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Creditor control rights and resource allocation within firms☆

Nuri Ersahin[a], Rustom M. Irani[b,*], Hanh Le[c]

[a] Broad College of Business, Michigan State University, 308 Eppley Center, East Lansing, MI 48824, USA
[b] Gies College of Business, University of Illinois at Urbana-Champaign, 1206 South Sixth Street, Champaign, IL 61820, USA
[c] College of Business, University of Illinois at Chicago, 601 South Morgan Street, Chicago, IL 60607, USA

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ABSTRACT

We examine the within-firm resource allocation and restructuring outcomes at firms violating debt covenants. Using establishment-level data from the US Census Bureau, we find that covenant violations are followed by reductions in employment, investment, and more frequent establishment closures among violating firms' noncore business lines and less productive establishments. These changes are concentrated among establishments at which manager-shareholder agency costs are pronounced and when key lenders have industry experience. Our findings suggest that enhanced creditor control reduces managerial agency costs and encourages a more efficient allocation of resources within the boundaries of firms in technical default.

1. Introduction

Governance by creditors not only has profound effects among bankrupt firms (Gilson, 1990; James, 1995; 1996), but it also extends to a broad range of firms through technical default. Debt covenant violations shift control rights to creditors, which, given their right to demand repayment, puts them in a strong position to influence corporate decision-making.[1] Firm-level empirical evidence confirms that covenant violations bring about more conservative corporate financing and liquidity management decisions (Roberts and Sufi, 2009), along with significant investment

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* Corresponding author.
E-mail addresses: ersahin2@msu.edu (N. Ersahin), rirani@illinois.edu (R.M. Irani), hanhle@uic.edu (H. Le).

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[1] Lenders may use the threat of calling the loan to influence firms through several mechanisms: waivers contingent on borrower improvements in financial condition, constraints on credit availability, contractual restrictions including limits on investment (Denis and Wang, 2014; Nini et al., 2009), enhanced monitoring of financial statements or collateral (Ivanov et al., 2016), or the appointment of creditor-friendly directors (Ferreira et al., 2018).

and employment cut backs (Chava and Roberts, 2008). While early evidence suggests that covenant violations are costly to shareholders (Beneish and Press, 1993), recent work argues that shifts in control rights to creditors can improve efficiency and increase shareholder value by reducing manager-shareholder agency costs (Bharath and Hertzel, 2019; Nini et al., 2009; 2012).[2]

So far, little is known about about the precise operational changes that could plausibly yield these value improvements. Simply put, as previous studies are all at the firm level, the firm itself is treated as a black box. In this paper, we seek to open up the black box of the firm using confidential establishment-level data from the US Census Bureau (henceforth, Census). This allows us to analyze which establishments get closed down and how resources are redeployed from one establishment to another, following covenant violations, to trace out the different micro mechanisms underlying the improvement.[3] Motivated by the literature on inefficient resource allocation within conglomerate firms, we analyze several establishment attributes connected to incentive conflicts between management and shareholders.[4] In doing so, we push forward our understanding of how state-contingent creditor control rights can have positive spillovers to shareholders.

Our main findings can be summarized as follows. First, we consider the industry focus of establishments within the firm. Peripheral business lines—activities outside the main scope of the firm—may arise from managers' privately beneficial "grandstanding" or "empire building" incentives (Williamson, 1964; Gompers, 1996), or management may lack experience relative to core activities (Scharfstein and Stein, 2000). Thus, withdrawing resources from these establishments and refocusing may improve operating efficiency and decrease the risk of failure, thus improving firm performance and value (Schoar, 2002). In line with this reasoning, we find that resources are withdrawn to a greater extent from establishments operating in peripheral industries. Violating firms lay off more employees at continuing peripheral establishments and, along the extensive margin, shutter them more often, relative to those within their core industry focus. This points to refocusing the scope of the firm as a first operational channel through which enhanced creditor control brings about performance improvements.

Second, we examine the importance of establishment productivity. Prior literature argues that "quiet life" incentives may induce managers to be slow to fire workers or to shutter underperforming plants at the expense of firm value (Bertrand and Mullainathan, 2003). To investigate whether creditor governance can undo such effects, we focus on the set of manufacturing firms for which the Census provides highly detailed information on factor inputs and output. This richness enables us to construct an array of establishment-level productivity measures—including total and individual labor and capital factor productivities—that we estimate both parametrically and non-parametrically. We uncover striking evidence that violating firms cut employment and investment at, and close down more often, those establishments that are classified as unproductive. Thus, resource withdrawal from relatively unproductive units is a second contributing factor to the improvement in firm performance.[5]

We also investigate the role of establishment operating risk. Given that creditors are exposed to losses on the downside, naturally we might expect them to push for risk reduction after the transfer of control rights. Measuring operating risk based on time-series and cross-sectional variation in establishment outcomes (e.g., operating margins), we find robust evidence that violating firms withdraw resources from riskier units. However, once we characterize how establishment risk and productivity interact, we observe cuts occurring almost exclusively among establishments classified as both risky and unproductive. Taken together, these findings suggest that the active role played by creditors after covenant violations can benefit both the creditors and shareholders of violating firms by reducing default risk and by improving economic efficiency.

To support our interpretation that enhanced creditor control benefits shareholders, we explore how these internal allocation effects vary among firms. We find that covenant violations have stronger effects among firms with greater managerial slack, including those firms operating in concentrated industries associated with weaker equity governance (e.g., Giroud and Mueller, 2010). Digging deeper, we construct establishment-level proxies for managers' private benefits: recent projects launched by the CEO and "hometown" establishments—those establishments located near a CEO's childhood home—that prior research has shown exhibit inefficient favoritism (Yonker, 2017). In both cases, we find pronounced resource withdrawals at these establishments around covenant violations. Thus, using multiple sources of variation, we find consistent evidence of a complementarity between creditor- and equity-based governance in minimizing manager-shareholder agency costs.

[2] Beneish and Press (1993) estimate an average cost of covenant violations between 1.2% and 2% of the market value of equity. In contrast, based on a broader sample, Nini et al. (2012) find that violating firms' stock returns (risk-adjusted) rebound at a rate of 5% per year within three months of the violation.

[3] Discussions in public filings point to within-firm restructuring activities surrounding covenant violations. For example, in the 2016 10-K filing of Ignite Restaurant Group, "forebearing lenders" were acknowledged as having discussions with management in their "pursuit of various strategic alternatives" to "enhance and preserve liquidity" and to "improve our capital structure." These strategic alternatives included closing underperforming restaurants; selling noncore assets; reducing labor, marketing, and operating expenses; eliminating new restaurant development; and reducing capital expenditures to maintenance levels.

[4] Shleifer and Vishny (1997) and Stein (2003) survey the literature on corporate governance and agency problems within conglomerates. These surveys highlight spillovers from entrenched managers' preferences to firm performance.

[5] In terms of economic magnitudes, we find a 4 percentage point reduction in firm-level employment at violator firms as compared with similar nonviolator firms, which is moderate relative to less frequent, but more severe, credit events such as bond defaults and bankruptcy filings (layoffs of 27% and 50%, respectively; see Agrawal and Matsa, 2013; Hotchkiss, 1995). Employment cutbacks are sharper at the peripheral (–9.3 percentage points) and unproductive (–10.7 percentage points) establishments of violator firms as compared with similar establishments of nonviolator firms.

Which types of lenders are more likely to bring about these positive spillovers? In our final set of tests, we conjecture that lenders with greater experience may be better able to offer expertise when negotiating with and advising management through tough times. We uncover novel evidence consistent with this mechanism. Specifically, for each firm, we characterize the industry specialization of relationship lenders in terms of whether they have significant lending to its industry (e.g., if they are a market leader in terms of loan origination volume). We do so using lending data from Reuters' Loan Pricing Corporation's Dealscan database, which allows us to connect firms to relationship lenders (i.e., loan lead arrangers) as well as to measure these lenders' historic industry-level loan origination activity. Based on this measurement, we show that only covenant violations in which relationship lenders have prior industry experience exhibit the array of within-firm resource allocation effects described above. We conclude that managerial agency costs are alleviated and performance gains are achieved primarily when key lenders bring industry-specific turnaround skill to bear on violating firms' operations.

Our findings contribute to empirical research on the importance of creditors in corporate governance, which builds on theoretical work analyzing optimal debt contracting in the presence of agency problems (e.g., Jensen and Meckling, 1976; Jensen, 1986). Earlier work has argued that regulatory and legal impediments—including prohibition of large equity investments and the threat of having their claims equitably subordinated in bankruptcy or litigation under lender liability laws—may limit the scope for creditor intervention outside of default states (Gilson and Vetsuypens, 1994). Prior empirical research therefore emphasizes creditor control through debt restructuring when borrowers are bankrupt (Gilson, 1990; Gilson et al., 1990; Wruck, 1990; James, 1995; 1996), including modern evidence on the role of nonbank lenders (Ivashina et al., 2016; Erel et al., 2018). More recently, (Nini et al., 2012) provides evidence suggesting an active and positive role of creditors in corporate governance outside of contracting and bankruptcy states. They argue that, following covenant violations, creditors are in a stronger position to influence firm decision-making and show that the transfer of control rights improves operating performance and this has positive spillovers to shareholders.[6,7]

Our paper provides novel evidence of a mechanism that is consistent with this positive role of creditors among underperforming firms: the way resources are allocated within firm boundaries. In particular, we show that performance improvements among firms in technical default are driven, at least in part, by pulling resources away from relatively unproductive and risky establishments as well as those operating outside of the firm's core competency. We provide new evidence that relationship lenders' past industry experience plays a central role in mitigating manager-shareholder agency costs and in achieving a more efficient resource allocation. Our findings therefore contrast with earlier research (e.g., Beneish and Press, 1993), which argues that creditors' demands following covenant violations may force firms to eliminate profitable investment projects, resulting in negative spillovers to shareholders.

We identify sources of efficiency gains that resemble those associated with major equity-centered governance interventions, notably mergers and acquisitions (Li, 2013; Maksimovic et al., 2011), private equity (PE) transactions (Davis et al., 2014), and hedge fund activism (Brav et al., 2015).[8] However, while the operational adjustments surrounding these interventions are similar, it is important to recognize that the types of firms violating covenants look very different from those targeted by activist shareholders. For example, hedge fund activist targets are mostly mature and generate free cash flow, whereas firms in technical default tend to be cash-strapped and underperforming. Moreover, on the financial side, hedge fund targets subsequently increase leverage and dividends, whereas firms in technical default do the opposite (Nini et al., 2012). Our findings therefore suggest that despite the fact that equity- and creditor-centered governance might be suitable for different firm types or firms at different stages in their life cycle, the effects of these interventions for capital allocation and restructuring are quite comparable.

More broadly, our paper relates to the literature on creditor rights and firm outcomes, including risk-taking. In a cross-country analysis, (Acharya et al., 2011) find that firms in creditor-friendly bankruptcy regimes have lower leverage and cash flow risk. In the US context, (Eisdorfer, 2008) finds evidence of risk-shifting among financially distressed firms, whereas (Gilje, 2016), in the context of the oil and gas industry, finds that firms with bank loans featuring stricter financial covenants reduce investment risk (i.e., exploratory drilling) as they approach bankruptcy. Between-firm evidence indicates that covenant violations are followed by conservatism in capital structure (Roberts and Sufi, 2009); reductions in firm-level investments, acquisitions, and employment (Chava and Roberts, 2008; Falato and Liang, 2016; Nini et al., 2009; Becher et al., 2018); and research and development (Chava et al., 2017). We complement these studies by providing new evidence on the operational effects of covenant violations within the boundaries of firms. The disaggregated nature of our data allows us to provide unique insights on how within-firm adjustments relate to key establishment

[6] Theoretically, creditor control may be value improving for underperforming firms since creditors' concave payoff structure creates incentives to monitor and constrain inefficient outcomes in the presence of private benefits (Aghion and Bolton, 1992; Dewatripont and Tirole, 1994; Zender, 1991). In the presence of agency conflicts between management and outside investors, creditor discipline may therefore increase the value of both debt and equity.

[7] Relatedly, (Bharath and Hertzel, 2019) show that firms with weaker external governance are more likely to issue bank debt (as opposed to bonds), consistent with creditors providing a substitute source of corporate governance.

