Essays on Corporate Finance and Corporate Governance

Jiaying Li

Department of Finance

Bayes Business School

City, University of London

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Abstract

This thesis includes three essays in corporate finance and corporate governance.

In the first paper, we investigate the redistribution effects among firm stakeholders of increased antitrust scrutiny. Using the cases opened by the European Commission between 1991 and 2019, we find that cartel investigations temporarily reduce profits for all firms in the affected industry and increase profits for their customers. In response to the negative shock to their profitability, firms engage in intense restructuring: they undertake mass layoffs and reduce employment; to lesser extents they increase leverage, cut investment and sell assets. The effects tend to be concentrated in firms that are less productive and closer to financial distress at the time of the shock.

The second paper investigates how shareholder protection affects an important stakeholder, labor. I examine the changes in firms' employment decisions around the staggered passage of the Universal Demand (UD) law - a law that weakens shareholder litigation rights and reduces managers' litigation risk. My findings document an increase in corporate employment following the UD adoption, which can be explained by managers' risk taking. The UD-induced workforce expansion is concentrated in firms that rely more on high-skilled labor, indicating a potential improvement in their labor structure. On aggregate, the workforce expansion is inefficient and harms shareholder interests.

The third paper presents new evidence that corporate customers play a governance role in disciplining managerial behavior. Using a comprehensive dataset of customer-supplier relationships, we show that major downstream firms respond to upstream firms' EPS manipulation - instrumented by variations in the incentive to manipulate - by severing business relationships. Ex ante, the threat of withdrawal by major customers appears to deter upstream firms from engaging in EPS manipulation. Suppliers with short-term incentives strategically reallocate trade credit among customers to retain their largest customers, which mitigates the ex-post impact of customer governance.

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Chapter 1

Trading Off Stakeholder Interests: Evidence from Antitrust Investigations

1.1 Introduction

A recent line of research has investigated the link between product market competition and macroeconomic outcomes. A key theme is that limited competition, by resulting in lower output, adversely affects employment. This implies that antitrust enforcement in product markets should lead to favorable labor outcomes.[1] Little is known, however, about the firm-level adjustments that take place along the path to a new industry equilibrium following antitrust investigations. How do incumbent firms respond to antitrust scrutiny in their industry? Do they engage in restructuring activities to enhance their productivity? Do they manage their cash flows differently in the face of reduced profit margins, cutting shareholders payouts or increasing leverage? In this paper we address these questions by focusing on cartel investigations.[2]

With these questions in mind, we perform an event study exploiting cross-industry variations in the cartel cases investigated by the European Commission from 1991 to 2019. Our sample consists of all the public firms included in Worldscope. A cartel investigation can be initiated by the EC or triggered by an application for leniency from a cartel member willing to cooperate with the authorities. Once the investigation is closed, the cartel members are subject to hefty fines if deemed guilty by the EC. However, the

[1]See for instance De Loecker, Eeckhout, and Unger (2020).

[2]"A cartel is a voluntary association of legally independent firms that aims to raise their joint profits through explicit agreements," Connor (2020). A collusive outcome may even be sustained without communication among firms, with no explicit agreement or exchange of relevant information: this is labeled as tacit collusion. Motta (2004) discusses the economics of collusive agreements and provides a review of competition policies against collusion in the EU and the US.

opening of the investigation is likely to represent a shock for both the cartel members and their non-cartel competitors: it should lead all players to intensify price competition due to the increased antitrust scrutiny over the industry and the cartel breakdown. Antitrust enforcement is thus likely to reduce the profitability of *all* incumbent firms in the affected industry, triggering a change in their corporate and financial strategies.[3]

For our analysis, we focus on the 3-digit SIC industries that have at least one 4-digit industry under investigation for anti-cartel law infringements during the sample period. Our treated group consists of all firms belonging to the 4-digit industries under scrutiny. Cartels are normally formed in very granular markets. However, non-cartelists just outside the relevant market may be affected, as products may become substitutes when the cartel's price increase is large (Inderst, Maier-Rigaud, and Schwalbe (2014)). The control group includes all firms that belong to the same 3-digit, but not the same 4-digit industries as the treated ones. Restricting attention to the 3-digit SIC industry alleviates the concern that the treated and control firms are intrinsically different. To further mitigate any potential bias, we adopt propensity score matching to select a control group with firms that are most similar to the treated ones, and use the matched sample throughout our analysis.

To assess whether the opening of the cartel investigation is a shock for the industry, we examine its impact on the operating performance of non-investigated firms in the cartelized industries. We find that the opening of a cartel investigation in the 4-digit industry is associated with a significant drop in performance: both Return on Assets and EBITDA/Assets decrease by $1 - 1.5$ percentage points in treated firms one year from the start of the investigation. The effects disappear within three years. The impact also translates into a decreased probability of survival, which is particularly pronounced for EU firms.

Consistently with a positive shock to competition, we show that the reduction in profitability for the treated firms is reflected in an increase in profitability in their customers. We rely on Factset Revere to identify the customers of firms in the treated and control set, and study the evolution of buyer performance before and after cartel investigations (to ensure a meaningful sample size we perform this analysis only on the global sample). In the two years following the investigation, customers see a 1.3 percentage point increase

[3] Cartel investigations have been shown to drive a decline in the stock prices of both investigated firms (Aguzzoni, Langus, and Motta (2013)) and European cartel outsiders (Bos, Letterie, and Scherl (2019)).

in Return on Assets and a $1.6 - 1.7$ percentage point increase in EBITDA over assets. However, the effect vanishes in the long run.

Having established that the cartel investigation is a (short-term) negative shock to firm profitability, we use the event study methodology to analyze the corresponding effects on firms' behavior. The short- and long-term effects of antitrust shocks on firms' demands for input (e.g., labor and capital) are not clear *a priori*. This is particularly evident in the case of anti-cartel enforcement. On the one hand, firms belonging to a cartelized industry may want to increase production as collusive agreements break down. This would boost their demand for inputs and employment. On the other hand, the intensification of competition triggered by an investigation may be a catalyst to cost-cutting and cash-flow boosting activities. This may translate in employee dismissals in some cases. How different firms reshape their demand for labor and capital following antitrust investigations is thus an open empirical question.

First, we focus on workforce restructuring, using a measure of mass layoffs as our main outcome variable. We find that following the opening of a cartel case in their industry, treated firms are significantly more likely to start mass layoffs. Among treated EU firms, the effect starts in the year when the investigation is opened, and becomes very pronounced in the second and third year (when the probability of a large employee dismissal increases by about 8 percentage points compared with the control firms). This represents an 80%-increase relative to the average probability of mass layoffs. The effect on mass layoffs completely wanes after 3 years. In the global sample, the effect is smaller in magnitude (the probability of a mass layoff for the treated firms is just 2.3 percentage points higher than for the control firms in the year of the shock) but still statistically significant.

We also find that in the EU sample, firm employment declines by $13 - 15\%$ in the first three years after the investigation starts (the effect in the global sample is a short-term reduction by $5 - 7\%$), and then regresses to pre-shock levels. Interestingly, this decline in employment in incumbent firms is associated with a permanent increase in sales per employee among treated EU firms. Taken together, these findings suggest that antitrust shocks force firms to restructure their labor force to boost their productivity. To shed more light on this hypothesis, in the global sample, we sort firms depending on their labor productivity before the shock: we label as productive the firms that have higher sales per employee than the industry median and unproductive the remaining ones. We find that less productive firms are more likely to start collective dismissals, in line with the hypothesis that cartel investigations are a catalyst for efficiency changes as the "fitter" firms are more likely to survive in a more competitive environment (as in Zingales (1998)). Pre-shock differences in labor productivity may explain the different results we uncover in the EU and global samples as EU firms are relatively more likely than non-EU firms to have low labor productivity before the cartel investigation.

Next, we study the effect of cartel investigations on firms' investment decisions. Among EU firms, investment as a proportion of total assets declines in the treated firms as compared with the control firms. The reduction in investment happens in the same year of the shock and survives also in the longer run, more than 3 years after the shock. The effect is stronger when measured in terms of net property, plants and equipment (PPE): treated firms shrink by about 10 percentage points (compared to control firms). An important driver of these changes seems to be an increase in asset sales: the latter ones become about 5 percentage-point more likely following the shock (which corresponds to a 30%-increase relative to the average probability of asset sales). We find no such effect in the global sample: treated firms do not alter their investment and asset growth policies following the cartel investigation compared with the control firms. Again, the difference between EU and non-EU firms might be due to differences in pre-shock characteristics. When we sort firms in the global sample depending on their financial strength before the shock, we find that the reductions in investment and the increases in asset sales are concentrated among firms that are close to financial distress (i.e., have an EBIT interest coverage ration below 2). We also find that EU firms are relatively more likely than non-EU firms to fit in the financial distress group.

Then, we proceed to examine whether cartel investigations have an effect on other firm stakeholders. To assess whether shareholders also share the costs of antitrust shocks, we look at changes in shareholder payouts (cash dividends and share repurchases) and net leverage. We find little or no significant change in either measure. To investigate whether customers and suppliers are affected by the shock, we study how firms manage their working capital: we look at the effect of the cartel investigation on accounts receivable, accounts payable, and inventory. On the one hand, firms may be forced to delay payments to suppliers, reduce inventories and limit the amount of trade credit extended, as a consequence of their worsened performance; on the other hand, firms with sufficient financial slack may see trade credit as a strategic tool to gain a competitive edge over their rivals, thus increasing the trade credit provided to customers or reducing the trade debt with their suppliers to take advantage of early payment discounts. The estimated coefficients for EU firms indicate a (insignificant) increase in account payable days, a reduction in accounts receivable and a significant reduction in inventory days (with pre-trends), which translate into a shorter cash conversion cycle.

Our key results survive a battery of robustness checks. We try to address concerns about the endogeneity of the cartel investigations: when an industry is in a bust, defection from the cartel via an application for leniency (and the subsequent cartel investigations) might be more likely. We follow Braun and Larrain (2005) to identify periods of industrial booms and busts: we find no change in our main results when we exclude industries that are experiencing a downturn before the shock. We show that results are robust to the exclusion of the largest sectors (pharmaceutical and banking), which are to some extent special industries. Although we conduct our analysis on a matched sample, the findings are not significantly different without matching. Furthermore, adopting a logit extimation for discrete dependent variables and the Sun and Abraham (2021a) methodology also yields similar results.

Overall, our results indicate that cartel investigations represent an adverse shock to profitability that spurs substantial mass layoffs and asset sales in incumbent firms. Restructuring activity is particularly concentrated in EU-based, less productive and financially weaker firms. Trade credit provision to customers (that see their performance improve) is unaffected, as are shareholder payouts and financial leverage.

1.1.1 Related literature

By studying the effect of antitrust investigations on restructuring decisions and cash flow management, our work fills a gap in the recent literature on competition policy and corporate finance/strategy. Dasgupta and Žaldokas (2019) examine how leniency legislation enabling more aggressive anti-cartel enforcement affects firms' capital structure: they find that firms respond by increasing investment and reducing leverage. Stricter anti-cartel laws may also induce firms to shift from price-fixing to other anti-competitive strategies (Bittlingmayer (1985); Mueller (1996)): in line with this, recent studies document an increase in horizontal M&A activity (Dong, Massa, and Žaldokas (2019) and Chung, Hasan, Hwang, and Kim (2023)) and in minority share horizontal acquisitions (Heim, Hüschelrath, Laitenberger, and Spiegel (2021)). The introduction of leniency laws has made investigations more frequent and effective in breaking up existing cartels. As expected, investigations adversely affect the stock prices of both investigated firms and their industry peers.[4] However, no previous work has studied how firms adjust when antitrust action busts a cartel in their industry: the results we present indicate that cartel investigations stimulate efficiency improvements, driving a short-lived but intense phase of labor restructuring and asset sales in less productive and financially weaker firms in the affected industry. Other papers rely instead on cartel detection data to investigate the behavior of colluding firms *before* being detected. Ferrés, Ormazabal, Povel, and Sertsios (2020) find that cartel firms have lower leverage ratios during collusion periods, while González, Schmid, and Yermack (2019) document that managers of cartel firms benefit in several ways from cartel participation: they enjoy greater job security, receive higher cash bonuses, and exercise more aggressively executive stock options.

Our firm-level analysis complements recent work investigating the link between product market competition and macroeconomic outcomes. Eeckhout (2021) argues that limited competition results in lower output, thus adversely affecting employment. Workers would then benefit from antitrust action in product markets. In line with this, Babina, Barkai, Jeffers, Karger, and Volkova (2023) document a positive impact of US antitrust enforcement on economic activity, as measured by aggregate industry employment and new business formation in non-tradable industries. Our paper investigates instead the firm-level response to antitrust investigations, which allows us to document heterogeneous adjustments among incumbent firms in the affected industries. We highlight that the effects of antitrust enforcement on labor are not obvious a priori, at least in the short run: while a cartel breakdown may boost production and labor demand, a sudden increase in competition may induce the less productive incumbents to restructure their workforce. We show that cartel investigations trigger in fact an intense (but short-lived) wave of mass layoffs, which is mainly concentrated among EU-based and less productive firms.

Our work also contributes to other lines of research. We add to the literature studying the impact of product market competition on firms' corporate strategy (see Sertsios (2020) for a survey). The work in Zingales (1998) on the effects of deregulation in the trucking

[4]See Aguzzoni, Langus, and Motta (2013);Gunster and van Dijk (2016); Bos, Letterie, and Scherl (2019).

industry served as inspiration for our analysis. Our results confirm that the impact of competition on investment is complex and differs across firms. Dasgupta and Žaldokas (2019) find an equity-funded increase in asset growth after the passage of leniency laws. By contrast, Frésard and Valta (2016) find that following import tariff reductions, incumbent firms reduce their investments when competitive actions are strategic substitutes, when they are financially weak and when entry costs are low. Covarrubias, Gutiérrez, and Philippon (2020) analyze the differential responses of leaders and laggards to foreign entry and find that leaders (laggards) increase (decrease) investment. Faccio and Zingales (2022) find no statistically significant change in investment following the introduction of pro-competition rules in the telecom industry. Although we look at investment, our main contribution is to study the effect of increased antitrust scrutiny on labor. Existing literature tend to focus only on the impact of competition on managerial incentives and compensation packages (see e.g. Cuñat, Gine, and Guadalupe (2012)).

The paper is also related to previous work on the redistributive effects of corporate restructuring. Both mass layoffs and asset sales are documented to improve firms' operating performance in distressed times (Denis and Kruse (2000)). Empirical studies have found that firms tend to resort to different types of restructuring strategies depending on their characteristics. Atanassov and Kim (2009a) show that layoffs are more likely in countries with stronger shareholder rights, and less likely where unions have more power. Koh, Durand, Dai, and Chang (2015) show that young firms are more inclined to lay off their employees when in distress while mature firms tend to adopt asset restructuring. We look at the restructuring activities associated with a specific shock to profitability (the breakdown of collusive practices due to the start of a cartel investigation). Hence, we have cleaner identification than previous papers.

Finally, our paper also complements the literature on antitrust and labor markets. Recent research has documented the pervasiveness of monopsony in labor markets (see e.g. Azar, Marinescu, Steinbaum, and Taska (2020)), which has led to more antitrust scrutiny of labor markets in recent years. We do not investigate the (still rare) cases of antitrust enforcement against wage-fixing cartels. However, our findings indicate that antitrust actions in product markets may affect workers and other input suppliers. Antitrust authorities should be aware that cartel investigations, by spurring mass layoffs, may have potential spillovers on labor markets.

The rest of the paper proceeds as follows. Section 2 presents the data and the empirical methodology. The main results are presented in Section 3. Cross-sectional tests are reported in Section 4; and robustness tests in Section 5. Section 6 concludes. We describe our sample of cartel cases, the definition of the variables, and the tables of robustness checks in the Appendix.

1.2 Data and Methodology

In our analysis, we construct two samples using the data in Worldscope from 1988 to 2019: (1) an EU sample where we focus only on publicly listed EU firms (including UK firms); and (2) a global sample, which includes public firms from all countries covered by Worldscope. We match both samples with hand-collected data on the European Commission cartel investigations.

1.2.1 Cartel Investigation Data

We collect information on all cartel investigations started by the European Commission in the 1991-2019 period: we document the opening and closing date of each investigation, details of firms and industries under scrutiny, as well as the imposed fines. Using the opening date of the initial investigation, we identify the year when antitrust authorities start the investigation in the corresponding industry. We treat as one cases that were re-opened after the closure of the initial investigation and cases with firms failing to settle with the EC at the same time as their "co-conspirators." We assign a 4-digit SIC code to each cartel case based on the specific target product market.[5] This procedure allows the identification of 112 cartel cases. The mean and median lengths of the investigation period are four years, the longest investigation in our sample lasted for fifteen years and the shortest took only one year.[6]

To rule out overlapping event windows, we exclude the 4-digit industries with two or more cartel investigations opened in different years in our sample. We keep the cases in which the industries have more than one cartel investigation opened in the same year, since the event windows perfectly coincide. This method yields a sample of 48 4-digit SIC industries that were under investigation some time during the 1991-2019 period, before

[5]To ensure the accuracy of the data, we omit the cases where we are not confident about the information, such as the SIC code. Details on our procedure are described in Appendix A.

[6]As four cartel investigations in our sample hadn't been closed when collecting the data, the summary statistics of the investigation lengths are based on the remaining 108 cases.

matching. Matching reduces further our sample to 37 industries in both the EU sample and global sample: the industries covered by these two samples do not perfectly overlap as the matching process was independently done for the two samples.

We report the details of the cartel cases and their industries from our post-matching global sample in Table 1.1.[7] For each investigated industry/year, the column *Treated Firms* lists the number of firms belonging to the investigated 4-digit SIC industries. We remove from the sample the firms that are directly under investigation. So *Treated Firms* are all the firms in the Worldscope samples (EU only and global) that are industry peers of the firms directly under investigation.

1.2.2 Firm-Level Data

We use Worldscope to collect all financial information on publicly listed firms around the world. We use two measures of firm profitability: *ROA* and *EBITDA/Assets*. We define *ROA* as a firm's net income before extraordinary items and preferred dividends over total assets. Similarly, *EBITDA/Assets* is the firm's EBITDA divided by total assets.

We use two measures of labor restructuring: *Mass Layoff* takes the value one in year t if the firm experiences a decrease in the number of employees greater than 20% over the previous one year or two years, and zero otherwise; and *log(Employees)*, which is the log value of the total number of employees as reported in Worldscope. While Worldscope has often been used in the literature to study changes in employment in international settings (see for instance Levine (2016), Atanassov and Kim (2009a), and Faccio and O'Brien (2021)), there may be concerns about the accuracy of the employment figures. As highlighted by Atanassov and Kim (2009a), the number of employees in Worldscope may not be updated as frequently as financial data, "because personnel information is subject to looser reporting and auditing requirements than financial variables." This in turn may underestimate the actual changes in the number of employees over a year. We try to mitigate this concern in a similarly way to Atanassov and Kim (2009a), by using *Mass Layoff* as our main indicator of labor restructuring.

We employ three measures of investment activity: *Investment*, which is measured as capital expenditures over total assets; *Asset Growth*, which is the annual percentage change in net Property Plant and Equipment (PPE); and *Asset Sales*, which is a dummy

[7] The cartel cases covered in the pre-matching sample and the post-matching EU sample are reported in Table 1.A.2 and Table 1.A.1, respectively.

variable proposed by Atanassov and Kim (2009a) that takes value one if the firm experiences a decrease in net PPE greater than 15% over the previous one year or two years, and zero otherwise.

To measure the impact on financial and operating leverage we focus on three variables: *Net Leverage*, which is the sum of interest-paying debt minus cash divided by total assets; *Shareholder Payout*, which is the sum of cash dividends and share repurchases over sales; and *Working Capital*, which measures the management of a firm's working capital: *Working Capital* is *AP Days* + *Inventory Days* - *AR Days*, where *AP Days* is the ratio of accounts payable over the cost of goods sold (COGS) multiplied by 365, *AR Days* is the ratio of accounts receivable over Sales multiplied by 365, and *Inventory Days* is the ratio of inventories over COGS multiplied by 365.

In our regressions, we include the logarithms of total assets, *log(Total Assets)*, as a standard firm-level control variable. This variable is lagged one year in the regressions and is winsorized at the 1% level. As further firm-level control variables we use the logarithms of totals sales, *log(Sales)*, and the ratio of the market value to the book value of equity, *M/B ratio*.

1.2.3 Identification Strategy

We employ a difference-in-difference methodology that exploits the opening of cartel investigations as a quasi-natural experiment. We label all firms whose primary 4-digit SIC code corresponds to a 4-digit industry in which a cartel investigation has been opened as treated firms. If more than one cartel investigation is opened in the same industry in one year, we consider them as one shock and treat them as one cartel investigation. As a robustness check, we also exploit the FactSet Revere data (as discussed in Section 1.2.5) to identify competitors of the firms under investigation as treated firms.

The underlying assumption is that the opening of a cartel investigation is likely to have an impact on all industry participants, whether directly subject or not to the antitrust investigation. This may happen for two reasons. First, the cartel breakdown will affect the many undetected cartel members that are not directly under investigation due to lack of hard evidence. Antitrust authorities often carry investigations on a subset of firms involved in the cartel. These are likely to be the ones named in the leniency application,[8]

[8]Leniency programs have long been considered as a powerful tool in detecting cartels, with the ultimate goal of deterring firms from colluding. The EC leniency policy (which was introduced in 1996 and reformed

while leaving other cartel members out of the case due to the lack of hard evidence of collusion. Secondly, as non-cartel members often benefit indirectly from a cartel operating in their industry, they will be affected by the intensification of competition following the cartel breakdown.[9] The effect of the investigation may somehow be mitigated for non-investigated firms, as they will not bear the financial burden of potential fines. However, the existing evidence on antitrust enforcement actions suggests that fines account for a small part of the impact on firms' values. Aguzzoni, Langus, and Motta (2013) document a 2.89% reduction in firms' share prices when a surprise inspection is carried out, but find that fines account for no more than 8.9% of the loss in firms' market value caused by the antitrust action.

In our analysis we exclude from our sample all cartel members directly subject to the investigation (including those applying for leniency), and study the response of the other firms in the 4-digit SIC industry (the "treated firms"). This partly mitigates endogeneity concerns due to the fact that applications for leniency and cartel investigations may be triggered by the actions of cartel members. More pragmatically, this approach allows us to have access to a larger sample of treated firms: if we restricted our attention to the firms in Worldscope that were directly indicted in an antitrust investigation we would be left with only a handful of treated companies and too little statistical power to conduct an empirical analysis.

We construct a pool of (clean) controls using the sample of never-treated firms whose core business is in the same 3-digit (but not the same 4-digit) SIC industry as the treated firms, and never experience a cartel investigation in their industry during the sample period. To assuage concerns that treated firms may be inherently different from control firms in the pre-investigation period due to the presence of cartels in their industries, we match treated and control firms using a multivariate propensity score matching method. We first exclude observations in extreme cases where the total asset is negative and *ROA* is less than -50%. Then, we use a logit model to estimate the probability of being treated using firm size (measured by the *log(Assets)*), performance (measured by *ROA*), and *M/B Ratio* as explanatory variables. To accomplish the matching, we select control firms with

in 2002) grants complete immunity from fines to the first firm reporting the cartel and providing key evidence to the EC.

[9] "Because successful cartels typically reduce quantities and increase prices, this . . . leads to a substitution away from the cartels' products toward substitute products produced by cartel outsiders." (Inderst, Maier-Rigaud, and Schwalbe (2014)).

the nearest propensity scores to the treated firms. For the global sample, we require that each allocated control firm operates in the same 3-digit (but not the same 4-digit) SIC industry as its corresponding treated firm. We cannot impose this restriction in our EU sample, due to the limited sample size. The approach yields a matched sample with 1414 (355) control firms and 1427 (469) treated firms in our global (EU) sample.[10]

The sample construction process only requires firms to be active for at least one year before and one year after the cartel investigation, and thus does not necessarily yield a balanced panel. It is possible that some treated firms are forced to exit the market due to intensified competition. A close inspection of our data shows that there are 257 (113) out of 1427 (469) treated firms that exit within three years after the investigation begins in our global (EU) sample. Our findings thus provide a lower bound of the estimated impact of cartel investigations on firms operating in the affected industry.

1.2.4 Descriptive Statistics

Table 1.2 presents descriptive statistics of the firm-level controls and outcomes, measured one year before the shock, separately for treated/control firms in our baseline analysis, after matching. All the reported firm-level characteristics are not significantly different between the treated and control firms in our global sample. For the EU sample, treated and control firms are similar in terms of size (measured by sales and assets), and do not differ across the outcome variables that we will examine, with the exception of *Investment*, *Asset Sales*, *log(Employees)*, and *Working Capital*. *Investment* and *Asset Growth* one year before the shock are larger in the treated group, suggesting that firms in the affected industries were investing and growing more before the shock. The *Working Capital* (defined as AR Days + Inventory Days - AP Days) is shorter in the treated firms as compared to the control firm: the treated firms are more efficient in their working capital management. To mitigate concerns that the samples are not comparable, we will pay particular attention in our empirical analysis to rule out the possibility that key firm outcomes display a different pre-treatment trend for treated firms versus never-treated firms.

[10] We also perform our analyses using the pre-matching full sample. The results are reported in Table 1.C.4 and are consistent with our baseline findings.

1.2.5 FactSet Revere Data

We use FactSet Revere to identify firms' customers. This database has been used in some finance and economics studies as well as in the operations management literature (e.g. Boehm and Sonntag (2020), Ding, Levine, Lin, and Xie (2021), and Son, Chae, and Kocabasoglu-Hillmer (2021)).

FactSet Revere is a specialized dataset that provides detailed information on the nature and duration of over 450,000 vertical and horizontal business relationships. It currently covers more than 31,000 publicly traded companies around the world. Its coverage of US firms begins in 2003; and the coverage has been expanded to include international firms starting from 2011. FactSet Revere reports 13 types of supply chain relationships and we limit our scope focusing only on the supplier-customer relationships. Each relationship is documented in the database with a relationship type, firm identifiers, and the start and end dates of the relationship.

The information on relationship networks is collected and updated on an ongoing basis as companies release their annual financial filings throughout the calendar year. The data sources also include company websites, corporate actions, investor presentations, news releases, press coverage. Although the coverage mainly focuses on large and mostly listed firms, many small and private firms appear in the data due to their relationships with large firms. The supplier-customer-relationship coverage of FactSet Revere is larger than the conventional database on supply chains such as Compustat Segment, which only includes large customers that represent more than 10% of a US supplier's sales.

As our analysis is at the annual level, we follow the literature and annualize the relationship data using the following procedure: when a relationship lasts for more than one calendar day in a specific year, we treat the relationship as active in that year. We use the ISIN code to merge supply-chain data with Worldscope.

1.2.6 Empirical Specification

We implement a pooled event study, which exploits the staggered nature of cartel investigations. We denote as $\tau = 0$ the year in which an investigation opens in a 4-digit industry s, and estimate the following dynamic specification:

$$y_{it} = \sum_{\tau=-3,\tau\neq-1}^{+3} \alpha_\tau I_{it}(\tau) + \bar{\alpha} I_{it}(4+) + \beta \cdot X_{it-1} + \lambda_i + \gamma_{kt} + \epsilon_{it}, \tag{1.1}$$

where y_{it} is an outcome observed for firm i (active in industry s and country k) at time t. The term $I_{it}(\tau)$ is a treatment indicator equal to 1 if in year t firm i is τ years away from the event, i.e. the opening of a cartel investigation in industry s, with $\tau \in [-3, +3]$. The term $I_{it}(4+)$ is a treatment indicator equal to 1 if in year t firm i is 4 or more years past the opening of a cartel investigation in its industry, thus putting into the same bin all longer term effects. As a normalization we exclude the first lead, $I_{it}(-1)$. We do not include the leading terms $\tau = -4$ and before, thus assuming away anticipation effects four or more periods before treatment. We include lagged firm-level controls (X_{it-1}), firm fixed effects (λ_i), and country×year fixed effects (γ_{kt}) to filter out possible country-specific trends that could simultaneously affect firms' corporate strategies and antitrust enforcement. We cluster our standard errors at the 4-digit SIC industry level in all regressions, because the investigation shocks are defined at this level. Since our main indicators of corporate restructuring (*Mass Layoff* and *Asset Sales*) are dummy variables, as a robustness check, in the Appendix we also estimate a logit model of specification (1.1) controlling for firm, country and year fixed effects.

Our difference-in-difference approach identifies the causal effect of a cartel investigation under the assumption that outcomes in treated and untreated firms display parallel trends in the absence of treatment. While this assumption cannot be tested directly, the coefficients on the leads will give us an indication of its plausibility. The coefficients on lags allow us instead to study the dynamics of the average firm response to a cartel investigation.

The two-way fixed effects estimator in equation (1.1) has been shown to be a weighted average of all possible 2×2 DiD estimators that compare firms in treated cohorts to never-treated firms, and firms in different treated cohorts to each other. In the presence of heterogeneous treatment effects, the 2×2 DiD components using already-treated units as controls may have negative weights (see Goodman-Bacon (2021)). To assess whether this issue biases our TWFE estimator, we verify the robustness of all our results to the use of the alternative "interaction-weighted" estimator proposed by Sun and Abraham (2021a). The results are discussed in Section 1.5, and figures are reported in the Appendix.

1.3 The Effect of Cartel Investigations

In this section, we explore how the opening of a cartel investigation in an industry affects firms belonging to that industry. We start from the impact on firm performance and then we examine firms' changes in employment, investment, working capital and financial policies.

1.3.1 Impact on Profitability

In Section 3.2.2 we argued that the opening of a cartel investigation represents a shock for *all* firms in the affected industry. In other words, the shock hits both the indicted firms in the cartel case and the firms in the industry that are not directly under investigation. To support this claim, we examine how the opening of cartel cases affects the performance of non-investigated firms in the cartelized industries.

In Table 1.3, we estimate equation (1.1) using the two measures of firms' performance (ROA and $EBITDA/Assets$). The results are presented for the EU sample (in columns 1 and 2) and for the global sample (in columns 3 and 4). The results do not indicate any significant differences between the two samples as the negative effects on short-term performance are similar both qualitatively and quantitatively across the two samples.

We find that cartel investigations have an adverse effect on the performance of the treated firms. Profitability decline by about 1 and 1.5 percentage points in the year of the shock (for $\tau = 0$) and in the year that follows ($\tau = +1$). The negative effect is still visible two years after the shock (at $\tau = +2$) and disappear completely afterwards (for $\tau = +3$ and $\tau > +3$). This finding suggests that the investigations trigger an intensification of competition for the whole industry, likely due to heightened antitrust scrutiny and the cartel breakdown.

