007) |
| *Controls* | No | No | No | No | Yes |
| Fixed Effects: | | | | | |
| *Quarter* | Yes | Yes | Yes | Yes | Yes |
| *Industry* × *Quarter* | No | Yes | Yes | Yes | Yes |
| *Lender* | No | No | Yes | Yes | Yes |
| *Lender* × *Quarter* | No | No | No | Yes | Yes |
| E[*Enforcement* \| *Breach* = 1] | 9.14% | 9.44% | 9.40% | 9.18% | 6.24% |
| Adjusted $R^2$ | 0.1773 | 0.2520 | 0.2657 | 0.2802 | 0.3270 |
| Obs. | 38,214 | 37,579 | 37,577 | 37,363 | 33,818 |

## Table 3. Lender Short-termism and Covenant Enforcement: *Slack* Robustness

This table presents fixed effects regression estimates of *Enforcement* on *Breach*, *STLender*, *Breach* × *STLender*, and control variables in a loan package-quarter panel. The specification is identical to the one presented in column (5) of Table 2. In Panel A, we use alternative definitions of *Slack* and *Breach* based on a subset of financial covenants that have relatively standard definitions. In column (1), we define *Slack* using Debt/EBITDA as in Demiroglu and James (2010). In columns (2) and (3), we define *Slack* using the current ratio and net worth (including tangible net worth) as in Chava and Roberts (2008). In these specifications, *Breach* is an indicator that equals one only if the re-defined slack is less than zero, which means that breaches of other financial covenants are not measured. In column (4), we construct combined measures of *Slack* and *Breach* using these three types of financial covenants, and in column (5) we estimate the same specification as in column (5) of Table 2 but restrict the sample to loan packages that include at least one of these three types of financial covenants. Panel B of this table presents estimates in subsamples to address additional concerns about measurement error in *Slack* and *Breach*. Columns (1) and (2) exclude loan packages with dynamic covenant thresholds and renegotiations during the life of the loan, respectively. Column (3) excludes observations after the year of the borrower's first breach of a covenant threshold of each loan package, and column (4) excludes loan packages in which no covenant threshold breaches are ever observed. Heteroskedasticity-robust standard errors are double clustered by lender and borrower, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

**Panel A. Slack Measurement**

| **Dependent variable:** *Enforcement* | | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Breach* × *STLender* | 0.038** | 0.090** | 0.060* | 0.048*** | 0.037*** |
| | (0.016) | (0.038) | (0.035) | (0.013) | (0.007) |
| *Slack definition* | *D/EDA* | *CR* | *NW* | *Combined* | *Restricted sample* |
| *Controls* | Yes | Yes | Yes | Yes | Yes |
| Fixed Effects: | | | | | |
| *Quarter* | Yes | Yes | Yes | Yes | Yes |
| *Industry* × *Quarter* | Yes | Yes | Yes | Yes | Yes |
| *Lender* | Yes | Yes | Yes | Yes | Yes |
| *Lender* × *Quarter* | Yes | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.3418 | 0.4360 | 0.3171 | 0.3411 | 0.3443 |
| Obs. | 21,556 | 2,375 | 10,302 | 27,915 | 27,982 |

**Panel B. Sample Restrictions**

| **Dependent variable:** *Enforcement* | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Breach* × *STLender* | 0.035** | 0.065*** | 0.032*** | 0.033*** |
| | (0.015) | (0.016) | (0.005) | (0.009) |
| *Sample restriction* | *No Dynamic Thresholds* | *No Renegotiations* | *Only First Breach* | *No Never-Breach Loans* |
| *Controls* | Yes | Yes | Yes | Yes |
| Fixed Effects: | | | | |
| *Quarter* | Yes | Yes | Yes | Yes |
| *Industry* × *Quarter* | Yes | Yes | Yes | Yes |
| *Lender* | Yes | Yes | Yes | Yes |
| *Lender* × *Quarter* | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.3265 | 0.3996 | 0.3318 | 0.3082 |
| Obs. | 21,275 | 6,374 | 20,750 | 23,306 |