[8] When targeted by a merger, private equity buyout, and hedge fund activist, employment at the continuing establishments falls by 2.1, 2.5, and 3.4 percentage points at the 3-year horizon, respectively. In each case, these effects become larger for relatively unproductive establishments. To illustrate, establishments targeted in a private equity buyout in the bottom tercile of the own-industry productivity distribution are 5.2 percentage points more likely to be closed or sold as compared with (matched) nontargeted establishments (see Davis et al., 2014).

attributes, including establishment-level proxies for CEO favoritism, that are motivated by research on corporate governance and misallocation within conglomerates (e.g., Stein, 2003). In addition, we provide evidence consistent with lender expertise enabling firms to achieve improvements in resource allocation and productive efficiency. Our findings therefore contrast with a narrow view of stronger lenders bargaining solely for risk reduction to protect their short-term interests and are more consistent with banks valuing relationships with borrowers as a going concern due to reputation costs of default or to future lending and cross-selling opportunities (e.g., Bharath et al., 2007).

2. Data and empirical methodology

2.1. Data sources

We use three establishment-level data sets provided by the Census. First, we use the Longitudinal Business Database (LBD), which annually tracks all business establishments in the United States with at least one paid employee. It provides longitudinal identifiers as well as data on the number of employees, payroll, location, and industry for each establishment. The LBD also records corporate affiliation, allowing us to identify establishment closures.

The Census of Manufactures (CMF) and Annual Survey of Manufactures (ASM) provide greater detail on activities for the subset of manufacturing establishments (Standard Industrial Classification (SIC) codes between 3000 and 3999). The CMF is a survey conducted every five years (years ending 2 and 7) and consists of all manufacturing establishments in the United States with at least one paid employee. The ASM is another survey conducted in noncensus years (i.e., when the CMF is not conducted) for a subset of these manufacturing establishments. This includes all establishments with greater than 250 employees and some with fewer employees, which are selected with a probability positively correlated with size. Reporting for both of these surveys is mandatory and misreporting is penalized, so the data is of the highest quality. Both the CMF and ASM include information on industry, corporate affiliation, output (total value of shipments), employment, capital expenditures, and material inputs of each establishment. The level of detail of these manufacturing data sets helps us construct various measures of productivity for each manufacturing establishment.

Our firm-level data come from Compustat. This database contains balance sheet and income statement data for publicly traded US corporations, which are the focus of this study. We gather a large number of standard accounting variables primarily to be used as control variables in our analysis. Our sample covers the period from 1996 to 2009. Following Nini et al. (2012), for a firm-year to be included in the sample, we require nonmissing data on total assets, total sales, common shares outstanding, and closing share price. We exclude (financial) firms with SIC codes between 6000 and 6999 as well as firms with book value of assets less than $10 million.

We use the longitudinal identifiers in LBD to merge the CMF and ASM. We then use the Compustat-SSEL bridge maintained by the Census to match each firm in Compustat to its establishments. The Compustat-SSEL bridge ends in 2005, so we extend the match to 2009 using employer characteristics including name, address, and employer identification number.

Our primary data on financial covenant violations is kindly provided online by Nini et al. (2012).[9] This is a quarterly data set that contains an indicator variable defining whether each firm-quarter in Compustat has violated a financial covenant. All companies with registered securities are required to disclose covenant violations in quarterly filings with the SEC under Regulation S-X (Roberts and Sufi, 2009; Beneish and Press, 1993). The authors use a combination of textual analysis and hand collection to carefully identify firms reporting a covenant violation. Their approach captures about 90% of actual reported violations. This data set begins in 1996—the first year in which electronic filing with the SEC became mandatory—and ends in 2009, which explains our choice of sample window.

In robustness tests, we use alternative measures of covenant violations based on loan contract terms at origination from Reuters' Loan Pricing Corporation's Dealscan database (henceforth, Dealscan) following Chava and Roberts (2008). Dealscan provides a large sample of loan contracts, including detailed information on maintenance covenants based on accounting ratios, that we match to Compustat.[10] We assume firms are bound by a given covenant threshold as stated at origination until the loan matures and take the tightest covenant at a given point in time.[11] In these tests, we restrict the sample merged to Compustat to firms having either net worth or current ratio covenants during the time period from 1996 until 2009. We focus on these covenants for two main reasons. First, Roberts and Sufi (2009) show that more than 95% of loan contracts include at least one financial covenant, with the net worth (leverage) and current ratio covenants being among the most common. Second, determining whether a violation has occurred or not for these two covenants is straightforward since the corresponding accounting variables are standard.

2.2. Variable construction and summary statistics

We capture how firms allocate resources using employment because of the completeness of the data provided in the LBD. In most tests, employment is measured as the annual change in the natural logarithm of the number of employees. At the establishment level, the number of

[9] These authors provide an excellent description of covenants in corporate credit agreements, including specific examples of violations from Securities and Exchange Commission (SEC) filings. They argue that covenants, while common in most debt contracts, are most prevalent and are often binding in bank loans (see also Taylor and Sansone, 2007).

[10] Thanks to Sudheer Chava and Michael Roberts for providing the Dealscan-Compustat link.

[11] Two caveats apply. First, firms may have overlapping deals (i.e., the first deal matures after the start of the second deal). Second, covenant thresholds can change over the tenure of the loan in a predetermined manner or, say, due to a renegotiation or refinancing of the deal. We address these challenges following Chava and Roberts (2008) (see their Appendix B). We assume firms are subject to a given covenant threshold for the longest maturity of all loans in each package and take the most restrictive covenant across packages.

employees comes directly from the LBD. At the firm level, the number of employees is summed across all of the firm's establishments. We consider additional employment measures for robustness and also to better understand the channels through which firms adjust resource allocation and potentially achieve cost improvements (i.e., reducing labor costs through the number of employees or wages per employee). We use four such measures based on data from the LBD. The first measure is the annual change in the natural logarithm of payroll. The second measure is the symmetric growth rate of employment, calculated by dividing the annual change in number of employees by the average of current and lagged number of employees. This measure accommodates both entry and exit and limits the effects of extreme values (Davis et al., 1998). For the third and fourth measures, we use the change in the number of employees and in payroll scaled by the average of current and lagged book value of assets, respectively.

Alongside employment, we also analyze establishment closure rates. Such closures represent an extreme form of resource withdrawal that may be less likely to occur absent outside pressure (Bertrand and Mullainathan, 2003). We use longitudinal identifiers from LBD to define for each establishment in year t a closure indicator variable that is set equal to one if the establishment is closed down in year $t+1$. This is a dependent variable in the establishment-level analysis. For the firm-level analysis, we use an indicator variable set equal to one if the firm closes any of its establishments in a given year.

In some tests we analyze the investment decisions of manufacturing firms based on data from the CMF and ASM. We calculate investment as the annual change in establishment-level capital expenditures scaled by the establishment-level capital stock. Establishment-level capital stock is estimated using the perpetual inventory method following Brav et al. (2015).

Our main independent variable is an indicator set equal to one if a firm violates a covenant in the current year. These violations are considered material information and must be disclosed in SEC filings. We aggregate the quarterly violation data to the annual frequency of the Census data. In light of this data constraint, we take a conservative approach when we measure the occurrence of a violation. To code a firm-year as a violation, we require a violation in at least one quarter of the current year and nonmissing covenant information without any violation in all four quarters of the previous year. Effectively, we focus on new covenant violations—those occurring in the current but not the previous year—which is a cleaner setting to observe the effects of creditor influence.

To complement our main approach, we also measure covenant violations based on at-origination loan contract terms (i.e., maintenance covenant thresholds) from the Dealscan data set. We focus on current ratio and net worth covenants due to their ubiquity and standardization. A covenant violation occurs in a given firm-year when the realized current or net worth ratio falls below the threshold specified by either covenant. As an additional robustness test, we restrict the sample to firm-year observations close to the threshold and conduct a regression discontinuity design (RDD) in the spirit of Chava and Roberts (2008). We discuss the identification assumptions underlying this test in the next section.

We include in our regressions firm-level accounting ratios on which covenants are written as well as variables to account for systematic differences between violator and nonviolator firms that could affect decision-making. We control for operating cash flow, leverage ratio, interest expense scaled by average assets, net worth over total assets, current ratio, and market-to-book ratio. These variables are winsorized at the 1% and 99% levels to limit the effects of outliers. In the establishment-level analysis, we further control for establishment age, the number of establishments per firm, and the number of establishments per three-digit industry segment of the parent firm. Appendix A defines all variables precisely.

With our data restrictions in place, particularly the Compustat-SSEL link, we construct a final sample containing 21,000 firm-year observations covering approximately 2,000,000 establishment-years for the period from 1996 until 2009. Table 1 presents summary statistics for the full sample as well as the subsamples of covenant violators and nonviolators.[12] The firm-level summary statistics are similar to Nini et al. (2012), reassuring us that sample selection resulting from the Compustat-Census match is not a problem. This is not surprising given the administrative nature of the Census data (i.e., it should cover the universe of Compustat firms). New covenant violations occur in 6.3% of firm-year observations.

Comparing violators with nonviolators, note that the change in employment is larger for violators both at the firm and establishment levels. In addition, establishments belonging to violating firms experience closures with greater frequency. There appears to be significant performance differences between violators and nonviolators: violators have lower net worth, current ratio, and market-to-book ratio; hold less cash; and are more levered. To ensure that our results do not simply reflect differences in these characteristics, it is crucial that we control flexibly for them in our regression framework. We also conduct several falsification and sensitivity tests to ensure that our results do not reflect systematic differences (or trends) between violators and nonviolators.

Finally, it is worth noting the differences between the LBD establishments (Panel B) and the subsample of manufacturing establishments from the CMF and ASM (Panel C). The rate of covenant violations is about the same for manufacturing (0.040) compared with all other establishments (0.041). Where manufacturing firms differ is that they tend to own fewer and older establishments. We control for these differences throughout our establishment-level analysis, including tests that focus specifically on manufacturing firms.

2.3. Identification and empirical model

We measure the firm-level effects of covenant violations for resource allocation following the literature (e.g.,

[12] As per Census disclosure requirements, we round off the number of observations in each table and quantile values are not reported in any summary statistics table.

Table 1
Summary statistics.
This table provides sample summary statistics. Panel A provides firm-level statistics. Panels B and C provide establishment-level statistics. The unit of observation in Panel A and Panels B and C, respectively, is a firm-year and establishment-year. All variables are defined in Appendix A.

	Full sample			Nonviolators			Violators		
	N [1]	Mean [2]	Std. [3]	N [4]	Mean [5]	Std. [6]	N [7]	Mean [8]	Std. [9]
Panel A: Firm level									
ΔLog(Employment)	21,000	−0.061	0.401	19,000	−0.002	0.399	2,000	−0.062	0.424
ΔLog(Payroll)	21,000	0.000	0.410	19,000	0.004	0.408	2,000	−0.047	0.431
Symmetric employment growth	21,000	0.018	0.308	19,000	0.018	0.306	2,000	0.029	0.334
ΔEmployees/Average assets	21,000	9.453	47.376	19,000	9.322	48.448	2,000	11.392	26.895
ΔPayroll/Average assets	21,000	0.349	2.697	19,000	0.347	2.776	2,000	0.388	0.966
Establishment closure	21,000	0.472	0.499	19,000	0.471	0.499	2,000	0.486	0.500
Covenant violation	21,000	0.063	0.244	19,000	0	0	2,000	1	0
Operating cash flow	21,000	0.075	0.246	19,000	0.077	0.250	2,000	0.050	0.174
Leverage	21,000	0.256	0.456	19,000	0.252	0.466	2,000	0.315	0.280
Interest expense	21,000	0.023	0.074	19,000	0.023	0.076	2,000	0.028	0.035
Net worth	21,000	0.432	0.967	19,000	0.435	0.995	2,000	0.393	0.371
Current ratio	21,000	2.772	4.615	19,000	2.821	4.744	2,000	2.048	1.724
Market-to-book	21,000	2.029	3.170	19,000	2.063	3.255	2,000	1.533	1.305
Panel B: Establishment level (LBD)									
ΔLog(Employment)	2,000,000	−0.138	0.664	1,900,000	−0.133	0.655	100,000	−0.251	0.832
Establishment closure	2,000,000	0.054	0.227	1,900,000	0.053	0.224	100,000	0.087	0.282
Covenant violation	2,000,000	0.041	0.197	1,900,000	0	0	100,000	1	0
Age	2,000,000	13.021	8.811	1,900,000	13.065	8.819	100,000	11.973	8.552
Establishments per firm	21,000	93.710	356.328	20,000	93.872	357	1,000	90	347
Establishments per segment	93,000	22.003	154.284	90,000	21.913	154	3,000	24.377	162
Core	2,000,000	0.764	0.424	1,900,000	0.761	0.427	100,000	0.841	0.365
Labor productivity	2,000,000	0.051	6.968	1,900,000	0.052	7.114	100,000	0.029	0.050
Panel C: Establishment level (CMF/ASM)									
ΔLog(Employment)	50,000	−0.193	0.814	48,000	−0.186	0.795	2,000	−0.378	1.158
ΔInvestment rate	50,000	−0.008	0.158	48,000	−0.007	0.157	2,000	−0.025	0.161
Establishment closure	50,000	0.035	0.185	48,000	0.034	0.18	2,000	0.077	0.267
Covenant violation	50,000	0.040	0.197	48,000	0	0	2,000	1	0
Age	50,000	20.973	9.127	48,000	21.034	9.122	2,000	19.527	9.116
Establishments per firm	8,000	7.427	14.091	7,000	7.654	14.412	1,000	4.337	8
Establishments per segment	21,000	2.959	4.675	20,000	2.985	4.700	1,000	2.436	4.105
Core	50,000	0.653	0.476	48,000	0.647	0.478	2,000	0.808	0.411
Total factor productivity	50,000	1.823	0.658	48,000	1.826	0.66	2,000	1.765	0.609
Labor productivity (Alt. 1)	50,000	114.415	288.128	48,000	116.309	293.188	2,000	69.333	104.312
Labor productivity (Alt. 2)	50,000	233.327	919.057	48,000	235.547	924.285	2,000	180.473	782.704
Labor productivity (Alt. 3)	50,000	0.019	0.031	48,000	0.020	0.032	2,000	0.018	0.016
Return on capital	50,000	5.920	604.419	48,000	6.110	617.968	2,000	1.714	4.135
Operating risk	50,000	2.428	15.417	48,000	2.422	15.612	2,000	2.569	9.349
Operating risk (Alt. 1)	50,000	15.161	67.914	48,000	15.372	68.896	2,000	10.141	40.702
Operating risk (Alt. 2)	50,000	0.014	0.012	48,000	0.014	0.012	2,000	0.016	0.013
Operating risk (Alt. 3)	50,000	0.017	0.011	48,000	0.017	0.011	2,000	0.018	0.012
Operating risk (Alt. 4)	50,000	0.016	0.011	48,000	0.016	0.011	2,000	0.017	0.014
Operating risk (Alt. 5)	50,000	25.904	169.89	48,000	26.180	171.685	2,000	19.801	134.152