The effect is temporary as the coefficient on the dummy for $\tau > +3$ is never statistically different from zero. We find no evidence of pre-trend effects, as the the coefficients on Shock($\tau = -3$) and Shock($\tau = -2$) are not statistically different from zero. This is supporting evidence in that the parallel trend assumption is plausible.

The effects we uncover are likely to be a lower estimate of the size of the negative shock to firm profitability associated with the antitrust investigation. This is because of a survivorship bias in our sample selection. As the event window we adopt spans seven years around the start of the antitrust investigation, it is possible that one of the effects of the increase competition is a bout of consolidations and exits.

To shed some light on this hypothesis, in Table 1.C.1 of the Appendix, we estimate a hazard model for the probability of exit. We find that the probability of exit increases significantly after the antitrust investigation. The effect is noticeably larger for the EU sample but it is also present in the global sample. For the EU sample, the hazard ratio is $2.9037 (= \exp(1.066))$, which indicates that when firms are affected by cartel investigations, there is an 190.37% increase in the probability of exit. For the global sample, the increase in the probability of exit is 135.61% ($\exp(0.857) = 2.3561$).

As a further check that our shock is indeed an increase in competition and not just a reduction in profitability (like an increase in the cost of input), we use FactSet Revere to identify the customers of our treated firms. Our assumption is that the antitrust investigation increases competition. Hence, we would expect the customers of the treated firms to benefit from it. Indeed, in Table 1.4, we find that customers see an increase in *ROA* and *EBITDA/Assets* between 1.3 and 1.7 percentage point following the antitrust investigations. As in the case of the treated firms, the effect on their customers is temporary: the coefficients on the time dummies are statistically different from zero only one ($\tau = +1$) and three ($\tau = +3$) years after the investigation. The results are presented only for the global sample as the matching with FactSet Revere reduced considerably the number of observations. We find no evidence of pre-trend effects.[11]

In conclusion, the results so far support the identifying assumption in our analysis: cartel investigations represent an increase in competition in the product market even for firms that are not directly targeted by the investigation but belong to the same industry. The question for the remaining of the paper is: how do the firms react to this shock? In basic cash flow analysis, a negative shock to profitability will have to be reflected in one or more of these changes: a reduction in operating costs (in terms of labor cost, in particular), a reduction in net investment (investment - asset sales), an increase in financial leverage (debt - cash) or a reduction in working capital, and a decrease in net shareholder payouts (dividend and share repurchases).

[11] In unreported results, we also use FactSet Revere to identify the suppliers of the treated firms. We find no significant effect of the shock on their profitability. This finding supports the view that the cartel investigation has no direct impact on the input markets.

1.3.2 Impact on Labor

Table 1.5 presents the estimated coefficients of specification (1.1) where the the outcome variables are employment measures: the *Mass Layoffs* indicator (in columns 1 and 4), *log(Employees)* (in columns 2 and 5), and *log(Sales/Employees)* (in columns 3 and 6). Results are presented separately for the EU sample (in columns 1 to 3) and for the global sample (in columns 4 to 6).

In the EU sample, the cartel investigation has a large effect on firms' labor policy. The cartel investigations are associated with an increase in the probability of mass layoffs and a decrease in the number of employees already at $\tau = 0$ (with respect to $\tau = -1$). The negative effects on employment increase over time, reaching a peak two year after the shock. The magnitude of these effects is important: for instance, two year after the opening of the investigation, the likelihood of a large employee dismissal increases by 8.8 percentage points, adding to a pre-event likelihood equal to 10.9% in treated firms (see Table 1.2); the decrease in employment peaks at 15.4%. The effect wanes after three years: the coefficient on the long-run indicator ($\tau > +3$) is not significant for mass layoffs and is barely significant for the log(employees). The reduction in employment is reflected in column (3) by an increase in labor productivity as measured by log(Sales/Employees). Two and three years after the start of the investigation, labor productivity increases by 16 percentage point, and stays higher also in the long run.

In the global sample, the effect of a cartel investigation on employment is much smaller and shorter lasting. It starts in the year when the investigation is opened (at $\tau = 0$) and continues in the year after (at $\tau = +1$); and is no longer significant afterwards. The magnitude of these effects is smaller than in the EU sample: the likelihood of a large employee dismissal increases by 2.3 percentage points at $\tau = 0$ (and the number of employees shrinks by 7.2 percentage points at the maximum). The effect wanes one year after the shock: the coefficients on $\tau = +2$, $\tau = +3$, and $\tau > +3$ are all not significantly different from 0 in columns 4 and 5. The lack of an effect on employment is reflected in column 6 by no discernible change in labor productivity, as measured by log(Sales/Employees).[12]

[12]In Table 1.C.2 and Table 1.C.3 in the Appendix, we exclude the Drug (SIC 283) and Banking (SIC 602) sectors from the sample, respectively. In the first case, the concern is that Pharmaceutical Preparations (SIC 2834) is a broadly defined industry that saw many players affected by the vitamin cartel investigation in 1999, but includes firms not involved in the production of these compounds. In

The results in Table 1.5 indicate that the antitrust shocks cause EU firms to engage in restructuring of their labor force, but have no such effect on non-EU firms. As indicated in the text of the table, the coefficients are significantly different between the two sets of regressions. The effects are mostly temporary, which is consistent with the interpretation that firms probably engage in mass layoffs to adapt the composition of their labor force (firing, then hiring different types of employees), not necessarily to downsize. The differential findings for the EU and global sample indicate that the need for restructuring is greater in the EU sample than in the non-EU sample. Why is that the case? EU firms might be more inefficient or financially weaker than non-EU firms. This is consistent with the result we mentioned before that the antitrust investigation increases the probability of exit more significantly for EU as compared with non-EU firms. We will turn to this question in more detail in Section 1.4.

1.3.3 Impact on Capital

We then examine the effect of cartel investigations on fixed assets (capital). In Table 1.6 we uncover a small, negative effect on firms' investment decisions restricted to the EU sample. Column 1 shows that investment as a proportion of total assets decline by about 1 percentage point in the treated firms as compared with the control firms. The effect is economically significant as the average investment is 6% of total assets. The reduction in investment is detectable in the same year of the shock ($\tau = 0$), continues in the next year ($\tau = +1$) and survives also in the longer run ($\tau > +3$), more than 3 years after the shock.

In column 2, we follow Atanassov and Kim (2009a) and compute asset sales as a dummy variable that takes value one when the firm experiences a drop of more than 15% in net PPE over the previous one or two years. According to this variable, there is an increase by 5.6 percentage points in asset sales 3 years after the shock; asset sales stay 4 percentage points higher than the matching firms in the longer run ($\tau > +3$). The results on asset sales exhibit some pre-trend effects, as the coefficients on $\tau = -3$ and $\tau = -2$ are both negative and statistically different from zero. Thus, they should be interpreted with caution.

In column 3, we obtain findings that are more similar to column 1 when we use asset

the second case, the concern is that extensive regulation makes the Banking Sector, which is the largest in our sample, significantly different from other industries. Our results are unchanged when we exclude these sectors.

growth as the dependent variable. We find that net PPE in treated firms start shrinking at the time of the shock ($\tau = 0$) and shrinks the most (by $10 - 11$ percentage points, compared to control firms) two and three years after the cartel investigations. The long run effect is not statistically different from zero.

These results confirms the extent of restructuring activity happening in EU firms following the start of the cartel investigation. Conversely we find no significant effects on investment, asset sales and asset growth in the global sample as shown in columns 4, 5 and 6, respectively. As indicated in the text of the table, the coefficients are significantly different between the two sets of regressions. Why is that the case? We will turn to this question in Section 1.4.

1.3.4 Impact on Other Stakeholders

Part of the shortfall in profits could be covered by changes in the firm financing policy. Specifically, firms may transfer some of the effects of the antitrust shock onto shareholders by reducing their payouts or by raising debt (and thus increasing equity risk). Another way in which firms may counter the reduction in profitability is by managing working capital more effectively.

In Table 1.7, we report the estimated coefficients from specification (1.1) when the outcome variables are *Net Leverage* (measured as debt minus cash over total assets) in columns 1 and 4, *Shareholder Payout* (measured as cash dividends and share repurchases over sales) in columns 2 and 5, and *Working Capital* (measured as Account Receivable Days plus Inventory Days - Account Payable Days) in columns 3 and 6. As done before we present the results separately for the EU sample (in columns 1-3) and for the global sample (in columns 4-6).

In column 1, we find a small contemporaneous effect ($\tau = 0$) on net leverage (an increase by 2.8 percentage point relative to the control group) and a larger long-term effect ($\tau > 3$): an increase of 5.9 percentage point in net leverage. This is a sizable effect as the median value for net leverage is 11 percentage points in the EU sample. In column 2, we find no effects on shareholder payout at all. In both regressions, there is no sign of pre-treatment trends. In column 3, we find a sizeable reduction in the working capital at the time of the shock ($\tau = 0$) and lasting until two years after the shock ($\tau = +2$). The effect is large in magnitude as the reduction by 24 days in working capital is about half of the median size of the working capital in the EU sample. However, there are pre-trend concerns with this result as the coefficient on $\tau = -3$ is large and statistically different from zero.[13]

Taken together, these results indicate that EU firms increase leverage and become more efficient at managing their working capital as a result of the cartel investigation. To a large extent, shareholders are shielded from the effects of the cartel investigation.

Except for a small increase in net debt, we find that the cartel investigations have no effect on net leverage, shareholder payout, and working capital in the global sample. This raises the question of why there is such a difference in finding between EU and non-EU firms, in spite of a similar effect on profitability. To answer to this question we turn to a cross-sectional analysis of the global sample.

1.4 Cross-Sectional Results

So far, our analysis indicates that the cartel investigations represent a negative shock to profitability with real impacts on employment policies and investment decisions, which are concentrated in the sample of EU firms. In this section we take advantage of the cross-sectional differences to shed more light on the mechanism at work.

1.4.1 EU versus non-EU Firms

At first impression, the fact that EU firms are more affected by the EC cartel investigations is not surprising. After all, the European Commission is likely to target firms that have a bigger impact on EU markets, which are likely to be EU firms. However, the results on profitability in Table 1.3 indicate that the profits of EU and non-EU firms are affected in a similar way by the cartel investigations. So, it is difficult to argue that EU firms are more exposed to the shock than non-EU firms.

An alternative, complementary explanation for the difference between EU and non-EU firms is that the EU firms are competitively weaker than the non-EU firms. The story would be that the anti-competitive practices targeted by the cartel investigations might have helped EU firms hide their competitive weakness. Once those practices stop and the profit margins drop, weaker firms are struggling: either they drop from the market or they need to restructure to become more competitive.

[13] In Table 1.C.5 in the Appendix, we split Working Capital into its components. Although the significant changes are concentrated in the Inventory Days, the results are much weaker, which suggests that it is the overall effect that counts.

The argument is that pre-shock differences between EU and non-EU firms might be behind the different results found in Table 1.5 and Table 1.6.

1.4.2 Pre-Shock Labor Productivity

To shed more light on this hypothesis, in the global sample, we sort firms depending on their labor productivity before the shock (at $\tau = -1$ and $\tau = -2$): we label as productive the firms that have higher sales per employee than the industry median and unproductive the remaining ones.

In Table 1.8, we find that less productive firms are more likely to start collective dismissals, reduce employees and increase labor productivity. This result is in line with the hypothesis that cartel investigations are a catalyst for efficiency changes. Interestingly, we find that the ex-ante classification into high/low labor productivity has no significant effects on investment, asset sales and asset growth. This is what one might expect given that labor productivity is the source of inefficiency.

When we look at the relation between ex-ante labor productivity of EU/non-EU firms, we find that EU firms are relatively more likely than non-EU firms to have low labor productivity before the cartel investigation: 50.53% are labelled as unproductive for the EU firms compared to 47.65% for the non-EU firms. So, this pre-shock difference in labor productivity may explain the difference in results we uncover in Table 1.5 between the EU and global samples.

1.4.3 Pre-Shock Financial Strength

Another important source of cross-sectional heterogeneity is the financial strength of the firm before the shock. It stands to reason to expect that the consequences of a negative shock to profitability should be more severe for firms closer to financial distress: after all, they have less margin to maneuver.

To test this hypothesis, in Table 1.9, we estimate the effect of cartel investigations on *Mass Layoffs*, *log(Employees)*, *log(Sales/Employees)*, *Investment*, *Asset Sales*, and *Asset Growth*, separately for firms with an EBIT interest coverage ratio smaller or larger than 2 prior to the cartel investigations. The assumption is that firms with a smaller coverage ratio face a greater risk of financial distress.

When we sort firms in the global sample depending on their financial strength before the shock (at $\tau = -1$ and $\tau = -2$), we find that the reductions in investment and the increases in asset sales are concentrated among firms that are close to financial distress (i.e., have an EBIT interest coverage ration below 2) before the start of the cartel investigation. These are also firms that are more likely to engage in mass layoffs and reduce employees.

Exploring the relation between financial strength and geography (EU versus non-EU), we find that EU firms are relatively more likely than non-EU firms to fit in the financial distress group. The percentage of financially-distress firms is 32.16% for the EU sample and 30.46% for non-EU firms in the global sample.

Taken all together, these findings indicate that the likely difference in the impact of the cartel investigations between EU and non-EU firms is driven by pre-shock differences between these two sets of firms. EU firms were more likely to have lower labor productivity and lower financial strength than the non-EU firms, making them more reliant on the protection afforded by the cartel agreements.

1.5 Robustness Tests

In this section, we discuss the robustness tests which are reported in the Internet Appendix: we estimate logit models for the dependent variables that are binary; we exclude firms involved in M&A activity from the sample; we control for industry-level business cycles; we focus on a subset of investigations that explicitly mention price-fixing as their type of collusive agreement; and finally, we implement the alternative "interaction-weighted" (IW) method proposed by Sun and Abraham (2021a).

1.5.1 Logit Model

Mass Layoff and *Asset Sales* are dummy variables that take only values 0 and 1. Therefore, the use of linear regression models might be inappropriate, and a logit regression model might be preferable. In Table 1.D.1 of the Appendix, we present the results of estimating specification (1.1) with a logit model. The cost of adopting logit is that we cannot include country $\times$ year fixed effects across our specifications due to data limitations. As we believe that these controls are critical to our identification strategy, we used OLS in our baseline analysis and present the logit model only as a robustness check. Across all specifications, we control for firm, year and country fixed effects. We include firm-level controls (the logarithm of total assets) in columns 2, 4, 6, and 8.

The results in Table 1.D.1 confirm that the likelihood of mass layoffs and asset sales increases significantly following the cartel investigations in the treated (as compared to the control) group. The effect on mass layoffs is concentrated in the three years following the start of the investigation, as the coefficient on the long term binned-lag is not statistically significant. However, the effect on asset sales appears to be long lasting. Overall, the results in Table 1.D.1 confirm the findings of our baseline analysis.

1.5.2 M&A Activity

Prior work has shown that merger activity picks up following the passage of leniency programs that facilitate cartel investigations (Dong, Massa, and Žaldokas (2019)). This in turn may trigger changes in employment. We verify in the Appendix that our results on mass layoffs are independent of merger activity. In Table 1.D.2 we show that the basic results on employment do not change when we exclude all firms that engage in M&As, or we exclude only the companies that act as acquirers or as targets. As in the basic findings, the results are stronger in the EU sample, but to a weaker extent also apply to the global sample.

1.5.3 Industry Booms and Busts

One concern with our identification strategy is whether industry-level downturns may be driving applications for leniency (and the subsequent antitrust investigations) *and* firm restructurings. This would be the case if cartel members are more prone to defect from the cartel and apply for leniency when their industry is facing a bust, a time when the benefit of receiving immunity from the antitrust authority outweighs the potential profit from collusion. To address this issue we identify periods of industrial booms and busts following the method used in Braun and Larrain (2005) and Boutin, Cestone, Fumagalli, Pica, and Serrano-Velarde (2013). Table 1.D.3 reports the percentages of treated and control industries that are facing a downturn prior to cartel investigations starting. For treated industries, 26.3% are identified as experiencing a downturn one year before the shock, compared with 24.5% in control firms. The percentage of treated (control) industries in a bust increases to 31.6% (30.9%) when we extend the period to include two years before the cartel investigations. However, although treated firms are slightly *more* likely to experience a downturn before the investigation start, as compared with non-treated 4-digit industries in their same 3-digit sector, they are also slightly *more* likely to experience a boom (this is in line with the fact that treated firms have higher investment pre-event compared with controls). This mitigates in part the concern that industry downturns are driving antitrust investigations and mass layoffs. As a further check, we provide a robustness test excluding from the sample firms operating in industries that are identified as being in a downturn one year before the cartel investigation starts.

Colluding firms may be more inclined to apply for leniency and trigger an investigation when the industry they operate in is experiencing a downturn. As an industry-level downturn may also induce deteriorated performance and restructuring, our results may suffer from an omitted variable bias. To mitigate this concern, we follow Braun and Larrain (2005) and Boutin, Cestone, Fumagalli, Pica, and Serrano-Velarde (2013) and identify the boom and bust periods of each industry in our sample. We then exclude all the firms operating in industries that are in recession one year before the cartel investigation begins. We re-run the baseline analysis using this cleaned sample and report our results in Table 1.D.4.

Our findings are both qualitatively and quantitatively similar to the baseline results. Firms that operate in the affected industries undergo a deterioration in their performance and a reduction in their long-term investment. In response to this, firms actively seek labor restructuring in order to boost productivity. The pattern of employment reconfirms the short-term characteristics of the workforce restructuring. In contrast, firms limit the negative impact of antitrust scrutiny on shareholders and customers.

1.5.4 Price-Fixing Cartels

Cartels can be categorized in different types based on the collusive agreements between colluding firms. The most common cartel types include price-fixing, market-sharing, and bid-rigging, etc. Considering that firms employ different strategies when engaging in different types of cartels, cartel investigations may have heterogeneous effects on affected firms subject to the cartel types.

According to the disclosed information on cartel cases detected by the European Commission, a proportion of the cartels are not colluding in prices. For instance, in some cases, firms collude by allocating market shares or engaging in big rigging. As shown in our previous findings, firms carry out restructuring activities in response to the deteriorated performance associated with the cartel investigations. Following this line of argument, price-fixing cartels should provide a cleaner setting as firms' markup and profitability are directly impaired by the cartel investigations.

We conduct our analysis using only cartel cases that are labeled as price-fixing as a robustness check. Table 1.D.5 presents the results. Although our sample size is reduced by restricting our attention to this cartel type, the results remain consistent with our baseline findings. Firms that are hit by antitrust investigations on price-fixing adjust in ways that affect workers (and other suppliers of inputs), while limiting the negative effects on shareholders.

1.5.5 Interaction-Weighted (IW) Method

Finally, we report in the Appendix the estimates obtained by implementing the alternative "interaction-weighted" (IW) method proposed by Sun and Abraham (2021a), using the STATA package `eventstudyinteract`. The results are in Figures 1.D.1- 1.D.4. Across all specifications the findings are both qualitatively and quantitatively similar to our baseline analysis.

1.6 Conclusion

This paper investigates how antitrust action against cartels affects different stakeholders in firms operating in the cartelized industries. We focus on firms that operate in the same industry as the cartel members under investigation, and document that investigations lead to a temporary decline in their performance. The adverse effect on these (non-investigated) firms is likely a consequence of the increased competition in the industry due to the breakdown of the cartel and the enhanced antitrust scrutiny. We investigate how this adverse event changes firms' corporate and financial strategy. Our findings suggest that firms react by restructuring the labor force, and, to lesser extents, by selling assets and cutting investment.

Exploiting the differential timing of cartel investigations initiated by the European Commission within a difference-in-differences setup, we show that antitrust enforcement spurs a significant increase in mass layoffs among firms operating in the affected industry. The impact of antitrust investigations manifests immediately after the case opens and dies away after three years. This effect is noticeably more pronounced for EU firms compared with the non-EU ones. We show that restructuring is driven by the pursuit of efficiency as EU firms exhibit a lower level of pre-shock labor productivity. Similarly, EU firms engage in asset sales as opposed to the non-EU firms. We show that the asset restructuring is concentrated in financially-distressed firms, and that EU firms are relatively more likely than non-EU firms to fit in the financial distress group. In sum, antitrust shocks cause firms to engage in a sharp restructuring of the labor force but do not cause long term effects. The more inefficient firms (i.e. those less fit before the shock) are the more affected by this restructuring activity (i.e., they shape up or exit).

Our results suggest that antitrust action against anti-competitive infringements in product markets has spillovers on the labor market, by driving a rise in mass layoffs among firms in the industry. After starting cartel investigations in product markets, the authorities should be alert to possible changes in labor market power which may facilitate abuse of dominance and collusion to fix wages. This is especially important in view of recent calls for more antitrust action related to labor market abuse (see Marinescu and Posner (2019)).

Table 1.1. Description of cartel cases

This table provides details of the cartel cases in our matched global sample. The specific dates of opening investigations, cartel industries and firms under investigations can be found on the European Commission website. *Open Year* denotes the year when the cartel investigation was initiated. *Treated Firms* reports the number of firms in our matched global sample that operate in the investigated 4-digit SIC industries.

Industry Description	SIC	Open Year	Treated Firms
Food crops grown under cover	182	2011	6
Structural steel erection	1791	2002	8
Canned fruits and specialties	2033	2013	43
Prepared feeds, nec	2048	1996	27
Malt beverages	2082	2000	87
Flavoring extracts and syrups, nec	2087	1999	13
Thread mills	2284	2001	2
Bags: plastic, laminated, and coated	2673	2001	15
Alkalies and chlorine	2812	1999	1
Industrial gases	2813	1997	4
Pharmaceutical preparations	2834	1999	139
Soap and other detergents	2841	2008	36
Asphalt paving mixtures and blocks	2951	2002	1
Rubber and plastics hose and beltings	3052	2007	4
Vitreous plumbing fixtures	3261	2004	10
Abrasive products	3291	2010	15
Copper rolling and drawing	3351	2001	43
Metal doors, sash, and trim	3442	2007	27
Architectural metalwork	3446	1991	6
Elevators and moving stairways	3534	2004	14
Industrial trucks and tractors	3537	2010	26
Ball and roller bearings	3562	2011	35
Refrigeration and heating equipment	3585	2009	57
Lighting equipment, nec	3648	2012	31
Electron tubes	3671	2007	3
Semiconductors and related devices	3674	2002	144
Electronic capacitors	3675	2013	1
Storage batteries	3691	2012	89
Magnetic and optical recording media	3695	2009	5
Carbon paper and inked ribbons	3955	1996	2
Switching and terminal services	4013	2013	6
Natural gas transmission	4922	2006	35
Scrap and waste materials	5093	2012	6
Fresh fruits and vegetables	5148	2005	8
National commercial banks	6021	1999	434
Federal savings institutions	6035	1997	34
Motion picture and video production	7812	2002	10
Total			1427

Table 1.2. Descriptive statistics

This table shows the descriptive statistics for the matched sample: Panel A uses the EU sample where all the treated and control firms are EU firms, and Panel B uses the global sample. All variables are measured at $\tau = -1$, i.e. one year before the opening of an investigation in the treated industry. We divide the matched sample into two groups of firms: *Treated Firms* and *Control Firms*. *Treated Firms* are the firms whose core business is in the 4-digit SIC industries that experience a cartel investigation in our sample period. *Control Firms* are their matched never-treated counterparts. Our sample period spans from 1988 to 2019. The full sample on which we perform the matching consists of all firms in the 3-digit SIC industries that have at least one 4-digit SIC industry under cartel investigation. This table reports the descriptive statistics for all firm-level variables (after winsorization). Differences in means between the treated and controls and their corresponding p-values are presented in the last two columns.

	Descriptive Statistics									
	Treated Firms				Control Firms				Difference	
	Mean	Median	SD	N	Mean	Median	SD	N	Difference	P-Value
Panel A: EU Sample										
log(Total Assets)	20.070	19.673	2.571	440	20.097	19.455	2.804	440	-0.027	0.882
log(Sales)	19.193	18.886	2.456	437	19.196	19.219	2.293	436	-0.003	0.986
M/B Ratio	1.482	1.095	1.146	440	1.507	1.106	1.185	440	-0.025	0.753
ROA	0.014	0.020	0.093	440	0.011	0.011	0.087	440	0.003	0.625
EBITDA/Assets	0.083	0.093	0.112	392	0.075	0.067	0.110	394	0.008	0.332
Investment	0.060	0.044	0.063	404	0.041	0.024	0.050	407	0.019***	0.000
Asset Sales	0.145	0.000	0.353	420	0.211	0.000	0.409	431	-0.066**	0.012
Asset Growth	0.170	0.032	0.633	419	0.147	0.013	0.632	429	0.023	0.600
Net Leverage	0.088	0.112	0.288	337	0.103	0.123	0.257	290	-0.016	0.469
Shareholder Payout	0.027	0.011	0.055	437	0.025	0.010	0.046	436	0.003	0.432
Working Capital	24.665	48.490	158.758	293	110.701	94.487	95.087	246	-86.036***	0.000
Layoff	0.109	0.000	0.313	402	0.086	0.000	0.280	408	0.024	0.257
log(Employees)	6.746	6.512	2.201	415	6.985	7.019	1.987	419	-0.240*	0.099
	Mean	Median	SD	N	Mean	Median	SD	N	Difference	P-Value
Panel B: Global Sample										
log(Total Assets)	19.868	19.506	2.395	1351	19.727	19.381	2.331	1351	0.141	0.120
log(Sales)	18.863	18.779	2.083	1333	18.747	18.597	2.128	1336	0.117	0.152
M/B Ratio	1.537	1.098	1.240	1351	1.490	1.088	1.197	1351	0.047	0.316
ROA	0.010	0.013	0.096	1351	0.006	0.012	0.104	1351	0.005	0.240
EBITDA/Assets	0.066	0.061	0.114	1206	0.062	0.057	0.121	1218	0.004	0.415
Investment	0.042	0.021	0.057	1260	0.042	0.023	0.053	1270	0.000	0.834
Asset Sales	0.117	0.000	0.321	1242	0.128	0.000	0.334	1253	-0.011	0.404
Asset Growth	0.226	0.052	0.655	1237	0.198	0.059	0.573	1249	0.028	0.265
Net Leverage	0.048	0.086	0.321	903	0.047	0.083	0.320	906	0.002	0.917
Shareholder Payout	0.034	0.011	0.058	1333	0.030	0.011	0.053	1336	0.003	0.141
Working Capital	97.076	89.971	105.224	674	104.903	91.358	98.981	658	-7.827	0.162
Layoff	0.109	0.000	0.312	1046	0.111	0.000	0.314	1038	-0.002	0.895
log(Employees)	6.683	6.683	1.872	1134	6.693	6.545	1.957	1128	-0.010	0.897

Table 1.3. Impact of cartel investigations on profitability

This table reports the estimated coefficients from equation (1.1), focusing on the impact of cartel investigations on ROA (measured as net income before extraordinary items and preferred dividends/total assets) and EBITDA/Assets (measured as EBITDA over total assets). Columns (1)-(2) use the matched EU sample, and columns (3)-(4) use the matched global sample. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The coefficients are not significantly different between the EU and global samples. The regressions also include the logarithms of total assets as the firm-level control, firm fixed effects and country$\times$year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EU Sample		*Global Sample*	
	ROA	EBITDA/Assets	ROA	EBITDA/Assets
	(1)	(2)	(3)	(4)
Shock($\tau = -3$)	0.004	0.004	0.001	0.000
	(0.003)	(0.004)	(0.003)	(0.004)
Shock($\tau = -2$)	-0.005	-0.006	-0.001	-0.004
	(0.003)	(0.004)	(0.005)	(0.006)
Shock($\tau = 0$)	-0.011**	-0.010*	-0.012***	-0.015***
	(0.004)	(0.005)	(0.004)	(0.005)
Shock($\tau = +1$)	-0.013***	-0.010*	-0.010**	-0.014***
	(0.005)	(0.005)	(0.004)	(0.005)
Shock($\tau = +2$)	-0.006	-0.001	-0.007*	-0.009**
	(0.005)	(0.006)	(0.004)	(0.005)
Shock($\tau = +3$)	0.004	0.005	0.000	0.000
	(0.008)	(0.010)	(0.005)	(0.006)
Shock($\tau > +3$)	-0.002	0.002	-0.004	-0.003
	(0.006)	(0.007)	(0.003)	(0.005)
Observations	15749	14374	47939	44047
R^2	0.503	0.621	0.493	0.608
Control Variables	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes

Table 1.4. Impact of cartel investigations on customers

This table reports the estimated coefficients from equation (1.1), focusing on the impact of cartel investigations on customer ROA (measured as net income before extraordinary items and preferred dividends/total assets) and EBITDA/total assets. The treated and control customers are defined as the customers of the treated and control firms in our matched global sample using FactSet Revere. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the logarithms of total assets as the firm-level control, firm fixed effects and country$\times$year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	ROA	EBITDA/Assets
	(1)	(2)
Shock($\tau = -3$)	0.003	0.006
	(0.005)	(0.006)
Shock($\tau = -2$)	-0.007	-0.005
	(0.007)	(0.008)
Shock($\tau = 0$)	-0.005	-0.004
	(0.006)	(0.008)
Shock($\tau = +1$)	0.008	0.017**
	(0.006)	(0.007)
Shock($\tau = +2$)	0.004	0.005
	(0.006)	(0.006)
Shock($\tau = +3$)	0.013**	0.016**
	(0.006)	(0.007)
Shock($\tau > +3$)	0.004	0.005
	(0.007)	(0.008)
Observations	5601	5507
R^2	0.451	0.545
Control Variables	Yes	Yes
Firm FE	Yes	Yes
Country*Year FE	Yes	Yes