## Table 4. Lender Short-termism and Covenant Enforcement: *STLender* Robustness

This table presents fixed effects regression estimates of *Enforcement* on *Breach*, *STLender*, *Breach* × *STLender*, and control variables in a loan package-quarter panel. The specification is identical to the one presented in column (5) of Table 2. However, each column represents an alternative definition of *STLender* and window around the zero earnings-per-share surprise cutoff. In column (1), we maintain the same STLender definition as in our baseline specification, but we expand the sample window to five cents around the zero earnings-per-share surprise cutoff. In columns (2) to (6), we further expand the sample window to include ten cents around the zero earnings-per-share surprise cutoff. For each of these subsequent columns, we use an alternative definition of *STLender*. In column (2), we maintain the same definition as in our baseline specification. In column (3), we focus only on the subset of firms that just-meet the cutoff with a surprise of zero cents of earnings-per-share. In column (4), we extend the definition from our baseline specification to include firms with a surprise of two cents of earnings-per-share. In column (5), we further extend that definition to include firms with a surprise of three cents of earnings-per-share. Finally, in column (6), following the findings in Bird, Karolyi, and Ruchti (2019) that a relatively high proportion of firms with negative one cent of earnings-per-share surprise also manipulated earnings based on the ex ante chance of meeting their benchmarks, we allow *STLender* to include firms with negative one cent of earnings-per-share surprise. Heteroskedasticity-robust standard errors are double clustered by lender and borrower, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

| **Dependent variable:** *Enforcement* | | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Breach* × *STLender* | 0.026** | 0.040*** | 0.038** | 0.023** | 0.024** | 0.027*** |
| | (0.011) | (0.010) | (0.014) | (0.011) | (0.010) | (0.009) |
| *STLender bins* | [0, 1] | [0, 1] | [0, 0] | [0, 2] | [0, 3] | [-1, 3] |
| *Lender surprise window* | [-5, 5] | [-10, 10] | [-10, 10] | [-10, 10] | [-10, 10] | [-10, 10] |
| *Controls* | Yes | Yes | Yes | Yes | Yes | Yes |
| Fixed Effects: | | | | | | |
| *Quarter* | Yes | Yes | Yes | Yes | Yes | Yes |
| *Industry* × *Quarter* | Yes | Yes | Yes | Yes | Yes | Yes |
| *Lender* | Yes | Yes | Yes | Yes | Yes | Yes |
| *Lender* × *Quarter* | Yes | Yes | Yes | Yes | Yes | Yes |
| E[*Violation* \| *Breach* = 1] | 7.29% | 6.92% | 7.50% | 7.22% | 6.99% | 6.65% |
| Adjusted $R^2$ | 0.3117 | 0.2960 | 0.2957 | 0.2955 | 0.2955 | 0.2956 |
| Obs. | 48,869 | 67,566 | 67,566 | 67,566 | 67,566 | 67,566 |

## Table 5. Lender Short-termism and Covenant Enforcement: Internal Validity

This table presents fixed effects regression estimates of *Enforcement* on *Breach*, *STLender*, *Breach* × *STLender*, and control variables in a loan package-quarter panel. These tests are identical to those presented in column (5) of Table 2 except that they allow for a triple interaction term for proxies for incremental benchmark incentives to test whether the short-termism induced enforcement of covenant breaches identified in previous tables is amplified when short-termism is better measured or performance benchmarks are more broadly followed. In columns (1) and (2), we explore the potential for analyst forecast dispersion to inflate or attenuate short-termism incentives driven by analysts' earnings-per-share benchmarks. In column (1), we measure *Disp*, which is the standardized dispersion in the banks' analyst forecasts for a given quarter, where dispersion is the standard deviation of analyst forecasts. In column (2), we use 1[*Low Disp*], an indicator that equals one if the bank-quarter is in the bottom quartile of the analyst forecast dispersion distribution and zero otherwise. In columns (3) and (4), we explore the potential for analyst coverage to inflate or attenuate short-termism incentives driven by analysts' earnings-per-share benchmarks. In column (3), we use *Cov*, which is the standardized number of analysts following the bank in a given quarter. In column (4), we use 1[*High Cov*], an indicator that equals one if the bank-quarter is in the top quartile of the analyst coverage distribution. Heteroskedasticity-robust standard errors are double clustered by lender and borrower, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

| **Dependent variable:** *Enforcement* | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Breach* × *STLender* × *Disp* | -0.068*** | | | |
| | (0.023) | | | |
| *Breach* × *STLender* × 1[*Low Disp*] | | 0.073*** | | |
| | | (0.020) | | |
| *Breach* × *STLender* × *Cov* | | | 0.023 | |
| | | | (0.017) | |
| *Breach* × *STLender* × 1[*High Cov*] | | | | 0.080** |
| | | | | (0.032) |
| *Controls* | Yes | Yes | Yes | Yes |
| Fixed Effects: | | | | |
| *Quarter* | Yes | Yes | Yes | Yes |
| *Industry* × *Quarter* | Yes | Yes | Yes | Yes |
| *Lender* | Yes | Yes | Yes | Yes |
| *Lender* × *Quarter* | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.3259 | 0.3296 | 0.3329 | 0.3313 |
| Obs. | 33,780 | 33,818 | 33,818 | 33,818 |