Roberts and Sufi, 2009; Nini et al., 2012):

$$\Delta y_{i,t+1} = \beta\ Covenant\ violation_{it} + \alpha_t + \alpha_k + \theta' \mathbf{X}_{it} + \epsilon_{it}, \quad (1)$$

where i indexes firms, t indexes years, and k indexes industries. The unit of observation is a firm-year. The dependent variable, $\Delta y_{i,t+1}$, captures resource allocation with either the within-firm annual change in the natural logarithm of the number of employees or in the establishment closure rate.[13] The main independent variable, *Covenant Violation*$_{it}$, is an indicator variable equal to one for a new covenant violation. The α_t and α_k denote year and industry (based on three-digit SIC codes) fixed effects, respectively. The industry fixed effects control for time-invariant differences between industries, and the year fixed effects control for aggregate economic shocks.[14] The error term, ϵ_{it}, is assumed to be correlated within-firm and potentially heteroskedastic (Petersen, 2009).

The variable labeled $\mathbf{X}_{it}$ contains a list of firm-level controls that account for common ratios on which covenants

[13] Census employment variables are measured as of March 12 each year. For this reason, if a violation occurs in the first or second (third or fourth) quarters of year t, we measure the annual change in employment from year t to $t+1$ ($t+1$ to $t+2$).

[14] Panel A of Appendix IA.I augments the regression with industry-by-state-by-year fixed effects and obtains similar results. To ensure direct comparability with firm-level analysis in the prior literature, we use Eq. (1) as our baseline model.

are written (Roberts and Sufi, 2009) as well as factors that may have an independent effect on employment and, more broadly, resource allocation decisions (e.g., Nickell and Wadhwani, 1991). These include operating cash flow, leverage ratio, interest expense scaled by average assets, net worth over total assets, current ratio, and market-to-book ratio. These accounting variables are included linearly, squared, and cubed, as indicated by the higher-order firm controls term, as well as their one-year lag.

The coefficient of interest, β, measures how a firm's resource allocation decisions respond to a new covenant violation, as compared with observationally similar firms that do not violate covenants. If firms reduce employment or shutter establishments after control rights shift to creditors, then β will be negative. The null hypothesis that covenant violations are irrelevant conditional on firm performance (because firms can find substitute financing or creditors cannot exert influence), which corresponds to a β equal to zero.

The key innovation of this paper is to examine establishment-level data to better understand the mechanisms through which the transfer of control rights might affect operating performance. While firms' establishments differ across several important dimensions, we focus primarily on two characteristics that have been emphasized by the literature on manager-shareholder agency problems and resource misallocation within conglomerates (e.g., Stein, 2003; Maksimovic and Phillips, 2008): establishment productivity and whether it operates in a core or peripheral industry of a firm. We also examine the role of establishment-level operating risk in determining the resource allocation decision. This analysis is based on the full sample of establishments covering all industries based on the LBD and the subsample of manufacturers based on the CMF and ASM. In the latter sample, we have detailed establishment data on investment, performance, and operating risk.

To examine the effect of violations on resource allocation across establishments within the same firm, for example, according to core peripheral status, we modify Eq. (1) in the spirit of Giroud and Mueller (2015):

$$\begin{aligned}\Delta y_{ij,t+1} = {} & \beta_1\ \textit{Covenant violation}_{it} \times \textit{Core}_{jt} \\ & + \beta_2\ \textit{Covenant violation}_{it} \times \textit{Peripheral}_{jt} \\ & + \beta_3\ \textit{Peripheral}_{jt} + \alpha_i + \alpha_{k(j)} \\ & \times \alpha_{s(j)} \times \alpha_t + \theta' \mathbf{X}_{ijt} + \epsilon_{ijt}, \end{aligned} \tag{2}$$

where i, j, t, $k(j)$, and $s(j)$ index for firms, establishments, years, and the industries and states (of establishment j), respectively. The unit of observation is an establishment-year. The dependent variable, $\Delta y_{ij,t+1}$, is the within-establishment annual change in resource allocation. Depending on the data source, this could be employment, investment, or establishment closures. The main independent variable, $\textit{Covenant violation}_{it}$, is an indicator variable equal to one if an establishment's owner firm violates a covenant. The indicator variable $\textit{Core}_{jt}$ ($\textit{Peripheral}_{jt}$) is set equal to one if the establishment belongs (does not belong) to a core industry of its firm at the beginning of year t.

We thus sort every establishment within a given firm at the beginning of the year, in this example, according to whether it operates in a core industry ($\textit{Core}_{jt} = 1$ and $\textit{Peripheral}_{jt} = 0$) or a peripheral industry of the firm ($\textit{Core}_{jt} = 0$ and $\textit{Peripheral}_{jt} = 1$). The *Peripheral* direct effect (β_3) allows for potential differences in resource utilization at peripheral nonviolator establishments. $\mathbf{X}_{ijt}$ now also contains a set of establishment-level controls, including establishment age, size, the number of establishments per firm, and the number of establishments per segment.[15] The α_i and $\alpha_{k(j)} \times \alpha_{s(j)} \times \alpha_t$ denote firm and industry-by-state-by-year (of the establishment) fixed effects, respectively.[16] We continue to cluster standard errors at the firm level to account for dependence across establishments of the same firm.

The coefficients of interest are β_1, which captures the average incremental effect of the covenant violation on resource utilization at the establishments with the attribute of interest, and β_2, which captures the effect on other establishments within the same firm. These estimates are benchmarked off the corresponding average among all establishments satisfying the attribute of interest at firms not violating covenants.[17] On the one hand, if firms violating covenants withdraw resources uniformly across establishments, then the coefficients β_1 and β_2 will both be negative and statistically indistinguishable from each other. On the other hand, if β_2 is larger in magnitude than β_1 (e.g., more negative in the case of employment cutbacks), then the cuts occur to a greater extent at establishments not satisfying the criterion (e.g., outside of the core industry focus of the firm). The null hypothesis is that covenant violations are irrelevant for establishment-level resource allocation decisions, conditional on firm performance, which corresponds to both β_1 and β_2 equal to zero.

The main identification challenge in the estimation of the βs is to separate out the effect of violations from expected changes in resource allocation based on differences in financial performance and other fundamentals between violators and nonviolators. Our approach addresses this challenge through a comparison of firms close to the covenant threshold by controlling flexibly for continuous functions of the underlying variables—on which covenants thresholds are contracted upon—and using the discontinuous change in firm behavior occurring at the time of a violation (Roberts and Sufi, 2009; Nini et al., 2012). In effect, the outcomes of violations are measured by comparing firms with similar previolation performance and thus a similar expected time-series path of outcomes. Specifically, we take the within-firm annual difference in dependent

[15] Appendix IA.II compares establishment *Age* and *Size* across subsamples and finds that, for example, core establishments tend to be larger than peripheral establishments. We therefore control for differences in both establishment size and age throughout our regression analysis.

[16] Panel B of Appendix IA.I augments the regression with establishment fixed effects and obtains similar results.

[17] For example, in Eq. (2) the core establishments of nonviolator firms are the omitted group. As a result, β_1 (β_2) is the incremental effect of a covenant violation on resource utilization among the core (peripheral) establishments, and β_3 captures resource utilization among peripheral nonviolator establishments.

variables, which sweeps out fixed differences in outcomes between violators and nonviolators. This approach is refined further in the establishment-level regressions, which use within-establishment annual differences. We also flexibly control for contemporaneous and lagged firm-level control variables known to affect outcomes, as described above, and thus control for previolation trend differences between violators and nonviolators.[18]

We complement our baseline approach with a standard RDD that incorporates the actual contractual level of covenants (Chava and Roberts, 2008). The RDD essentially compares firms that just violate covenants to those that closely avoid doing so. We focus on the net worth and current ratio thresholds and define a firm-year to be in violation if the observed accounting ratio falls below the threshold specified by the contract. Thus, the covenant violation is a discontinuous function of the distance between the accounting ratio and the threshold, which constitutes the basis of the RDD approach.[19]

We use this alternative definition of a violation in two sets of robustness tests. The first simply uses it as a substitute independent variable in Eq. (1). The second restricts the sample to firm-year observations within a reasonably tight window, say, ± 10%, around the threshold. In this case, a violation can plausibly be considered as good as random. The RDD approach offers two advantages. First, it allows us to tackle borrower selection into loan contracts and covenant thresholds at origination. In particular, the analysis is conditional on firms receiving similar covenants in their contracts. We can also control for the distance to covenant thresholds at origination (e.g., covenant strictness; see Murfin, 2012) and at the time of technical default. Doing so mitigates the concern that covenant strictness conveys information about investment opportunities. Second, using a narrow bandwidth around the threshold ensures the violation is close to random and thus is unlikely to correlate with firm characteristics (Bakke and Whited, 2012).[20]

[18] We also incorporate a difference-in-differences matching estimator that controls nonparametrically for differences among violator and nonviolator firms. In Section 3.3, we show explicitly that our matching procedure eliminates systematic differences in performance metrics both at the time of the violation and pretrends.

[19] The RDD uses "locally" exogenous variation in violations arising from the distance to the threshold. The main identification assumption is local continuity, which amounts to continuity of all factors besides the violation through the covenant threshold. This requires that firms cannot perfectly sort themselves on one side of the threshold (Lee and Lemieux, 2010). In our context, this would require that firms manipulate accounting ratios to avoid violations, an outcome mitigated by the institutional features of the US loan market (Chava and Roberts, 2008). In Section 3.3, we verify the internal validity of our RDD estimates via formal tests of covariate balance and the continuity of the density of the running variable around the covenant threshold (McCrary, 2008).

[20] The disadvantage of this approach is that the restricted RDD sample size renders much of our analysis infeasible, and we therefore choose specification (1) as our baseline model. While our baseline approach does not incorporate explicit covenant thresholds, we can proxy for the unobserved thresholds by including lags of the firm controls. In support of this approximation, (Chava and Roberts, 2008) show that covenant violations tend to occur two years after origination, on average.