Table 1.5. Impact of cartel investigations on labor

This table reports the estimated coefficients from equation (1.1), focusing on the impact of cartel investigations on mass layoffs (measured as a dummy variable that identifies drops by more than 20% in the number of employees over the previous one or two years), firm employment (measured as the logarithm of the number of employees), and labor productivity (measured as the logarithm of sales per employee). Columns (1)-(3) use the matched EU sample, and columns (4)-(6) use the matched global sample, both of which exclude the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The coefficients are significantly different between the EU and global samples for *Mass Layoffs* and *log(Sales/Employees)*. The regressions also include the logarithms of total assets as the firm-level control, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EU Sample			*Global Sample*		
	Mass Layoffs	log(Employees)	log(Sales/Employees)	Mass Layoffs	log(Employees)	log(Sales/Employees)
	(1)	(2)	(3)	(4)	(5)	(6)
Shock($\tau = -3$)	-0.007	-0.045	0.064	-0.003	-0.015	0.002
	(0.016)	(0.035)	(0.042)	(0.010)	(0.022)	(0.022)
Shock($\tau = -2$)	0.027	-0.079	0.089	-0.005	-0.029	0.005
	(0.020)	(0.048)	(0.054)	(0.010)	(0.024)	(0.029)
Shock($\tau = 0$)	0.045**	-0.132***	0.095	0.023**	-0.072**	0.016
	(0.018)	(0.046)	(0.063)	(0.011)	(0.032)	(0.030)
Shock($\tau = +1$)	0.065***	-0.130**	0.116	0.018*	-0.055*	0.031
	(0.023)	(0.053)	(0.074)	(0.010)	(0.033)	(0.029)
Shock($\tau = +2$)	0.088***	-0.154***	0.167***	0.015	-0.062	0.047
	(0.025)	(0.052)	(0.061)	(0.012)	(0.040)	(0.036)
Shock($\tau = +3$)	0.079**	-0.150**	0.166**	0.006	-0.049	0.063
	(0.030)	(0.064)	(0.074)	(0.013)	(0.039)	(0.043)
Shock($\tau > +3$)	0.020	-0.107*	0.162*	0.005	-0.031	0.056
	(0.015)	(0.059)	(0.082)	(0.009)	(0.044)	(0.063)
Observations	14566	14744	14654	39345	40478	40299
R^2	0.235	0.964	0.844	0.247	0.957	0.855
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.6. Impact of cartel investigations on capital

This table reports the estimated coefficients from equation (1.1), focusing on the impact of cartel investigations on investment (measured as capex/total assets), asset sales (measured as a dummy variable that identifies a drop in net PPE greater than 15% over the previous one or two years), and asset growth (measured as the growth rate of net PPE). Columns (1)-(3) use the matched EU sample, and columns (4)-(6) use the matched global sample, both of which exclude the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The coefficients are significantly different between the EU and global samples for all the outcome variables. The regressions also include the logarithms of total assets as the firm-level control, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EU Sample			*Global Sample*		
	Investment	Asset Sales	Asset Growth	Investment	Asset Sales	Asset Growth
	(1)	(2)	(3)	(4)	(5)	(6)
Shock($\tau = -3$)	0.005	-0.037*	0.023	-0.002	-0.018**	-0.044**
	(0.004)	(0.020)	(0.035)	(0.002)	(0.008)	(0.019)
Shock($\tau = -2$)	0.001	-0.038*	0.045	-0.002	-0.016	-0.013
	(0.005)	(0.022)	(0.047)	(0.002)	(0.011)	(0.033)
Shock($\tau = 0$)	-0.006*	0.028	-0.059*	-0.002	0.012	-0.015
	(0.003)	(0.023)	(0.035)	(0.003)	(0.012)	(0.021)
Shock($\tau = +1$)	-0.007**	0.037	-0.082	-0.003	0.027	-0.024
	(0.003)	(0.023)	(0.051)	(0.005)	(0.017)	(0.027)
Shock($\tau = +2$)	-0.005*	0.060	-0.110***	-0.003	0.011	-0.030
	(0.003)	(0.041)	(0.034)	(0.004)	(0.019)	(0.026)
Shock($\tau = +3$)	-0.006*	0.056*	-0.102**	-0.003	-0.019	0.008
	(0.004)	(0.030)	(0.041)	(0.005)	(0.024)	(0.029)
Shock($\tau > +3$)	-0.011**	0.040*	-0.059	-0.003	0.005	-0.021
	(0.005)	(0.023)	(0.045)	(0.005)	(0.015)	(0.023)
Observations	14428	15640	15627	45088	47642	47613
R^2	0.582	0.247	0.222	0.565	0.242	0.233
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.7. Impact of cartel investigations on other stakeholders

This table reports the estimated coefficients from equation (1.1), focusing on the impact of cartel investigations on Net Leverage (measured as (debt - cash)/total assets), Shareholder Payout (measured as (cash dividends + share repurchases)/sales), and Working Capital (measured as AR Days + Inventory Days - AP Days). Columns (1)-(3) use the matched EU sample, and columns (4)-(6) use the matched global sample, both of which exclude the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The coefficients are significantly different between the EU and global samples for *Shareholder Payout* and *Working Capital.* The regressions also include the logarithms of total assets as the firm-level control, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EU Sample			*Global Sample*		
	Net Leverage	Shareholder Payout	Working Capital	Net Leverage	Shareholder Payout	Working Capital
	(1)	(2)	(3)	(4)	(5)	(6)
Shock($\tau = -3$)	-0.012	0.002	-18.985***	-0.008	-0.001	-2.130
	(0.012)	(0.003)	(6.708)	(0.008)	(0.002)	(2.530)
Shock($\tau = -2$)	-0.006	0.001	-4.272	-0.006	-0.001	3.356
	(0.016)	(0.003)	(5.620)	(0.011)	(0.002)	(2.525)
Shock($\tau = 0$)	0.019	0.001	-18.780***	0.002	-0.000	-0.880
	(0.018)	(0.003)	(6.914)	(0.011)	(0.003)	(2.987)
Shock($\tau = +1$)	0.028*	0.000	-20.314**	0.009	-0.003	-3.124
	(0.016)	(0.003)	(7.779)	(0.012)	(0.003)	(3.306)
Shock($\tau = +2$)	0.028	0.004	-24.480***	0.015	0.002	-1.199
	(0.019)	(0.003)	(8.216)	(0.011)	(0.003)	(3.389)
Shock($\tau = +3$)	0.036	-0.003	-9.736	0.020	0.003	-3.769
	(0.023)	(0.004)	(8.824)	(0.012)	(0.004)	(3.860)
Shock($\tau > +3$)	0.059***	0.001	0.887	0.027*	0.004	-4.697
	(0.022)	(0.005)	(10.408)	(0.015)	(0.004)	(3.495)
Observations	11133	15621	9773	31771	47648	26182
R^2	0.650	0.498	0.741	0.720	0.532	0.694
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.8. Differences in pre-shock labor productivity - global sample

This table reports the cross-sectional impact of cartel investigations depending on firm productivity using the matched global sample. We use firms' pre-shock productivity level to identify productive/unproductive firms. A firm is considered as productive if its averaged sales per employee over the two years prior to the cartel investigation is higher than its industry median. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The coefficients are significantly different between the productive and unproductive firms for all variables apart from *Investment* and *Asset Growth*. The regressions also include the logarithms of total assets as the firm-level control, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Mass Layoffs		log(Employees)		log(Sales/Employees)		Investment		Asset Sales		Asset Growth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Shock($\tau = -3$)	0.016	-0.010	0.016	-0.021	-0.033	0.027	0.003	-0.005**	-0.039**	0.009	-0.029	-0.050*
	(0.016)	(0.014)	(0.027)	(0.025)	(0.026)	(0.030)	(0.004)	(0.002)	(0.018)	(0.013)	(0.032)	(0.027)
Shock($\tau = -2$)	-0.005	0.001	0.016	-0.056*	-0.042	0.054	-0.000	-0.004*	-0.018	-0.011	0.026	-0.039
	(0.020)	(0.012)	(0.027)	(0.031)	(0.029)	(0.042)	(0.002)	(0.002)	(0.017)	(0.016)	(0.055)	(0.023)
Shock($\tau = 0$)	0.050**	0.004	-0.076*	-0.067	0.018	0.023	-0.003	-0.003	-0.025*	0.039*	-0.011	-0.016
	(0.022)	(0.014)	(0.041)	(0.047)	(0.025)	(0.048)	(0.003)	(0.004)	(0.015)	(0.024)	(0.028)	(0.027)
Shock($\tau = +1$)	0.023	0.017	-0.063	-0.040	0.036	0.036	-0.003	-0.003	-0.000	0.045*	-0.027	-0.017
	(0.020)	(0.013)	(0.042)	(0.043)	(0.032)	(0.042)	(0.006)	(0.004)	(0.024)	(0.027)	(0.040)	(0.027)
Shock($\tau = +2$)	0.014	0.012	-0.071	-0.029	0.071*	0.028	-0.003	-0.005	0.007	0.020	-0.001	-0.052*
	(0.021)	(0.015)	(0.047)	(0.043)	(0.040)	(0.048)	(0.006)	(0.004)	(0.025)	(0.024)	(0.039)	(0.027)
Shock($\tau = +3$)	0.007	0.002	-0.065	-0.015	0.111**	0.020	-0.001	-0.004	-0.015	-0.009	-0.007	0.008
	(0.023)	(0.014)	(0.049)	(0.043)	(0.043)	(0.057)	(0.006)	(0.004)	(0.029)	(0.026)	(0.040)	(0.029)
Shock($\tau > +3$)	0.003	0.010	-0.081	0.025	0.104	0.006	-0.004	-0.002	-0.009	0.018	-0.018	-0.028
	(0.012)	(0.013)	(0.055)	(0.051)	(0.075)	(0.076)	(0.006)	(0.005)	(0.020)	(0.017)	(0.029)	(0.025)
Observations	17874	18664	18357	18950	18311	18918	20133	18617	20850	20389	20839	20382
R^2	0.262	0.270	0.958	0.962	0.829	0.803	0.593	0.599	0.283	0.245	0.230	0.276
Productivity	Low	High	Low	High	Low	High	Low	High	Low	High	Low	High
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.9. Differences in pre-shock financial strength - global sample

This table reports the cross-sectional impact of cartel investigations depending on corporate financial distress using the matched global sample. We define a firm as financially-distressed if its EBIT interest coverage ratio is smaller than 2 over the two years prior to the cartel investigation. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The coefficients are significantly different between the financially-distressed and financially-sound firms for all variables apart from *log(Sales/Employees)*. The regressions also include the logarithms of total assets as the firm-level control, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Mass Layoffs		log(Employees)		log(Sales/Employees)		Investment		Asset Sales		Asset Growth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Shock($\tau = -3$)	-0.027	0.007	-0.070	0.010	0.043	-0.019	0.003	-0.003	-0.019	-0.025***	-0.004	-0.037**
	(0.027)	(0.010)	(0.052)	(0.022)	(0.032)	(0.019)	(0.005)	(0.002)	(0.029)	(0.008)	(0.064)	(0.017)
Shock($\tau = -2$)	0.025	-0.012	-0.119**	0.025	-0.011	0.005	-0.005	-0.001	0.055*	-0.030**	-0.038	-0.005
	(0.029)	(0.010)	(0.051)	(0.025)	(0.042)	(0.029)	(0.003)	(0.002)	(0.031)	(0.012)	(0.060)	(0.024)
Shock($\tau = 0$)	0.083**	0.004	-0.151**	-0.020	-0.040	0.022	-0.006	-0.000	0.072**	-0.026*	-0.076	0.026
	(0.035)	(0.011)	(0.064)	(0.031)	(0.053)	(0.033)	(0.004)	(0.003)	(0.028)	(0.014)	(0.046)	(0.017)
Shock($\tau = +1$)	0.051*	0.003	-0.115	-0.019	-0.004	0.036	-0.005	-0.001	0.085***	-0.012	-0.097**	0.023
	(0.028)	(0.012)	(0.069)	(0.034)	(0.052)	(0.029)	(0.005)	(0.004)	(0.025)	(0.013)	(0.041)	(0.027)
Shock($\tau = +2$)	0.032	0.013	-0.167*	-0.018	0.016	0.038	-0.004	-0.002	0.065**	-0.011	-0.101**	-0.001
	(0.026)	(0.011)	(0.085)	(0.039)	(0.080)	(0.034)	(0.004)	(0.005)	(0.029)	(0.015)	(0.051)	(0.016)
Shock($\tau = +3$)	0.023	0.012	-0.119	-0.042	0.027	0.063	-0.010**	-0.001	0.042	-0.030*	-0.070	0.028
	(0.038)	(0.012)	(0.076)	(0.040)	(0.070)	(0.040)	(0.005)	(0.004)	(0.040)	(0.016)	(0.057)	(0.018)
Shock($\tau > +3$)	-0.009	0.013	-0.037	-0.033	0.013	0.064	-0.000	-0.003	0.022	0.008	-0.031	-0.006
	(0.022)	(0.008)	(0.088)	(0.052)	(0.084)	(0.055)	(0.004)	(0.005)	(0.027)	(0.013)	(0.042)	(0.017)
Observations	8393	24811	8696	25478	8637	25463	9982	28226	10678	29666	10666	29656
R^2	0.316	0.235	0.934	0.962	0.839	0.872	0.497	0.591	0.293	0.226	0.339	0.214
Financially-Constrained	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Appendix

1.A Cartel Case Collection

The cartel cases used in this paper are documented on the European Commission website. See `https://ec.europa.eu/competition/cartels/cases/cases.html`. We collect all the cases that were closed between 2000 and 2019 as the EC website arranges the cases using the closing year rather than the opening year of the investigation. We use the opening year as the shock year. For each case, we read thoroughly through the decision files starting from the EC's *Summary decision*. When the information we need is not disclosed in the *Summary decision* file, we resort to the *Non-confidential version of the decision*, which is the detailed version of the *Summary decision*. From these sources, we collect data on when the investigation was started, the cartel firms, the cartel periods, the cartel industries, any immunity or fine reduction applications, and the granted cases of immunity and fine reductions.

The opening year of an investigation is the year when the EC first initiated the case. In many cases, the initiation is provoked by the leniency application submitted by one of the cartel firms. We take the year of the first-submitted leniency application as the opening year in those cases. Regarding the cartel firms, we note down all the legal entities (not just undertakings) and then assign them ISIN codes if they are public firms, or EIKON PermID if they are private firms. For cartel periods, given the complexity of the relations among all the legal entities and undertakings – some legal entities may be acquired by another parent company or merge with other legal entities during the collusion period – we take the collusion periods of the undertakings and assign them to their subsequent legal entities. Similarly, the application and granting of immunity or fine reduction are also documented at the undertaking level. As for the cartel industries, the EC discloses a three-digit or four-digit NACE Rev.2 code to each case. In order to merge this with our Worldscope dataset, we first manually search for the four-digit SIC codes based on the product description in each decision file, we then double check the accuracy of the SIC codes through the industry codes matching table. All the cases that we are uncertain about the industry codes are excluded from our sample.

Table 1.A.1. Description of cartel cases - EU sample

This table provides details of the EU cartel cases in our post-matching EU sample. The specific dates of opening investigations, cartel industries and firms under investigations can be found on the European Commission website. *Open Year* denotes the year when the cartel investigation was initiated. *Treated Firms* reports the number of firms in our matched sample that operate in the investigated 4-digit SIC industries.

Industry Description	SIC	Open Year	Treated Firms
Plastering, drywall, and insulation	1742	2001	1
Structural steel erection	1791	2002	1
Canned fruits and specialties	2033	2013	4
Prepared feeds, nec	2048	1996	5
Malt beverages	2082	2000	41
Flavoring extracts and syrups, nec	2087	1999	2
Bags: plastic, laminated, and coated	2673	2001	2
Industrial gases	2813	1997	4
Pharmaceutical preparations	2834	1999	46
Soap and other detergents	2841	2008	6
Petroleum refining	2911	2005	8
Rubber and plastics hose and beltings	3052	2007	3
Flat glass	3211	2005	1
Vitreous plumbing fixtures	3261	2004	6
Abrasive products	3291	2010	2
Copper rolling and drawing	3351	2001	6
Metal doors, sash, and trim	3442	2007	8
Architectural metalwork	3446	1991	2
Elevators and moving stairways	3534	2004	3
Industrial trucks and tractors	3537	2010	8
Ball and roller bearings	3562	2011	2
Refrigeration and heating equipment	3585	2009	7
Transformers, except electric	3612	2007	7
Lighting equipment, nec	3648	2012	4
Electron tubes	3671	2007	1
Semiconductors and related devices	3674	2002	24
Storage batteries	3691	2012	5
Switching and terminal services	4013	2013	1
Freight transportation arrangement	4731	2003	6
Electric services	4911	2012	87
Natural gas transmission	4922	2006	5
Water supply	4941	2008	14
Scrap and waste materials	5093	2012	1
Grocery stores	5411	2019	21
National commercial banks	6021	1999	86
Federal savings institutions	6035	1997	12
Motion picture and video production	7812	2002	27
Total			469

Table 1.A.2. Description of cartel cases - full sample

This table provides details of the EU cartel cases in our pre-matching sample. The specific dates of opening investigations, cartel industries and firms under investigations can be found on the European Commission website. *Open Year* denotes the year when the cartel investigation was initiated. *Treated Firms* reports the number of firms in our pre-matching sample that operate in the investigated 4-digit SIC industries.

Industry Description	SIC	Open Year	Treated Firms
Food crops grown under cover	182	2011	21
Beef cattle, except feedlots	212	2001	10
Plastering, drywall, and insulation	1742	2001	9
Structural steel erection	1791	2002	40
Canned fruits and specialties	2033	2013	70
Prepared feeds, nec	2048	1996	125
Malt beverages	2082	2000	231
Flavoring extracts and syrups, nec	2087	1999	28
Thread mills	2284	2001	8
Bags: plastic, laminated, and coated	2673	2001	61
Alkalies and chlorine	2812	1999	8
Industrial gases	2813	1997	87
Pharmaceutical preparations	2834	1999	1804
Soap and other detergents	2841	2008	70
Petroleum refining	2911	2005	211
Asphalt paving mixtures and blocks	2951	2002	16
Rubber and plastics hose and beltings	3052	2007	28
Flat glass	3211	2005	42
Vitreous plumbing fixtures	3261	2004	18
Abrasive products	3291	2010	36
Copper rolling and drawing	3351	2001	148
Metal doors, sash, and trim	3442	2007	52
Architectural metalwork	3446	1991	18
Elevators and moving stairways	3534	2004	41
Industrial trucks and tractors	3537	2010	49
Ball and roller bearings	3562	2011	70
Refrigeration and heating equipment	3585	2009	141
Transformers, except electric	3612	2007	134
Electric lamps	3641	2009	8
Lighting equipment, nec	3648	2012	77
Electron tubes	3671	2007	5
Semiconductors and related devices	3674	2002	1172
Electronic capacitors	3675	2013	13
Storage batteries	3691	2012	195
Magnetic and optical recording media	3695	2009	18
Carbon paper and inked ribbons	3955	1996	2
Fasteners, buttons, needles, and pins	3965	2001	12
Switching and terminal services	4013	2013	9
Freight transportation arrangement	4731	2003	118
Electric services	4911	2012	1384
Natural gas transmission	4922	2006	84
Water supply	4941	2008	192
Scrap and waste materials	5093	2012	22
Fresh fruits and vegetables	5148	2005	32
Grocery stores	5411	2019	386
National commercial banks	6021	1999	869
Federal savings institutions	6035	1997	637
Motion picture and video production	7812	2002	327
Total			9138

1.B Variable Definitions

Outcome Variables:

ROA: Net income before extraordinary items and preferred dividends divided by total assets.

EBITDA/Assets: EBITDA divided by total assets.

Net Leverage: Net debt (defined as debt minus cash and short-term investment) divided by total assets.

Shareholder Payout: The sum of cash dividends and share repurchases over sales.

Working Capital: The Working Capital is measured as *AR Days* + *Inventory Days* - *AP Days*, where *AR Days* is accounts receivable divided by sales times 365, *Inventory Days* is inventory divided by COGS times 365, and *AP Days* is accounts payable divided by COGS times 365.

Investment: A firm's CAPEX divided by its total assets.

Asset Sales: A dummy variable that equals 1 if a firm's net PPE experiences a drop of at least 15% over the last year or last two years, and 0 otherwise.

Asset Growth: The annual growth rate of a firm's net PPE.

Mass Layoffs: A dummy variable that equals 1 if a firm's number of employees drops by at least 20% over the previous year or previous two years, and 0 otherwise.

log(Employees): The log value of a firm's number of employees.

Sales per Employee: The log value of a firm's sales divided by the number of employee.

Control Variables:

Total Assets: The log value of a firm's total assets.

Interest Coverage: The ratio of EBIT divided by firms' total interest expense.

log(Sales): The natural logarithm of totals sales.

M/B ratio: The ratio of the market value to the book value of equity.

1.C Ancillary Results

Table 1.C.1. Impact of cartel investigations on firm exit - Cox hazard model

This table reports the estimated impact of cartel investigation on firm exit using the cox hazard model. Column (1) uses the matched EU sample, and column (2) uses the matched global sample, both of which exclude the investigated firms. The regressions also include the logarithms of total assets as the firm-level control, and country×year strata (in replace of the fixed effects). All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EU Sample	*Global Sample*
	Exit	
	(1)	(2)
Shock	1.066***	0.857***
	(0.288)	(0.201)
Observations	15801	48210
Control Variables	Yes	Yes
Country*Year Strata	Yes	Yes

Table 1.C.2. Impact of cartel investigations on labor - excluding pharmaceutical sector

This table reports the estimated coefficients from equation (1.1), focusing on the impact of cartel investigations on mass layoffs (measured as a dummy variable that identifies drops by more than 20% in the number of employees over the previous one or two years), firm employment (measured as the logarithm of the number of employees), and labor productivity (measured as the logarithm of sales per employee). We exclude all firms from the pharmaceutical sector (3-digit SIC code 283). Columns (1)-(3) use the matched EU sample, and columns (4)-(6) use the matched global sample, both of which exclude the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the logarithms of total assets as the firm-level control, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EU Sample			*Global Sample*		
	Mass Layoffs	log(Employees)	log(Sales/Employees)	Mass Layoffs	log(Employees)	log(Sales/Employees)
	(1)	(2)	(3)	(4)	(5)	(6)
Shock($\tau = -3$)	-0.014	-0.042	0.063	-0.002	-0.013	0.005
	(0.016)	(0.037)	(0.044)	(0.010)	(0.023)	(0.024)
Shock($\tau = -2$)	0.026	-0.075	0.086	-0.005	-0.032	0.010
	(0.022)	(0.050)	(0.056)	(0.011)	(0.026)	(0.031)
Shock($\tau = 0$)	0.049**	-0.137***	0.093	0.027**	-0.079**	0.021
	(0.019)	(0.047)	(0.066)	(0.012)	(0.035)	(0.029)
Shock($\tau = +1$)	0.067***	-0.136**	0.113	0.020*	-0.058*	0.040
	(0.024)	(0.055)	(0.079)	(0.010)	(0.035)	(0.027)
Shock($\tau = +2$)	0.092***	-0.162***	0.168**	0.014	-0.061	0.068*
	(0.027)	(0.055)	(0.065)	(0.012)	(0.040)	(0.036)
Shock($\tau = +3$)	0.095***	-0.156**	0.153*	0.008	-0.051	0.076*
	(0.028)	(0.068)	(0.077)	(0.014)	(0.039)	(0.039)
Shock($\tau > +3$)	0.018	-0.112*	0.162*	0.009	-0.040	0.071
	(0.016)	(0.064)	(0.087)	(0.009)	(0.044)	(0.048)
Observations	13640	13804	13737	35908	36989	36849
R^2	0.240	0.963	0.849	0.249	0.958	0.870
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.C.3. Impact of cartel investigations on labor - excluding banking sector

This table reports the estimated coefficients from equation (1.1), focusing on the impact of cartel investigations on mass layoffs (measured as a dummy variable that identifies drops by more than 20% in the number of employees over the previous one or two years), firm employment (measured as the logarithm of the number of employees), and labor productivity (measured as the logarithm of sales per employee). We exclude all firms from the banking sector (3-digit SIC code 602). Columns (1)-(3) use the matched EU sample, and columns (4)-(6) use the matched global sample, both of which exclude the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the logarithms of total assets as the firm-level control, firm fixed effects and country$\times$year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EU Sample			*Global Sample*		
	Mass Layoffs	log(Employees)	log(Sales/Employees)	Mass Layoffs	log(Employees)	log(Sales/Employees)
	(1)	(2)	(3)	(4)	(5)	(6)
Shock($\tau = -3$)	-0.002	-0.046	0.050	-0.009	-0.018	0.000
	(0.020)	(0.035)	(0.039)	(0.012)	(0.027)	(0.022)
Shock($\tau = -2$)	0.026	-0.074	0.064	-0.005	-0.039	0.002
	(0.024)	(0.051)	(0.042)	(0.014)	(0.031)	(0.035)
Shock($\tau = 0$)	0.038*	-0.121**	0.063	0.032**	-0.098**	0.016
	(0.022)	(0.049)	(0.050)	(0.013)	(0.038)	(0.034)
Shock($\tau = +1$)	0.068**	-0.140***	0.083	0.023	-0.086**	0.015
	(0.028)	(0.053)	(0.058)	(0.014)	(0.038)	(0.038)
Shock($\tau = +2$)	0.097***	-0.166***	0.127**	0.028**	-0.112***	0.031
	(0.032)	(0.055)	(0.049)	(0.011)	(0.041)	(0.043)
Shock($\tau = +3$)	0.076**	-0.157**	0.119**	0.017	-0.093**	0.050
	(0.036)	(0.070)	(0.052)	(0.016)	(0.042)	(0.049)
Shock($\tau > +3$)	0.016	-0.105	0.078	0.012	-0.082	0.066
	(0.019)	(0.070)	(0.054)	(0.012)	(0.053)	(0.062)
Observations	10529	10679	10620	25850	26716	26631
R^2	0.242	0.962	0.841	0.255	0.945	0.848
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.C.4. Impact of cartel investigations on labor - pre-matching full sample

This table reports the estimated coefficients from equation (1.1), focusing on the impact of cartel investigations on mass layoffs (measured as a dummy variable that identifies drops by more than 20% in the number of employees over the previous one or two years), firm employment (measured as the logarithm of the number of employees), and labor productivity (measured as the logarithm of sales per employee). We use the pre-matching full sample where all firms in the same 3-digit SIC industry as the 4-digit investigated one are included as controls. Columns (1)-(3) use the EU sample, and columns (4)-(6) use the global sample, both of which exclude the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the logarithms of total assets as the firm-level control, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EU Sample			*Global Sample*		
	Mass Layoffs	log(Employees)	log(Sales/Employees)	Mass Layoffs	log(Employees)	log(Sales/Employees)
	(1)	(2)	(3)	(4)	(5)	(6)
Shock($\tau = -3$)	-0.015	-0.024	0.040	-0.012	-0.018	0.038
	(0.013)	(0.030)	(0.042)	(0.009)	(0.019)	(0.028)
Shock($\tau = -2$)	0.015	-0.058	0.060	-0.002	-0.034	0.044
	(0.017)	(0.043)	(0.050)	(0.006)	(0.032)	(0.034)
Shock($\tau = 0$)	0.030	-0.094**	0.079	0.001	-0.055	0.055
	(0.019)	(0.041)	(0.053)	(0.007)	(0.035)	(0.038)
Shock($\tau = +1$)	0.041*	-0.106**	0.118*	0.001	-0.078**	0.093**
	(0.022)	(0.046)	(0.063)	(0.006)	(0.035)	(0.038)
Shock($\tau = +2$)	0.061***	-0.120***	0.153***	0.004	-0.079*	0.100**
	(0.023)	(0.045)	(0.048)	(0.007)	(0.041)	(0.038)
Shock($\tau = +3$)	0.044	-0.096*	0.146**	-0.009	-0.072	0.088*
	(0.029)	(0.057)	(0.063)	(0.008)	(0.046)	(0.045)
Shock($\tau > +3$)	0.013	-0.047	0.116*	-0.004	-0.029	0.073
	(0.013)	(0.053)	(0.069)	(0.005)	(0.050)	(0.059)
Observations	22074	22416	22300	129180	134188	133661
R^2	0.231	0.965	0.853	0.245	0.958	0.869
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.C.5. Impact of cartel investigations on working capital

This table reports the estimated coefficients from equation (1.1), focusing on the impact of cartel investigations on firms' working capital management, measured as AP days (measured as accounts payable×365/COGS), AR days (measured as accounts receivable×365/Sales), and inventory days (measured as inventory×365/COGS). Columns (1)-(3) use the matched EU sample, and columns (4)-(6) use the matched global sample, both of which exclude the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the logarithms of total assets as the firm-level control, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EU Sample			*Global Sample*		
	AP Days	AR Days	Inventory Days	AP Days	AR Days	Inventory Days
	(1)	(2)	(3)	(4)	(5)	(6)
Shock($\tau = -3$)	3.541	-0.416	-3.900*	-1.750	-0.390	-3.290*
	(2.974)	(1.656)	(2.220)	(1.859)	(0.986)	(1.925)
Shock($\tau = -2$)	0.427	0.969	-3.382	-1.267	0.418	0.692
	(3.246)	(1.179)	(2.242)	(1.309)	(1.258)	(1.651)
Shock($\tau = 0$)	4.870	-0.090	-2.227	0.781	-1.573	0.067
	(3.020)	(1.807)	(3.409)	(2.434)	(1.555)	(2.236)
Shock($\tau = +1$)	3.260	-1.137	-4.123	-1.479	-1.994	0.511
	(3.442)	(2.410)	(3.723)	(2.214)	(1.848)	(1.936)
Shock($\tau = +2$)	2.823	-3.720	-8.254**	-0.220	-2.549	1.501
	(3.515)	(2.492)	(3.921)	(1.541)	(1.956)	(2.276)
Shock($\tau = +3$)	-1.151	-1.870	-4.554	-0.562	-1.573	1.199
	(3.050)	(2.708)	(4.336)	(1.608)	(1.962)	(2.346)
Shock($\tau > +3$)	-0.853	-2.041	-0.179	-0.477	-4.757*	0.252
	(6.707)	(2.749)	(6.305)	(2.321)	(2.406)	(2.942)
Observations	10421	9943	10669	30727	26439	31277
R^2	0.716	0.708	0.780	0.643	0.750	0.732
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes

1.D Robustness tests

Table 1.D.1. Logit Estimates

This table reports the estimated coefficients from equation (1.1) obtained with logit regressions, using the *Mass Layoff* and *Asset Sales* indicators as the outcome. Columns (1)-(4) use the matched EU sample, and columns (5)-(8) use the matched global sample, both of which exclude the investigated firms. We control for firm, year and country fixed effects. The natural logarithm of total assets is included as firm controls in column 2, 4, 6, and 8. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EU Sample				*Global Sample*			
	Mass Layoffs		Asset Sales		Mass Layoffs		Asset Sales	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
main								
Shock($\tau = -3$)	-0.134	-0.130	-0.297	-0.287	-0.059	-0.058	-0.187	-0.230*
	(0.227)	(0.228)	(0.187)	(0.188)	(0.150)	(0.154)	(0.119)	(0.122)
Shock($\tau = -2$)	0.295	0.301	-0.355**	-0.357*	-0.034	-0.057	-0.173	-0.190*
	(0.203)	(0.208)	(0.180)	(0.183)	(0.146)	(0.149)	(0.113)	(0.114)
Shock($\tau = 0$)	0.468**	0.502**	0.195	0.205	0.220*	0.242*	0.045	0.054
	(0.201)	(0.201)	(0.168)	(0.168)	(0.132)	(0.133)	(0.105)	(0.105)
Shock($\tau = +1$)	0.711***	0.674***	0.280*	0.245	0.243*	0.155	0.322***	0.268***
	(0.206)	(0.212)	(0.170)	(0.173)	(0.138)	(0.144)	(0.102)	(0.104)
Shock($\tau = +2$)	0.892***	0.850***	0.525***	0.448**	0.207	0.179	0.243**	0.173
	(0.206)	(0.212)	(0.172)	(0.177)	(0.141)	(0.145)	(0.105)	(0.108)
Shock($\tau = +3$)	0.831***	0.832***	0.410**	0.364**	0.092	0.088	-0.032	-0.059
	(0.216)	(0.218)	(0.178)	(0.180)	(0.151)	(0.154)	(0.113)	(0.114)
Shock($\tau > +3$)	0.169	0.180	0.165	0.142	0.157	0.114	0.174**	0.141*
	(0.161)	(0.164)	(0.122)	(0.124)	(0.105)	(0.107)	(0.075)	(0.076)
Observations	8355	8213	12920	12769	20580	20105	34603	34016
R^2	0.030	0.029	0.042	0.042	0.015	0.015	0.026	0.027
Firm-Level Controls	No	Yes	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.D.2. Excluding M&As

This table reports the coefficients from equation (1.1) using the matched sample but excluding firms that engage in M&As after the cartel investigations. Columns (1)-(6) use the matched EU sample, and columns (7)-(12) use the matched global sample, both of which exclude the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the logarithms of total assets as the firm-level control, firm fixed effects and country$\times$year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EU Sample						*Global Sample*					
	Excl. M&As		*Excl. Acquirors*		*Excl. Targets*		*Excl. M&As*		*Excl. Acquirors*		*Excl. Targets*	
	Mass Layoffs	log(Employees)	Mass Layoffs	log(Employees)	Mass Layoffs	log(Employees)	Mass Layoffs	log(Employees)	Mass Layoffs	log(Employees)	Mass Layoffs	log(Employees)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Shock($\tau = -3$)	-0.033	-0.024	-0.010	-0.055	-0.025	-0.020	-0.015	0.008	-0.010	-0.014	-0.009	0.006
	(0.020)	(0.045)	(0.021)	(0.039)	(0.015)	(0.038)	(0.013)	(0.021)	(0.011)	(0.023)	(0.011)	(0.020)
Shock($\tau = -2$)	0.014	-0.064	0.016	-0.095*	0.025	-0.056	-0.005	0.003	-0.005	-0.021	-0.008	-0.001
	(0.026)	(0.061)	(0.024)	(0.052)	(0.020)	(0.052)	(0.014)	(0.025)	(0.009)	(0.026)	(0.013)	(0.024)
Shock($\tau = 0$)	0.041*	-0.133**	0.026	-0.148***	0.055**	-0.127**	0.027*	-0.053*	0.023**	-0.066**	0.029**	-0.054*
	(0.024)	(0.054)	(0.020)	(0.047)	(0.022)	(0.050)	(0.015)	(0.031)	(0.011)	(0.032)	(0.013)	(0.031)
Shock($\tau = +1$)	0.047*	-0.124*	0.040*	-0.150***	0.070**	-0.125**	0.024	-0.032	0.019	-0.042	0.024*	-0.039
	(0.028)	(0.070)	(0.024)	(0.054)	(0.027)	(0.062)	(0.016)	(0.037)	(0.012)	(0.034)	(0.015)	(0.038)
Shock($\tau = +2$)	0.094***	-0.159**	0.088***	-0.177***	0.088***	-0.153**	0.024*	-0.059	0.014	-0.053	0.024*	-0.064
	(0.032)	(0.068)	(0.028)	(0.052)	(0.031)	(0.061)	(0.013)	(0.043)	(0.012)	(0.043)	(0.013)	(0.044)
Shock($\tau = +3$)	0.118***	-0.147*	0.088***	-0.156**	0.105***	-0.165**	0.019	-0.053	0.011	-0.033	0.017	-0.064
	(0.038)	(0.086)	(0.032)	(0.066)	(0.037)	(0.077)	(0.018)	(0.044)	(0.013)	(0.040)	(0.017)	(0.046)
Shock($\tau > +3$)	0.018	-0.115	0.007	-0.101	0.028	-0.134	0.002	-0.052	0.003	-0.014	0.007	-0.062
	(0.024)	(0.091)	(0.017)	(0.064)	(0.023)	(0.083)	(0.012)	(0.051)	(0.010)	(0.045)	(0.011)	(0.056)
Observations	9525	9669	13031	13195	10522	10674	23426	24259	34405	35441	26392	27283
R^2	0.260	0.960	0.244	0.965	0.252	0.960	0.279	0.957	0.251	0.957	0.277	0.956
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.D.3. Boom & bust industries before the cartel Investigations

This table reports the percentage of industries that are in boom or bust before the cartel investigations begin, for both the treated and the control firms in our matched global sample. We identify periods of industrial booms and busts following the method used in Braun and Larrain (2005) and Boutin, Cestone, Fumagalli, Pica, and Serrano-Velarde (2013). Booms and busts are identified from the fluctuations of industry sales based on a peak-to-trough criterion. We first estimate the cyclical component of the industry-level sales as a proxy for the state of industry, where the cyclical industry sales is measured as the difference between the actual sales and a trend computed using a Hodrick-Prescott filter with a smoothing parameter of 100. A trough occurs when the log of industry sales is below the trend by more than one standard deviation, where the standard deviation is calculated using the cyclical industry sales. For each trough, we go back in time until we find a local peak, which is defined as the closest preceding year in which cyclical industry sales is higher than in both the previous and posterior years. A bust goes from the year after the local peak to the year of the trough. Similarly, a peak occurs when the cyclical industry sales is more than one standard deviation above zero. Once a peak is identified, we go back in time until we find a local trough (the closest proceeding year in which the cyclical industry sales in lower than both the previous and posterior year). The boom goes from the year after the local trough until the year of the peak.

	Percentage of Industries in Booms and Busts	
	Treated Firms	Control Firms
1 Year before Cartel Investigations (Bust)	26.3%	24.5%
1 Year before Cartel Investigations (Boom)	39.5%	32.7%
1-2 Years before Cartel Investigations (Bust)	31.6%	30.9%
1-2 Years before Cartel Investigations (Boom)	44.7%	42.7%

Table 1.D.4. Excluding busting industries

This table reports the coefficients from equation (1.1) using the matched sample, where all the industries that are in a recession one year before the cartel investigations begin are excluded. Columns (1)-(3) use the matched EU sample, and columns (4)-(6) use the matched global sample, both of which exclude the investigated firms. The regressions include the logarithms of total assets as the firm-level control. We control for firm, country*year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EU Sample			*Global Sample*		
	Mass Layoffs	log(Employees)	log(Sales/Employees)	Mass Layoffs	log(Employees)	log(Sales/Employees)
	(1)	(2)	(3)	(4)	(5)	(6)
Shock($\tau = -3$)	-0.031*	-0.047	0.077	0.000	-0.016	-0.000
	(0.016)	(0.041)	(0.048)	(0.012)	(0.031)	(0.026)
Shock($\tau = -2$)	0.008	-0.084	0.111*	0.002	-0.015	-0.018
	(0.028)	(0.054)	(0.064)	(0.015)	(0.030)	(0.029)
Shock($\tau = 0$)	0.039*	-0.134***	0.117	0.022	-0.066*	0.021
	(0.022)	(0.048)	(0.073)	(0.015)	(0.037)	(0.033)
Shock($\tau = +1$)	0.073**	-0.150**	0.152	0.021*	-0.047	0.032
	(0.030)	(0.063)	(0.093)	(0.012)	(0.038)	(0.029)
Shock($\tau = +2$)	0.086**	-0.181***	0.182**	0.003	-0.071	0.057
	(0.034)	(0.060)	(0.080)	(0.013)	(0.043)	(0.038)
Shock($\tau = +3$)	0.096***	-0.191***	0.167*	-0.009	-0.047	0.050
	(0.032)	(0.070)	(0.089)	(0.019)	(0.041)	(0.042)
Shock($\tau > +3$)	0.032**	-0.200***	0.206**	0.016	-0.023	0.007
	(0.014)	(0.059)	(0.100)	(0.011)	(0.047)	(0.043)
Observations	11357	11491	11428	25308	26090	25947
R^2	0.255	0.966	0.858	0.274	0.961	0.880
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes

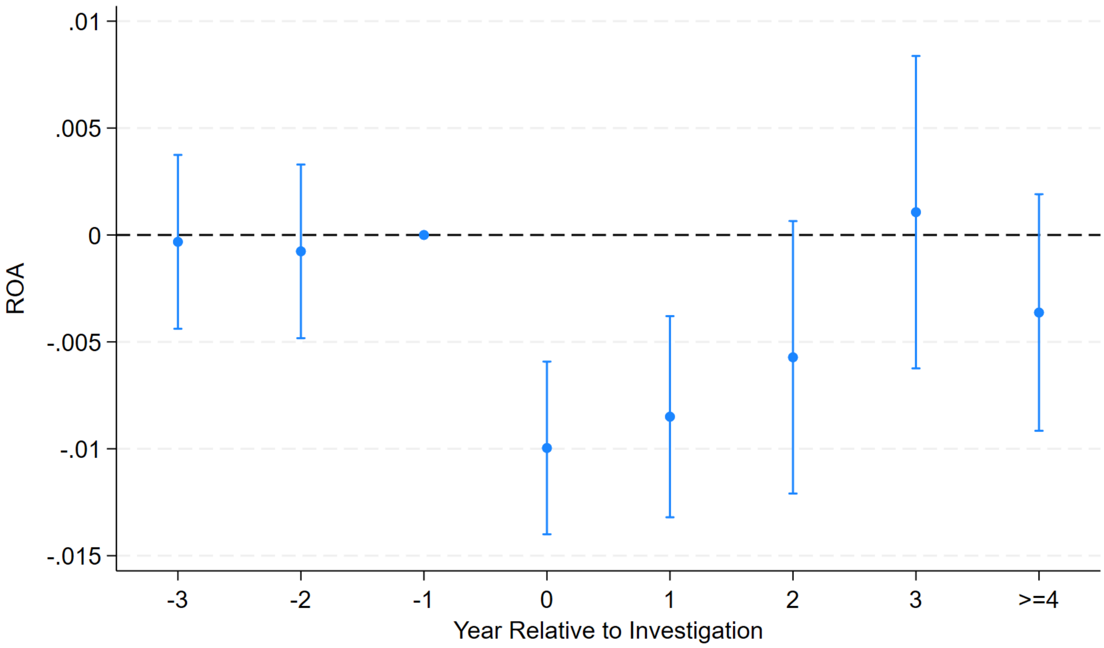
Table 1.D.5. Price-fixing cartels

This table reports the coefficients from equation (1.1) using the matched sample but including only the price-fixing cartels. Columns (1)-(3) use the matched EU sample, and columns (4)-(6) use the matched global sample, both of which exclude the investigated firms. We run the regressions allowing for leads and lags τ of the shock indicator (which is a dummy variable that identifies the cartel investigation event): we include each indicator $\tau \in [-3, +3]$, with the exception of $\tau = -1$ (which is the reference year), and $\tau > +3$ (to capture the long-run effects). The regressions also include the logarithms of total assets as the firm-level control, firm fixed effects and country×year fixed effects. All standard errors are clustered at 4-digit code SIC industry level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	EU Sample			*Global Sample*		
	Mass Layoffs	log(Employees)	log(Sales/Employees)	Mass Layoffs	log(Employees)	log(Sales/Employees)
	(1)	(2)	(3)	(4)	(5)	(6)
Shock($\tau = -3$)	-0.014	-0.021	0.024	-0.002	-0.016	0.013
	(0.019)	(0.042)	(0.035)	(0.010)	(0.023)	(0.022)
Shock($\tau = -2$)	-0.002	-0.023	0.036	-0.006	-0.032	0.013
	(0.020)	(0.042)	(0.041)	(0.009)	(0.026)	(0.030)
Shock($\tau = 0$)	0.046*	-0.109**	0.043	0.018*	-0.081**	0.021
	(0.025)	(0.052)	(0.051)	(0.010)	(0.033)	(0.028)
Shock($\tau = +1$)	0.058*	-0.108*	0.045	0.023**	-0.069*	0.046
	(0.031)	(0.061)	(0.058)	(0.011)	(0.035)	(0.028)
Shock($\tau = +2$)	0.079**	-0.135**	0.127**	0.021	-0.075*	0.057*
	(0.030)	(0.060)	(0.061)	(0.013)	(0.043)	(0.034)
Shock($\tau = +3$)	0.043	-0.097	0.098	0.013	-0.055	0.073*
	(0.027)	(0.066)	(0.078)	(0.013)	(0.042)	(0.041)
Shock($\tau > +3$)	0.014	-0.050	0.118	0.007	-0.052	0.081
	(0.019)	(0.070)	(0.081)	(0.010)	(0.048)	(0.063)
Observations	10071	10196	10121	35224	36238	36058
R^2	0.238	0.967	0.850	0.255	0.960	0.854
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Country*Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Figure 1.D.1: The impact of cartel investigations on performance: IW estimates

The figure plots the IW estimates for each relative time period, obtained implementing Sun and Abraham (2021a) "interaction weighted" estimator, together with 95% confidence intervals. We use the matched global sample and control for firm and country $\times$ year fixed effects as well as firm-level controls. The top panel reports estimates for ROA, and the bottom panel for $EBITDA/Assets$.



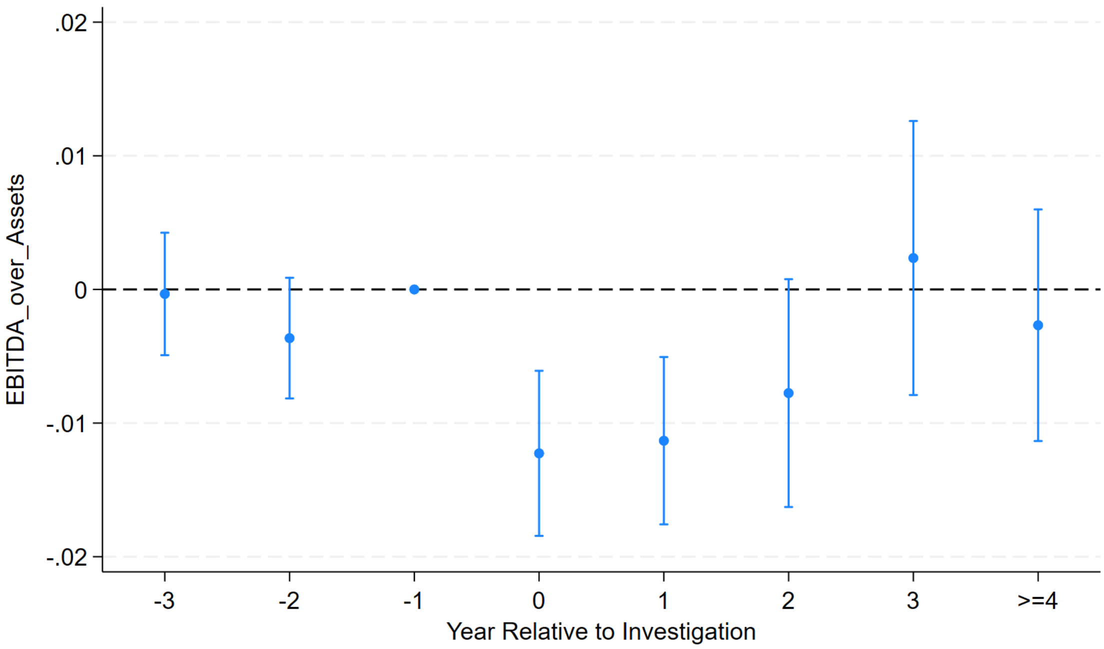


Figure 1.D.2: The impact of cartel investigations on labor: IW estimates

The figure plots the IW estimates for each relative time period, obtained implementing Sun and Abraham (2021a) "interaction weighted" estimator, together with 95% confidence intervals. The top panel reports estimates for *Mass Layoffs*; the middle panel for *Employment*; and the bottom panel for *Productivity*. We use the matched global sample and control for firm and country × year fixed effects as well as firm-level controls.

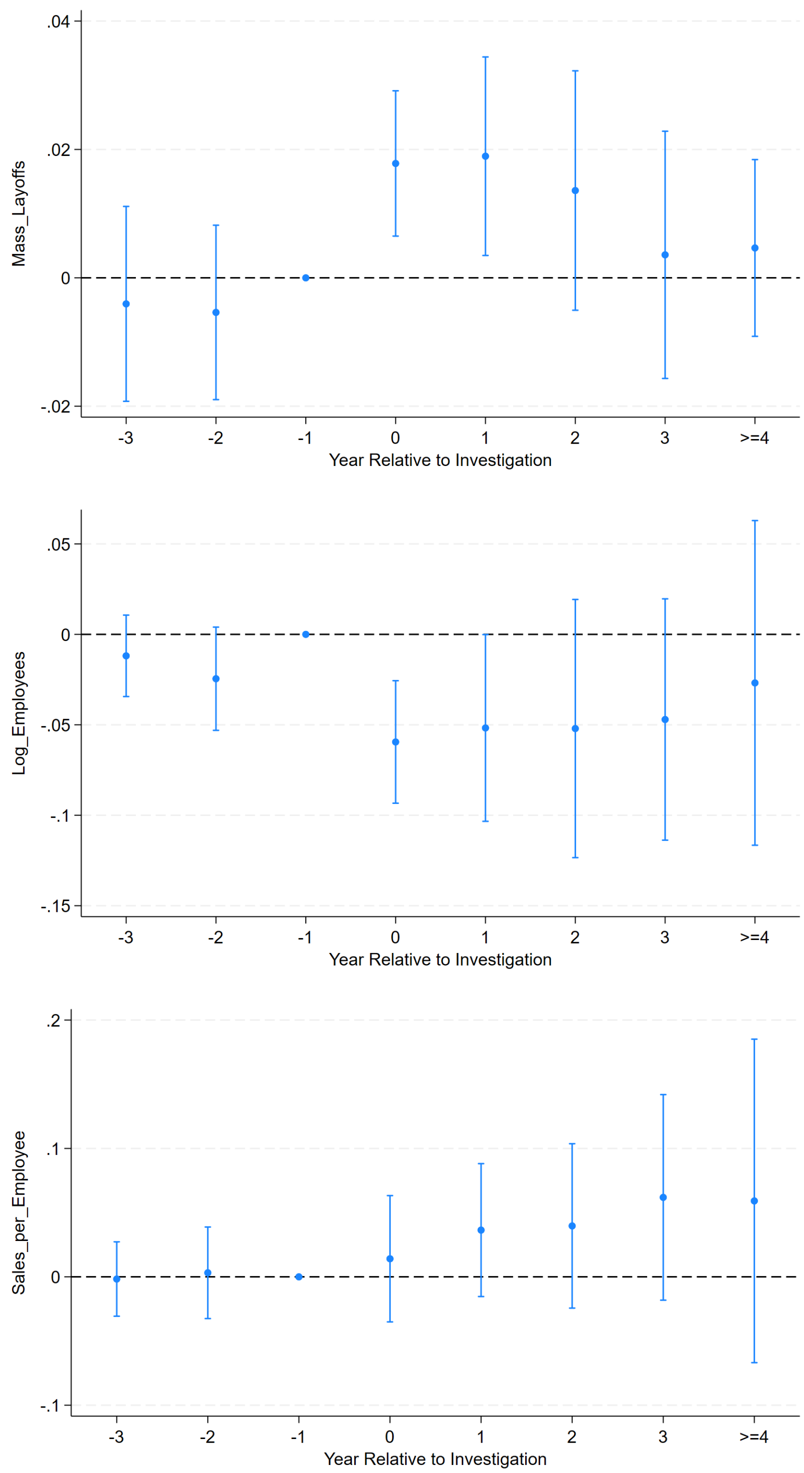


Figure 1.D.3: The impact of cartel investigations on investment: IW estimates

The figure plots the IW estimates for each relative time period, obtained implementing Sun and Abraham (2021a) "interaction weighted" estimator, together with 95% confidence intervals. The top panel reports estimates for *Investment*; the middle panel for *Asset Sales*; and the bottom panel for *Asset Growth*. We use the matched global sample and control for firm and country × year fixed effects as well as firm-level controls.

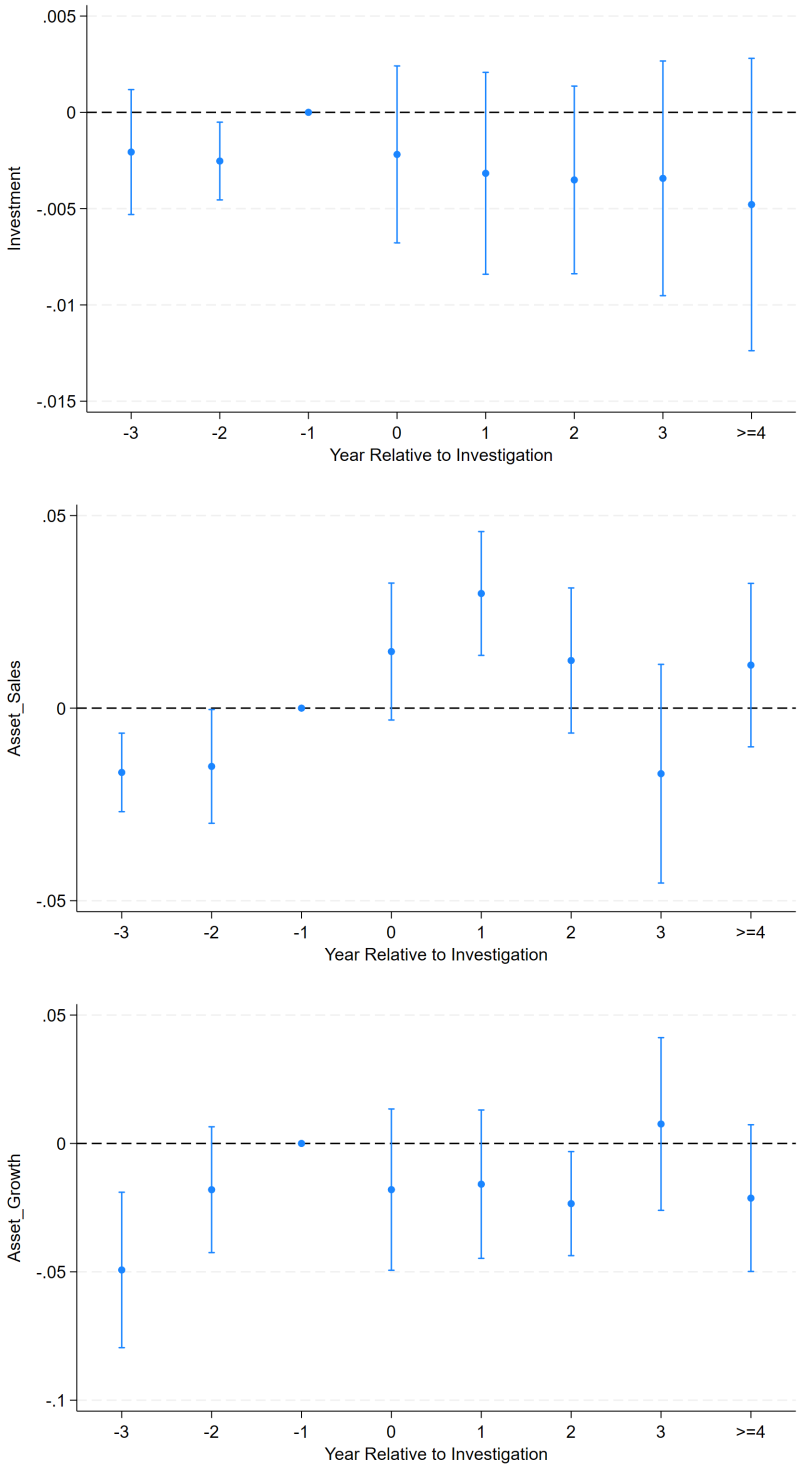


Figure 1.D.4: The impact of cartel investigations on working capital: IW estimates

The figure plots the IW estimates for each relative time period, obtained implementing Sun and Abraham (2021a) "interaction weighted" estimator, together with 95% confidence intervals. The top panel reports estimates for *AP Days*; the middle panel for *AR Days*; and the bottom panel for *Inventory Days*. We use the matched global sample and control for firm and country × year fixed effects as well as firm-level controls.

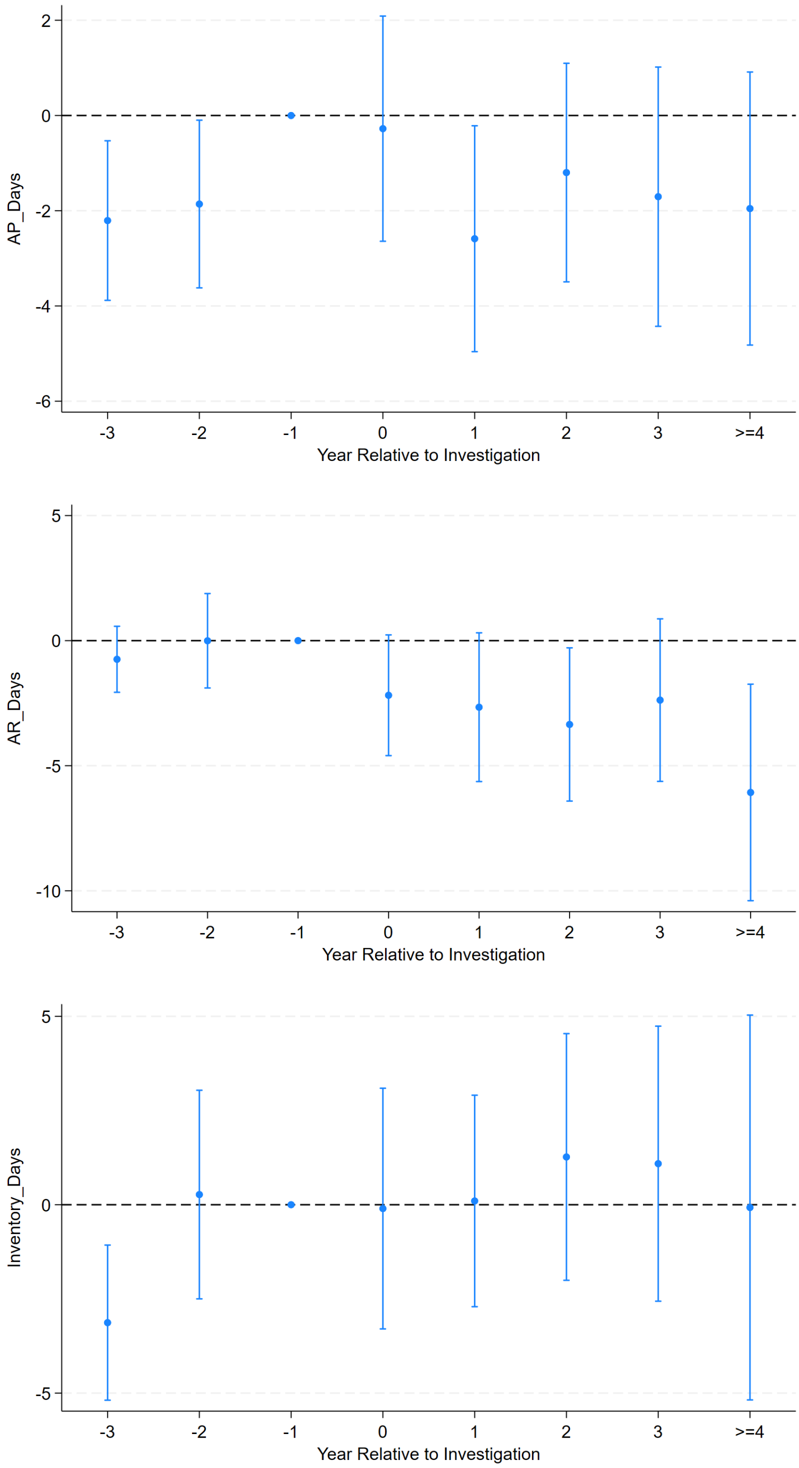
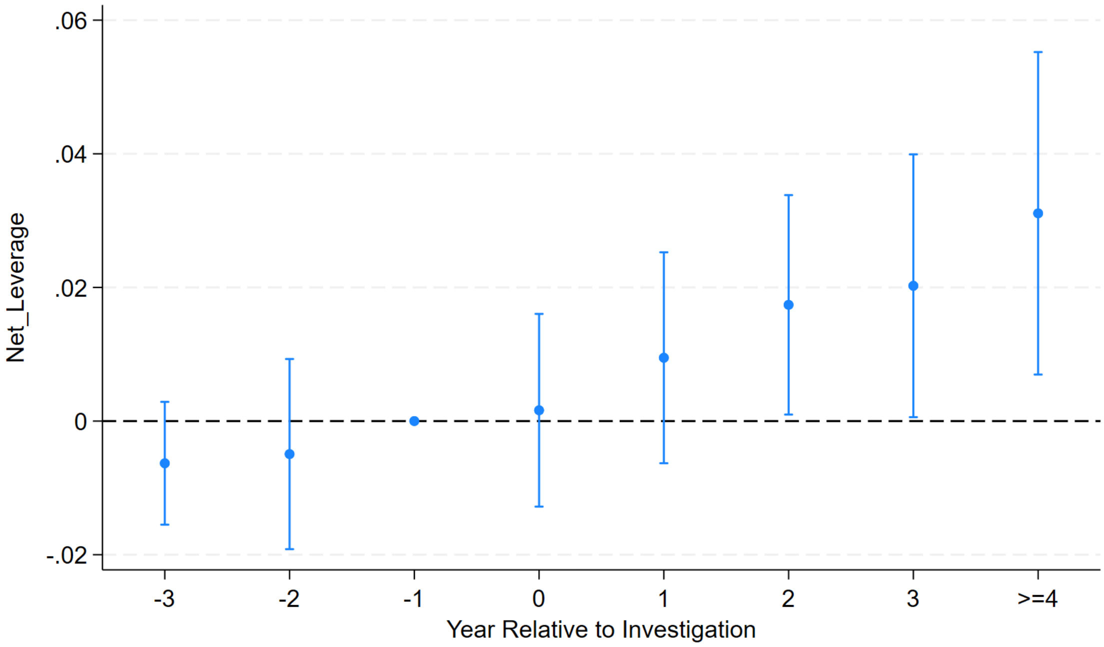
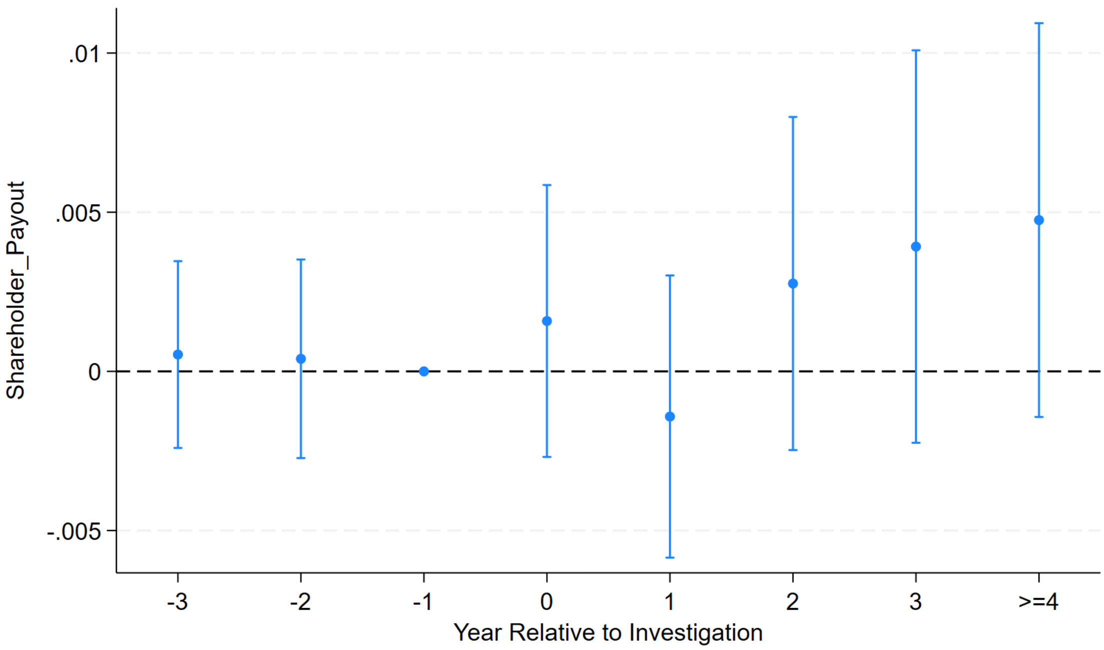


Figure 1.D.5: The impact of cartel investigations on financing: IW estimates

The figure plots the IW estimates for each relative time period, obtained implementing Sun and Abraham (2021a) "interaction weighted" estimator, together with 95% confidence intervals. The top panel reports estimates for *Net Leverage*; and the bottom panel for *Shareholder Payout*. We use the matched global sample and control for firm and country × year fixed effects as well as firm-level controls.