## Table 6. Lender Short-termism and Covenant Enforcement: Alternative Measures of Short-termism

This table presents fixed effects regression estimates of *Enforcement* on *Breach* and *Breach* interacted with alternative measures of short-termism incentives as well as control variables in a loan package-quarter panel. In column (1), our alternative measure of short-termism incentives is *%manip*, which is an ex ante structural measure of the proportion of bank-quarter observations within each cent of earning-per-share surprise that manipulated earnings as in Bird, Karolyi, and Ruchti (2019). In columns (2) and (3), we follow prior literature and focus on retirement age and abnormally long-tenured bank CEOs using *RetirementAge*, which is an indicator that equals one if the bank CEO is at least 64 years old (Cheng 2004), and *LongTenure*, which is an indicator that equals one if the bank CEO is in the top quartile of the CEO tenure distribution and zero otherwise. In columns (4) and (5), we explore other states of the world in which banks may want to accelerate income recognition, specifically when they have low capital or liquidity. We use *LowCapital*, an indicator that equals one if the bank's capital ratio is less than 8% and zero otherwise, and *LowLiquidity*, an indicator that equals one if the bank's liquidity ratio is less than 6% and zero otherwise. Heteroskedasticity-robust standard errors are double clustered by lender and borrower, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

| **Dependent variable:** *Enforcement* | | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Breach* × *%manip* | 0.372*** | | | | |
| | (0.088) | | | | |
| *Breach* × *RetirementAge* | | 0.187** | | | |
| | | (0.072) | | | |
| *Breach* × *LongTenure* | | | 0.097** | | |
| | | | (0.040) | | |
| *Breach* × *LowCapital* | | | | 0.100*** | |
| | | | | (0.031) | |
| *Breach* × *LowLiquidity* | | | | | 0.047* |
| | | | | | (0.026) |
| *Controls* | Yes | Yes | Yes | Yes | Yes |
| Fixed Effects: | | | | | |
| *Quarter* | Yes | Yes | Yes | Yes | Yes |
| *Industry* × *Quarter* | Yes | Yes | Yes | Yes | Yes |
| *Lender* | Yes | Yes | Yes | Yes | Yes |
| *Lender* × *Quarter* | Yes | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.3269 | 0.3423 | 0.3348 | 0.3308 | 0.3274 |
| Obs. | 33,818 | 27,281 | 27,708 | 33,818 | 33,818 |

## Table 7. Ex Ante Lender Short-termism

This table presents fixed effects regression estimates of *EnforcementRate*, the fraction of loans with a material covenant violation, on *ExAnteSTLender* and control variables in a bank-quarter panel. *ExAnteSTLender* is an indicator that equals one if the bank is in the [-2, 0) window of the latent EPS surprise distribution and zero otherwise. In columns (1)-(4), *EnforcementRate* is defined as the fraction of the bank's entire loan portfolio with a material covenant violation, and in columns (5)-(8), it is defined as the fraction of loans in breach of a covenant threshold with a material covenant violation. As described in Section 4.4, the latent EPS surprise distribution is based on the distribution of bank earnings before taking into account income generated from enforcing covenant breaches. Similar to Almeida et al. (2016), this measure is intended to capture the ex ante incentive of banks to manage earnings via covenant enforcement. We incrementally include quarter and lender fixed effects to eliminate fixed differences in covenant enforcement across banks and secular changes in economic conditions that may relate to both benchmark incentives and covenant enforcement. Other controls include bank characteristics (i.e., fraction of loans in breach of a covenant threshold, total loans, and commercial loan share). We isolate variation in banks' ex ante short-termism incentives by focusing on a two-cent window around zero latent EPS surprise. Heteroskedasticity-robust standard errors are clustered by lender, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