3. Empirical results

We first confirm that new covenant violations have important firm-level effects on resource allocation.[21] Table 2 shows results based on the estimating several versions of Eq. (1). Starting with employment, we see that the coefficients of interest on *Covenant violation,* β, are negative (ranging between –0.040 and –0.063) and is statistically significant at the 1% confidence level (columns 1–4). This indicates that covenant violations induce firms to lay off employees. More precisely, the size of the point estimate implies that a typical covenant violation is associated with between a 4 and 6.3 percentage point decrease in the number of employees, which constitutes about 10 to 15.7% of its standard deviation (0.401) among the full sample of firms.[22] Covenant violations also lead firms to withdraw resources on a larger scale through establishment closures. Column 5 shows a point estimate is 0.024, significant at the 5% level, which indicates a violating firm is 2.4 percentage points more likely to close an establishment than a nonviolator. This estimated effect is moderate given the coarse measurement of closures at the firm level: about 50% of all firms close an establishment in a given year.[23] We shall see our estimates become sharper and more economically meaningful in our establishment-level analysis.

3.1. Within-firm effects of debt covenant violatons

3.1.1. Establishments operating in core and peripheral business lines

From this point on, our empirical analysis moves beyond this aggregated evidence and conducts an establishment-level analysis to dissect the effects of creditor control on resource allocation within the boundaries of the firm. We first test for a heterogeneous response among establishments operating in core and peripheral business lines. Since peripheral business lines are outside the main scope of the firm, these activities may be less developed and could arise from managers' private incentives, or management may lack experience relative to core business lines (e.g., Gompers, 1996; Scharfstein and Stein, 2000). Therefore, withdrawing resources from these establishments and refocusing may improve operating efficiency and decrease the risk of failure, thus improving firm performance and value (e.g., Lang and Stulz, 1994; Schoar, 2002). On the other hand, diversification from an operational standpoint could increase the value of debt—provided cash flows are not perfectly correlated (i.e., a "coinsurance" effect, as in Lewellen, 1971)—in which case we might see no change in focus.

[21] While our measurement—notably of employment and establishment closures—offers some advantages, these firm-level results echo prior literature (e.g., Falato and Liang, 2016) and therefore serve as a "sanity check" to confirm that firms react to covenant violations within the Census sample.

[22] Appendix IA.III shows the robustness of this result to alternative measures of employment and indicates that violating firms reduce labor costs by cutting both the number of employees and wages per employee.

[23] In unreported results, we find a positive and statistically significant relation between covenant violations and the percentage of establishments closed in the subsequent year.

Table 2
Covenant violations and firm-level resource allocation.

This table shows estimates of the firm-level impact of debt covenant violations on asset allocation. The unit of observation in each regression is a firm-year pair. The dependent variable is the annual change in the natural logarithm of the number of employees aggregated across establishments (columns 1–4) and an indicator for whether the firm closed any establishment in a year (column 5). A covenant violation occurs when a firm reports a covenant violation in a SEC 10-K or 10-Q filing in the current but not in the previous year. Firm controls include operating cash flow scaled by average assets, leverage, interest expense, net worth, current ratio, and market-to-book ratio. Higher-order and lagged firm controls refer to the second and third power and one-year lag of the firm-level controls, respectively. All variables are defined in Appendix A. Industry fixed effects are based on three-digit SIC codes. Standard errors (in parentheses) are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:		*ΔLog(Employment)*			*Est. closure*
	[1]	[2]	[3]	[4]	[5]
Covenant violation	−0.063***	−0.042***	−0.042***	−0.040***	0.024**
	(0.007)	(0.008)	(0.009)	(0.009)	(0.012)
Operating cash flow		0.013***	0.061**	0.119***	0.143***
		(0.013)	(0.028)	(0.036)	(0.036)
Leverage		0.048**	−0.063*	−0.095	−0.157
		(0.020)	(0.032)	(0.078)	(0.126)
Interest expense		−0.085	−0.372	0.332	4.033***
		(0.182)	(0.257)	(0.848)	(1.268)
Net worth		0.073***	0.032	0.050	0.007
		(0.014)	(0.026)	(0.032)	(0.043)
Current ratio		0.001	−0.007***	0.000	−0.016
		(0.001)	(0.002)	(0.006)	(0.011)
Market-to-book		0.019***	0.022***	0.061***	−0.038**
		(0.001)	(0.002)	(0.010)	(0.016)
Lagged firm controls	N	N	Y	Y	Y
Higher-order firm controls	N	N	N	Y	Y
Industry fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
Rounded *N*	30,000	26,000	21,000	21,000	21,000
R^2	0.02	0.12	0.11	0.11	0.32

To test for the importance of industry focus in resource allocation, we turn to the establishment-level data from LBD. We follow Maksimovic and Phillips (2002) and, for each firm, classify a three-digit SIC industry as core (peripheral) if its payroll summed across establishments is more (less) than 25% of the firm's total payroll. Each establishment within the firm is characterized as core or peripheral on a year-to-year basis based on its industry classification. We then estimate our establishment-level regression model (2), allowing for differential sensitivities among establishments operating in the firm's core or peripheral business lines following a new covenant violation. The estimated coefficients on *Covenant violation* × *Core* and *Covenant violation* × *Peripheral* measure these heterogeneous responses. Table 3 shows the results.

In columns 1–4 the dependent variable is the establishment-level change in the natural logarithm of the number of employees. In column 1, we perform the estimation without any firm controls and find that covenant violations result in a decrease in employment of 2.8 percentage points in core establishments and 8.1 percentage points in peripheral establishments. Recall that these estimates are measured with respect to changes among the corresponding establishment types of nonviolator firms. Both point estimates are significant at conventional levels. In column 2, we add firm controls, and the coefficients of interest are estimated to be −0.026 and −0.089, still statistically significant at conventional levels. Columns 3 and 4 include further controls, but the finding does not change: firms decrease employment significantly at both core and peripheral establishments, but the effect is about twice as large at peripheral establishments.[24,25]

We further examine the robustness of these results to our classifications of core and peripheral industries. We conduct two tests. First, in column 5, we use finer information on establishment industry codes to classify industries. In particular, we focus on four-digit SIC codes and maintain the 25% threshold (e.g., Giroud and Mueller, 2015). In column 6, we maintain the use of three-digit SIC codes but now adopt a 50% payroll threshold to classify industries within a firm as core or peripheral. For both sets of tests, we find very similar results, thus indicating that this finding is not an artifact of our industry classification scheme.

Finally, column 7 reports results from regressions where the dependent variable is an indicator variable for establishment closure. In this case, the dependent variable is equal to one if the establishment is closed in the subsequent year and is zero otherwise. Here, a similar pattern emerges: the coefficients of interest are significantly positive for both types of establishment, but the coefficient for peripheral establishments is much larger (0.013 versus 0.031). Once again, this difference is statistically significant at the 1% level based on an *F*-test.

[24] The estimated direct effect of peripheral establishment status—β_3 in Eq. (2)—ranges between −0.004 and −0.015 in columns 1–4 and is statistically insignificant at conventional levels.

[25] We test whether these coefficients are statistically distinct using *F*-tests. In each case, we find the difference between coefficients is significantly different from zero at the 1% confidence level.

Table 3
Within-firm resource allocation by establishment industry focus.
This table presents estimates of the within-firm impact of debt covenant violations on resource allocation among establishments within the core and peripheral industry focus of the firm. The unit of observation in each regression is an establishment-year pair. The dependent variables in columns 1–6 and 7 are the annual change in the (log) number of employees and a dummy variable indicating whether an establishment is closed or not, respectively. Core (peripheral) establishments are establishments operating in three-digit SIC industries that account for more than (less than) 25% of the firm's total employment expenditures. Column 5 instead considers four-digit SIC industries, and column 6 instead uses a 50% cutoff. A covenant violation occurs when a firms reports a covenant violation in a SEC 10-K or 10-Q filing in the current but not in the previous year. Establishment controls include age, the number of establishments, and the number of establishments per segment. Firm controls are described in Table 2. Industry fixed effects are based on establishments' three-digit SIC codes. As detailed in Eq. (2), each regression includes direct effects (point estimates not shown). All variables are defined in Appendix A. Standard errors (in parentheses) are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:				ΔLog(Employment)			Est. closure
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Covenant violation × *Core*	−0.028*	−0.026*	−0.049***	−0.050***	−0.051***	−0.051***	0.013***
	(0.015)	(0.016)	(0.018)	(0.019)	(0.019)	(0.019)	(0.008)
Covenant violation × *Peripheral*	−0.081**	−0.089**	−0.099**	−0.093**	−0.085***	−0.084**	0.031***
	(0.037)	(0.036)	(0.040)	(0.037)	(0.033)	(0.034)	(0.013)
Establishment controls	Y	Y	Y	Y	Y	Y	Y
Firm controls	N	Y	Y	Y	Y	Y	Y
Lagged firm controls	N	N	Y	Y	Y	Y	Y
Higher-order firm controls	N	N	N	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y
Industry × state × year fixed effects	Y	Y	Y	Y	Y	Y	Y
Rounded *N*	3,000,000	2,500,000	2,000,000	2,000,000	2,000,000	2,000,000	2,000,000
R^2	0.13	0.14	0.15	0.15	0.15	0.15	0.06

Overall, these establishment-level results indicate a large withdrawal of resources from violating firms' operations, particularly establishments operating in peripheral industries. Specifically, following covenant violations, firms decrease employment more at their continuing peripheral establishments and, along the extensive margin, close them significantly more often.

3.1.2. Establishment productivity

We next analyze the effects of covenant violations on within-firm resource allocation across productive and unproductive establishments. Managers may prefer to delay cutbacks at underperforming units at the expense of shareholder value (Bertrand and Mullainathan, 2003), or bargaining between headquarters and lower management might result in a misallocation of resources across units (Scharfstein and Stein, 2000). If operating performance improves due to heightened creditor influence, then, naturally, we expect managers to withdraw resources from less productive establishments.

We focus primarily on the subsample of manufacturers using the CMF and ASM. These data provide detailed information on manufacturing establishments, including output and factor inputs, allowing us to construct an array of productivity measures. We can measure total, labor, and capital productivity several ways both parametrically and nonparametrically, which gives us confidence that measurement error is not driving our results. We first use total factor productivity (TFP) to estimate establishment productivity. We follow the literature to compute TFP using Census data (e.g., Foster et al., 2008). TFP is estimated as the difference between actual and predicted output, where the latter is estimated using a log-linear Cobb-Douglas production function with capital, labor, and materials as inputs.

We rank establishments on the basis of their within-firm productivity ranking—productive (unproductive) establishments fall above (below) the median of TFP of the establishments belonging to the same firm—and consider the within-industry ranking later in a robustness test.[26] Note that establishments are resorted every year. Given the richness of the manufacturing data, we examine effects of covenant violations on establishment-level investment, in addition to employment and closures. To implement our tests, we estimate Eq. (2) allowing high and low productivity establishments to display different incremental resource utilization effects after covenant violations.

Panel A of Table 4 shows the within-firm effects of productivity on employment and closures. In columns 1–8, the dependent variable is the annual change in the natural logarithm of the number of employees. Column 1 indicates that firms cut employment at both productive and unproductive establishments, although layoffs are considerably larger at unproductive establishments. The coefficients show a decrease in number of employees of 7.4 and 16.8 percentage points for productive and unproductive establishments, respectively, as compared with the corresponding establishment types of nonviolator firms. As we introduce firm controls, the estimated effect on productive establishments diminishes in size and statistical significance. In column 4, with the full set of controls, layoffs at productive establishments are indistinguishable from zero. In contrast, unproductive establishments experience employment cuts that are large and statistically significant at the 1% level. *F*-tests confirm that the difference in the estimates

[26] If industry production is heterogeneous in terms of capital, labor, and TFP, then within-firm productivity rankings might be misleading, especially for firms spread across several industries.