Chapter 2

Litigation Risk and Employment: Evidence from the Universal Demand Laws

2.1 Introduction

Apart from "voice" and "exit", shareholder litigation is often used as a third approach to mitigate agency conflicts. In particular, shareholder litigation protects shareholder interests by enforcing the fiduciary duties of directors and officers through legal proceedings. As the interests of shareholders and other stakeholders may not always align, this leads to a natural question on whether and how shareholder protection affects firms' strategies towards other stakeholders, which is of key importance given the increasing awareness of the stakeholder society concept (Tirole (2001)).

To answer this question, this paper focuses on one important corporate stakeholder, labor, and studies how weakened shareholder litigation rights affect the size and structure of a firm's labor force. On the one hand, low litigation risk may motivate managers to implement pro-employee policies at the expense of shareholders. On the other hand, it may lead to worsened governance, generating ambiguous externalities on labor. For instance, managers seeking a "quiet life" (Bertrand and Mullainathan (2003)) may be reluctant to invest in new projects and also avoid laying off unproductive workers, which may yield zero net effect on workforce. By contrast, managers may engage in empire building (Baumol (1959); Marris (1964); Williamson (1964)) or take excessive risks (Houston, Lin, and Xie (2018); Ni and Yin (2018)), which would lead to a subsequent workforce expansion.

In this paper, I study the effect of shareholder protection on firms' employment strategies using the staggered passage of the U.S. state-level Universal Demand (UD) laws in a diff-in-diff setting. The UD laws introduce procedural hurdles for shareholders to initiate derivative lawsuits against managers and directors, effectively reducing the number of derivative lawsuits at both the firm and state level (e.g., Appel (2019); Chu and Zhao (2015)). As corporate employment decisions and firms' litigation risk may be simultaneously affected by some unobservable factors, the UD adoption provides a setting to capture exogenous changes in shareholder litigation rights and to estimate a suggestive causal effect.

My main finding shows that the UD adoption triggers a 5 percentage points increase in firms' employment growth. This effect is economically important and represents an increase of approximately 16% of the pre-treatment standard deviation. This indicates that the weakened shareholder litigation rights have positive externalities on corporate employment level. The results remain consistent when using a matched sample based on firm characteristics, alleviating the concern that firms incorporated in UD states and non-UD states are intrinsically different.

Is the UD-induced employment growth a by-product of managers' self-dealing behaviors or is it a result of pro-labor corporate policies? I examine this by studying the evolution of wage around the passage of UD laws. I do not find any significant changes in wage following the UD adoption, which suggests that managers are not implementing policies that improve employees' welfare at the expense of shareholders.

I next proceed to explore whether firms' workforce expansion is triggered by the UD-induced deterioration in governance, focusing on managers' risk-taking behaviors. I find a significant increase in firms' capital expenditure as well as R&D expenses. As innovation is often considered as risky, my results suggest that firms are more prone to take risks following the UD adoption. This coincides with the findings of Lin, Liu, and Manso (2021) - reduced litigation risks spur corporate innovation. Furthermore, I find that the likelihood of a firm engaging in acquisitions as well as the size of the deals increase following the UD adoption. Specifically, I find that firms undertake more non-diversifying M&As. This provides further evidence on the risk-taking channel as prior evidence suggests that managers use diversifying acquisitions to reduce firms' risk (Gormley and Matsa (2016)).

Can the expansion in workforce be fully explained by the increase in investment and acquisitions? In order to test this, I follow Benmelech, Bergman, and Seru (2021) and directly include variables on investment and acquisitions in the baseline regression. The coefficients of the UD indicator remain positive and significant, indicating the existence of alternative channels through which UD laws lead to the growth in employment.

Do firms' intensified R&D activities trigger workforce expansion and further, a change in the labor structure? As human capital is a crucial input of innovation, firms may increase their demand for high-skilled labor following the UD adoption. I construct four measures of human capital intensity and find that firms operating in human-capital-intensive industries experience a larger increase in employment. This indicates that the UD-induced innovation contributes to the employment growth and that there may be an improvement in the skill levels of labor for the affected firms.

Finally, I ask whether the workforce expansion is efficient or not. On the one hand, managers are prone to take excessive risks after the UD adoption, suggesting that the subsequent employment growth may exceed the optimal level. On the other hand, firms' increased demand for high-skilled labor indicates a potential enhancement in the labor structure, which could have positive implications on the long-term performance. Using a labor model to predict firms' optimal level of employment, my results show that on aggregate, the workforce expansion is inefficient.

My research contributes to several strands of literature. First, it adds value to the sprouting literature on how derivative litigations shape corporate finance and corporate governance. Early studies in this field exploit the specific lawsuits and document evidence on the positive impact of derivative lawsuits on corporate governance. Erickson (2010) deems derivative litigation as a new form of shareholder activism in corporate governance and Ferris, Jandik, Lawless, and Makhija (2007) document the evidence on improved board quality following derivative lawsuits.

More recent studies exploit the staggered adoption of the U.S. state-level Universal Demand laws as a negative shock on firms' derivative litigation risk and further shed light on its impact on corporate governance issues such as managerial entrenchment (Appel (2019)), CEO turnover (Hayes, Peng, and Wang (2020)), board quality (Masulis, Shen, and Zou (2020)), and its peer effect on governance (Foroughi, Marcus, Nguyen, and Tehranian (2019)). Contrary to the conventional wisdom that shareholder litigation is frivolous and incurs high attorney fees, researchers have also unveiled its impact on a wide range of corporate finance issues. Some of the papers demonstrate the bright side of the UD laws and find evidence on their positive influence on corporate innovation (Lin, Liu, and Manso (2021)), takeover efficiency (Chu and Zhao (2015)), and value of cash

(Nguyen, Phan, and Sun (2018)). Other papers disclose the dark side of the UD laws and show that their adoption leads to an increase in the cost of capital (Houston, Lin, and Xie (2018)), cost of debt (Ni and Yin (2018)), and insider trading (Adhikari, Agrawal, and Sharma (2021)). My findings add value to this research area by bridging the litigation literature and the labor literature.

Firms' employment strategies are determined by various factors. A growing number of papers have revealed the implications of corporate events on firms' employment decisions. Davis, Haltiwanger, Jarmin, Lerner, and Miranda (2011) examine the effect of leveraged buyouts on job creation and destruction, while Borisov, Ellul, and Sevilir (2021) investigate how IPOs affect corporate employment growth through different channels such as the relaxation of financial constraints. Similarly, Benmelech, Frydman, and Papanikolaou (2019) and Giroud and Mueller (2017) find that financial frictions as well as corporate leverage play an important role in firms' employment level in the setting of the Great Depression. Aside from the financing factor, conflicts between different stakeholders also exert an influence on firms' employment strategies. Atanassov and Kim (2009b) find that weak investor protection and strong union laws stimulate the emergence of a manager-worker alliance, thus reducing the risk of mass layoffs when firms are under-performing. In a related paper, Falato and Liang (2016) show a substantial employment cut in the presence of creditor rights after firms violate loan covenants. In addition, corporate characteristics affect corporate decision making on labor. Ellul, Pagano, and Schivardi (2018) find that family firms provide more employment insurance than non-family firms, and Landier, Nair, and Wulf (2009) show that geographic dispersion negatively affects firms' employment policies. My research contributes to this strand of literature by dissecting the firm-level employment strategies from a new angle - firms' litigation risk.

2.2 Institutional Background

2.2.1 Derivative Lawsuits and the Universal Demand Laws

Corporate directors and officers are required to fulfil two main fiduciary duties, "the duty of loyalty" and "the duty of care". When the fiduciary duty is breached, shareholders can initiate legal proceedings against the directors and officers to protect their rights. Derivative lawsuits provide a legal mechanism targeting the misconducted directors and officers. In a derivative lawsuit, plaintiff shareholders sue directors or officers on behalf of the corporation and the subsequent financial recovery goes to the company itself.

Despite the argument that the indemnifications in derivative lawsuits hardly impose financial burdens on sued directors and officers [1], managers must bear their own attorney fees, time loss, and reputation loss (Houston, Lin, and Xie (2018); Lin, Mai, Zhang, and Zhang (2021)). Moreover, the settlements of derivative lawsuits usually focus on corporate governance reforms (Erickson (2010)), which may substantially affect the behaviors of directors and officers.

In the United States, shareholders are required to demand that the board takes corrective action before bringing forward a derivative lawsuit. The board can either accept or reject the demand. Given that at least some of the directors are named as defendants in a typical case, the board almost always rejects the demand. In reality, once a board rejects the demand, in most cases the court follows the board's decision citing the business judgement rule. However, shareholders can circumvent the demand requirement by providing evidence that the demand is a futile act. In practice, shareholders almost always argue that the demand is futile instead of making the demand, which entails substantial judicial time and resource and engenders a large number of litigations.

Between 1989 and 2005, 23 states passed the Universal Demand Laws, which remove shareholders' option of pleading demand futility when they wish to bring forward a derivative lawsuit. The timing of the UD laws adoption is reported in Table 2.A.1 in the Appendix.[2] Similar to the anti-takeover laws, a state UD law applies to firms incorporated in that state.

Another major type of litigation is the direct litigation, which addresses harms to shareholders. Direct lawsuits are normally initiated by a single shareholder or multiple shareholders. In the case of multiple shareholders alleging the harm, they can file a class action lawsuit, where federal or state securities laws will be enforced. Overall, class actions and derivative litigations are different in nature and have different procedural requirements. In this paper, I focus exclusively on derivative lawsuits.

[1]The D&O insurance policies and the exculpatory charter provisions adopted by most states both protect directors and officers from the personal liability incurred by derivative litigations (Appel (2019)).

[2]In most cases, the UD laws adopted by states are based on a version of the rule from the Model Business Corporation Act (MBCA), which is the basis for corporate statutes in 32 states (Appel (2019)).

2.2.2 UD Laws as An Exogenous Shock

The difference-in-difference setting in this paper relies on the exogeneity of the UD adoption. It is possible that the passage of UD laws was influenced by various economic and political forces: although the UD appears to be a non-partisan legislation, it was affected by the lobbying from interest groups.[3] Of all the states that have adopted the UD laws, only Pennsylvania passed it by the state Supreme Court and thus can be viewed as a clean setting.[4] Therefore, I conduct several empirical tests to alleviate the concerns over potential endogeneity issues.

First, I conduct some preliminary empirical tests on examining whether the timing of UD adoption is driven by the previous employment growth aggregated at state level. Following Beck, Levine, and Levkov (2010) and Lin, Liu, and Manso (2021), I exploit the Weibull hazard model where the dependent variable is the log value of time expected until the adoption of UD. The null hypothesis here is that employment growth does not affect the timing of the UD adoption. I use two measures of firm-level employment growth to form the state-aggregated employment growth, the percentage change of the employee numbers between year t and $t-1$, and the change of the log value of employee numbers between year t and $t-1$. Moreover, I control for several state characteristics such as GDP, union coverage, and unemployment rate, to capture regional economic factors that may confound the results. Column (1) - (4) in Table 2.B.2 report the results of the Weibull regression. All the coefficients of the state-level employment growth measures are insignificant, with and without state-level controls. This suggests that the adoption of UD laws is irrelevant to corporate employment growth, which verifies the exogeneity of UD laws.

Second, firms may strategically choose their states of incorporation. It is possible that firms "shop" the states of incorporation based on UD laws. To address this concern, I run the regression using the log value of the number of firms incorporated in each state in a given year as the dependent variable. I use the UD indicator which equals one if a state has passed a UD law as the main explanatory variable and control for the state fixed effects and year fixed effects. The standard errors are clustered at the state

[3] Appel (2019) documents the example of lobbying activities over UD in New York State. Even though the action was eventually put off, it certifies that the attitude of a state towards UD is the revelation of negotiations among interest parties.

[4] Following the literature, I include only the firms incorporated in Pennsylvania as treated firms as a robustness check. The result is shown in Section 6.2.

of incorporation level. As is shown in Column (5) and (6) in Table 2.B.2, the number of firms incorporated in each state is not affected by the UD adoption, ruling out the possibility of state of incorporation shopping.

2.2.3 Effect of UD Laws on Litigation

The identifying assumption of this paper is that the UD adoption effectively reduces firms' derivative litigation risk. To test this, I assemble a derivative lawsuit database relying on two sources: Audit Analytics (AA) and SEC filings from the EDGAR system. Following the literature (e.g. Appel (2019); Lin, Liu, and Manso (2021)), I identify derivative lawsuits as those that are categorised as both "shareholder suits" and "derivative" in AA. Due to the limitations of the AA database[5], I supplement the sample with derivative lawsuits disclosed in 10-K filings. To be specific, I web scrape all the 10-K filings disclosed in the EDGAR system between 1994 and 2010 and identify filings that use the terms "derivative lawsuit/suit" or "derivative litigation". For those filings, I collect the corresponding firm identifier (CIK number), year, and state of incorporation and append them to the AA database after omitting the overlapped cases.[6]

Using the combined dataset, I construct an indicator of firm-level derivative lawsuits and conduct an OLS regression on a sample where states that had adopted UD laws before 1994 are excluded. The results are reported in the first two columns of Table 2.1. The dependent variable is the derivative lawsuit indicator that equals one if a firm has a derivative lawsuit in a given year, zero otherwise; and the main explanatory variable is the UD indicator that equals one if a state has adopted the UD, zero otherwise. I include firm fixed effects and industry*year fixed effects in both specifications and in Column (2), state-level characteristics such as GDP, unemployment rate and union coverage are also controlled for. In both columns, the coefficients of the UD indicator are significantly negative, suggesting that UD laws cause a substantial drop in firms' derivative litigation risks.

As another major form of shareholder litigations, class actions might serve as a sub-

[5] The coverage of the AA database only begins in 2000, and only includes lawsuits that are filed in federal courts.

[6] Note that firms usually disclose their derivative lawsuits in all the years between when the lawsuit was started and when it got settled, I thus only take into account of the first year in a row that a firm reports its derivative lawsuits in its 10-K. For instance, a derivative lawsuit was filed by shareholders against Firm A in 2006 and was settled in 2008, Firm A usually reports this lawsuit in its 10-K filings in all the years between 2006 and 2008. To avoid counting the derivative lawsuits repeatedly, I only count the derivative lawsuit of Firm A in 2006, not in 2007 or 2008.

stitute to derivative lawsuits. I therefore examine whether the UD adoption is associated with changes in firms' risks of being sued in class actions. I extract class action lawsuits data from the Stanford Securities Class Action Clearinghouse (SCAC), which covers all the class actions filed in federal courts since 1996. I then construct the class action indicator in the same way as the derivative lawsuit indicator and regress it on the UD indicator. The results are presented in Column (3) and (4) of Table 2.1. No evidence is found on UD's effect on class actions, contradicting the hypothesis that shareholders may resort to class actions when derivative lawsuits are more difficult to initiate.

To sum up, the results regarding UD's effect on litigation are in line with the literature (e.g., Appel (2019), Lin, Liu, and Manso (2021), Chu and Zhao (2015)) - the UD adoption causes a substantial drop in the risk of derivative lawsuits. This confirms the rationale of using UD as a negative shock to managers' derivative litigation risks.

2.3 Data and Methodology

2.3.1 Data and Sample Construction

My sample consists of all the U.S. public firms between 1994 and 2010 excluding financial firms. I obtain the firm-level financial data from Compustat and the stock price data from CRSP. The sample period begins in 1994 because this is the first year when the electric filings are available on the EDGAR system, which enables me to accurately retrieve the historical state of incorporation data.[7] Note that some states adopted the UD laws before 1994, which may introduce bias to my results, I omit those states from the final sample. My sample ends in 2010 to ensure that there is 5 years' data after the last UD adoption in 2005.

To gauge a firm's governance, I obtain the widely used E index and G index from Bebchuk, Cohen, and Ferrell (2009) and Gompers, Ishii, and Metrick (2003) respectively.[8] I also calculate firms' total institutional ownership holdings and their concentration as other proxies for governance, of which the data is extracted from the Refinitiv 13F Institutional

[7]Firms' historical state of incorporation data is extracted from the Augmented 10-X Header Data provided by Bill McDodald. See `https://sraf.nd.edu/data/augmented-10-x-header-data/`.

[8]The E index (entrenchment index) is based on six corporate governance provisions (staggered boards, supermajority voting requirements for mergers, limits on shareholder bylaws amendments, limits on shareholder charter amendments, poison pills, and golden parachute arrangements). The G index (governance index) also uses governance provisions but covers a wider range (up to 24 provisions). Both of these two indices proxy for a firm's governance quality.

Holdings dataset.

Regarding the state-level data, I rely on the Bureau of Economic Analysis to obtain data on real GDP. The data on unemployment rate is extracted from the Local Area Unemployment Statistics provided by the Bureau of Labor Statistics. Furthermore, I retrieve the union coverage data from the Union Membership and Coverage Database (Hirsch and Macpherson (2003)).[9] In order to rule out the potential impact other state-level laws may exert, I construct indicators using the anti-takeover laws and labor protection laws following Gormley and Matsa (2016), Karpoff and Wittry (2018), Chava, Danis, and Hsu (2020), and Serfling (2016). All the summary statistics are reported in Table 2.2.

2.3.2 Methodology

The Universal Demand laws mitigate managers' litigation risks by reducing the threat of derivative lawsuits. The adoption of UD laws provides a natural experiment that can be used in a difference-in-difference setting - firms incorporated in the UD-adopted states are viewed as the treated and firms in other states serve as controls. The baseline regression model is defined as below:

$$Emp\_Growth_{ijst} = \alpha + \beta UD_{s,t-1} + \theta_i + \gamma_{jt} + \epsilon_{ijst} \tag{2.3.1}$$

where Emp_Growth_{ijst} represents the employment growth of firm i in industry j from year $t-1$ to t, with the state of incorporation being s. UD is defined as a dummy variable that equals one if a firm incorporates in a state that has adopted the UD law, zero otherwise. θ_i denotes the firm fixed effects, and γ_{jt} denotes the industry_year fixed effects.

Note that it is possible that firms change their states of incorporation seeking better business conditions. Although there is no evidence of state of incorporation shopping in my sample (see section 2.2), I exclude firms that moved their states of incorporation across the UD-adopted and the non UD-adopted states to further rule out any potential endogeneity issues.[10]

The conventional method used in the staggered difference-in-difference setting has recently raised questions in the econometrics field. The potential heterogeneity problem

[9] See www.unionstats.com.

[10] I exclude firms that moved their states of incorporation from a state without the UD laws to a state with UD laws, and vice versa.

of the treatment effects between groups or over time can severely bias the TWFE results. To address this concern, I conduct an analysis using the method proposed by Sun and Abraham (2021b) as a validation test to the traditional TWFE analysis. The results are reported in Section 4.1.

To mitigate the concern over "bad controls", I do not include control variables in the baseline regressions. In other specifications, I include common firm-level controls such as leverage, firm size (log of assets), asset tangibility (ratio of plant, property and equipment to total assets), and ROA, all lagged by one year. Regarding state-level controls, I include unemployment rate, union coverage, and real GDP to capture the economic conditions at the state level.

2.4 Empirical Results

In this section, I present the empirical results of the main analysis on how the UD adoption affects firm-level employment.

2.4.1 The UD Adoption Increases Firm-Level Employment Growth

Table 2.3 reports the main results of the staggered difference-in-difference setting. To sum up, I find that firm-level employment growth increases significantly after a state adopts the UD law. To control for unobservable firm-level characteristics and industry-level trends, I include the firm fixed effects and the industry_year fixed effects. Column (1) is the baseline regression without any control variables, whilst Column (2) and (3) include firm-/state-level controls. As is shown in Table 2.3, the magnitude of the effect varies from 4.82% to 5.24%, depending on the specification. This effect is economically important and indicates an increase by 16% of a standard deviation over the pre-treatment sample (the standard deviation of the pre-treatment sample is 0.3041, see Table 2.B.1).

It has been shown by some recent papers that the conventional TWFE estimators could be biased in the presence of heterogeneous treatment effects (e.g., De Chaisemartin and d'Haultfoeuille (2020); Sun and Abraham (2021b)). The conventional DiD method and its subsequent test of parallel trends through conducting dynamic regressions are hence problematic. To confirm the validity of my baseline results and further provide evidence on the parallel trend assumption, I exploit the event study method proposed by Sun and Abraham (2021b) and estimate the "interaction-weighted" coefficients. The event study plot is shown in Figure 2.1 (a). In line with the baseline results, firms' employment growth increases significantly after the UD adoption, which peaks at $\tau = 1$ and $\tau = 2$. Furthermore, there is no pre-trend on employment growth prior to the passage of UD laws.

As an additional check, I conduct an event-specific analysis where I run the baseline regression using only one treated state each time (Cengiz, Dube, Lindner, and Zipperer (2019); Donelson, Kettell, McInnis, and Toynbee (2021)). This method can fully take into account of the heterogeneities in the treatment effects across treated states. Figure 2.B.1 plots the corresponding point and confidence interval estimates using the full sample and the (-5,+5) event window. For a total number of 14 events, the UD adoption causes a significant increase in employment growth in the majority of events and many of those workforce expansions are significantly different from zero (red diamond markers). On average, there are approximately three events in which the effect of UD adoption on employment growth appears to be negative. However, in all these events, the economic magnitude is smaller than that of the other events. Also, the point estimates are statistically insignificant when I control for the headquarter state*year fixed effects. In short, the event-specific analysis further validates my baseline results.

I also use two other common measures of the employment growth to demonstrate that the results are not driven by the choice over measurement. First, I calculate the difference between the log value of employee numbers between year t and $t-1$ following Ellul, Pagano, and Schivardi (2018). Second, I adopt the symmetric employment growth measure used by Davis, Haltiwanger, and Schuh (1998) and Falato and Liang (2016). This measure is calculated as the ratio of the difference between numbers of employees in year t and $t-1$ over one half of the sum of these two years' numbers of employees. The advantage of the second measure is that it addresses the asymmetries between large employment increases and cuts. Table 2.4 reports the results using these two additional measures. There is consistent evidence that the passage of UD laws significantly increases corporate employment growth - the increase ranges from 2.95 percentage points to 3.49 percentage points, depending on the specification.

2.4.2 Matched Sample

Although the baseline results survive after controlling for various firm-/state-level characteristics, it is possible that my finding is driven by some unobservable factors which make the treated and control firms intrinsically different. To assuage this concern, I perform a propensity score matching using leverage, firm size, asset tangibility, and ROA to ensure the comparability between the treated and controls.

Specifically, for each year that a UD law is adopted, I identify all firms incorporated in states that have newly passed the UD in that year as the treated firms. I then obtain all the firms incorporated in states that have never adopted and will not adopt the UD laws in the future as controls ("clean controls"). I then rely on a probit model to estimate a firm's probability of being treated using various firm-specific characteristics one year before the UD adoption. Each treated firm is matched with one control firm that has the nearest propensity score. This matching process eventually yields a sample with 327 treated firms and 327 control firms.

Results using the matched sample are reported in Table 2.5. Panel A shows the descriptive statistics of the treated and control firms one year before UD adoption. The differences of the means of each matching variable between the treated and controls and the corresponding p-values are also reported - there is no significant difference between the treated and control firms prior to the UD adoption. Panel B shows the regression results exploiting different measures of employment growth. There is a consistent significant increase in the firm-level employment growth with and without control variables. Overall, the effect of UD adoption on the main employment growth measure Emp_Growth is about 5.13%. This amounts to an approximate increase by 15.7% of a standard deviation over the pre-treatment sample (untabulated) for the treated firms, which is similar to the estimated increase using the full sample.

2.5 Underlying Channels

In this section, I explore the potential channels that cause the workforce expansion following UD adoption. In particular, I focus on managers' risk-taking behaviors due to the deterioration of governance and firms' increased demand for high-skilled labor.

2.5.1 Risk-Taking

2.5.1.1 Investment and R&D

Derivative litigation serves as a way of corporate governance (Ferris, Jandik, Lawless, and Makhija (2007)); Appel (2019)). The UD adoption, by reducing managers' litigation risk, exacerbates the agency conflicts, thus induces managers to carry out risk-taking activities.

In order to test this, I first examine how the UD adoption shapes firms' investment strategies. I exploit two conventional investment proxies, the ratio of capex over beginning-of-year property, plant and equipment, and the R&D expense. Table 2.6 reports the regression results. Across all the specifications, I control for firm fixed effects and industry*year fixed effects. There is an 2.72 percentage points increase in investment and 6.03 percentage point increase in firms' R&D expenditure after including all the firm and state controls. Compared with the pre-treatment sample, the increases in investment and R&D are 7.09% and 7.99% of a standard deviation respectively.

These results suggest that the UD adoption leads to an increase in investment, especially for risky investment such as R&D, which are in line with the findings in Lin, Liu, and Manso (2021). The results also suggest that managers are not seeking a "quiet life" where new investment opportunities would not be pursued. Consistent with my conjecture, the findings validate that managers take more risks when their exposure to litigation is limited. The post-UD employment growth can thus be at least partially explained by this investment expansion.

2.5.1.2 Mergers and Acquisitions

M&A deals are often used as a measure of risk-taking as managers have the incentive to pursue acquisitions when they are exposed to less monitoring. In light of this, I examine whether firms are more likely to engage in corporate acquisitions as well as whether the size of acquisitions increases following the UD adoption. I rely on the M&A data extracted from the SDC database and construct two variables or interest, *Acquiror* and *Deal Value/Mkt Cap*. *Acquiror* is an indicator variable that equals one if a firm acquires a target firm in a given year, zero otherwise. It captures firms' likelihood of expanding through acquisitions. *Deal Value/Mkt Cap* is calculated using the sum of the values of all M&A deals a firm has in one year, scaled by the firm's market capitalization. This variable captures the size of a firm's acquisitions of a given year.

Panel A in Table 2.7 presents the evidence that UD adoption leads to an increase in both the likelihood and the size of acquisitions. Column (1) and (2) show the regression results on a firm's chance of being an acquiror, and Column (3) and (4) demonstrate the results on the total size of a firm's acquisitions. All coefficients of the UD indicators are significantly positive. After controlling for firm and state characteristics, there is a 4.71 percentage point increase in a firm's likelihood of going through an acquisition, which amounts to a 13.28% increase of a pre-treatment standard deviation. Similarly, firms' sizes of acquisitions increase by 1.59 percentage points after including controls, which indicates a 12.12% increase of a pre-treatment standard deviation.

One can argue that the increase in takeover activities is not necessarily a manifestation of risk-taking. In fact, it is possible that managers resort to diversifying M&A deals in order to reduce risk (Gormley and Matsa (2016)). I therefore follow the literature (e.g. Gormley and Matsa (2016); Ni and Yin (2018)) and categorize all the M&A deals into diversifying M&As and non-diversifying M&As. To be specific, I define a non-diversifying M&A if the acquiror and target have the same prime SIC code, and define all other deals as the diversifying ones. According to my conjecture, as managers are inclined to undertake excessive risk following the UD adoption, we should see an increase in firms' non-diversifying deals. Note that I do not rule out the possibility of a firm engaging in diversifying deals, since the UD adoption increases the overall M&A efficiency by eliminating managers' need to make suboptimal merger decisions (Chu and Zhao (2015))

Panel B and C in Table 2.7 present the results of my analysis. As Panel B shows, firms' sizes of diversifying acquisitions increase significantly after the UD adoption. However, the likelihood of a firm being an acquiror in a diversifying M&A remains almost unchanged. This indicates that firms are more likely to carry out larger diversifying M&A deals compared with the pre-UD period. Regarding the non-diversifying M&As, there is an increase in both the probability and size of acquisitions. This confirms my conjecture that firms undertake more risk following the UD adoption, and conducting horizontal mergers is one of the many strategies managers pursue.