| **Dependent variable:** *Enforcement Rate* (%) | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| **Denominator:** | *Loans* | | | | *Breaches* | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *ExAnteSTLender* | 0.043*** | 0.044*** | 0.037*** | 0.037*** | 0.094*** | 0.097*** | 0.076*** | 0.076*** |
| | (0.004) | (0.004) | (0.004) | (0.005) | (0.009) | (0.009) | (0.009) | (0.009) |
| *Controls* | No | No | No | Yes | No | No | No | Yes |
| Fixed Effects: | | | | | | | | |
| *Quarter* | No | Yes | Yes | Yes | No | Yes | Yes | Yes |
| *Lender* | No | No | Yes | Yes | No | No | Yes | Yes |
| E[*Enforcement Rate*] | 5.42% | | | | 12.15% | | | |
| Adjusted $R^2$ | 0.868 | 0.1331 | 0.2481 | 0.2540 | 0.1250 | 0.1774 | 0.3022 | 0.3048 |
| Obs. | 2,765 | 2,765 | 2,698 | 2,698 | 2,045 | 2,045 | 1,996 | 1,996 |

## Table 8. Lender Short-termism and Covenant Enforcement: Alternative Manipulation Tools

This table presents fixed effects regression estimates of *Enforcement* on *Breach*, *STLender*, *Breach* × *STLender*, and control variables in a loan package-quarter panel. These tests are identical to those presented in column (5) of Table 2 except that they allow for a triple interaction term for alternative earnings manipulation tools to test whether the short-termism induced enforcement of covenant breaches identified in previous tables is used alongside other earnings management tools that are commonly used by banks. In column (1), *Repurchase*, which is an indicator equal to one if the bank had a net repurchase of common shares during the quarter and zero otherwise. In column (2), we interact *Breach* × *STLender* with $\Delta LLR$, which is the standardized quarter-over-quarter change in loan loss reserves. In column (3), we interact *Breach* × *STLender* with *LLP*, which is the standardized amount of loan loss provisions. In column (4), we interact *Breach* × *STLender* with 1[*LargeUnrealizedLoss*], which is an indicator that identifies observations with above average levels of unrealized losses and zero otherwise. Heteroskedasticity-robust standard errors are double clustered by lender and borrower, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

| **Dependent variable:** *Enforcement* | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Breach* × *STLender* × 1[*Repurchase*] | 0.064*** | | | |
| | (0.018) | | | |
| *Breach* × *STLender* × $\Delta LLR$ | | -0.064** | | |
| | | (0.031) | | |
| *Breach* × *STLender* × *LLP* | | | -0.046** | |
| | | | (0.022) | |
| *Breach* × *STLender* × 1[*LargeUnrealizedLoss*] | | | | 0.139** |
| | | | | (0.063) |
| *Controls* | Yes | Yes | Yes | Yes |
| Fixed Effects: | | | | |
| *Quarter* | Yes | Yes | Yes | Yes |
| *Industry* × *Quarter* | Yes | Yes | Yes | Yes |
| *Lender* | Yes | Yes | Yes | Yes |
| *Lender* × *Quarter* | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.3292 | 0.3206 | 0.3277 | 0.3277 |
| Obs. | 33,818 | 27,559 | 33,818 | 33,818 |

## Table 9. Lender Short-termism and Covenant Enforcement: Cross-Sectional Heterogeneity

This table presents fixed effects regression estimates of *Enforcement* on *Breach*, *STLender*, *Breach* × *STLender*, and control variables in a loan package-quarter panel. These tests are identical to those presented in column (5) of Table 2 except that they allow for a triple interaction term for proxies for incremental benchmark incentives to test whether the short-termism induced enforcement of covenant breaches identified in previous tables is mediated by lender governance or executive compensation. In column (1), we use *Delta*, the standardized average measure of pay-for-performance sensitivity for the bank's CEO and CFO as constructed by Coles, Daniel, and Naveen (2006). In column (2), we use *VariableCompensation*, the standardized fraction of performance-based compensation for the bank's CEO. In column (3), we use the percentage of shares held by institutional owners. In column (4), we use the standardized concentration (i.e., Herfindahl-Hirschman Index) of institutional ownership. In column (5), we use *ExperiencedBoard*, an indicator that equals one if the members of the lender's board of directors have longer than average board experience. Heteroskedasticity-robust standard errors are double clustered by lender and borrower, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