Table 4
Within-firm resource allocation by establishment productivity.
This table presents estimates of the within-firm impact of debt covenant violations on resource allocation among productive and unproductive establishments. The sample is restricted to manufacturing firms. The unit of observation in each regression is an establishment-year pair. Panels A and B show the effects for employment and establishment closures and investment, respectively. In Panel A, the dependent variable in columns 1–8 is the annual change in the (log) number of employees and in column 9 a dummy variable indicating whether the establishment is closed. In columns 1–4 and 9 each establishment is classified as productive or unproductive depending on its within-firm total factor productivity (TFP) ranking. An establishment is considered productive (unproductive) if its corresponding TFP rank is above (below) the median TFP of the establishments belonging to the firm in a given year. Column 5 uses the within-industry total factor productivity to rank establishments. Columns 6–8 use three measures of labor productivity: value added per labor hour, output divided by total labor hours, and wage per hour. In Panel B, the dependent variable the annual change in investment given by establishment-level capital expenditures over capital stock. In columns 1–4 each establishment is classified as productive or unproductive depending on its within-firm total factor productivity (TFP) ranking. An establishment is considered productive (unproductive) if its corresponding TFP rank is above (below) the median TFP of the establishments belonging to the firm in a given year. Column 5 uses the within-industry total factor productivity to rank establishments. Column 6 uses return on capital to measure capital productivity. A covenant violation occurs when a firm reports a covenant violation in a SEC 10-K or 10-Q filing in the current but not in the previous year. Establishment controls include age, the number of establishments, and the number of establishments per segment. Firm controls are described in Table 2. Industry fixed effects are based on establishments' three-digit SIC codes. As detailed in Eq. (2), each regression includes direct effects (point estimates not shown). All variables are defined in Appendix A. Standard errors (in parentheses) are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Employment and establishment closure Dependent variable:	*ΔLog(Employment)*								*Est. closure*
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Covenant violation × *Productive*	−0.074**	−0.067**	−0.066*	−0.060	−0.043	−0.022	−0.041	−0.008	0.012
	(0.030)	(0.033)	(0.037)	(0.039)	(0.041)	(0.040)	(0.041)	(0.045)	(0.008)
Covenant violation × *Unproductive*	−0.168***	−0.167***	−0.114**	−0.107**	−0.120***	−0.131***	−0.118***	−0.144***	0.023***
	(0.037)	(0.040)	(0.045)	(0.047)	(0.045)	(0.044)	(0.045)	(0.050)	(0.008)
Establishment controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm controls	N	Y	Y	Y	Y	Y	Y	Y	Y
Lagged firm controls	N	N	Y	Y	Y	Y	Y	Y	Y
Higher-order firm controls	N	N	N	Y	Y	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry × state × year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Rounded *N*	80,000	65,000	50,000	50,000	50,000	50,000	50,000	50,000	60,000
R^2	0.30	0.32	0.34	0.34	0.34	0.34	0.34	0.34	0.32

Panel B: Investment Dependent variable: *ΔInvestment rate*						
	[1]	[2]	[3]	[4]	[5]	[6]
Covenant violation × *Productive*	−0.005	0.005	0.005	0.003	0.005	−0.003
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Covenant violation × *Unproductive*	−0.025***	−0.019***	−0.021***	−0.022***	−0.020***	−0.014**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)
Establishment controls	Y	Y	Y	Y	Y	Y
Firm controls	N	Y	Y	Y	Y	Y
Lagged firm controls	N	N	Y	Y	Y	Y
Higher-order firm controls	N	N	N	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y	Y	Y
Industry × state × year fixed effects	Y	Y	Y	Y	Y	Y
Rounded *N*	70,000	60,000	50,000	50,000	50,000	50,000
R^2	0.25	0.26	0.26	0.26	0.26	0.26

between productive and unproductive establishments is always statistically significant at conventional levels.[27]

We next examine the robustness of employment outcomes to alternative measures of productive efficiency. In column 5, we consider a within-industry (three-digit SIC code) TFP ranking of establishments and find a similar result as compared to using the within-firm productivity ranking. The estimates indicate that following a violation firms decrease the number of employees at unproductive establishments by 12 percentage points, whereas the change in employment at productive establishments is statistically insignificant.

We consider three more refined measures of labor productivity commonly used in the literature (e.g., Brav et al., 2015). First, in column 6, we use value-added per labor hour, which is total value of shipments minus material and energy costs divided by total labor hours. Second, in column 7, we use output divided by total labor hours. Finally, in column 8, we use wage per hour. Each time, we use a within-industry productivity ranking to determine which establishments are relatively productive. It can be seen that following covenant violations, the withdrawal of labor resources occurs most strongly at establishments with low labor productivity. In contrast to the productive establishment interaction, the unproductive establishment interac-

[27] The estimated direct effect of unproductive establishment status—β_3 in Eq. (2)—ranges between −0.045 and −0.055 in columns 1–4 and is always statistically insignificant at the 1% confidence level.

tion is always negative, larger in magnitude, and statistically significant at the 1% confidence level.[28] Finally, in column 9, based on the within-firm TFP ranking, we examine establishment closures and find that, along the extensive margin, firms only close unproductive establishments.

In Panel B of Table 4 we uncover similarly striking patterns for investment. We consider the investment rate as a dependent variable, which we measure as the annual change in establishment-level capital expenditures scaled by the establishment-level capital stock. Following covenant violations, violating firms incrementally cut investment by between 1.9 and 2.5 percentage points at unproductive establishments, as compared with the unproductive establishments of nonviolators. In contrast, there is a virtually zero effect on the investment rate among the productive establishments of covenant violating firms. This pattern holds either for the within-firm TFP ranking (columns 1–4) or the within-industry TFP ranking (column 5).

In column 6 we proxy for capital productivity based on return on capital (ROC), which has the advantage of being a simple and nonparametric measure. ROC is calculated as total value of shipments minus labor, material, and energy costs scaled by capital stock. Very similar results emerge: compared with the investment rate of productive nonviolator establishments, the investment rate decreases by 0.014 among violating firms' establishments with below-median within-firm ROC (significant at the 5% level) and is indistinguishable from zero in the case of productive establishments.

We next analyze how establishment productivity and industry focus interact in the response of firms to covenant violations.[29] To this end, we modify Eq. (2) to include the interaction of these two establishment characteristics as follows:

$$\begin{aligned}\Delta y_{ij,t+1} = {} & \beta_1\ Covenant\ violation_{it} \times Core_{jt} \times Productive_{jt} \\ & + \beta_2\ Covenant\ violation_{it} \times Core_{jt} \times Unproductive_{jt} \\ & + \beta_3\ Covenant\ violation_{it} \times Peripheral_{jt} \times Productive_{jt} \\ & + \beta_4\ Covenant\ violation_{it} \times Peripheral_{jt} \times Unproductive_{jt} \\ & + \beta_5\ Core_{jt} \times Unprod._{jt} + \beta_6\ Peri._{jt} \\ & \times Prod._{jt} + \beta_7\ Peri._{jt} \times Unprod._{jt} \\ & + \alpha_i + \alpha_{k(j)} \times \alpha_{s(j)} \times \alpha_t + \theta' \mathbf{X}_{ijt} + \epsilon_{ijt}. \end{aligned} \quad (3)$$

The coefficients of interest (β_1 through β_4) capture the incremental changes in the resource utilization rate among the establishments due to a covenant violation. The lower-level terms (β_5, β_6, and β_7) account for potential differences in resource utilization rates across the various establishment types absent covenant violations. The omitted group in this regression is the set of *Core* × *Productive* establishments at nonviolator firms.

The results of estimating Eq. (3) are shown in Table 5. Two key results obtain. First, we observe that the cuts occurring at manufacturing establishments outside of the core focus of violating firms are in line with the estimates for all industries (see Table 3). Second, on the interaction between focus and productivity, we see that the cuts occur among unproductive establishments in both core and peripheral industries; however, they are far larger in magnitude at the peripheral establishments. For example, column 2 shows, among covenant violating firms, a 10 percentage point reduction in employment at *Core* × *Unproductive* establishments (significant at the 10% level), about half the size of the 24.4 percentage point cut at *Peripheral* × *Unproductive* establishments (significant at the 1% level). This finding is consistent with managers withdrawing resources primarily from less productive establishments, although the peripheral characteristic appears to play an important amplification role.

In summary, this evidence highlights the central importance of establishment productivity in firm decision-making following covenant violations. We find strong evidence that violating firms cut employment and investment at unproductive establishments and close them down more frequently.

3.1.3. Establishment operating risk

Next, we examine the importance of establishment operating risk for resource allocation decisions after the transfer of control rights to creditors. Risk-taking on the operational side might expose the firm to large potential losses. Management might undertake excessively risky investments due to a lack of information or skill. Alternatively, these operating decisions might be optimal from the perspective of shareholders who reap the gains on the upside but at the expense of creditors who are exposed to the losses on the downside. Consequently, in the presence of shareholder-creditor conflicts of interest, creditors may prefer to shift resources away from projects that have high operating risk.

We construct industry-level measures of operating risk based on the variance of establishment outcomes. Following Maksimovic et al. (2011), our main measure of risk is the cross-sectional standard deviation of operating margins across manufacturing establishments in the same three-digit SIC code, where operating margins are calculated as the total value of shipments minus all input costs divided by the value of shipments. Operating margins can only be calculated using the CMF/ASM data, so we continue to focus on manufacturing establishments. We also wish to examine the interactions between operating risk and productivity, further necessitating the focus on manufacturers. For each three-digit SIC code and each year, we calculate operating risk and classify an establishment as *Risky* if it belongs to an industry with above-median standard deviation of operating margins and *Safe* otherwise.

Table 6 presents the results of estimating Eq. (2) under this risk-based classification of establishments.[30] In column 1, the dependent variable is the annual change in the natural logarithm of the number of employees. The

[28] Appendix IA.IV further analyzes the role of labor productivity using wage-based and value-added-based measures following Silva (2019) and finds similar patterns.

[29] Appendix IA.V confirms that productivity is not highly correlated with focus among establishments.

[30] Note that the (colinear) industry × state × year fixed effects subsume the direct effect of operating risk.

Table 5
Interaction between establishment industry focus and productivity.

This table presents estimates of how the within-firm impact of debt covenant violations on resource allocation among establishments within the core and peripheral industry focus of the firm interacts with establishment productivity. The sample is restricted to manufacturing firms. The unit of observation in each regression is an establishment-year pair. Core (peripheral) establishments are establishments operating in three-digit SIC industries that account for more than (less than) 25% of the firm's total employment expenditures. The dependent variables in columns 1–3 and 4–6 are the annual change in the (log) number of employees and a dummy variable indicating whether an establishment is closed or not, respectively. In columns 2 and 5 (3 and 6) each establishment is classified as productive or unproductive depending on its within-firm (within-three-digit SIC industry) total factor productivity (TFP) ranking. An establishment is considered productive if its corresponding TFP rank is above the median TFP of the establishments belonging to the firm (industry) in a given year and unproductive otherwise. A covenant violation occurs when a firms reports a covenant violation in a SEC 10-K or 10-Q filing in the current but not in the previous year. Establishment controls include age, the number of establishments, and the number of establishments per segment. Contemporaneous, lagged, and higher-order firm controls are included in every regression (see description in Table 2). Industry fixed effects are based on establishments' three-digit SIC codes. As detailed in Eq. (2) and (3), each regression includes direct effects and intermediate interaction terms (point estimates not shown). All variables are defined in Appendix A. Standard errors (in parentheses) are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	*ΔLog(Employment)*			*Establishment closure*		
	[1]	[2]	[3]	[4]	[5]	[6]
Covenant violation × *Core*	−0.058*			0.013**		
	(0.031)			(0.005)		
Covenant violation × *Peripheral*	−0.153***			0.031***		
	(0.043)			(0.011)		
Covenant violation × *Core* × *Productive*		−0.028	−0.014		0.006	0.009
		(0.048)	(0.050)		(0.009)	(0.007)
Covenant violation × *Core* × *Unproductive*		−0.100*	−0.112*		0.020**	0.020**
		(0.060)	(0.058)		(0.008)	(0.009)
Covenant violation × *Peripheral* × *Productive*		−0.057	0.018		0.026	0.033*
		(0.101)	(0.090)		(0.018)	(0.018)
Covenant violation × *Peripheral* × *Unproductive*		−0.244**	−0.294**		0.037**	0.033*
		(0.122)	(0.125)		(0.019)	(0.019)
Establishment controls	Y	Y	Y	Y	Y	Y
Firm controls	Y	Y	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y	Y	Y
Industry × state × year fixed effects	Y	Y	Y	Y	Y	Y
Rounded *N*	50,000	50,000	50,000	60,000	60,000	60,000
R^2	0.34	0.34	0.34	0.32	0.32	0.32

estimates indicate that layoffs are present only at risky establishments. The estimated coefficients show a decrease in number of employees of 15.4 percentage points for risky establishments (significant at the 1% level), whereas layoffs at safe establishments are indistinguishable from zero.[31] Columns 4 and 7 repeat this estimation for establishment closures and investment, respectively. In a consistent manner, we find a higher incidence of closures and large cuts in investment among risky establishments only. These findings collectively support the idea that creditor influence brings about a decline in operational risk-taking through the allocation of resources within firm boundaries following covenant violations.