2.5.1.3 Over-Investment in Labor

To what extent can the risk-taking driven expansion explain the employment growth? In order to test whether the capital investment is fully responsible for the labor force expansion, I follow Benmelech, Bergman, and Seru (2021) and include investment-related variables as controls in the baseline regression. In particular, I control for the changes in investment (*Investment Growth*) and the level of investment (*Investment*) in the previous year to capture firms' direct capital investment; I also control for the indicator of whether a firm acquires a target in the previous year (*Acquiror*) and the size of acquisitions (*Deal Value/Mkt Cap*) as a measure of firms' M&A activities. If the increased investment and M&As can fully explain the employment growth, the corresponding control variables should absorb the effect and the coefficient of UD should be approximately zero and insignificant.

Table 2.8 reports the results of this analysis. In Column (1)-(2), I include investment-related controls aside from other firm-level and state-level control variables. As is expected, the coefficient of *Investment* is significantly positive, suggesting that capital investment contributes to the employment growth. In Column (3)-(4), I further include the M&A measures. Similarly, the coefficients of M&A-related controls are also positive and significant. This coincides with the expectation that firms' labor force expand after acquiring targets' employees. Most importantly, the inclusion of these extra controls barely affects the statistical significance of the UD indicator, with only a slight drop in the economic magnitude. This result provides further support for the positive effect of UD adoption on firms' employment, and infers that there exists other channels apart from managers' risk-taking behaviors.

2.5.2 Increased Demand for High-Skilled Labor

In this section, I explore the underlying channel that contributes to the part of the UD induced increase in workforce that cannot be explained by the investment expansion. Specifically, I explore whether firms increase hiring and change the structure of their labor force due to the increased innovation activities. The UD adoption stimulates firms to undertake more R&D activities as a result of reduced litigation risk. In the presence of UD laws, managers no longer need to worry about being sued if there is a failure in innovation and if this further leads to a unrealized short-term earning goal. As an important input of innovation, high-skilled labor would be more in demand following the UD adoption, which may spur a shift in the component of firms' workforce. Due to the data limitation, it is impossible to directly measure the proportion of high-skilled labor for each firm. I therefore follow Borisov, Ellul, and Sevilir (2021) and rely on four industry-specific measures of human capital intensity to examine whether firms operating in those industries experience a higher employment growth following the UD adoption. All of the four measures are at the 3-digit SIC industry level.

I construct the first industry-specific measure of human capital intensity following Borisov, Ellul, and Sevilir (2021). As wage is correlated with skill, I use the average annual wage for each industry from the Occupational Employment and Wage Statistics (OEWS) as a proxy for industry-level skill. Since the OEWS survey began using the

Office of Management and Budget (OMB) Standard Occupational Classification (SOC) system in 1999, I collect the wage data starting from 1999. Moreover, the survey used SIC as the industry classification code before 2002 and switched to NAICS afterwards. Given that the industry codes used in this paper are SIC codes, I rely on the average wage data collected from 1999 to 2001 for all occupations within each industry to rank the industries. I classify the industries with the average annual wage in the top tercile as the human capital intensive industry and define the indicator *High Skill* as one if a firm operates in such an industry, zero otherwise.

The second measure of human capital intensity is similar to the first one, but exploits the data on the proportion of skilled worker in each industry. Specifically, *Management Occupations*, *Computer and Mathematical Occupations*, *Architecture and Engineering Occupations*, and *Life, Physical, and Social Science Occupations* are often considered as human capital intensive occupations. I again employ the OEWS data and calculate the total percentage of workers in high-skilled occupations for each industry. I then define the *High Skill Percentage* indicator equals one if an industry's high-skilled worker proportion is in the top tercile, zero otherwise.

The last two measures use the industry-level innovation inputs and outputs to categorize industries that rely more on human capital. In particular, using the average R&D expenditure of each industry, I assign the indicator *High R&D* of value one if an industry's R&D expenditure is in the top tercile of the sample, zero otherwise. In a similar vein, the fourth human capital intensity measure, *High Patents*, relies on the average number of patents in an industry. If an industry's number of patents is in the top tercile of the sample, *High Patents* equals one, zero otherwise.

Table 2.9 presents the results of cross-sectional analysis. I augment the baseline regression by including the interaction terms of the UD indicator and the industry-level human capital intensity measures. Of all the four measures, three of the corresponding interaction terms have positive and significant coefficients. This indicates that human capital intensive industries are more inclined to experience a boom in employment growth following the UD adoption. This is consistent with the view that UD laws affect firms' employment policies through stimulating corporate innovation, which eventually leads to an increase in firms' demand for high-skilled labor.

2.5.3 Labor Investment Inefficiency

As discussed in the previous section, firms expand their workforce following the UD adoption due to managers' risk-taking behaviours as well as firms' increased demand for high-skilled labor. However, the analysis does not shed light on whether this expansion in workforce is efficient or not. If managers avoid risky but value-enhancing projects before the UD adoption, the resultant increase in employment growth would be efficient. For instance, managers could be reluctant to innovate due to the potential litigation risk, then the UD adoption would mitigate this concern and leads to a healthy expansion in firms' workforce. Nevertheless, if the UD laws stimulate managers to take excessive risks and over-invest, the subsequent workforce expansion would be inefficient. In this section, I aim to answer this question.

I examine the overall efficiency of firms' labor investment exploiting the labor demand model proposed by Pinnuck and Lillis (2007). The model takes into account of an extensive list of firm-specific characteristics and has been widely used in the literature (e.g., Khedmati, Sualihu, and Yawson (2020); Ghaly, Dang, and Stathopoulos (2020)). The model is defined as below:

$$
\begin{aligned}
Emp\_Growth_{it} =& \alpha + \beta_1 Sales\_Growth_{it} + \beta_2 Sales\_Growth_{i,t-1} + \beta_3 Profit_{it} + \beta_4 \Delta Profit_{it} + \\
& \beta_5 \Delta Profit_{i,t-1} + \beta_6 Return_{it} + \beta_7 Size_{i,t-1} + \beta_8 Quick\_Ratio_{i,t-1} + \\
& \beta_9 \Delta Quick\_Ratio_{i,t-1} + \beta_{10} \Delta Quick\_Ratio_{it} + \beta_{11} Leverage_{i,t-1} + \\
& \Sigma_{l=1}^{5} \delta_l Loss\_Bins_{itl} + \gamma_j + \epsilon_{it}
\end{aligned}
$$

where i, j, t denotes firm, industry, and year, respectively. Emp_Growth represents the percentage change in a firm's number of employees. Similarly, $Sales\_Growth$ denotes the percentage change in a firm's sales. $Profit$ is the net income scaled by beginning-of-year total assets, and $\Delta Profit$ is the change in net income scaled by beginning-of-year total assets. $Return$ represents the total annual stock return for the given fiscal year. $Size$ is the market value of equity ranked into percentiles. $Quick\_Ratio$ is the ratio of cash and short-term investments plus receivables over current liabilities. $Leverage$ is calculated using the total debt divided by total assets. $Loss\_Bin_{itl}$ is defined as a dummy variable that equals one if a firm's $Profit$ falls in the corresponding 0.005 interval between -0.025

and 0.[11] The model also includes γ_j as the industry fixed effects to control for unobserved industry characteristics.

I rely on this labor demand model to estimate firms' expected employment growth, which is the fitted value of the regression. The inefficiency of firms' labor investments is measured as the absolute value of the difference between firms' actual hiring and expected hiring, which is exactly the regression residual - the unexplained portion of employment growth from the model.[12] The regression results of this labor demand model are reported in Table 2.B.3.

Table 2.10 presents the results regressing the labor investment inefficiency proxy on the UD indicator. There is a significant increase in labor investment inefficiency after the UD adoption, suggesting that firms are over-investing in labor. This suggests that the overall effect of UD laws on firms' employment is inefficient - the dark side of managers' risk-taking behaviors outweighs the innovation gains.

2.6 Additional Analysis and Robustness Checks

2.6.1 Additional Analysis on Confounding Factors

In this section, I extend the analysis by exploring potential omitted factors that may affect firms' employment growth and confound the results.

First, the underlying mechanisms driving the employment growth following the UD adoption can be affected by corporate governance. For instance, firms with better governance may suffer less from agency conflicts and are less prone to over-invest in labor, which would affect the results. I therefore include four governance indicators in the regression, firms' institutional ownership, institutional ownership concentration (IO HHI), E index, and G index. The institutional ownership is measured as the sum of a firm's total institutional holdings, whilst the institutional ownership concentration is calculated as the Herfindahl-Hirschman index of ownership. The E index and G index are obtained from the data used in Bebchuk, Cohen, and Ferrell (2009) and Gompers, Ishii, and Metrick (2003). Note that the E index and G index are only available for approximately every three years from 1990 to 2006, I exploit the conventional method used in the literature

[11] For instance, $Loss\_Bin_{it1}$ takes the value one if the profit of firm i in year t is between -0.005 and 0.

[12] To better capture firms' labor investment inefficiency (e.g., to obtain a more accurate firm size ranked in percentile), I apply this model using the data merged from Compustat and CRSP before omitting firms with no accurate historical state of incorporation data from the EDGAR filings.

and fill in the missing values with the nearest non-missing values.

Panel A of Table 2.11 reports the regression results controlling for corporate governance proxies. Column (1) and (2) control for firms' overall institutional ownership and IO HHI; Column (3) and (4) include firms' E index whilst the last two columns control for G index. As is shown in the table, the UD adoption continues to play a significant role in firms' employment growth. This suggests that the effect of UD laws on employment is not sensitive to controlling for various governance measures.

Second, I proceed to examine if firms' employment growth is affected by other state laws. The timing of UD adoption overlaps with some of the state-level anti-takeover laws, which may obscure the results. Moreover, the passage of anti-takeover laws, by strengthening firms' takeover defenses, can also affect corporate labor investment. For instance, with the anti-takeover laws being enacted, managers are protected from takeovers and may thus seek a "quiet life", (Bertrand and Mullainathan (2003)), which would bias my results. To address this issue, I follow Ni and Yin (2018) and Chu and Zhao (2015) and control for all the confounding law changes in the state-level anti-takeover legal regime documented in Karpoff and Wittry (2018). To be specific, I include the control share acquisition laws (CS), business combination laws (BC), fair price laws (FP), directors' duties laws (DD), and poison pill laws (PP). Similar to the UD indicator, I generate an indicator variable for each anti-takeover law and define it as one once a state passes the law, and zero otherwise. If the increase in employment growth is driven by these laws, the coefficient of the UD indicator should no longer be significant once these laws are controlled for.

Panel B of Table 2.11 reports the corresponding results. In Column (1)-(5), each column controls for one anti-takeover law; Column (6) includes all the anti-takeover laws in the regression. Across all the specifications, I include firm-level and state-level characteristics. I also control for firm fixed effects and industry*year fixed effects. The coefficients of the UD indicator remain significant in all columns, suggesting that the results are not driven by state-level anti-takeover laws.

2.6.2 Robustness Checks

I next proceed to carry out a battery of robustness checks and present the results in Table 2.12. I first address the potential endogeneity issue of the UD adoption by narrowing down the treated states to including only Pennsylvania. Pennsylvania is the only state where the UD was adopted by the Supreme Court - its UD adoption can therefore be deemed as a clean shock without concerns over lobbying. Although evidence on the exogeneity of the timing of UD adoption relative to employment growth has been demonstrated in section 2.2, this test can further mitigate the endogeneity concern. Column (1) and (2) in Table 2.12 present the regression results. The coefficients of the UD indicator remain significantly positive, suggesting a solid causal effect.

Second, I refine my sample with only the control states that have closely followed the MBCA (Appel (2019)), whilst the treated states remain the same as those in the main analysis.[13] Given that the UD adopted by many states are originated from MBCA, the previous findings can be subject to spurious correlation. The regression results using this refined sample are reported in Column (3) and (4). The effect of UD is still positive and statistically significant, suggesting that the previous results can not be attributed to the latent effect of MBCA.

Third, the effect of UD on employment growth can be confounded by legal changes regarding other types of shareholder litigations. For instance, a ruling by the Ninth Circuit in 1999 is often viewed as a shock for the class action litigations (Crane and Koch (2018); Chu (2017)).[14] After 1999, firms incorporated in states that are covered by the Ninth Circuit are more protected from class action lawsuits due to the increased difficulty in their initiation.[15] I subsequently control for this Ninth Circuit ruling in the regression and present the results in Column (5) and (6). As is shown by the results, the effect of UD is not absorbed by the Ninth Circuit ruling, which further verifies the main results.

I also conduct robustness checks addressing the "Delaware Effect". Previous studies have shown that Delaware corporate law improves firm value (Daines (2001)). Given that firms often make the decision on incorporation based on corporate laws such as anti-takeover statutes (Bebchuk and Cohen (2003)), many firms choose to incorporate in Delaware regardless of their headquarter states. This yields a sample with over half of the public firms incorporated in Delaware, which may introduce the "Delaware Effect" bias to my results. I therefore exclude all the firms that are incorporated in Delaware to alleviate this concern. As is shown in Column (7) and (8), the effect of UD remains positive and significant after restricting the sample in this manner.

[13] These states are specifically AL, CO, IL. KY, MD, NM, ND, OR, SC, TN, and WA.

[14] See the Silicon Graphics case.

[15] States that are covered by the Ninth Circuit are Alabama, Arizona, California, Hawaii, Idaho, Nevada, Oregon, and Washington.

Note that the UD laws are determined by the state of incorporation, which is similar to the Business Combination Laws exploited in Bertrand and Mullainathan (2003). This allows the comparison between firms that are headquartered in the same state but are incorporated in different states, and are thus subject to different legislations. I therefore include headquarter state*year fixed effects in the baseline regression to rule out possible noise introduced by regional economic shocks. Column (9) and (10) report the results. There is still a positive and significant effect of UD laws on firms' employment growth, indicating that my results are not affected by the inclusion of different fixed effects.

Finally, Donelson, Kettell, McInnis, and Toynbee (2021) raise the concern over the validity of using the UD adoption as a negative shock to firms' litigation risk. In their paper, they exploit a slightly different timing of the passage of UD laws. I re-perform my baseline analysis using the UD adoption time in Donelson, Kettell, McInnis, and Toynbee (2021) and report the results in Table 2.B.4. The sample period is extended to allow for the inclusion of DC and LA as treated states.[16] Panel A of Table 2.B.4 uses the sample from 1994 to 2018, which includes three years' data after the last UD adoption in the sample. Note that Donelson, Kettell, McInnis, and Toynbee (2021) uses the sample period 1996 – 2015, I therefore refine my sample to fully reconcile to their paper and report the corresponding results in Panel B. Across all the regressions, I consistently find a significant increase in employment growth following the UD adoption.

2.7 Conclusion

Despite the debate over the frivolity of shareholder litigations, recent literature has documented their real effects on various corporate issues ranging from governance mechanisms to financing and innovation strategies (e.g., Appel (2019); Nguyen, Phan, and Sun (2018); Lin, Liu, and Manso (2021)). In this paper, I study the relationship between derivative litigations and firms' employment growth. For identification, I exploit the staggered passage of the U.S. state-level Universal Demand laws as a quasi-natural experiment and adopt a difference-in-difference setting. The UD laws bring forward procedural hurdles in initiating a derivative lawsuit, which mitigates firms' litigation risk. This allows me to investigate how the threat of litigation affects corporate hiring.

[16] DC and LA adopted the UD laws in 2011 and 2015 respectively. In Donelson, Kettell, McInnis, and Toynbee (2021), they treat the UD adoption year of Utah as 2000 instead of 1992. To reconcile with their paper, I use 2000 as the UD adoption year of Utah in this analysis.

My findings highlight a negative relationship between litigation risks and firms' employment growth - firms hire more after their states of incorporation adopt the UD laws. I also explore the channels that could explain this observed relation. First, the UD adoption leads to a deterioration of corporate governance and exacerbates agency conflicts. As a result, managers are prone to undertake excessive risks and engage in risk-taking projects. My findings suggest that firms increase their investment, innovation, and M&As following the UD adoption, which can partially explain the employment growth. Second, firms' increased innovation activities indicate a potential shift in the labor component, where high-skilled labor is more in demand. Specifically, I find that firms operating in human capital intensive industries experience larger workforce expansion compared with firms from other industries.

Overall, this paper contributes to the ongoing debate on shareholder litigations by building a bridge between the litigation literature and the labor literature. My findings emphasize the unintended effect of legal changes regarding shareholder litigations on corporate employment growth. Although the reduced threat of litigation expands firms' labor force, it brings inefficiency and may harm shareholders' interests in the long run.

Table 2.1. Identifying assumption: UD laws And litigation

This table shows the results of the test for the identifying assumption - whether the UD adoption effectively reduces firms' litigation risk. Column (1) and (2) report the effect of UD laws on derivative lawsuits. In these two specifications, the dependent variable is an indicator that equals one if a firm has a derivative lawsuit in that year, zero otherwise. Column (3) and (4) report the results of the firm-level regression on class action lawsuits, where the dependent variable is an indicator that equals one if a firm has a class action lawsuit in that year, zero otherwise. Firm and industry*year fixed effects are controlled for; in some specifications state-level and firm-level controls are included. *UD* is a dummy variable indicating whether a state has enacted the UD laws. All standard errors are clustered by the state of incorporation and are reported in parentheses.

	Litigation Risk			
	Derivative Lawsuits		Class Action	
	(1)	(2)	(3)	(4)
UD	-0.0124***	-0.0120***	0.0045	0.0044
	(0.0037)	(0.0032)	(0.0032)	(0.0036)
Leverage		0.0046		-0.0043
		(0.0049)		(0.0039)
Size		0.0136***		0.0193***
		(0.0012)		(0.0021)
Tangibility		0.0251**		-0.0064
		(0.0115)		(0.0079)
ROA		-0.0107***		-0.0087*
		(0.0030)		(0.0052)
Unemployment Rate		-0.0013		0.0027
		(0.0026)		(0.0025)
Union Coverage		0.0005		-0.0006
		(0.0006)		(0.0007)
GDP		0.0034		-0.0016
		(0.0026)		(0.0030)
Observations	38737	38737	38260	38260
R^2	0.223	0.225	0.205	0.209
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes

Table 2.2. Descriptive statistics

This table shows the descriptive statistics for the full sample excluding states that passed the UD laws before the sample begins. The sample period spans from 1994 to 2010. All continuous variables are winsorized at 1% level.

	Mean	Median	SD	Obs
Employment Growth Measures				
Emp_Growth	0.0908	0.0293	0.3517	46821
Log_Emp_Growth	0.0467	0.0290	0.2766	46813
Sym_Emp_Growth	0.0310	0.0231	0.2457	39996
Firm-Level Variables				
Leverage	0.2174	0.1754	0.2141	46821
Size	5.5381	5.4304	2.0102	46821
Tangibility	0.2643	0.1913	0.2270	46821
ROA	0.0505	0.1065	0.2280	46821
Investment	0.3648	0.2238	0.4577	46380
R&D Expense	0.2649	0.0026	1.2265	46426
Deal Value	0.0437	0.0000	0.1514	46801
Acquiror	0.1838	0.0000	0.3873	46821
State-Level Variables				
Unemployment Rate	4.8516	4.4583	1.5553	46821
Union Coverage	14.9499	14.1000	3.9334	46821
GDP	11.5847	11.0112	1.1163	46821

Table 2.3. Effect of UD laws on employment growth

This table reports the results of my baseline regressions. *UD* is a dummy variable indicating whether a state has enacted the UD laws. I include firm fixed effects and industry*year fixed effects. Firm-level controls included in the regressions are leverage, size, asset tangibility, and ROA. State-level controls included in the analysis are unemployment rate, union coverage, and real GDP. All standard errors are clustered at the state of incorporation level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Employment Growth		
	(1)	(2)	(3)
UD	0.0524**	0.0482***	0.0487***
	(0.0205)	(0.0174)	(0.0174)
Leverage		-0.2041***	-0.2045***
		(0.0249)	(0.0251)
Size		-0.1353***	-0.1353***
		(0.0067)	(0.0067)
Tangibility		-0.3223***	-0.3228***
		(0.0290)	(0.0289)
ROA		0.2453***	0.2452***
		(0.0121)	(0.0121)
Unemployment Rate			0.0056
			(0.0040)
Union Coverage			0.0021
			(0.0013)
GDP			-0.0052
			(0.0054)
Observations	38737	38737	38737
R^2	0.293	0.332	0.332
Firm FE	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes

Table 2.4. Effect of UD laws on employment growth - additional measures

This table reports the results of my baseline regressions using two additional measures of employment growth, *Log_Emp_Growth* and *Sym_Emp_Growth*. *Log_Emp_Growth* is measured as the difference between the log value of the numbers of employees at year t and $t-1$. *Sym_Emp_Growth* denotes the symmetric employment growth and is calculated as the ratio of the difference of numbers of employees between year t and $t-1$ over one half of the sum of these two years' numbers of employees. Column (1)-(2) show the results using *Log_Emp_Growth* as the dependent variable, whilst Column (3)-(4) present the regression results on *Sym_Emp_Growth*. *UD* is a dummy variable indicating whether a state has enacted the UD laws. Various fixed effects and firm-/state-level controls are included across the specifications. Firm-level controls included in the regressions are leverage, size, asset tangibility, and ROA. State-level controls included in the analysis are unemployment rate, union coverage, and real GDP. All standard errors are clustered at the state of incorporation level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Employment Growth			
	Log_Emp_Growth		Sym_Emp_Growth	
	(1)	(2)	(3)	(4)
UD	0.0349*	0.0323**	0.0318*	0.0295**
	(0.0173)	(0.0147)	(0.0168)	(0.0143)
Observations	38731	38731	38737	38737
R^2	0.302	0.340	0.309	0.347
Controls	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes

Table 2.5. Effect of UD laws on employment growth - matched sample

This table reports the results of regressions on how firms' employment growth is affected by UD adoption using the matched sample. In this sample, each treated firm is matched with one control firm based on leverage, size, asset tangibility, and ROA. Panel A reports the summary statistics of both the treated and control firms. The differences between the means of the treated and control firms are shown in Column (5), whilst the p-values are reported in Column (6). Panel B reports the regression results using the main dependent variable *Emp_Growth* as well as the two additional measures *Log_Emp_Growth* and *Sym_Emp_Growth*. The regressions control for various fixed effects and include firm-level and state-level controls such as leverage, size, tangibility, ROA, unemployment rate, union coverage, and real GDP. All standard errors are clustered at the state of incorporation level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Matched Sample					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Treated Firms		Control Firms		Difference	
	Mean	Median	Mean	Median	Diff	p
Leverage	0.198	0.170	0.199	0.163	0.001	0.958
Size	4.997	4.837	4.842	4.631	-0.155	0.266
Tangibility	0.327	0.242	0.323	0.254	-0.004	0.831
ROA	0.104	0.125	0.108	0.127	0.005	0.684
Emp_Growth	0.092	0.033	0.101	0.040	0.009	0.725
Log_Emp_Growth	0.055	0.032	0.066	0.039	0.012	0.565
Sym_Emp_Growth	0.015	0.019	0.032	0.019	0.017	0.335
Panel B	Emp_Growth		Log_Emp_Growth		Sym_Emp_Growth	
UD	0.0513***	0.0513***	0.0284**	0.0282*	0.0239*	0.0231
	(0.0157)	(0.0166)	(0.0133)	(0.0143)	(0.0134)	(0.0142)
Observations	4513	4513	4511	4511	4513	4513
R^2	0.379	0.408	0.383	0.413	0.393	0.425
Control Variables	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.6. Channels: risk-taking I

This table reports the regression results of the UD adoption's effect on corporate investment. *Investment* is measured as the ratio of CAPEX over the beginning-of-year PPE . *R&D* is calculated as the ratio of the R&D expenditure over firms' total assets. Column (1)-(2) show the results using *Investment* as the dependent variable, whilst Column (3)-(4) present the regression results on *R&D*. *UD* is a dummy variable indicating whether a state has enacted the UD laws. Various fixed effects and firm-/state-level controls are included across the specifications. Firm-level controls included in the regressions are leverage, size, asset tangibility, and ROA. State-level controls included in the analysis are unemployment rate, union coverage, and real GDP. All standard errors are clustered at the state of incorporation level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Investment		*R&D*	
	(1)	(2)	(3)	(4)
UD	0.0368**	0.0272**	0.0730**	0.0603**
	(0.0178)	(0.0133)	(0.0302)	(0.0278)
Observations	38405	38405	38464	38464
R^2	0.385	0.459	0.721	0.726
Controls	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes

Table 2.7. Channels: risk-taking II

This table reports the regression results of the UD adoption's effect on M&As. *Acquiror* is a dummy variable that equals one if a firm has acquired a target in a given year, and *Deal Value/Mkt Cap* denotes the total value of all the acquisitions a firm goes through in one year divided by its market capitalization. Panel A shows the regression results using all the M&A deals when calculating the dependent variables. Panel B focuses on diversifying M&As, where only deals with different prime SIC industries of the acquiror's and target's are included. Panel C only includes non-diversifying M&As, and I define them as the rest of M&A deals apart from those in Panel B. *UD* is a dummy variable indicating whether a state has enacted the UD laws. Various fixed effects and firm-/state-level controls are included across the specifications. Firm-level controls included in the regressions are leverage, size, asset tangibility, and ROA. State-level controls included in the analysis are unemployment rate, union coverage, and real GDP. All standard errors are clustered at the state of incorporation level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Acquiror		*Deal Value/Mkt Cap*	
	(1)	(2)	(3)	(4)
Panel A: All M&A				
UD	0.0490**	0.0471*	0.0165***	0.0159***
	(0.0240)	(0.0259)	(0.0044)	(0.0045)
Observations	38737	38737	38722	38722
R^2	0.306	0.313	0.263	0.273
Controls	No	Yes	No	Yes
Panel B: Diversifying M&A				
UD	0.0169	0.0157	0.0112***	0.0109***
	(0.0157)	(0.0154)	(0.0029)	(0.0031)
Observations	38737	38737	38722	38722
R^2	0.304	0.309	0.268	0.275
Controls	No	Yes	No	Yes
Panel C: Non-Diversifying M&A				
UD	0.0303**	0.0294*	0.0037*	0.0036*
	(0.0138)	(0.0147)	(0.0020)	(0.0020)
Observations	38737	38737	38722	38722
R^2	0.275	0.278	0.244	0.247
Controls	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes

Table 2.8. Over-investment in labor compared with capital

This table reports the regression results of UD adoption's effect on employment controlling for firms' capital investment as well as M&A deals. Column (1)-(2) include measures of firms' direct capital investment. *Investment* is measured as the ratio of CAPEX over the beginning-of-year PPE . *Investment Growth* is calculated as the percentage increase in firms' investment. Column (3)-(4) further include M&A related controls. *Acquiror* is a dummy variable that equals one if a firm has acquired a target in a given year, and *Deal Value/Mkt Cap* denotes the total value of all the acquisitions a firm goes through in one year divided by its market capitalization. *UD* is a dummy variable indicating whether a state has enacted the UD laws. Various fixed effects and firm-/state-level controls are included across the specifications. Firm-level controls included in the regressions are leverage, size, asset tangibility, and ROA. State-level controls included in the analysis are unemployment rate, union coverage, and real GDP. All standard errors are clustered at the state of incorporation level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Employment Growth			
	(1)	(2)	(3)	(4)
UD	0.0482***	0.0543***	0.0515***	0.0526***
	(0.0165)	(0.0116)	(0.0113)	(0.0109)
Investment	0.0454***	0.0403***	0.0380***	0.0377***
	(0.0046)	(0.0059)	(0.0058)	(0.0059)
Investment Growth		-0.0002	-0.0006	-0.0006
		(0.0020)	(0.0020)	(0.0020)
Acquiror			0.0532***	
			(0.0039)	
Deal Value/Mkt Cap				0.1560***
				(0.0122)
Observations	38365	34649	34649	34639
R^2	0.334	0.332	0.335	0.336
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes

Table 2.9. Channels: human capital

This table reports the results of the cross-sectional analysis with respect to industry-level human capital intensity. Specifically, I construct four measures of industry-level human capital intensity and categorize an industry as human capital intensive if its value of the measure is in the top tercile of the sample. *High Skill* exploits the average annual wage of an industry as the proxy for the corresponding skill level. *High Skill Percentage* relies on the proportion of high-skilled labor of each industry as a measure of human capital intensity. *High R&D* uses the averaged industry R&D expenses as the proxy whilst *High Patents* uses the averaged industry number of patents. Various fixed effects and firm-/state-level controls are included across the specifications. Firm-level controls included in the regressions are leverage, size, asset tangibility, and ROA. State-level controls included in the analysis are unemployment rate, union coverage, and real GDP. All standard errors are clustered at the state of incorporation level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Employment Growth			
	(1)	(2)	(3)	(4)
UD	0.0362*	0.0114	0.0290	0.0193
	(0.0195)	(0.0251)	(0.0208)	(0.0290)
UD × High Skill	0.0284			
	(0.0223)			
UD × High Skill Percentage		0.0612**		
		(0.0263)		
UD × High R&D			0.0324**	
			(0.0153)	
UD × High Patents				0.0462*
				(0.0273)
Observations	36405	36405	38737	38737
R^2	0.327	0.327	0.332	0.332
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes

Table 2.10. Labor investment inefficiency

This table reports the regression results of the UD adoption's effect on firms' labor investment inefficiency. *Labor Investment Inefficiency* captures firms' inefficiency in labor investing and is measured as the absolute value of residuals of the labor demand model used in the literature (e.g., Pinnuck and Lillis (2007); Ghaly, Dang, and Stathopoulos (2020); Khedmati, Sualihu, and Yawson (2020)). *UD* is a dummy variable indicating whether a state has enacted the UD laws. Various fixed effects and firm-/state-level controls are included across the specifications. Firm-level controls included in the regressions are leverage, size, asset tangibility, and ROA. State-level controls included in the analysis are unemployment rate, union coverage, and real GDP. All standard errors are clustered at the state of incorporation level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Labor Investment Inefficiency		
	(1)	(2)	(3)
UD	0.0341***	0.0328***	0.0334***
	(0.0047)	(0.0047)	(0.0044)
Leverage		-0.0630***	-0.0631***
		(0.0110)	(0.0109)
Size		-0.0431***	-0.0431***
		(0.0019)	(0.0019)
Tangibility		-0.1128***	-0.1133***
		(0.0183)	(0.0180)
ROA		-0.0447***	-0.0447***
		(0.0129)	(0.0129)
Unemployment Rate			0.0009
			(0.0033)
Union Coverage			0.0022***
			(0.0008)
GDP			-0.0015
			(0.0028)
Observations	37193	37193	37193
R^2	0.351	0.362	0.362
Firm FE	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes

Table 2.11. Effect of UD laws on employment growth - controlling for confounding factors

This table reports the regression results of UD adoption's effect on employment growth after controlling for confounding factors. In Panel A, various corporate governance proxies are included. Column (1)-(2) include firms' total institutional ownership and IO concentration as measures of corporate governance; Column (3)-(4) exploit the E index whilst Column (5)-(6) use the G index. In Panel B, different anti-takeover laws are controlled for to mitigate the concern that the employment growth is not driven by UD laws. *CS Law*, *BC Law*, *FP Law*, *DD Law*, *PP Law* are indicators for anti-takeover laws that equal one if a state has passed the corresponding law, zero otherwise. Various fixed effects and firm-/state-level controls are included across the specifications. Firm-level controls included in the regressions are leverage, size, asset tangibility, and ROA. State-level controls included in the analysis are unemployment rate, union coverage, and real GDP. All standard errors are clustered at the state of incorporation level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Employment Growth					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Corporate Governance						
UD	0.0417***	0.0343***	0.0395**	0.0328**	0.0308**	0.0270**
	(0.0135)	(0.0114)	(0.0155)	(0.0127)	(0.0146)	(0.0110)
Institutional Ownership	0.0515***	0.1118***				
	(0.0080)	(0.0082)				
IO HHI	-0.0127	-0.0774***				
	(0.0133)	(0.0240)				
E Index			0.0045	0.0060		
			(0.0037)	(0.0037)		
G Index					-0.0025	-0.0005
					(0.0023)	(0.0022)
Observations	29735	29735	16084	16084	13555	13555
R^2	0.344	0.384	0.350	0.389	0.349	0.388
Controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Anti-Takeover Laws						
UD	0.0488***	0.0547***	0.0487***	0.0487***	0.0489***	0.0551***
	(0.0174)	(0.0107)	(0.0174)	(0.0171)	(0.0171)	(0.0104)
CS Law	0.0308					0.0379
	(0.0590)					(0.0866)
BC Law		-0.0316				-0.0328
		(0.0249)				(0.0248)
FP Law			0.0085			-0.0043
			(0.0287)			(0.0557)
DD Law				0.0011		0.0079
				(0.0204)		(0.0216)
PP Law					-0.0034	-0.0105
					(0.0143)	(0.0156)
Observations	38737	38737	38737	38737	38737	38737
R^2	0.332	0.332	0.332	0.332	0.332	0.332
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes

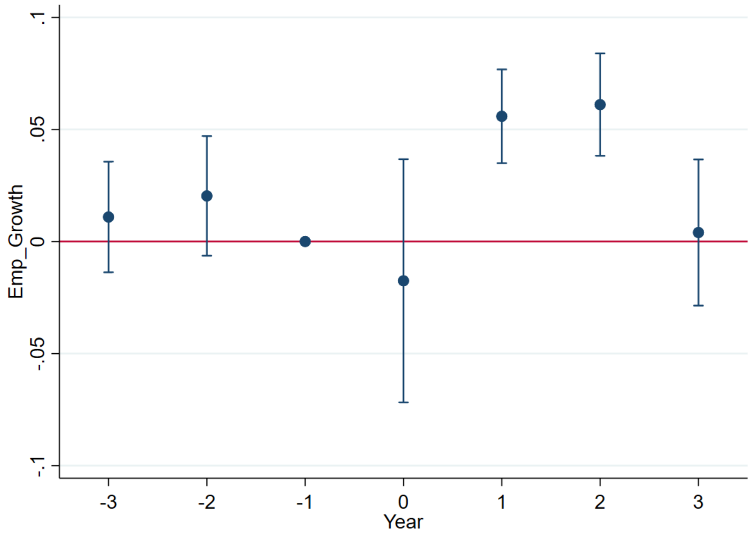
Table 2.12. Robustness results

This table presents the results of robustness checks. Column (1)-(2) use only firms that are incorporated in Pennsylvania as treated firms. Column (3)-(4) exclude control states that do not follow MBCA. Column (5)-(6) control for the Ninth Circuit indicator after 1999 to rule out the possibility that the previous results are affected by legal changes with respect to class action lawsuits. Column (7)-(8) omit firms incorporated in Delaware to alleviate the concern over the "Delaware Effect". Column (9)-(10) further include Headquarter_State*Year fixed effects to compare the effects of UD laws between firms that are headquartered in the states but are subject to different legislations. *UD* is a dummy variable indicating whether a state has enacted the UD laws. Various fixed effects and firm-/state-level controls are included across the specifications. Firm-level controls included in the regressions are leverage, size, asset tangibility, and ROA. State-level controls included in the analysis are unemployment rate, union coverage, and real GDP. All standard errors are clustered at the state of incorporation level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

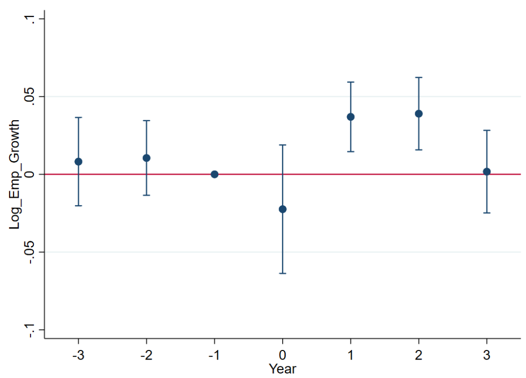
	Employment Growth									
	Only PA		Only MBCA		Ninth Circuit		Excl DE		Hdqt_State*Year FE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
UD	0.0879***	0.0705***	0.0505***	0.0542***	0.0525**	0.0488***	0.0364**	0.0396**	0.0278**	0.0286**
	(0.0091)	(0.0088)	(0.0157)	(0.0139)	(0.0205)	(0.0174)	(0.0174)	(0.0189)	(0.0136)	(0.0112)
Observations	36042	36042	5153	5153	38737	38737	13407	13407	38617	38617
R^2	0.296	0.335	0.395	0.424	0.293	0.332	0.346	0.378	0.311	0.349
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Headquarter_State*Year FE									Yes	Yes

Figure 2.1: Dynamic effect of UD laws

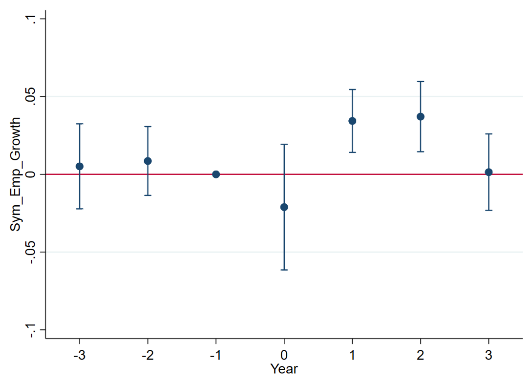
This figure depicts the dynamic effect of UD laws using the method proposed by Sun and Abraham (2021b). The graph is plotted using the Stata package eventstudyinteract.



(a)



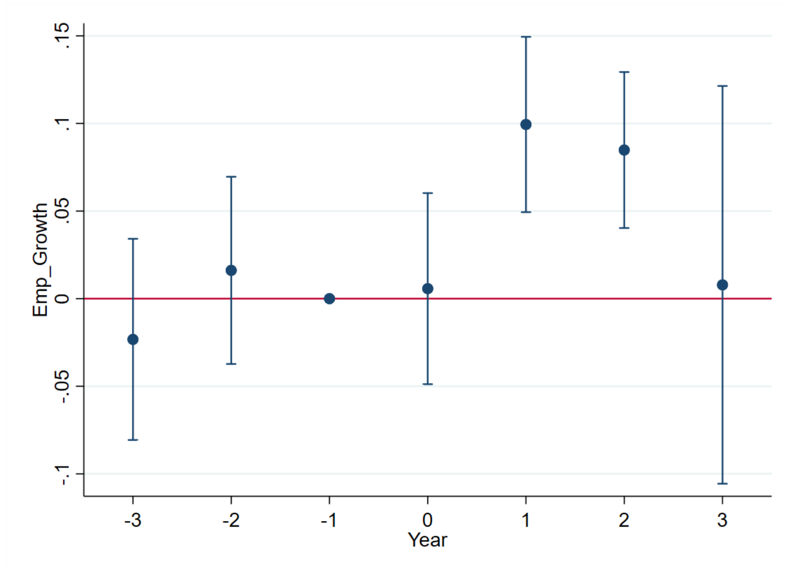
(b)



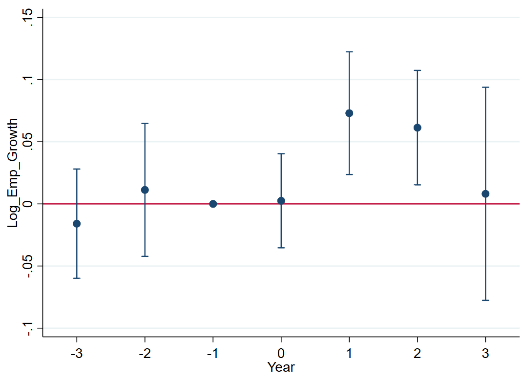
(c)

Figure 2.2: Dynamic effect of UD laws - matched sample

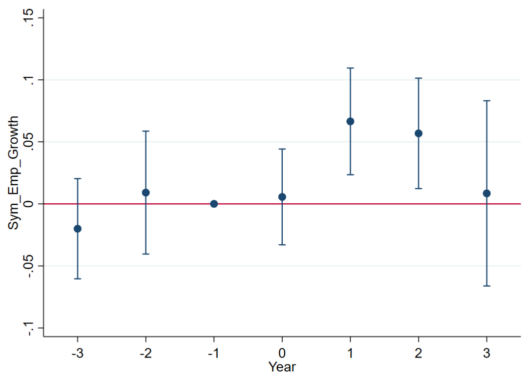
This figure depicts the dynamic effect of UD laws using the method proposed by Sun and Abraham (2021b) on the matched sample. There are 327 treated firms and 327 control firms after propensity score matching. The graph is plotted using the Stata package eventstudyinteract.



(a)



(b)



(c)

Appendix

2.A Universal Demand Laws

Table 2.A.1. Adoption of UD laws

This table provides the details of the timing of UD adoption in different states and their corresponding references.

Year	State	Citation
1989	GA	Ga. Code Ann. §14-2-742
1989	MI	Mich. Comp. Laws Ann. §450.1493a
1990	FL	Fla. Stat. Ann. §607.07401
1991	WI	Wis. Stat. Ann. §180.742
1992	MT	Mont. Code. Ann. §35-1-543
1992	VA	Va. Code Ann §13.1-672.1B
1992	UT	Utah Code. Ann. §16-10a-740(3)
1993	NH	N.H. Rev. Stat. Ann. §293-A:7.42
1993	MS	Miss. Code Ann. §79-4-7.42
1995	NC	N.C. Gen. Stat. §55-7-42
1996	AZ	Ariz. Rev. Stat. Ann. §10-742
1996	NE	Neb. Rev. Stat. §21-2072
1997	CT	Conn. Gen. Stat. Ann. §33-722
1997	ME	Me. Rev. Stat. Ann. 13-C, §753
1997	PA	*Cuker v. Mikalauskas* 692 A.2d 1042
1997	TX	Tex. Bus. Org. Code. Ann. §21.553
1997	WY	Wyo. Stat. §17-16-742
1998	ID	Idaho Code §30-1-742
2001	HI	Haw. Rev. Stat. §414-173
2003	IA	Iowa Code Ann. §490.742
2004	MA	Mass. Gen. Laws. Ann. Ch. 156D, §7.42
2005	RI	R.I. Gen. Laws. §7-1.2-710(C)
2005	SD	S.D. Codified Laws §47-1A-742

2.B Ancillary Results

Table 2.B.1. Descriptive statistics - treated firms before the UD adoption

This table shows the descriptive statistics for the treated firms prior to the UD adoption. States that passed the UD laws before 1994 are excluded. Treated firms are those that are incorporated in UD-adopted states. The full sample period spans from 1994 to 2010, but only observations before the UD adoption are included. All continuous variables are winsorized at 1% level.

	Mean	Median	SD	Obs
Emp_Growth	0.0760	0.0234	0.3041	1336
Log_Emp_Growth	0.0423	0.0231	0.2420	1336
Sym_Emp_Growth	0.0237	0.0160	0.2064	948
Leverage	0.1863	0.1230	0.2020	1336
Size	5.1835	5.0447	1.6874	1336
Tangibility	0.2963	0.2130	0.2297	1336
ROA	0.0957	0.1201	0.1580	1336
Investment	0.3406	0.2228	0.3835	1312
R&D Expense	0.1427	0.0128	0.7548	1333
Deal Value	0.0327	0.0000	0.1312	1335
Acquiror	0.1475	0.0000	0.3547	1336

Table 2.B.2. Validity tests on UD adoption as an exogenous shock

This table shows the results of the validity tests for using UD adoption as a natural experiment. The sample period spans from 1994 to 2010. Column (1) to (4) report the regression results using Weibull hazard model. The dependent variable is the log expected time for the enforcement of UD laws and the main explanatory variables are the employment growth/log value of the employment growth aggregated at state level. In Column (2) and (4), state-level controls such as the unemployment rate, union coverage, and real GDP are included. Column (5) and (6) report the regression results on state of incorporation shopping. The independent variable is the log value of the number of firms incorporated in each state in each year. *UD* is a dummy variable indicating whether a state has enacted the UD laws. Year and state of incorporation fixed effects are included and Column (6) includes state-level controls. All standard errors are clustered by the state of incorporation and are reported in parentheses.

	Timing of the UD Laws				State of Incorp Shopping	
	(1)	(2)	(3)	(4)	(5)	(6)
Mean_Emp_Growth	1.3530	2.4669				
	(2.3924)	(2.4878)				
Mean_Log_Emp_Growth			0.9269	1.7376		
			(2.0452)	(2.0708)		
UD					0.0988	0.0969
					(0.1174)	(0.1117)
Observations	478	478	478	478	619	619
Controls	No	Yes	No	Yes	No	Yes
Year FE					Yes	Yes
Incorp_State FE					Yes	Yes

Table 2.B.3. Model of labor demand

This table reports the estimates of the labor demand model used in the literature (e.g., Pinnuck and Lillis (2007); Ghaly, Dang, and Stathopoulos (2020); Khedmati, Sualihu, and Yawson (2020)). The expected sign of each explanatory variable is presented in the column "Expected Sign". All standard errors are clustered at the firm level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Net Hiring	Expected Sign
Sales Growth	.3151***	+
	(0.007)	
L.Sales Growth	.0247***	+
	(0.004)	
Profit	.1792***	+
	(0.010)	
Delta_Profit	-.223***	-
	(0.012)	
L.Delta_Profit	.0414***	+
	(0.010)	
Return	.0409***	+
	(0.002)	
L.Size	.0196***	+
	(0.004)	
L.Quick_Ratio	.0038***	+
	(0.001)	
Delta_Quick_Ratio	-.0159***	+/-
	(0.003)	
L.Delta_Quick_Ratio	.0198***	+
	(0.002)	
L.Leverage	-.0565***	+/-
	(0.006)	
Loss_Bin1	-.0253***	-
	(0.008)	
Loss_Bin2	-.0174**	-
	(0.007)	
Loss_Bin3	-.0254***	-
	(0.009)	
Loss_Bin4	-.0404***	-
	(0.008)	
Loss_Bin5	-.0228***	-
	(0.008)	
Cons	.0134***	+/-
	(0.003)	
Obs	86940	
R_2	0.2507	
Industry FE	Yes	

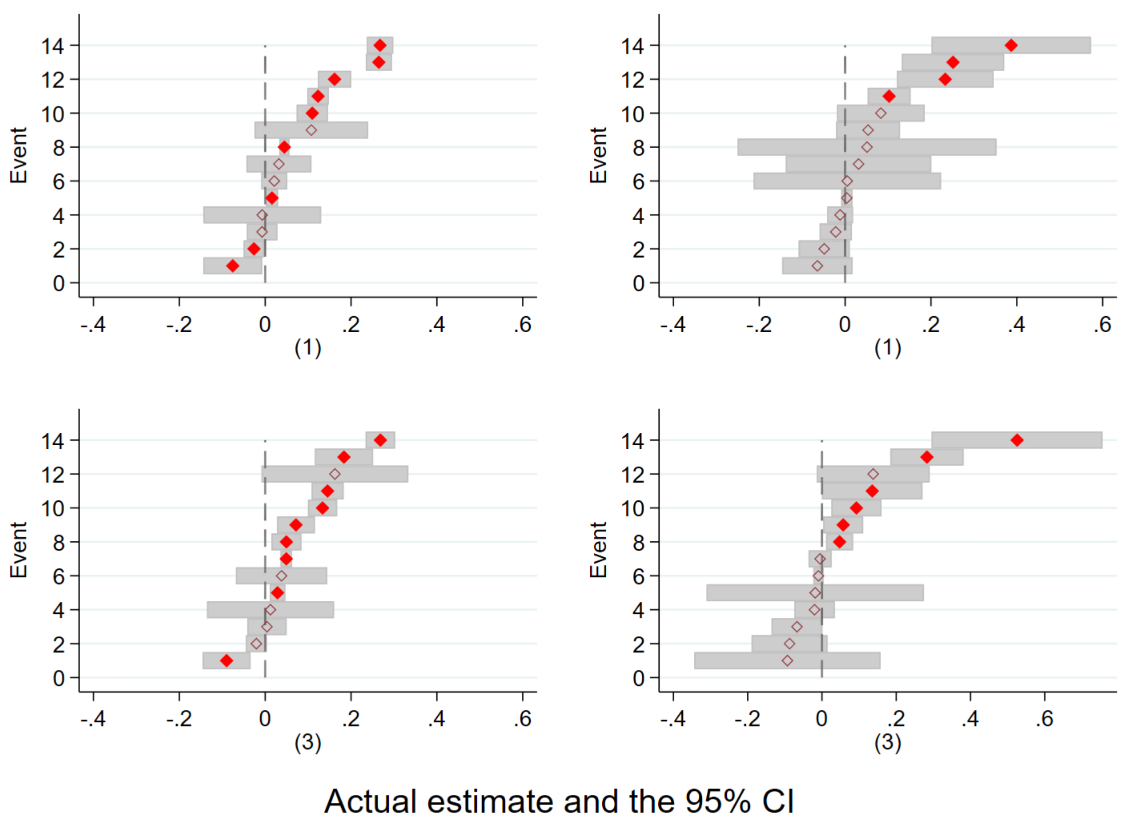
Table 2.B.4. Effect of UD laws on employment growth - refined sample

This table reports the results of my baseline regressions using refined samples following Donelson, Kettell, McInnis, and Toynbee (2021). Panel A extends the sample from 2010 to 2018 and include the two states that adopted the UD laws after 2010 (DC and LA); I also follow Donelson, Kettell, McInnis, and Toynbee (2021) and change the timing of UD adoption for Utah from 1992 to 2000. Panel B further limits the sample to 1996-2015 in order to fully adapt to the sample period used in Donelson, Kettell, McInnis, and Toynbee (2021). *UD* is a dummy variable indicating whether a state has enacted the UD laws. I include firm fixed effects and industry_year fixed effects across all specifications. Firm-level controls included in the regressions are leverage, size, asset tangibility, and ROA. State-level controls included in the analysis are unemployment rate, GDP growth, union coverage, and real GDP. All standard errors are clustered at the state of incorporation level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Employment Growth		
	(1)	(2)	(3)
Panel A: 1994-2018			
UD	0.0349*	0.0338*	0.0325*
	(0.0205)	(0.0187)	(0.0184)
Observations	54706	54694	54694
R^2	0.279	0.291	0.291
Panel B: 1996-2015			
UD	0.0389*	0.0385*	0.0375*
	(0.0229)	(0.0208)	(0.0207)
Observations	47283	47272	47272
R^2	0.282	0.295	0.295
Firm-Level Controls	No	Yes	Yes
State-Level Controls	No	No	Yes
Firm FE	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes

Figure 2.B.1: Event-specific estimates for employment growth

This figure shows the event-specific point estimates (diamond markers) and confidence intervals for firm-level employment growth. The point estimates and CIs are obtained by running the regression in Equation (1) using only one treated state each time. Figure (1) and (3) control for firm and industry_year FE, whilst Figure (2) and (4) further include headquarter_state_year FE. Regarding the samples, Figure (1) and (2) exploit the full sample after excluding the treated states that are irrelevant in a specific event; Figure (3) and (4) further trim the sample to a (-5,+5) event window. The vertical gray dash line indicates the null hypothesis that UD adoption has no effect on employment growth in a certain state. If the 95% confidence interval bars don't cross the gray dash line, the null hypothesis would be rejected. Red diamond markers represent significant effects of the UD adoption, regardless of the sign; hollow diamond markers indicate that the null hypothesis is failed to be rejected. Across all the figures, most of the diamond markers lie right to the gray dash line of 0, and the ones that fall on the left are all insignificant. This suggests that overall, the UD adoption has a positive and significant impact on firm-level employment growth.



Chapter 3

Do Customers Play a Corporate Governance Role?

3.1 Introduction

The corporate governance literature typically focuses on the governance functions performed by shareholders, product market competition, and regulators (e.g., Gillan and Starks (2000); Giroud and Mueller (2010); La Porta, Lopez-de Silanes, Shleifer, and Vishny (1997)). Anecdotal evidence suggests that customers also play a significant role in disciplining their suppliers. In 2017, Boeing raised anti-competition concerns over a proposed M&A deal of a supplier, threatening to cancel contracts. In 2019, Nestlé stopped buying from Cargill Inc. when the soybeans supplier failed to provide evidence that its oilseeds were not produced on converted land. In this paper, we examine whether these anecdotes are examples of a broader empirical pattern.

Specifically, we ask whether customers perform a governance role vis-à-vis their suppliers. Do customers monitor suppliers and exit the relationship after detecting governance issues? Does customer monitoring deter poor governance *ex ante*? We answer these questions by focusing on a specific governance issue, earnings management, where firms chase short-term earnings goals at the expense of long-term economic value. Some types of short-term earnings management, such as EPS-boosting repurchases, exhaust financial resources and reduce innovation and investment (Almeida, Fos, and Kronlund (2016)). Customers may thus be incentivized to monitor and exit, as earnings manipulation may affect product quality and supply stability.

However, capturing customers' response to suppliers' earnings management is challenging for two reasons. First, endogeneity issues may bias the results: for instance, a supplier's decision to manipulate earnings and a customer's decision to exit can be simultaneously influenced by unobservable factors, such as poor performance. Second, it is difficult to identify whether a relationship termination is triggered by customer monitoring, as it can be initiated by either the supplier or the customer.

We address the endogeneity problem by using a discontinuity in firms' incentive to conduct EPS-boosting repurchases when they are about to miss their analysts' EPS forecast. This regression discontinuity design (RDD) largely ensures the similarity and comparability between the treated (firms just about to miss analysts' forecast) and controls (firms just meeting analysts' forecast). Any differentially higher frequency of customer exits for treated firms would thus be due to EPS-boosting repurchases caused by their stronger incentive to manipulate.

We perform our analysis using the supplier-customer relationships between U.S. firms and their customers from 2003 to 2019 covered by FactSet Revere. We show that *major customers* - the ones representing more than 10% of the supplier's sales - sever the supplier-customer relationship when their suppliers have short-term EPS incentives. As major customers have stronger monitoring incentives (Cen, Dasgupta, Elkamhi, and Pungaliya (2016)) and are less likely to be relinquished by suppliers (Costello (2020)), our results suggest that the break is initiated by the customer and triggered by customer monitoring.

Why do customers care about suppliers' EPS-boosting repurchases and how can they monitor them? Customers care because these repurchases are very costly and would impose an adverse impact on firm outcomes, potentially affecting their own production. Replicating the analysis in Almeida, Fos, and Kronlund (2016), we find a significant reduction in investment and financial capacity subsequent to firms' short-term EPS incentives, which may motivate customer exits. Customers can monitor as they have an information advantage - they frequently engage in business interactions with their suppliers and may detect the financial cost of EPS manipulation from changes in inventory as well as trade credit provision (Cen, Dasgupta, Elkamhi, and Pungaliya (2016)). By contrast, analysts and investors typically learn about repurchases several months after they are initiated from disclosures in firms' quarterly or annual filings (Hribar, Jenkins, and Johnson (2006)).[1]

[1]The SEC recently updated the disclosure requirements regarding share repurchases by requiring either quarterly or semi-annual disclosure of firms' daily share repurchases starting from October 1, 2023. However, the retrospective nature of the disclosure is not changed as the information is typically disclosed in 10-Q and 10-K filings.

Our baseline finding indicates that major downstream firms in supply chains perform an ex-post governance function on upstream suppliers. The economic magnitude of this effect is large: short-term EPS incentives increase firms' probability of losing each of their major customers by 4.9 percentage points, an increase of 181% relative to the average probability of losing a major customer, and a $40.8 million loss in annual sales according to a back-of-the-envelope calculation.

Our identification strategy captures the effects of short-term EPS incentives, which induce EPS-boosting repurchases but may also incorporate other types of manipulation. However, most types of earnings manipulation do not consume financial resources, at least not in the short term, while repurchases do. Moreover, we observe a large increase in repurchases for firms with short-term EPS incentives. Thus, it is likely that the customer exits are caused by EPS-boosting share repurchases. We also conduct several ancillary tests to further support this claim.

If customer exits are caused by EPS-boosting share repurchases depleting suppliers' finances, this effect should be concentrated in suppliers with limited financial resources. We therefore split the sample based on firms' *ex-ante* financial constraints and find that major customer losses are concentrated in financially constrained firms. This suggests that customers care about EPS manipulation in so far as by worsening suppliers' financial conditions, it impairs product quality, trade credit provision or in the worst case, supply chain stability.

What factors favor customer governance? We first look at a customer's reliance on a supplier for its input. On the one hand, customers that buy a large proportion of inputs from a specific supplier have stronger incentives to monitor and possibly exit, as the supplier's performance can have a large effect on their own production. On the other hand, a customer who is heavily dependent on a supplier might face high switching costs and thus be less likely to exit the relationship. To investigate which effect prevails, we categorize customers into two groups based on how large the relationship-level trade volume is relative to their cost of goods sold. Customer monitoring and exits are concentrated in relationships where not only the customer is a major customer but also the supplier is important to the customer.

We then ask whether customers with more outside options make better monitors. We capture the degree of competition among suppliers using two measures: the number of competitors a firm has from FactSet Revere and the Herfindahl-Hirschman index (HHI) developed by Hoberg and Phillips (2016). We find that short-termist firms operating in competitive product markets experience more customer losses. This confirms the governance function of product market competition and shows that customers with more outside options are more likely to discipline suppliers.

We also look at switching costs as a factor that might hinder customer governance. We use three proxies for product specificity: a firm's R&D expenditure, the product similarity between the firm and its rivals, and whether it operates in a durable goods sector.[2] We find that customer exits are less likely when the supplier with short-term EPS incentives produces unique products. While customers buying specific inputs might be more concerned about their suppliers' financial conditions, they may also struggle to find alternative suppliers they can switch to (e.g., Barrot and Sauvagnat (2016); Custódio, Ferreira, and Garcia-Appendini (2022)) and thus be less prone to exit.

We next examine whether the expectation of customer governance deters EPS manipulation. We show that suppliers with major customers are less likely to conduct EPS-boosting repurchases when they are about to miss their analysts' earnings forecasts. This result holds using three different proxies for the existence and importance of major customers: whether a firm has at least one major customer, the number of major customers, and the sales concentration (HHI) of major customers. The ex-ante effect of customer governance is economically important: firms' probability of conducting accretive share repurchases when they are about to miss an earnings forecast drops by 38.4% (compared to the sample average) in the presence of major customers.

Finally, we study whether firms with short-term EPS incentives try to prevent the exit of their major customers. Using the heterogeneity in suppliers' sales dependence on different major customers, we find that the likelihood of relationship breaks does not increase between firms and their *largest customer*. This suggests that firms strategically prioritize their *largest customer* in order to minimize the cost of customer losses.

How do suppliers persuade their *largest customer* to stay? We propose that firms with short-term EPS incentives offer financial concessions to their *largest customer*. Specifically, we look at trade credit - an important financial tool supporting supply-chain stability and facilitating trade (Ersahin, Giannetti, and Huang (2021), Breza and Liberman

[2]Banerjee, Dasgupta, and Kim (2008) argue that firms in durable goods sectors produce differentiated products that are more specific to each individual customer, especially to major customers.

(2017)). We follow Freeman (2023) and manually read through firms' 10-K filings to collect supplier-customer relationship-level trade credit data. Our findings show that firms with short-term EPS incentives do not change their overall trade credit terms but do strategically re-allocate trade credit across customers. In particular, firms extend more trade credit to their *largest customer* and cut trade credit to other major customers.

To summarize, our results show that downstream firms in supply chains play a governance role on their upstream suppliers. We show that major customers exit the supply chain relationships when their suppliers have the incentive to manipulate EPS. Although short-termist firms may use various manipulation methods, our analyses suggest that EPS-boosting repurchases is the most likely mechanism. We also document an ex-ante effect of governance by showing that suppliers with major customers are less likely to conduct such repurchases when they are about to miss their EPS forecasts. Finally, our results suggest that short-termist suppliers prioritize the relationships with their *largest customer*, using trade credit as a way to avoid the impact of customer governance.

3.1.1 Related Literature

This paper contributes to the corporate governance literature by highlighting the monitoring and governance role customers play. Existing studies focus on understanding the governance role played by shareholders (e.g., Gillan and Starks (2000); Brav, Jiang, Partnoy, and Thomas (2008)), product market competition (e.g., Giroud and Mueller (2010),Giroud and Mueller (2011)), as well as legislative and regulatory actions (e.g., La Porta, Lopez-de Silanes, Shleifer, and Vishny (1997); Larcker, Ormazabal, and Taylor (2011)). Few papers have studied the role played by customers. Cen, Dasgupta, Elkamhi, and Pungaliya (2016) find that long-term relationships with major customers lead to lower loan spreads and looser loan covenants, pointing to the certification role of major customers via their screening and monitoring function. Cai and Zhu (2020) confirm the monitoring role of major customers by showing a negative relation between the presence of major customers and suppliers' cost of debt in the bond market. Chen, Su, Tian, Xu, and Zuo (2023) provides further evidence on the disciplinary role of major customers by showing that firms with major customers are less likely to commit misconduct. Our paper extends this small literature by showing relationship-level evidence that corporate customers monitor and discipline their suppliers by severing the relationship with short-termist suppliers and their subsequent strategic interactions.