| **Dependent variable:** *Enforcement* | | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Breach* × *STLender* × *Delta* | 0.036** | | | | |
| | (0.015) | | | | |
| *Breach* × *STLender* × *VariableCompensation* | | 0.021*** | | | |
| | | (0.008) | | | |
| *Breach* × *STLender* × *IO(%)* | | | -0.203*** | | |
| | | | (0.077) | | |
| *Breach* × *STLender* × *OwnConcentration* | | | | -0.015** | |
| | | | | (0.006) | |
| *Breach* × *STLender* × *ExperiencedBoard* | | | | | -0.038** |
| | | | | | (0.015) |
| *Controls* | Yes | Yes | Yes | Yes | Yes |
| Fixed Effects: | | | | | |
| *Quarter* | Yes | Yes | Yes | Yes | Yes |
| *Industry* × *Quarter* | Yes | Yes | Yes | Yes | Yes |
| *Lender* | Yes | Yes | Yes | Yes | Yes |
| *Lender* × *Quarter* | Yes | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.3456 | 0.3258 | 0.3311 | 0.3268 | 0.3109 |
| Obs. | 22,576 | 16,063 | 31,384 | 33,514 | 10,489 |

## Table 10. Lender Short-termism and Relationship Dynamics

This table presents fixed effects regression estimates of *Switch* on *Breach*, *STLender*, *Breach* × *STLender*, and control variables in a loan package-quarter panel. *Switch* is an indicator that equals one if the borrower's next loan is issued by a new lead arranger and zero otherwise. The unconditional probability of switching lead arrangers is 33.02%. In Panel A, *Switch* is defined using subsequent loans within two years of the breach. Panel B presents estimates corresponding to the most restrictive specification in Panel A (i.e., column (5)) for alternative definitions of *Switch* based on subsequent loans within one, two, three, four, or five years of the breach. *Slack* and *Breach* are based on the minimum covenant slack observed in the prior four quarters to account for the fact that lenders may have up to one year to enforce a covenant breach. In all specifications, we control for the distance to covenant breaches with *Slack* and for the lender's earnings surprise using indicator functions at each cent of earnings-per-share. We incrementally include quarter, industry-by-quarter, lender, and lender-by-quarter fixed effects to eliminate industry dynamics and lender-specific enforcement behavior, which adjusts for characteristics that lead borrowers to match with a specific lender in a specific quarter. Other controls include loan characteristics (i.e., amount, spread, maturity, collateral indicator, term loan indicator, tranche B indicator, number of tranches, and loan purpose indicators), and borrower characteristics (i.e., Whited-Wu index, leverage, M/B, and ROA). We isolate variation in lender short-termism by focusing on a two-cent window around the zero earnings-per-share surprise cutoff. Heteroskedasticity-robust standard errors are double clustered by lender and borrower, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

**Panel A.** Main Estimates

| **Dependent variable:** *Switch* | | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Breach* × *STLender* | 0.026** | 0.032** | 0.043*** | 0.048** | 0.046** |
| | (0.012) | (0.016) | (0.015) | (0.019) | (0.018) |
| *Controls* | No | No | No | No | Yes |
| Fixed Effects: | | | | | |
| *Quarter* | Yes | Yes | Yes | Yes | Yes |
| *Industry* × *Quarter* | No | Yes | Yes | Yes | Yes |
| *Lender* | No | No | Yes | Yes | Yes |
| *Lender* × *Quarter* | No | No | No | Yes | Yes |
| Adjusted $R^2$ | 0.0550 | 0.0771 | 0.1204 | 0.0748 | 0.1042 |
| Obs. | 10,320 | 9,536 | 9,532 | 9,320 | 9,320 |

**Panel B.** Varying Measurement Horizon for Lender Switching

| **Dependent variable:** *Switch* | | | | | |
|---|---|---|---|---|---|
| **Horizon:** | 1 year | 2 years | 3 years | 4 years | 5 years |
| | (1) | (2) | (3) | (4) | (5) |
| *Breach* × *STLender* | 0.083** | 0.046** | 0.030** | 0.027** | 0.030*** |
| | (0.039) | (0.018) | (0.015) | (0.014) | (0.011) |
| *Controls* | Yes | Yes | Yes | Yes | Yes |
| Fixed Effects: | | | | | |
| *Quarter* | Yes | Yes | Yes | Yes | Yes |
| *Industry* × *Quarter* | Yes | Yes | Yes | Yes | Yes |
| *Lender* | Yes | Yes | Yes | Yes | Yes |
| *Lender* × *Quarter* | Yes | Yes | Yes | Yes | Yes |
| Adjusted $R^2$ | 0.0600 | 0.1042 | 0.1288 | 0.1352 | 0.1290 |
| Obs. | 3,871 | 9,320 | 12,790 | 14,760 | 15,864 |