In the remaining columns of the table, we characterize how establishment productivity and operating risk interact based on Eq. (3). We see very clearly that layoffs are concentrated among the establishments that are considered to be both unproductive and risky. For example, column 2 shows a 16 percentage point reduction in employment at *Unproductive* × *Risky* establishments (significant at the 1% level) and nowhere else. In this column we use our preferred measures of productivity and risk; however, this finding persists under the within-industry productivity ranking defined above (see column 3). This large and statistically robust effect holds for establishment closures and is particularly stark for investment. Thus, while riskier operations experience cuts, resources are withdrawn from unproductive units and therefore are likely to benefit both creditors and shareholders by both reducing default risk and improving productive efficiency.[32]

3.2. Exploring cross-sectional variation in within-firm effects

To strengthen a causal interpretation of our results and to shed further light on the underlying mechanism, in this section we analyze how the resource allocation effects of covenant violations vary within the cross-section of borrowers and lenders.

3.2.1. Heterogeneity among borrowers

We first consider borrowers' characteristics, in particular, manager-shareholder agency costs and financial strength. Given the role of covenants in mitigating such agency problems (Aghion and Bolton, 1992; Dewatripont and Tirole, 1994), we expect larger effects from creditors among poorly governed firms that have greater opportunity for managerial slack. In addition, the shift in control

[31] *F*-tests indicate that the difference between risky and safe establishments point estimates is statistically significant at at least the 5% confidence level.

[32] Appendices IA.VI, IA.VII, and IA.VIII confirm these results hold under alternative measures of operating risk.

Table 6
Importance of establishment operating risk for within-firm resource allocation.

This table presents estimates of the impact of debt covenant violations on within-firm resource allocation as a function of the operating risk. The sample is restricted to manufacturing firms. The unit of observation in each regression is an establishment-year pair. In columns 1–3 the dependent variable is the annual change in the (log) number of employees, in 4–6 it is a dummy variable indicating whether the establishment is closed, and in 7–9 it is the annual change in investment given by establishment-level capital expenditures over capital stock. Each establishment is classified as safe or risky depending on the cross-sectional standard deviation of operating margins across Census establishments in the same three-digit SIC code. Operating margins are calculated as the total value of shipments minus all input costs divided by the value of shipments made by the establishment. An establishment is considered safe (risky) if its corresponding industry standard deviation of operating margins is below (above) the median of all industries in a given year. In addition, each establishment is classified as productive or unproductive depending on its within-firm total factor productivity (TFP) ranking (within-industry in columns 3, 6, and 9). An establishment is considered productive if its corresponding TFP rank is above the median TFP of the establishments belonging to the firm (industry in columns [3], [6], and [9]) in a given year and unproductive otherwise. A covenant violation occurs when a firm reports a covenant violation in a SEC 10-K or 10-Q filing in the current but not in the previous year. Establishment controls include age, the number of establishments, and the number of establishments per segment. Contemporaneous, lagged, and higher-order firm controls are included in every regression (see description in Table 2). Industry fixed effects are based on establishments' three-digit SIC codes. As detailed in Eq. (2) and (3), each regression includes direct effects and intermediate interaction terms (point estimates not shown). All variables are defined in Appendix A. Standard errors (in parentheses) are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	*ΔLog(Employment)*			*Establishment closure*			*ΔInvestment rate*		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Covenant violation × *Safe*	−0.010			0.006			−0.004		
	(0.044)			(0.008)			(0.007)		
Covenant violation × *Risky*	−0.154***			0.031***			−0.014*		
	(0.033)			(0.006)			(0.008)		
Covenant violation × *Productive* × *Safe*		0.046	−0.016		−0.007	0.002		0.010	0.011
		(0.064)	(0.062)		(0.012)	(0.015)		(0.011)	(0.009)
Covenant violation × *Productive* × *Risky*		−0.088	−0.054		0.023**	0.015		0.000	0.002
		(0.059)	(0.046)		(0.009)	(0.010)		(0.009)	(0.010)
Covenant violation × *Unproductive* × *Safe*		−0.037	0.007		0.012	0.003		−0.011	−0.011
		(0.096)	(0.073)		(0.013)	(0.017)		(0.010)	(0.011)
Covenant violation × *Unproductive* × *Risky*		−0.160**	−0.165***		0.024**	0.030***		−0.027***	−0.023**
		(0.071)	(0.049)		(0.011)	(0.010)		(0.009)	(0.010)
Establishment controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry × state × year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Rounded *N*	80,000	50,000	50,000	100,000	60,000	60,000	50,000	50,000	50,000
R^2	0.30	0.34	0.34	0.27	0.32	0.32	0.26	0.26	0.26

should matter more when creditors are in a stronger bargaining position with respect to management. For example, firms lacking outside financing options might be more likely to make operational changes to satisfy creditors.

To explore the importance of manager-shareholder agency frictions, we employ industry-level measures of product market competition, based on the idea that managerial slack is more severe in industries that feature less discipline from competitors (e.g., Giroud and Mueller, 2010). We calculate product market competition using the Herfindahl Hirschman Index (HHI) at the four-digit SIC industry level, split industries at the median to classify establishments into competitive industry (Z=0) and concentrated industry (Z=1) groups, and repeat our establishment-level analyses based on Eq. (3). Consistent with covenant violations alleviating manager-shareholder agency costs, in columns 1–4 of Panel A of Table 7, we observe a shift in resources away from peripheral and unproductive establishments operating in concentrated industries. For example, the coefficient on *Covenant violation* × *Peripheral* × *Concentrated* (Z=1) in column [1] indicates that the average change in log employment at peripheral establishments operating in concentrated industries and belonging to firms violating covenants exhibit a 33.9 percentage point cutback as compared with the corresponding adjustment at *Peripheral* × *Concentrated* (Z=1) nonviolator establishments.

In columns 5–8, we proxy for the strength of borrower bargaining position using financial slack, as measured by the presence of a credit rating. We use long-term credit ratings issued by S&P and recorded in Compustat and sort firms each year according to whether they have a rating or not. The point estimates show our benchmark establishment-level results are only present among firms without a credit rating.

We next dig deeper into establishment-level variation within firms to further understand the importance of agency frictions. Motivated by the literature on internal capital markets and control rights, we conduct two tests more closely connected to managers' private benefits. First, as argued by Gertner et al. (1994), external control enhances monitoring incentives but may dull managers' incentives because they may not be able to see their new projects through to completion (due to a potential loss of control). The testable implication is that the covenant-induced shift in control reverses the CEO's recent projects or ideas. Second, (Aghion and Bolton, 1992)

Table 7
Importance of manager-shareholder agency frictions for within-firm effects.

This table presents estimates of how the within-firm impact of debt covenant violations on resource allocation among establishments with varying borrower and industry characteristics that proxy for manager-shareholder agency frictions. The unit of observation in each regression is an establishment-year pair. Panel A examines borrowers' industry competition and credit rating status. The sample is restricted to manufacturing firms. Core (peripheral) establishments are establishments operating in three-digit SIC industries that account for more than (less than) 25% of the firm's total employment expenditures. An establishment is considered productive if its within-firm total factor productivity (TFP) rank is above the median TFP of the establishments belonging to the firm in a given year and unproductive otherwise. An establishment operates in an uncompetitive industry if its industry Herfindahl-Hirschman Index (HHI) is above median. A borrower is unrated if it does not have an S&P Long-Term Domestic Issuer Credit Rating. Panel B examines establishment-level measures of managerial private benefits. Columns 1 and 2 consider establishments born or purchased during the tenure of the CEO that violated the covenant. Columns 3 and 4 classifies establishments depending on the distance to covenant violating CEO's home county. An establishment is close to home if the corresponding distance to CEO's home county is below (above) the median distance of the establishments belonging to the firm in a given year. The dependent variables, a covenant violation, and control variables are described in Table 3 and are defined in Appendix A. Contemporaneous, lagged, and higher-order firm controls are included in every regression. As detailed in Eq. (2) and (3), each regression includes direct effects and intermediate interaction terms (point estimates not shown). Standard errors (in parentheses) are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Firm-level agency frictions

Borrower classification (Z=1):	Operates in concentrated industry				Has no credit rating			
Dependent variable:	*ΔLog(Employment)*		*Establishment closure*		*ΔLog(Employment)*		*Establishment closure*	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Covenant violation × *Core* × (Z=0)	−0.043		0.014*		−0.052		0.011	
	(0.040)		(0.007)		(0.049)		(0.010)	
Covenant violation × *Core* × (Z=1)	−0.104*		0.012		−0.059		0.012	
	(0.061)		(0.013)		(0.038)		(0.008)	
Covenant violation × *Peripheral* × (Z=0)	−0.035		0.018		−0.079		0.026	
	(0.075)		(0.017)		(0.118)		(0.026)	
Covenant violation × *Peripheral* × (Z=1)	−0.339***		0.061**		−0.201**		0.045***	
	(0.131)		(0.024)		(0.091)			(0.016)
Covenant violation × *Productive* × (Z=0)		−0.004		0.014		−0.070		0.010
		(0.051)		(0.010)		(0.072)		(0.015)
Covenant violation × *Productive* × (Z=1)		−0.079		0.007		−0.031		0.014
		(0.076)		(0.016)		(0.061)		(0.010)
Covenant violation × *Unproductive* × (Z=0)		−0.071*		0.016*		−0.019		0.015
		(0.050)		(0.009)		(0.097)		(0.012)
Covenant violation × *Unproductive* × (Z=1)		−0.254***		0.039***		−0.172**		0.025**
		(0.077)		(0.014)		(0.071)		(0.011)
Establishment controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Industry × state × year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Rounded N	50,000	50,000	60,000	60,000	50,000	50,000	60,000	60,000
R^2	0.34	0.34	0.29	0.32	0.32	0.33	0.29	0.29

Panel B: Establishment-level measurement of managerial private benefits

Establishment classification (Z=1):	CEO's own project		Close to CEO's home	
Dependent variable:	*ΔLog(Emp.)*	*Est. closure*	*ΔLog(Emp.)*	*Est. closure*
	[1]	[2]	[3]	[4]
Covenant violation × (Z=0)	−0.026***	0.009***	−0.065***	0.029***
	(0.009)	(0.004)	(0.021)	(0.005)
Covenant violation × (Z=1)	−0.060***	0.020***	−0.054***	0.025***
	(0.014)	(0.007)	(0.018)	(0.005)
Establishment controls	Y	Y	Y	Y
Firm controls	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y
Industry × state × year fixed effects	Y	Y	Y	Y
Rounded N	1,500,000	1,500,000	600,000	600,000
R^2	0.22	0.19	0.19	0.15

argue that when control shifts to lenders, the CEO loses private benefits. The testable implication is therefore that covenant-induced resource withdrawals are concentrated among establishments that benefit the CEO or the CEO personally likes.

To operationalize these concepts, we rely on S&P's Execucomp database, which identifies the CEOs of firms in each year during the period from 1996 until 2009.[33] In our first test, we identify recent projects launched by the CEO. We exploit the fact that the LBD pinpoints

[33] Execucomp mostly covers firms in the S&P 1500. Appendix IA.IX shows there are limited differences in the accounting ratios between the subsamples analyzed in these tests and our main sample.

establishments born or purchased in each firm-year. Doing this allows us to classify a subset of establishment-years as recent projects that were launched by the CEO in charge at the time of the covenant violation.[34] In our second test, we consider "hometown" establishments—establishments located near a CEO's childhood home—that exhibit inefficient favoritism due to "place attachment" in terms of human resource allocation and establishment closures (Yonker, 2017). Hometown establishments are identified using birth county data for a subset of CEOs from Gennaro et al. (2016), which can easily be linked to each LBD establishment via ZIP code.[35] We calculate the distance between each establishment and the current CEO's hometown using the great-circle distance formula. Establishments that are close to home have a below-median proximity to hometown among the establishments belonging to the firm in a given year.

Panel B of Table 7 shows the results of estimating Eq. (2) based on these two classification schemes. In columns 1 and 2, we separate out those projects launched during the tenure of the CEO that was in charge when the covenant was violated. We see that the incremental effect of the covenant violation on resource withdrawals—employment cutbacks and establishment closures—is about twice as large for such establishments. Moreover, *F*-tests confirm that for both columns the differences in the estimates between CEO-own (Z=1) and non-CEO-own (Z=0) projects are statistically significant at conventional levels. Next, columns 3 and 4 indicate that establishments both close to and far from the CEO's home county experience an increase in closures and employment cutbacks. Thus, the CEO favoritism toward hometown establishments uncovered in Yonker (2017) is undone when control shifts to creditors. These results are therefore consistent with the state-contingent shift in control from managers to creditors leading to a reduction in investments that are more likely to be motivated by manager-shareholder agency conflicts.