## Table 11. Lender Short-termism and Loan Term Dynamics

This table presents fixed effects regression estimates of $\Delta Spread(\%)$ or $\Delta Amount(\%)$ on *Breach*, *STLender*, *Breach* × *STLender*, and control variables in a loan package-quarter panel. $\Delta Spread(\%)$ and $\Delta Amount(\%)$ are the percentage change in the all-in-drawn spread and loan amount, respectively, between the borrower's next loan and the present loan. Panel A presents estimates for $\Delta Spread(\%)$, and Panel B presents estimates for $\Delta Amount(\%)$. In these specifications, *Slack* and *Breach* are based on the minimum covenant slack observed in the prior four quarters to account for the fact that lenders may have up to one year to enforce a covenant breach. In all specifications, we control for the distance to covenant breaches with *Slack* and for the lender's earnings surprise using indicator functions at each cent of earnings-per-share. We incrementally include quarter, industry-by-quarter, lender, and lender-by-quarter fixed effects to eliminate industry dynamics and lender-specific enforcement behavior, which adjusts for characteristics that lead borrowers to match with a specific lender in a specific quarter. Other controls include loan characteristics (i.e., amount, spread, maturity, collateral indicator, term loan indicator, tranche B indicator, number of tranches, and loan purpose indicators), and borrower characteristics (i.e., Whited-Wu index, leverage, M/B, and ROA). We isolate variation in lender short-termism by focusing on a two-cent window around the zero earnings-per-share surprise cutoff. Heteroskedasticity-robust standard errors are double clustered by lender and borrower, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

**Panel A.** $\Delta Spread(\%)$

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| *Breach* × *STLender* | 0.041** | 0.064*** | 0.052*** | 0.069*** | 0.068*** |
| | (0.017) | (0.018) | (0.014) | (0.017) | (0.019) |
| *Controls* | No | No | No | No | Yes |
| Fixed Effects: | | | | | |
| *Quarter* | Yes | Yes | Yes | Yes | Yes |
| *Industry* × *Quarter* | No | Yes | Yes | Yes | Yes |
| *Lender* | No | No | Yes | Yes | Yes |
| *Lender* × *Quarter* | No | No | No | Yes | Yes |
| Adjusted $R^2$ | 0.0724 | 0.0687 | 0.0925 | 0.0743 | 0.1286 |
| Obs. | 9,591 | 8,827 | 8,823 | 8,616 | 7,751 |

**Panel B.** $\Delta Amount(\%)$

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| *Breach* × *STLender* | 0.126** | 0.192*** | 0.196*** | 0.161** | 0.155** |
| | (0.055) | (0.060) | (0.063) | (0.064) | (0.070) |
| *Controls* | No | No | No | No | Yes |
| Fixed Effects: | | | | | |
| *Quarter* | Yes | Yes | Yes | Yes | Yes |
| *Industry* × *Quarter* | No | Yes | Yes | Yes | Yes |
| *Lender* | No | No | Yes | Yes | Yes |
| *Lender* × *Quarter* | No | No | No | Yes | Yes |
| Adjusted $R^2$ | 0.0175 | 0.0104 | 0.0342 | 0.0019 | 0.1037 |
| Obs. | 9,591 | 8,827 | 8,823 | 8,616 | 7,751 |

## Table 12. Real Effects of Lender Short-termism

This table presents fixed effects regression estimates of $\Delta Investment(\%)$ or $\Pr(\Delta Investment(\%) < -20\%)$ on *Breach*, *STLender*, *Breach* × *STLender*, and control variables in a loan package-quarter panel. $\Delta Investment(\%)$ and $\Pr(\Delta Investment(\%) < -20\%)$ are the quarterly growth in total investment and the probability that total investment has a quarter-over-quarter decline of at least 20%, respectively. Panel A presents estimates for $\Delta Investment(\%)$, and Panel B presents estimates for $\Pr(\Delta Investment(\%) < -20\%)$. The baseline probability of total investment falling by 20% or more in a given quarter is 34.5% in our estimation sample. In these specifications, *Slack* and *Breach* are based on the minimum covenant slack observed in the prior four quarters to account for the fact that lenders may have up to one year to enforce a covenant breach. In all specifications, we control for the distance to covenant breaches with *Slack* and for the lender's earnings surprise using indicator functions at each cent of earnings-per-share. We incrementally include quarter, industry-by-quarter, lender, and lender-by-quarter fixed effects to eliminate industry dynamics and lender-specific enforcement behavior, which adjusts for characteristics that lead borrowers to match with a specific lender in a specific quarter. Other controls include loan characteristics (i.e., amount, spread, maturity, collateral indicator, term loan indicator, tranche B indicator, number of tranches, and loan purpose indicators), and borrower characteristics (i.e., Whited-Wu index, leverage, M/B, and ROA). We isolate variation in lender short-termism by focusing on a two-cent window around the zero earnings-per-share surprise cutoff. Heteroskedasticity-robust standard errors are double clustered by lender and borrower, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

**Panel A.** $\Delta Investment(\%)$

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| *Breach* × *STLender* | -0.056** | -0.075*** | -0.077*** | -0.095*** | -0.078*** |
| | (0.026) | (0.022) | (0.021) | (0.021) | (0.023) |
| *Controls* | No | No | No | No | Yes |
| Fixed Effects: | | | | | |
| *Quarter* | Yes | Yes | Yes | Yes | Yes |
| *Industry* × *Quarter* | No | Yes | Yes | Yes | Yes |
| *Lender* | No | No | Yes | Yes | Yes |
| *Lender* × *Quarter* | No | No | No | Yes | Yes |
| Adjusted $R^2$ | 0.0250 | 0.0894 | 0.0917 | 0.1038 | 0.1080 |
| Obs. | 28,067 | 27,483 | 27,477 | 27,302 | 24,395 |

**Panel B.** $\Pr(\Delta Investment(\%) < -20\%)$

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| *Breach* × *STLender* | 0.031*** | 0.042*** | 0.041*** | 0.047*** | 0.041*** |
| | (0.012) | (0.009) | (0.010) | (0.011) | (0.015) |
| *Controls* | No | No | No | No | Yes |
| Fixed Effects: | | | | | |
| *Quarter* | Yes | Yes | Yes | Yes | Yes |
| *Industry* × *Quarter* | No | Yes | Yes | Yes | Yes |
| *Lender* | No | No | Yes | Yes | Yes |
| *Lender* × *Quarter* | No | No | No | Yes | Yes |
| Adjusted $R^2$ | 0.0403 | 0.1085 | 0.1101 | 0.1191 | 0.1209 |
| Obs. | 28,067 | 27,483 | 27,477 | 27,302 | 24,395 |

## Table 13. Short-term Borrower Market Reactions to Lender Short-termism

This table presents estimates from an event study of the market reaction to earnings announcements in which a covenant breach is revealed. The dependent variable is the cumulative abnormal return during the three days around the announcement. Rows of the table correspond to different models of abnormal returns, including raw, market-adjusted, market model, Fama-French three factor model, and Fama-French three factor plus momentum model. The independent variables include *Enforcement* and *Enforcement × STLender* as well as controls for the lender's earnings surprise and the borrower's covenant slack. We present two sets of standard errors below each estimate for comparison. Robust standard errors are presented in parentheses, and robust standard errors that are clustered at the borrower and lender levels are presented in brackets. ***, **, and * denote results significant at the 1%, 5%, and 10% levels, and are shown beside the corresponding standard error estimates.