3.2.2. Heterogeneity among lenders

Our final set of cross-sectional tests identify heterogeneity among lenders. Lenders with experience may use their knowledge and turnaround expertise to offer advice and to monitor operational improvements. Consistent with this idea, prior research has shown that some lenders specialize in extending credit to certain firms or markets (Boot, 2000; Paravisini et al., 2017), and this information advantage may confer benefits to the management of struggling borrowers.[36] Moreover, specialized lenders—particularly those with a significant market share—may value successful turnarounds due to reputation costs of default or to future lending and cross-selling opportunities (Bharath et al., 2007). We therefore test whether past lender industry experience and market share are associated with pronounced resource allocation outcomes around covenant violations.

For each firm-year in our sample, we identify the names of lead lenders on active loans from Dealscan.[37] If a firm-year has more than one lead arranger—due to multiple lead arrangers per loan or multiple loans with unique lead arrangers—then we assign the lead lender that arranges the most credit across all deals.[38] For each lender-year pair, we characterize lending behavior across industries and construct two lender experience measures. First, we classify lenders as having industry experience if the lead arranger has active credit extended to at least one other firm in the same industry in the current year. Second, we consider the industry market share of the lead lender by cumulating active credit extended by each lead as a fraction of total credit outstanding to the industry over the previous year. We classify lenders as having high market share if a given lead arranger has an above-median industry market share.[39]

In Table 8 we estimate our establishment-level Eq. (3) now interacting establishment characteristics with the lender experience variables. The coefficients on the peripheral and unproductive establishment interactions have the expected sign and are statistically significant at conventional levels. This holds for changes in employment and establishment closures as well as for both lender experience measures. Thus, only those firms in technical default whose main lender has industry experience exhibit allocation effects that are consistent with operational improvements.

In Table 9 we go a step further by analyzing how lender experience interacts with the role of covenant violations in alleviating manager-shareholder conflicts. For each measure of borrower agency frictions, we partition the set of firm-years according to whether the firm is borrowing from an experienced or inexperienced lender. In odd columns, we consider whether the lender has any experience in the borrower's industry and, in even columns, whether the lender has an above- or below-median market share of lending in the borrower's industry. For example, the coefficient on *Covenant violation* × *Has industry experience* × (Z=1) in column 1 measures the average change in log employment at establishments (i) operating in concentrated industries, (ii) borrowing from a lenders with any prior industry experience, and (iii) belonging to firms violating covenants. This measurement is benchmarked against the adjustment at the corresponding nonviolator establishment types (i.e., satisfying conditions (i) and (ii) only).

A striking pattern emerges from the table: following covenant violations, cutbacks occur at establishments prone to manager-shareholder agency problems predominantly when lenders have greater industry experience.

[34] Naturally, CEO's own projects tend to be younger than legacy establishments inherited from prior management (see Appendix IA.II). We therefore continue to control for establishment age in these regressions.

[35] Thanks to Vineet Bhagwat for providing the CEO birth county data.

[36] Acharya et al. (2012) and Bernstein and Sheen (2016) find that PE partners' past industry experience improves the performance of the portfolio company and the operating performance of PE-backed firms, respectively.

[37] Thanks to Michael Schwert for providing the Dealscan-Compustat link for lenders.

[38] Similar results obtain if we use the maximum experience in the case of multiple lead arrangers.

[39] Similar results obtain if we split lead lenders at the 75th percentile of market share.

Table 8
Importance of lender industry experience for within-firm effects.
This table presents estimates of how the within-firm impact of debt covenant violations on resource allocation among establishments with varying lead lender experience. The sample is restricted to manufacturing firms. The unit of observation in each regression is an establishment-year pair. We examine lenders' industry experience defined according to whether the borrower's lead lender lends to other firms in the same industry or if they have a significant (above-median) market share of lending to the borrower's industry or not. If a borrower has multiple lead lenders, then the lead bank arranging the most amount of credit in dollar terms is selected. In columns 1–3 and 4–6 the dependent variables are the annual change in the (log) number of employees and a dummy variable indicating whether the establishment is closed, respectively. Core (peripheral) establishments are establishments operating in three-digit SIC industries that account for more than (less than) 25% of the firm's total employment expenditures. An establishment is considered productive if its within-firm total factor productivity (TFP) rank is above the median TFP of the establishments belonging to the firm in a given year and unproductive otherwise. An establishment is considered safe (risky) if its industry standard deviation of operating margins is below (above) the median of all industries in a given year. A covenant violation occurs when a firm reports a covenant violation in a SEC 10-K or 10-Q filing in the current but not in the previous year. Establishment controls include age, the number of establishments, and the number of establishments per segment. Contemporaneous, lagged, and higher-order firm controls are included in every regression (see description in Table 2). Industry fixed effects are based on establishments' three-digit SIC codes. All variables are defined in Appendix A. As detailed in Eq. (3), each regression includes intermediate interaction terms (point estimates not shown). Standard errors (in parentheses) are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Lender characteristic (Z=1):	Has industry experience				Has high market share			
Dependent variable:	Δ*Log(Employment)*		*Establishment closure*		Δ*Log(Employment)*		*Establishment closure*	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Covenant violation × *Core* × (Z=0)	−0.023		−0.006		−0.002		−0.003	
	(0.074)		(0.020)		(0.116)		(0.024)	
Covenant violation × *Core* × (Z=1)	−0.074		0.010		−0.070		0.010	
	(0.044)		(0.010)		(0.043)		(0.009)	
Covenant violation × *Peripheral* × (Z=0)	0.050		−0.013		0.035		−0.023	
	(0.085)		(0.029)		(0.138)		(0.030)	
Covenant violation × *Peripheral* × (Z=1)	−0.156***		0.037***		−0.136***		0.032***	
	(0.037)		(0.012)		(0.050)		(0.009)	
Covenant violation × *Productive* × (Z=0)		0.041		−0.016		−0.037		0.003
		(0.117)		(0.027)		(0.121)		(0.032)
Covenant violation × *Productive* × (Z=1)		−0.067		0.013		−0.071*		0.015
		(0.048)		(0.011)		(0.042)		(0.009)
Covenant violation × *Unproductive* × (Z=0)		−0.029		−0.004		0.112		−0.023
		(0.062)		(0.017)		(0.100)		(0.026)
Covenant violation × *Unproductive* × (Z=1)		−0.161***		0.027**		−0.140***		0.024***
		(0.046)		(0.011)		(0.042)		(0.009)
Establishment controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Industry × state × year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Rounded N	40,000	40,000	40,000	40,000	40,000	40,000	40,000	40,000
R^2	0.33	0.35	0.31	0.31	0.33	0.35	0.34	0.31

Continuing with the example, column 1 shows a 13.4 percentage point drop in employment among establishments satisfying all three conditions above. The three remaining categories of establishments do not exhibit any differential behavior in response to covenant violations. Similar results emerge for unrated firms as well as establishments classified as CEO's own projects or close to the CEO's hometown.

This final set of results suggest that advice or enhanced monitoring by lenders with industry expertise is a channel for alleviating managerial agency costs and achieving operational improvements among firms in technical default. Furthermore, the fact that we only observe these changes in borrower behavior in industries in which lenders have expertise—and less so in other industries—helps to strengthen a causal interpretation of our results.[40]

3.3. Robustness checks

The firm- and establishment-level results survive a wide array of robustness tests reported in the Internet Appendix. First, we investigate the internal validity of our baseline results by checking for preexisting trends in employment, investment, and establishment closures between violators and nonviolators, conditional on our firm performance metrics. Specifically, Appendix IA.XII examines the difference in outcomes between violators and nonviolators in one or two years prior to the new covenant violation. We shift the violation forward by one or two years to a time, by construction, that we know there was no covenant violation. For both the one- and two-year placebos, the resulting point estimates of the impact of a covenant violation on all outcomes of interest are small in magnitude and are statistically indistinguishable

[40] To buttress this interpretation, we verify that lender industry experience is unlikely to proxy for other differences among lenders, including bank size or risk. First, Appendix IA.X matches lead lenders from Dealscan that are commercial banks to bank holding company regulatory filings and shows a lack of meaningful differences between high and low market share lenders in terms of size, leverage, credit performance, and liquidity risk. Second, Appendix IA.XI appends the regressions from Table 8 with lender size and lender fixed effects (Panel A) and lender-by-year fixed effects (Panel B) and obtains similar results.

Table 9
Interacting borrower manager-shareholder agency frictions and lender characteristics.

This table presents estimates of how the effect of debt covenant violations on establishment-level resource allocation depends jointly on manager-shareholder agency frictions and lender characteristics. The unit of observation in each regression is an establishment-year pair. Establishments are partitioned according to the measures of agency frictions described in Table 7. An establishment operates in a concentrated industry (Z=1) if its industry Herfindahl-Hirschman Index (HHI) is above median (Z=0 otherwise). A borrower is unrated if it does not have an S&P Long-Term Domestic Issuer Credit Rating. A CEO's own project is an establishment that was born or purchased during the tenure of the CEO that violated the covenant. An establishment is close to home if the corresponding distance to CEO's home county is below (above) the median distance of the establishments belonging to the firm in a given year. Lenders' industry experience is defined according to whether the borrower's lead lender lends to other firms in the same industry or if they have a significant (above-median) market share of lending to the borrower's industry or not. If a borrower has multiple lead lenders, then the lead bank arranging the most amount of credit in dollar terms is selected. The dependent variables, a covenant violation, and control variables are described in Table 3 and defined in Appendix A. Contemporaneous, lagged, and higher-order firm controls are included in every regression. As detailed in Eq. (3), each regression includes intermediate interaction terms (point estimates not shown). Standard errors (in parentheses) are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable: $\Delta Log(Employment)$								
Borrower classification (Z=1):	Concentrated industry		No credit rating		CEO's own project		Close to CEO's home	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Covenant violation × *No industry experience* × (Z=0)	0.029		−0.008		−0.023		−0.068	
	(0.127)		(0.087)		(0.048)		(0.052)	
Covenant violation × *No industry experience* × (Z=1)	−0.001		0.010		−0.023		−0.073	
	(0.084)		(0.092)		(0.058)		(0.054)	
Covenant violation × *Has industry experience* × (Z=0)	−0.002		−0.086		−0.021		−0.064***	
	(0.080)		(0.062)		(0.029)		(0.024)	
Covenant violation × *Has industry experience* × (Z=1)	−0.134***		−0.134**		−0.085*		−0.048***	
	(0.039)		(0.045)		(0.051)		(0.018)	
Covenant violation × *Low market share* × (Z=0)		0.116		0.045		0.002		0.053
		(0.218)		(0.090)		(0.050)		(0.051)
Covenant violation × *Low market share* × (Z=1)		0.003		−0.210		0.067		0.047
		(0.101)		(0.400)		(0.079)		(0.115)
Covenant violation × *High market share* × (Z=0)		−0.008		−0.092		−0.030		−0.067***
		(0.070)		(0.064)		(0.028)		(0.021)
Covenant violation × *High market share* × (Z=1)		−0.121***		−0.098***		−0.072*		−0.056***
		(0.035)		(0.037)		(0.041)		(0.018)
Establishment controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Industry × state × year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Rounded N	40,000	40,000	40,000	40,000	1,500,000	1,500,000	600,000	600,000
R^2	0.35	0.34	0.34	0.34	0.08	0.08	0.15	0.15

from zero.[41] Importantly, this null result holds when we partition establishments within firms by industry focus, productivity, and operating risk. This indicates that the internal resource allocation effects that we uncover are due to the covenant violation and not to some preexisting trend in firm behavior in the years before the violation.

Second, we nonparametrically control for trends in the financial condition of violators and nonviolators prior to technical default. Naturally, in the four quarters prior to a covenant violation, the financial condition of violators deteriorates relative to the average firm (see Table 1 and Nini et al., 2012). While our regressions control extensively for performance-related differences both at the time of violation and the year before, it is possible that our linear framework might not adequately account for heterogeneity between violators and nonviolators, especially if observable and unobservable differences are correlated (e.g., Roberts and Whited, 2013).

To evaluate this conjecture, we implement a difference-in-differences matching estimator. We construct a control sample of nonviolators that are matched to violator firms along a set of firm control variables measured in the year of and the year prior to technical default.[42] We adopt a nearest-neighbor propensity score matching scheme. We first run a probit regression of an indicator variable that equals one if a particular firm-year is classified in technical default (and zero otherwise) on our matching variables. The estimated coefficients are then used to predict probabilities of treatment (propensity scores), which allow us to perform a nearest-neighbor match with replacement using a 10^{-5} caliper.

Appendix IA.XIII displays the results. Panel A shows firm-level summary statistics for the violator and (matched) nonviolator samples. These statistics indicate that we achieve covariate balance among the two samples in terms of both current and lagged financial condition. Panel B estimates our baseline firm- and establishment-level models using the matched sample. The point estimates for employment and establishment closures, as well as the reshuffling of resources within firms, are similar to

[41] The one exception is the firm-level establishment closure rate, which is negative and statistically significant at the 10% level two years prior to the first violation.