| **Estimated coefficients:** | *Enforcement* | | *Enforcement × STLender* | |
|---|---|---|---|---|
| **Lender surprise window:** | [-2, 2] | [-10, 10] | [-2, 2] | [-10, 10] |
| | (1) | (2) | (3) | (4) |
| **Model:** | | | | |
| *Raw Returns* | -0.022 | -0.030 | -0.017 | -0.009 |
| Robust S.E. | (0.005)*** | (0.002)*** | (0.006)*** | (0.004)** |
| Clustered S.E. | [0.010]** | [0.004]*** | [0.008]** | [0.005]* |
| *Market-Adjusted Returns* | -0.021 | -0.029 | -0.017 | -0.009 |
| Robust S.E. | (0.005)*** | (0.002)*** | (0.006)*** | (0.004)** |
| Clustered S.E. | [0.010]** | [0.004]*** | [0.008]** | [0.004]** |
| *Market Model* | -0.018 | -0.022 | -0.013 | -0.009 |
| Robust S.E. | (0.004)*** | (0.002)*** | (0.005)** | (0.004)** |
| Clustered S.E. | [0.008]** | [0.004]*** | [0.006]** | [0.004]** |
| *FF3 Model* | -0.019 | -0.022 | -0.012 | -0.009 |
| Robust S.E. | (0.004)*** | (0.002)*** | (0.005)** | (0.004)** |
| Clustered S.E. | [0.008]** | [0.004]*** | [0.007]* | [0.004]** |
| *FF3 + MOM Model* | -0.017 | -0.022 | -0.014 | -0.009 |
| Robust S.E. | (0.004)*** | (0.002)*** | (0.005)*** | (0.004)** |
| Clustered S.E. | [0.009]* | [0.004]*** | [0.007]** | [0.004]** |
| Obs. | 7,646 | 19,280 | 7,646 | 19,280 |

## Table 14. Long-Term Lender Market Reactions to Lender Short-termism

This table presents estimates from a long run event study of the market reaction to lenders' earnings announcements conditional on whether the lender is an *STLender* and the amount the lender increased its enforcement rate during the quarter. The dependent variable is the cumulative abnormal return during the three years following the announcement. Rows of the table correspond to different models of abnormal returns, including raw, market-adjusted, market model, Fama-French three factor model, and Fama-French three factor plus momentum model. The independent variables include $\Delta EnforcementRate$ and $\Delta EnforcementRate \times STLender$ as well as controls for the lender's earnings surprise and the borrower's covenant slack. In the specifications presented in columns (2) and (3) of Panel B, *EnforcementRate* is defined as the fraction of the bank's entire loan portfolio with a material covenant violation, and in columns (4) and (5), it is defined as the fraction of loans in breach of a covenant threshold with a material covenant violation. $\Delta EnforcementRate$ is the year-over-year change in enforcement rates. We present two sets of standard errors below each estimate for comparison. Robust standard errors are presented in parentheses, and robust standard errors that are clustered at the borrower and lender levels are presented in brackets. ***, **, and * denote results significant at the 1%, 5%, and 10% levels, and are shown beside the corresponding standard error estimates.

| | *STLender* | *STLender* | $\Delta EnforcementRate \times STLender$ | *STLender* | $\Delta EnforcementRate \times STLender$ |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| **Model:** | | | | | |
| *Raw Returns* | -0.064 | -0.062 | -0.109 | -0.063 | -0.087 |
| Robust S.E. | (0.021)*** | (0.021)*** | (0.061)* | (0.021)*** | (0.063) |
| Clustered S.E. | [0.025]*** | [0.024]** | [0.072] | [0.024]** | [0.071] |
| *Market-Adjusted Returns* | -0.039 | -0.037 | -0.077 | -0.038 | -0.046 |
| Robust S.E. | (0.017)** | (0.018)** | (0.051) | (0.017)** | (0.053) |
| Clustered S.E. | [0.019]** | [0.019]** | [0.055] | [0.019]** | [0.059] |
| *Market Model* | -0.080 | -0.076 | -0.244 | -0.079 | -0.159 |
| Robust S.E. | (0.038)** | (0.039)** | (0.112)** | (0.039)** | (0.116) |
| Clustered S.E. | [0.042]* | [0.041]* | [0.139]* | [0.041]* | [0.121] |
| *FF3 Model* | -0.048 | -0.046 | -0.213 | -0.048 | -0.151 |
| Robust S.E. | (0.038) | (0.038) | (0.110)* | (0.038) | (0.114) |
| Clustered S.E. | [0.038] | [0.038] | [0.157] | [0.038] | [0.141] |
| *FF3 + MOM Model* | -0.082 | -0.077 | -0.222 | -0.080 | -0.154 |
| Robust S.E. | (0.039)** | (0.040)** | (0.115)** | (0.040)** | (0.119) |
| Clustered S.E. | [0.041]** | [0.040]* | [0.151] | [0.041]** | [0.140] |
| *Enforcement Rate* denominator | | | *Loans* | | *Amount* |
| Obs. | 1,276 | | 1,276 | | 1,276 |