[42] In unreported results, we obtain similar matching estimates when we match firms on the basis of observable characteristics in the two quarters immediately prior to technical default.

our baseline models in terms of statistical and economic magnitude, further supporting the notion that the effects we show reflect the causal impact of technical default.

Third, we consider threshold-based approaches to measuring covenant violations based on the Dealscan database of private credit agreements. This database provides actual covenant threshold levels for loan contracts at the time of origination, which allows us to implement a sharp RDD based on imputed rather than actual violations, albeit for a smaller sample (Chava and Roberts, 2008). We code a firm-year as a violation whenever the current value of the accounting ratio (net worth or current) is below the threshold specified in the loan contract. We continue to consider only new violations, meaning both accounting variables must exceed their respective thresholds in every quarter of the prior year and all data required to compute violations must be nonmissing.

Appendix IA.XIV shows the results of this alternative approach. Column 1 of Panel A defines a violation based on the net worth and/or current ratio thresholds. Column 2 combines the definitions based on Dealscan and SEC filings and code violations to occur when either accounting variable falls below its threshold or a violation is reported to the SEC. Column 3 instead uses a standard instrumental variables approach in which, in the first stage, a SEC-reported covenant violation is regressed on the distance to the threshold and in the second stage, employment is regressed on the fitted value of *Covenant violation*. This last method resembles a fuzzy RDD and allows for the possibility that lenders might waiver minor violations. Under each of alternative approaches, we see that the employment effects have a similar magnitude and remain significant at the 1% level.

Columns 4, 5, and 6 revert to the violation definition based on covenant thresholds and restricts the sample to firm-year observations within increasingly narrow intervals around the threshold (from $\pm$ 20% to $\pm$ 10%). By narrowing the bandwidth, we mitigate the concern that information about future investment opportunities (not measured by the control variables) may be captured by distance to the covenant threshold.[43] Column 7 instead selects the bandwidth based on the (Calonico et al., 2014) implementation of the (Imbens and Kalyanaraman, 2011) mean square error-optimal rule, yielding an interval of $\pm$ 19%. Columns 4–7 report the results of the estimation only including contemporaneous firm controls, as we implement a conventional RDD here. In each case the coefficient of interest is large and statistically significant at conventional levels. Column 4 shows that, on average, the number of employees decreases by 4 percentage points postviolation, which is in line with our baseline estimates. Panel B extends the RDD analysis to employment and closures at the establishment level and confirms our baseline results. These findings once again reassure us that we are identifying the effect of covenant violations on resource allocation separately from changes driven by differences in fundamentals between violators and nonviolators.

Fourth, we examine a setting where we are confident that action by creditors has taken place and therefore the postviolation adjustment in employment is less likely to reflect voluntary action on the part of the borrower. We follow Nini et al. (2009) and consider covenant violations that lead to the introduction of new capital expenditure restrictions in renegotiated loan contracts (i.e., a setting in which creditors are active following a covenant violation). These restrictions usually apply to annual cash capital expenditures plus new capital leases, expressed either in dollar terms or as a percentage of earnings or revenue.[44] These authors demonstrate that, upon the introduction of a new restriction, investment promptly dips below the level specified in the contract, which strongly suggests that these restrictions influence investment over and above any effects from underperformance. Under the assumption that capital and labor are complements in the production function, we therefore expect to find similar effects for employment in our setting.

Data for this exercise are kindly provided online by Nini et al. (2009). These data contain a representative sample of 3,720 private credit agreements between lenders and 1,931 publicly traded US corporations pulled from SEC filings and identified at the firm-year level. About 30% of these contracts contain capital expenditure restrictions. We focus on the intersection of this data set and our Compustat-LBD firm-year-level sample. We compare employment before and after the renegotiation for three groups of firms: (1) firms with new contracts that do not restrict capital expenditures, (2) firms with new contracts that contain a new restriction and whose prior contracts do not contain a restriction, and (3) firms receiving a contract that contains a restriction and whose prior contracts already contain a capital expenditure restriction (or for which we are missing the prior contract). Based on these three groups, we define two indicator variables: *New capital expenditure restriction* (second group) and *Old capital expenditure restriction* (third group). The first group of firms without any capital expenditure restriction either before or after the renegotiation are the omitted group in the regression.

Appendix IA.XV estimates the employment effects of capital expenditure restrictions across these three groups of firms. Column 1 indicates that the introduction of a new capital expenditure restriction leads to a 9 percentage point reduction in employment. This effect is significant at the 1% level. There is no effect for firms signing a new contract without a new restriction. The remaining columns repeat the estimation controlling for firm performance, and the estimate remains negative—although the magnitude reduces to –0.065 with the full set of controls—and

[43] Panel C conducts balancing tests within $\pm$ 20% distance around the threshold. We find no discontinuous jump in violating firms' characteristics, which alleviates concerns that differences in observable characteristics might be driving resource allocation outcomes. We also conduct a formal (McCrary, 2008) density test (results unreported) and rule out the possibility of manipulation of the running variable (i.e., the distance-to-technical-default) around the threshold. In unreported results, we confirm that the resource allocation effects are similar when we control explicitly for distance to the covenant threshold either at the time of technical default or at origination.

[44] While creditors are in a position to adjust other contract terms after the covenant violation, the elasticity of capital expenditure restrictions with respect to violations is largest in magnitude (Nini et al., 2009).

significant at conventional levels for the new capital expenditure restriction group only. These results indicate that the employment effects are the outcome of creditor actions, as opposed to self-correcting behavior on the part of borrowers.

Finally, an important remaining concern is the potential impact of measurement error in the market to book ratio for our estimates (Erickson and Whited, 2000). We tackle this concern using two complementary approaches, the results of which are shown in Appendix IA.XVI. First, we assume that market-to-book is measured with error and follow the estimation method that exploits higher-order cumulants of the data (Erickson et al., 2014).[45] Second, we substitute *Macro-q*—defined as the sum of total book debt and the market value of equity less inventory divided by lagged capital stock—into our regression models since it is likely to improve the measurement of Tobin's *q* relative to *Market-to-book* (Erickson and Whited, 2000). As shown in columns 1–4 and 5–8, respectively, we obtain similar qualitative results, although the magnitudes of the point estimates are slightly larger under either approach. This finding suggests that measurement error in our setting might be leading us to underestimate the resource allocation effects of covenant violations.

4. Conclusion

Using establishment-level data from the US Census Bureau, we provide detailed evidence on how US publicly traded corporations adjust their operations in response to debt covenant violations. We first show that covenant violations are followed by significant cutbacks, about 5% of the labor force. Then, using the Census micro-data, we look inside the black box of the firm and show two patterns of within-firm resource allocation following covenant violations. First, we show that firms refocus the scope of their operations by withdrawing resources significantly more from peripheral establishments outside of the firm's core business lines. Second, total and individual factor productivities drive resource allocation, whereby violating firms pull resources entirely at unproductive establishments. This second channel contrasts with the idea that lenders' demands destroy firm value by forcing borrowers to eliminate profitable investment projects (e.g., Beneish and Press, 1993). Crucially, we provide new evidence that these changes are prominent when key lenders specialize in a borrower's industry, which is consistent with creditors valuing relationships and offering expertise and knowledge when advising management through difficult times. Overall, these within-firm effects help to rationalize the surprising gains in both operating performance and equity returns following violations (Nini et al., 2009; 2012). Taking a step back, these findings fit with the view that creditors can alleviate manager-shareholder agency costs and thereby play a positive role in the corporate governance of underperforming firms.

Regulatory changes in the wake of the Great Recession and recent financial innovations may impede the ability of lenders to perform this role. Notably, stricter capital regulation and new liquidity requirements levied on banks increase the cost of originating and holding corporate loans, particularly long-term loans to risky borrowers that may benefit most from monitoring. In addition, the prevalance of "covenant-light" loan contracts with weaker lender protection—namely, loans excluding maintenance covenants (Ivashina and Becker, 2016; Berlin et al., 2017)—may reduce the occurrence of covenant violations and the potential for creditor influence. Finally, relatively new credit risk transfer tools such as credit default swaps separate control rights from potential losses (Parlour and Winton, 2013), which may weaken incentives to intervene when borrowers violate covenants (Bolton and Oehmke, 2011; Chakraborty et al., 2015). Investigating the role of banks and the broadening spectrum of other creditors in corporate governance in rapidly evolving, modern credit markets remains an exciting area for future research.

[45] We implement the estimator using the Stata command "xtewreg," which has kindly been made available at toni.marginalq.com/ewestimators.html.

Appendix A. Variable definitions

Table A.1
This appendix presents definitions for the variables used throughout the paper.

Variable	Definition	Source
Panel A: Firm level variables		
Δ Log(Employment)	Annual change in the natural logarithm of number of employees summed across establishments	LBD
Δ Log(Payroll)	Annual change in the natural logarithm of payroll summed across establishments	LBD
Symmetric employment growth	Twice the annual change in total employees over the sum of current and lagged employment	LBD
Δ Employees/Average assets	Annual change in the number of employees divided by the average of current and lagged book assets	LBD, Compustat
Δ Payroll/Average assets	Annual change in payroll divided by the average of current and lagged book assets	LBD, Compustat
Establishment closure	Indicator variable equal to one if the firm closes any establishment in the current year	LBD
Covenant violation	Indicator variable equal to one if the firm violates a covenant in the current but not previous year	Nini, Smith, and Sufi (2012)
New capital expenditure restriction	Indicator variable equal to one if the new contract contains a capital expenditure restriction and the previous contract for the same borrower does not	Nini, Smith, and Sufi (2009)
Old capital expenditure restriction	Indicator variable equal to one if the new contract contains a capital expenditure restriction and *New capital expenditure restriction* is equal to zero	Nini, Smith, and Sufi (2009)
Operating cash flow	Operating income before depreciation divided by average assets	Compustat
Leverage	Sum of debt in current liabilities and long-term debt divided by total assets	Compustat
Interest expense	Interest expense divided by average assets	Compustat
Net worth	Stockholders equity divided by total assets	Compustat
Current ratio	Current assets divided by current liabilities	Compustat
Market-to-book	Market value of equity minus book equity (adjusted for deferred taxes) divided by total assets	Compustat
Macro-q	Sum of total book debt and market value of equity less inventory divided by lagged capital stock (net PPE)	Compustat
Panel B: Establishment level variables		
Δ Log(Employment)	Annual change in the establishment-level natural logarithm of number of employees	LBD
Δ Investment rate	Annual change in establishment-level capital expenditures divided by capital stock	CMF/ASM
Establishment closure	Indicator variable equal to one if the establishment is closed	LBD
Covenant violation	Indicator variable equal to one if the parent firm had a covenant violation in the current but not in the previous year	Nini, Smith, and Sufi (2012)
Age	Number of years since the first year the establishment first appears in the LBD	LBD
Establishments per firm	The total number of establishments of the parent firm	LBD
Establishments per segment	The average number of establishments per three-digit industry segment of the parent firm	LBD
Core	Establishment operates in three-digit SIC industry containing at least 25% of firm employment	LBD
Total factor productivity	Establishment-level log total factor productivity computed following Foster, Haltiwanger, and Syverson (2013)	CMF/ASM
Labor productivity	Average wage defined as payroll divided by number of employees	LBD
Labor productivity (Alt. 1)	Value added per labor hour defined as sales minus materials and energy costs divided by labor hours	CMF/ASM
Labor productivity (Alt. 2)	Output divided by total labor hours	CMF/ASM
Labor productivity (Alt. 3)	Wage per hour defined as payroll divided by total labor hours	CMF/ASM
Return on capital	Sales minus material and energy costs and payroll divided by establishment-level capital stock	CMF/ASM
Operating risk	Cross-sectional volatility of establishment operating margins at three-digit SIC code level	CMF/ASM
Operating risk (Alt. 1)	Cross-sectional volatility of firm operating margins at the three-digit SIC code level	CMF/ASM
Operating risk (Alt. 2)	Five-year time-series volatility of average industry operating margin at the three-digit SIC level	Compustat
Operating risk (Alt. 3)	Ten-year time-series volatility of average industry operating margin at the three-digit SIC level	Compustat
Operating risk (Alt. 4)	Five-year time-series volatility of average industry ratio of operating cash flows to assets at the three-digit SIC level	Compustat
Operating risk (Alt. 5)	Cross-sectional volatility of establishment return on capital at three-digit SIC code level	CMF/ASM

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