This paper also extends the literature on corporate short-termism caused by managers' incentives to meet performance - especially EPS - goals. In the survey of Graham, Harvey, and Rajgopal (2005), 74% of Chief Financial Officers (CFO) describe analysts' consensus EPS estimate as the most important performance target, and 78% of them admit their willingness to take real economic actions (e.g., decreasing discretionary spending or delaying a new project) to meet this target. Bennett, Bettis, Gopalan, and Milbourn (2017) confirm the pervasive use of EPS as a performance metric in executive pay - around 45% of non-equity and equity grants link payouts to an EPS goal. They also find that firms that just exceed their EPS goals have higher abnormal accruals and lower R&D expenditures.

Our paper is most closely related to two recent studies documenting the negative effects of firms using repurchases to meet analysts' EPS forecasts. Almeida, Fos, and Kronlund (2016) and Almeida, Ersahin, Fos, Irani, and Kronlund (2019) show that firms experience a decline in employment, investment, cash holdings, and productivity when they have incentives to manipulate EPS. Although the market seems to be oblivious to EPS-driven share repurchases, our paper shows that corporate customers are not. This suggests that the impact of corporate myopia extends beyond firm boundaries and that short-termist earnings management has real consequences for firms' customers.

Finally, our paper contributes to the literature on trade credit and its role in supply chains. Ersahin, Giannetti, and Huang (2021) find that when firms are struck by natural disasters, they both obtain and extend more trade credit in order to stabilize supply chains. Analysing the trade relationships between a large Chilean retailer and its suppliers, Breza and Liberman (2017) show that restrictions on trade credit extensions reduce the likelihood of trade, inducing the large customer to shift away from affected suppliers. Our results confirm that firms use trade credit as means of preserving relationships with customers when they have incentives to manipulate EPS.

The rest of the paper proceeds as follows. Section 3.2 presents the data and the empirical methodology. Section 3.3 shows the main empirical results. Section 3.4 analyzes firms' responses to major customers' monitoring and exit. Section 3.5 contains additional analyses and robustness checks. Section 3.6 concludes. We describe the definition of variables and some ancillary results in the Appendix.

3.2 Data and Methodology

3.2.1 Data and Sample Construction

We use a large panel of U.S. public firms from 2003 to 2020 to study the impact of firms' short-term incentives on their supply chain relationships. We combine four datasets to construct our final sample: accounting data and international stock return data from Compustat, supply-chain network data from FactSet Revere and Compustat Segment, stock returns of U.S. firms from CRSP, and analysts' earnings forecasts and actual earnings information from I/B/E/S. Our final sample uses the overlapping coverage of these four datasets.

3.2.1.1 Firm-Level Data

We obtain the accounting data of U.S. firms from Compustat North America. We first extract firm-quarter observations from the Compustat Quarterly database to construct adjusted earnings per share and the repurchase measures. We exclude highly-regulated utility firms (SIC 4900-4999) and financial firms (SIC 6000-6999) as well as firm-quarters with missing or non-positive assets. As our relationship-level analysis is at the annual level, we retrieve firm-level accounting data from Compustat Annual and merge it with the annualized EPS data. We extract stock return data from CRSP and focus on common stocks with share class code 10 or 11. The richness of our supply chain data allows us to also examine firms' relationships with their international customers. To obtain accounting and stock return data for the international trading partners, we use Compustat Global as our data source.[3] We convert accounting variables denominated in foreign currencies into USD using the exchange rates at the end of each calendar year reported by the Compustat Conversion File.

3.2.1.2 Supply Chain Data

We use FactSet Revere to build the supply chain relationships between suppliers and customers. FactSet Revere is a specialized database that describes vertical and horizontal

[3] We additionally compare the coverage offered by Datastream and Worldscope for our international sample. We find Compustat Global has the largest coverage among these three datasets with the largest number of non-missing accounting variables. The difference in coverage is attributed to the greater coverage on large firms in FactSet Revere, and Compustat Global has the best data availability for large international firms as reported by Dai (2012). To ensure the consistency in data collection method, we solely rely on Compustat Global for our international sample.

relationships of large and mostly listed firms. It includes around 450,000 global business relationships starting from 2003. This data has been used in finance and economics studies such as Ding, Levine, Lin, and Xie (2021) and Boehm and Sonntag (2020), and is widely adopted in the supply chain management literature (e.g., Son, Chae, and Kocabasoglu-Hillmer (2021)).[4]

FactSet Revere reports thirteen types of business relationships, of which we limit our scope focusing only on the supplier-customer relationships. FactSet Revere uses a proprietary research method and collects business information annually from primary public sources such as 10-K fillings, investor presentations, and press coverage. The coverage is thus noticeably broader than the Compustat Segment Customer File, which only covers the information of customers that represent more than 10% of a firm's annual sales as disclosed in 10-K filings.[5] We define a customer as a major customer of a supplier if that supplier-customer relationship is covered by both FactSet Revere and Compustat Segment in that year. Additionally, we define a customer as a supplier's largest customer if it accounts for the highest sales proportion of that supplier in that year as reported by Compustat Segment.

For each documented relationship, FactSet Revere reports detailed information including the start date, end date, relationship type and firm identifiers. It is worth emphasizing that the detailed information on relationship duration allows us to accurately track the termination of supplier-customer relationships. Specifically, when a relationship between a supplier and one of its major customers no longer appears in our sample, it means that the relationship is severed, not that the sales towards this major customer is reduced below the 10% threshold.

As our study is at the annual level, we follow the literature and annualize the relationship data: when the distance between the start date and end date of a relationship is longer than one calendar day, we treat the relationship as active in that year. We use CUSIP to link the supply chain data from FactSet Revere with CRSP and I/B/E/S, and use the CRSP/Compustat link table to merge the data with Compustat North America. When merging FactSet Revere with Compustat Global, we use the ISIN code. Consistent

[4]See Boehm and Sonntag (2020) and Ding, Levine, Lin, and Xie (2021) for a detailed discussion on the coverage and structure of FactSet Revere.

[5]We compare the Compustat Segment Customer File with FactSet Revere, and find that 97% of the customer relationship in Compustat with a disclosed ID has been recorded in FactSet Revere. In our final sample, 8.86% of the relationships are covered by Compustat Segment.

with our sample of suppliers, we exclude customers that operate in the utility industry (SIC 4900-4999) or financial industry (SIC 6000-6999).

3.2.1.3 Trade Credit Data

Conventional databases only provide trade credit data at the firm level. This limitation introduces an obstacle in understanding the extension of trade credit at the supplier-customer-relationship level. To unveil any re-allocation effect of trade credit resources across a firm's major customers, we follow Freeman (2023) and manually read through firms' 10-K filings to collect the pair-level trade credit data.

There are two sets of SEC reporting regulations regarding the disclosure of pair-level trade credit in 10-K filings. The Statement of Financial Accounting Standards (SFAS) No.14 and No.131 require public firms to disclose customers representing more than 10% of their sales, whilst Financial Accounts Standards Board (FASB) No.105 requires the disclosure of concentrations of credit risk. For firms that see trade credit extension to major customers as an important part of credit risk, either the dollar amount or the percentage of accounts receivable towards each major customer would be disclosed in their 10-K filings.

The nature of these regulations implies that only a proportion of firms would disclose the pair-level trade credit data and, when they do, they typically only disclose the data for major customers. This would inevitably lead to a limited sample size and a potential concern over self-selection. However, when studying the role of trade credit as a financial concession tool, there are two underlying assumptions. First, customers are important enough for suppliers to offer the concession; and second, the dollar value of the financial concession should be large enough to mitigate the cost customers have to bear if they choose not to exit. In our setting, we look at the governance role played by major as opposed to minor customers, which satisfies the first assumption. To persuade major customers to stay, the financial concession is likely to be large and form part of the credit risk, which satisfies the second assumption. Nevertheless, our findings obtained using this data should be interpreted with caution.

3.2.2 Identification Strategy

Estimating the causal effect of EPS manipulation on supplier-customer relationship stability presents empirical challenges due to two major endogeneity concerns. First, the analysis may suffer from reverse causality as the characteristics of supplier-customer relationships, such as relationship-specific investment, may affect actual earnings management (Raman and Shahrur (2008)). Second, empirical results of standard OLS models may suffer from omitted variable bias: firms' decision to manipulate earnings and customers' decision to exit can be simultaneously influenced by unobservable factors, such as poor performance. To address theses concerns, we follow Almeida, Fos, and Kronlund (2016) and adopt a fuzzy regression discontinuity design, exploiting the discontinuity in managers' incentive to manipulate EPS using share repurchases.

The underlying idea of this identification strategy is that firms have strong incentives to meet or beat analysts' EPS forecasts - managers may use share buybacks to boost EPS if firms are only a few cents away from meeting their analysts' forecasts.[6] Firms that barely meet or miss the earnings forecast can be seen as having similar economic fundamentals but have a discontinuous jump in their incentive to conduct EPS-boosting share repurchases. Exploiting this allows us to tease out the effect of short-term EPS incentives on supplier-customer relationships, where EPS-boosting repurchases is the most likely mechanism.

3.2.2.1 Pre-Repurchase EPS Surprise

To establish the discontinuity design, we first calculate the pre-repurchase EPS surprises following Almeida, Fos, and Kronlund (2016).[7] The pre-repurchase earnings surprise for firm i in quarter q, denoted as $Sue\_adj_{i,q}$, is defined as the quarterly difference between the repurchase-adjusted EPS and the median value of analysts' forecasted EPS, standardized by the end-of-quarter stock price[8]:

$$Sue\_adj_{i,q} = \frac{EPS\_adj_{i,q} - Median\_EPS_{i,q}}{Price_{i,q}}. \tag{3.2.1}$$

When there are multiple forecasts, we take the latest updated analysts' median EPS forecast of each quarter (99.9% of the forecast was updated less than three months before the actual earnings were released). This to some extent alleviates the concern that

[6]See Bhojraj, Hribar, Picconi, and McInnis (2009) and Almeida, Fos, and Kronlund (2016) for a detailed discussion and an example on how firms use repurchases to boost their earnings per share.

[7]This method is also used in Almeida, Ersahin, Fos, Irani, and Kronlund (2019) and Almeida, Fos, Hsu, Kronlund, and Tseng (2020).

[8]We perform robustness checks where the pre-repurchase earnings surprise is not normalized by the end-of-quarter stock price, the results remain both qualitatively and quantitatively similar.

missing/meeting the analysts' EPS forecast is due to sudden changes in the economic environment.

This method could potentially lead to two endogeneity concerns. First, analysts' EPS forecasts may be influenced by firm managers. To alleviate this concern, we take the median value of all analysts' EPS forecasts, as it is unlikely for managers to be able to influence all analysts. Second, it is likely that analysts' forecasts already reflect their anticipations regarding firms' future earnings. However, if analysts were to anticipate worsened earnings of a firm and incorporate the anticipations in the forecasts, we would expect a lower $Median\_EPS_{i,q}$. This would in turn reduce a firm's likelihood of having negative pre-repurchase earnings surprises, introducing bias agasint our baseline findings.

To calculate the *repurchase-adjusted EPS* in Equation (3.2.1), we use the following formula:

$$EPS\_adj_{i,q} = \frac{E\_adj_{i,q}}{S\_adj_{i,q}} = \frac{(E_{i,q} + I_{i,q})}{(S_{i,q} + \Delta S_{i,q})} \tag{3.2.2}$$

where $E_{i,q}$ is the reported earnings calculated as the actual earnings per share times the number of shares outstanding; $I_{i,q}$ is the estimated forgone interest due to the repurchases and is calculated as the after-tax return a firm would have obtained if it invested the repurchase stock in a 3-month T-bill; $S_{i,q}$ is the number of shares outstanding at the end of each quarter, and $\Delta S_{i,q}$ is the estimated number of shares repurchased calculated as the repurchase amount divided by the average daily stock price. $EPS\_adj_{i,q}$ measures what the EPS would have been in the absence of share repurchases.

The repurchase-adjusted EPS is an estimated measure and its accuracy depends on the underlying assumptions and calculations. For instance, $\Delta S_{i,q}$ is an extrapolated measure as we do not have information on the exact number of shares being repurchased, which may be biased in the case of high stock price volatility. It is admittedly hard to gauge the direction and degree of this bias. However, the fact that we use the averaged daily stock price as the denominator to some extent mitigates the influence of extreme stock prices. Also, $I_{i,q}$ may be an underestimation of the forgone value when firms conduct repurchases as firms may invest the cash used for repurchases in projects with returns higher than the 3-month T-bill. Nevertheless, for this to have a real impact on our measure, the return of this investment opportunity should be paid off within the same quarter, which is relatively unlikely in most cases.

Building on the pre-repurchase EPS surprises measure, we further define a negative pre-repurchase EPS surprise dummy and an accretive repurchase dummy. The negative pre-repurchase EPS surprise dummy, Neg_Sue, equals one if the pre-repurchase EPS surprise $Sue\_adj_{i,q}$ is negative, zero otherwise. The accretive repurchase dummy equals one if the repurchase increases EPS by at least one cent, zero otherwise.[9]

Having calculated firms' pre-repurchase EPS surprises, we next proceed to examine whether there is a discontinuity in firms' likelihood to engage in share repurchases around the zero pre-repurchase EPS-surprise threshold. We show that firms with negative surprises are 4.9% more likely to conduct short-termist accretive share buybacks (representing a 35.5% increase compared with the sample average). The results are presented in Table 3.E.1 and the graphical evidence using an RDD plot is shown in Figure 3.F.1.[10]

3.2.2.2 Regression Specifications

In our main analysis, we adopt this regression discontinuity design to examine the effect of EPS-boosting repurchases - instrumented by variations in the incentive to manipulate - on supplier-customer relationship stability. Specifically, we estimate the reduced-form regression using the formula below:

$$\begin{aligned} Y_{i,j,t} = \alpha &+ \beta_1 Neg\_Sue_{i,t} + \beta_2 Neg\_Sue_{i,t} Major\,Customer_{i,j,t} + \beta_3 Major\,Customer_{i,j,t} \\ &+ \beta_4 Sue\_adj_{i,t} + \beta_5 Neg\_Sue_{i,t} Sue\_adj_{i,t} + \beta_6 Major\,Customer_{i,j,t} Sue\_adj_{i,t} \\ &+ \beta_7 Neg\_Sue_{i,t} Sue\_adj_{i,t} Major\,Customer_{i,j,t} + \beta_8 X_{i,t} + \theta_{j,t} + \eta_{i,j} + \gamma_{ind_i ind_j,t} + \epsilon_{i,j,t}, \end{aligned} \tag{3.2.3}$$

where i and j index firms and their customers respectively. $Y_{i,j,t}$ is the outcome variable *Relationship Break*$_{i,j,t}$, which equals one if the relationship between firm i and firm j is active in year t but inactive in year $t+1$, and zero otherwise. The identification of relationship break further limits our sample period to 2003-2019. $Neg\_Sue_{i,t}$ is our independent variable of interest, which is an indicator that equals one if a firm has an annualized negative pre-repurchase EPS surprise. $Sue\_adj_{i,t}$ is the annualized pre-repurchase EPS surprise. X is a vector of controls of suppliers. We saturate our regressions with fixed effects to control for the unobservable characteristics at different levels. Specifically,

[9] For firms whose Sue_adj is between (-0.003,0) and conducted accretive repurchases, 67% eventually met the earnings forecast. For firms whose Sue_adj is between (-0.002,0) or (-0.001,0), 71% or 80% of firms that did accretive repurchases successfully met the earnings forecast.

[10] Similar to Almeida, Fos, and Kronlund (2016), we find no evidence of any discontinuity in the probability of decretive share repurchases around the zero earnings surprise threshold (Figure 3.F.2). We define the decretive repurchase dummy as one if the repurchases conducted by a firm decrease its EPS by at least one cent, and zero otherwise.

we include the firm-pair fixed effects to control for time-invariant supplier-customer-level factors. We also include customer-year and industry-pair-year fixed effects to control for time-varying heterogeneities.

Given that our supply-chain data is at the annual level whilst the EPS data is at quarterly level, we follow Almeida, Ersahin, Fos, Irani, and Kronlund (2019) and limit our analysis only to the earnings in the fourth quarter of a firm's fiscal year.[11] To alleviate the concern that firms having negative pre-repurchase earnings surprises are intrinsically different from those with positive surprises, we follow Almeida, Fos, and Kronlund (2016) and limit the sample to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$ (98.61% of the firms using repurchases to meet their earnings forecasts fall within this window in our sample). In later analyses, we use different windows and our results remain both qualitatively and quantitatively similar.

One potential concern is that the relationship break can be driven by other factors, such as poor performance, instead of firms' short-term EPS incentives. Although limiting the sample to a small window ($-0.003 \leq Sue_{adj} \leq 0.003$) to some extent mitigates this problem, we perform three robustness tests to address this issue. First, for firms with negative pre-repurchase earnings surprises, we exclude those that did not conduct EPS-boosting repurchases. Second, we restrict the sample to firms with negative pre-repurchase earnings surprises and compare the likelihood of losing customers between firms that are closer/further away from the zero-earnings threshold. As it is more feasible for firms with less negative earnings surprises to meet their EPS forecasts using repurchases, a higher probability of losing customers for these firms would indicate that the customer losses are more likely to be triggered by repurchases instead of other factors. Third, we again only look at firms with negative pre-repurchase earnings surprises but this time we compare the probability of a customer exit between firms that did repurchases and firms that did not. We find that customer exits are concentrated in firms that carried out EPS-boosting share repurchases. Although these tests cannot fully eliminate all other possible factors, our findings suggest that EPS-boosting repurchases is the most likely mechanism in triggering customer exits. The details of these tests are discussed in Section 3.5.1.

[11] We also alternatively aggregate the quarterly earnings surprise to an annual frequency by setting Neg_Sue as one if the firm's quarterly pre-repurchase EPS surprise is negative for at least two quarters in that year. Our results are qualitatively and quantitatively similar. Specifically, the continuous variable Sue_{adj} for that year is set to be the minimum of the pre-repurchase EPS surprises across negative/positive surprise quarters if the Neg_Sue dummy for that year equals one/zero.

Considering that our empirical setting is fuzzy RDD, we further estimate a 2SLS regression using $Neg\_Sue_{i,t}$ as an instrument to the level of firms' share repurchases,

$$\begin{aligned}Y_{i,j,t} = \alpha &+ \beta_1 Repurchases_{i,t} + \beta_2 Repurchases_{i,t} Major\,Customer_{i,j,t} + \beta_3 Major\,Customer_{i,j,t} \\ &+ \beta_4 Sue\_adj_{i,t} + \beta_5 Neg\_Sue_{i,t} Sue\_adj_{i,t} + \beta_6 Major\,Customer_{i,j,t} Sue\_adj_{i,t} \\ &+ \beta_7 Neg\_Sue_{i,t} Sue\_adj_{i,t} Major\,Customer_{i,j,t} + \beta_8 X_{i,t} + \theta_{j,t} + \eta_{i,j} + \gamma_{ind_i ind_j,t} + \epsilon_{i,j,t},\end{aligned} \tag{3.2.4}$$

where $Repurchases_{i,t}$ is defined as a firm's net repurchases normalized by assets if it conducts accretive repurchases, and zero otherwise. We follow Fama and French (2001) and construct net repurchases as the increase in common treasury stock if treasury stock is not zero or missing. If the treasury stock is zero in the current and prior quarter, we measure net repurchases as the difference between stock repurchases and stock issuance. If either of these two values is negative, we set net repurchases as zero.

3.2.3 Summary Statistics

Table 3.1 reports the summary statistics at both firm and relationship levels. These descriptive statistics are calculated using the 1270 firms included in our final sample, that is, firms whose pre-repurchase earnings surprises are within (-0.003, 0.003). We also impose the requirement that all the included firms and their customers have available accounting and financial data. We report the statistics for both the full sample as well as the split sample where firms with slightly negative pre-repurchase earnings surprises (treated firms) are separated from the ones with slightly positive earnings surprises (control firms). Our measure of relationship stability is at the relationship level, of which the unconditional mean is 12.8%, regardless of the short-term EPS incentives. When limiting the relationships to only include major customers, the average probability of a relationship break is dropped to 2.7%. Treated firms are more likely to lose a major customer (3.3%) compared to control firms (2.4%).

We then collapse our relationship-level data to the firm level and calculate the corresponding descriptive statistics. Treated firms have higher unconditional means of repurchase amount and a higher probability of conducting accretive repurchases as opposed to control firms. In our sample, an average firm has eight customers each year, one of which is identified as a major customer. Firms on average have two major customers each year when we limit the sample to only include firms with at least one major customer.

We present the industry and country distributions of firms whose pre-repurchase earnings surprises fall within the $-0.003 \leq Sue\_adj \leq 0.003$ window as well as that of their customers in Table 3.A.2. Both suppliers and customers are representative of the entire sample (without the window restriction) in terms of industry and country/region distributions. The total numbers of firms in the industry/country distribution table may not match the actual numbers of firms used in our sample for two reasons. First, firms may modify their industry classifications during the sample period, resulting in duplicated counts. Second, certain customer firms lack available country data, leading to some missing customers in the distribution table. We show the time trend of our sample in Table 3.A.3. As expected, we do no observe a clear time trend in the number of firms as our analysis is restricted to a small window.

3.2.4 Preliminary Results: Reduced Financial Capacity

A key channel through which EPS-boosting repurchases affect the relationship with customers is firms' financial capacity. Manipulating EPS using share buybacks is expensive: to finance the repurchases, firms may need to drain their cash holdings or cut real investment (Almeida, Fos, and Kronlund (2016)). As a result, firms may suffer from weakened financial muscle and an increased probability of distress, which in turn may affect customers.

To test this assumption, we conduct firm-level analyses investigating whether firms' short-term EPS incentives have an adverse impact on corporate outcomes and report the evidence in Table 3.2. Consistent with Almeida, Fos, and Kronlund (2016), we find that firms with short-term EPS incentives suffer from a drop in cash holdings and future investment. There is also a reduction in firms' interest coverage, suggesting that their ability to pay for the interest expenses deteriorates. Furthermore, we observe a significant reduction in firms' future sales, which indicates changes in the way firms interact with customers. We also estimate the corresponding 2SLS regressions, where Neg_Sue is used as instrumental variable for the repurchase amount in accretive repurchases in first stage regressions. The results are presented in Table 3.E.2 and are consistent with the reduced-form findings.

3.3 Customer Governance

3.3.1 Major Customer Exit

Our first question is whether firms' EPS manipulation leads to customer losses. Supplier-customer relationship may break for two reasons. On the one hand, the cost of EPS manipulation could result in a decrease in production, which may in turn limit firms' ability to fulfill all customer demands and trigger relationship termination with customers. If this is the case, firms are more likely to sacrifice their minor customers as major customer concentration is associated with higher profitability (Campello and Gao (2017)). On the other hand, customers may observe the financial cost of suppliers' EPS manipulation and decide to switch to other suppliers. In this case, the supplier-customer relationship breaks are initiated by customers and would thus be concentrated in major customers. This is because major customers have a stronger incentive and easier access to monitor their suppliers (Cen, Dasgupta, Elkamhi, and Pungaliya (2016)).

Table 3.3 reports the results of the baseline reduced-form regression stated in Equation 3.2.3. We find that the short-term EPS incentive significantly increases the risk of relationship breakdown between a firm and its major customers. As is shown in columns 2 and 3, when firms have the incentive to conduct short-termist share buybacks, there is a 4.7 (4.9) percentage points increase in the likelihood of a relationship break with each of their major customers in the following year without (with) control variables. The economic magnitude of this effect is significant, representing a 181% increase relative to the average probability of losing a major customer in our sample. Furthermore, our back-of-the-envelope calculation suggests that suppliers' short-term EPS incentive leads to around \$40.8 million losses in annual sales (we arrive at this number by multiplying the average sales proportion a major customer represents (17.8%), with the average annual sales of the suppliers in our sample (\$4.7 billion) and the increase in the probability of relationship breaks with major customers (column 3 in Table 3.3)).

We next use the Cox Hazard Model given that our variable of interest measures relationship survival. In this analysis, we allow the baseline hazard to differ across industries, year, and supplier-customer industry pairs in lieu of fixed effects. Columns 4-7 of Table 3.3 report the estimated results. We find the results are consistent with our OLS analyses. The coefficient of the interaction term in column 7 can be used to calculate the hazard ratio, $1.806 = exp(0.591)$, which can be interpreted as an 80.6% increase in the probability of major customer losses.[12]

We also estimate the corresponding 2SLS regression, where Neg_Sue is used as the instrumental variable for the repurchase amount in accretive repurchases in first stage regressions (Equation 3.2.4). This specification allows us to identify the Local Average Treatment Effect (LATE) of accretive repurchases on the stability of supplier-customer relationships. The analysis results are presented in Table 3.E.3. Consistent with the reduced-form findings, we show that the relationships between suppliers and their major customers are destabilized, where EPS-boosting share repurchases appear to be an important driving force. In our sample, the average ratio of repurchase amount relative to total assets for firms that used accretive repurchases to meet analysts' EPS forecast is 7.9%. This suggests that for an average firm conducting EPS-boosting repurchases, there is a 95% increase in the probability of a relationship break with each of its major customers.

We present the above-mentioned evidence in graphical form in Figure 3.1. We collapse all earnings surprises into 20 bins that fall between the range $(-0.008, 0.008)$ and calculate the averaged probability of customer relationship breaks for each bin. The dots represent the calculated probability for each bin and the lines are second-order polynomials fitted through the estimated relationship break probabilities on each side of the zero threshold. As shown in the figure (a) and (b), only major customers exhibit differentially higher frequency of exit when suppliers have negative pre-repurchase earnings surprises, which is consistent with our results from the regression analyses.

3.3.1.1 Alternative Stories and Pre-Trend Analysis

Firms may establish relationships with new major customers seeking efficiency or profitability to mitigate the adverse impact of short-term EPS incentives. To alleviate this concern, we examine the relationship creation of short-termist firms and find no evidence for this. This suggests that the observed major customer losses are not driven by changes in suppliers' corporate strategy targeting new customers.

Our identification strategy allows us to capture how firms' short-term EPS incentives affect the stability of supply chain relationships. Despite our effort in identifying a threshold where there exists only the discontinuity in firms' likelihood of conducting EPS-boosting repurchases, we are unable to completely rule out other discontinuities that may

[12]Details regarding the Cox Hazard Model are discussed in 3.B.

also affect the relationship stability. For instance, firms that locate to the left of the zero pre-repurchase earnings surprise threshold may also be prone to engage in other types of EPS manipulation. However, we argue that the existence of other possible types of manipulation does not affect our interpretation of the results: EPS-boosting repurchases play an important role in triggering major customer exits as they incur real consequences that raise customers' concerns. We perform a battery of robustness tests to support this argument in later sections.

Although our setting to a large extent mitigates the endogeneity issues, we cannot fully rule out the possibility that our results may suffer from reverse causality. As we cannot track down the exact date of relationship breaks between each customer and supplier, it is thus crucial for us to show that there exists no pre-trends of supply chain relationship breaks for suppliers with short-term EPS incentives.

To perform this test, we follow Almeida, Fos, and Kronlund (2016) and examine whether firms' short-term EPS incentives in the current year have any impact on the supply chain relationship breaks i years in advance and report the results in Table 3.E.4. When we compare the supply chain relationship stability of firms with small negative and positive pre-repurchase earnings surprises, firms on either side of the zero earnings surprise threshold have very similar trends with respect to relationship stability prior to having the surprises. Our graphical evidence presented in Figure 3.F.3 also confirms the reduced-form regression results. This validates our no pre-trend assumption and mitigates the concern of reverse causality.

Another potential concern that could cast doubt on our results is the possibility of a discontinuity in the existence of major customers for firms that narrowly miss or meet analysts' earnings forecasts. However, we conduct a balance check and find no such discontinuity.

3.3.2 Reduced Financial Capacity as A Key Mechanism

Why are customers motivated to monitor and exit the relationship with suppliers that have short-term EPS incentives? Our evidence suggests that financial capacity is a key mechanism. In the previous section, we have shown that short-term EPS incentives reduce firms' financial resources, which may have an adverse impact on product quality, trade credit extension, and even trigger financial distress. Because suppliers that are ex-ante relatively financially constrained are prone to suffer from these issues, we should expect to observe more supply chain relationship breaks happening to these suppliers.

We adopt two proxies to measure whether a firm is financially constrained: the Hadlock-Pierce index developed by Hadlock and Pierce (2010) and the Whited-Wu index developed by Whited and Wu (2006). We split the sample using the sample medians of these proxies and conduct our baseline analyses on these sub-samples. The results reported in Table 3.4 show that the customer losses are concentrated in relatively constrained firms, which lends support to our claim that reduced financial capacity is a key mechanism behind major customer exits. Our findings also suggest that the negative impact of short-term EPS incentives on a firm's customer base can be seen as one of the indirect costs of financial distress.[13]

It is likely that in the presence of severe financial distress, EPS manipulation becomes a second-order priority and that firms become less prone to conduct EPS-boosting repurchases (Almeida, Fos, and Kronlund (2016)). In this sense, our results may be driven by the adverse financial condition itself instead of the repurchases. To tackle this concern, we first argue that the financial constraint measures we construct are in relative rather than absolute terms. This means that while financially constrained firms may be less inclined to engage in share repurchases for EPS enhancement, (some of) these firms still possess the capacity to do so. We validate this through a thorough examination of the data. Additionally, we conduct a robustness test using a restricted sample where for treated firms, we only include those that actually did EPS-boosting repurchases. We present the results in Table 3.E.5 and show that financially constrained firms still suffer more from major customer exits.

3.3.3 What Factors Favor or Hinder Customer Governance?

3.3.3.1 Important suppliers regarding trade volume

In preceding sections, we have shown that major customers discipline their suppliers through monitoring and potentially severing the supply chain relationship. We conjecture that the incentive to monitor varies across different customers depending on their reliance on the corresponding suppliers.

It is *a priori* unclear whether a customer's dependence on a supplier favors or hinders

[13] We also use firms' interest coverage ratio (calculated as the ratio of a firm's EBIT divided by its interest expenses) as a proxy for financial conditions and find that firms with ex-ante lower interest coverage suffer more from major customer losses.

its likelihood to exit. On the one hand, customers that buy a fairly large proportion of inputs from a specific supplier are expected to have higher incentives to monitor the supplier and further *exit* the relationship if the supplier is short-term oriented. On the other hand, dependent customers may face a high switching cost and are thus less likely to exit the relationship. However, the fact that customers are on average much larger than the suppliers in our sample implies that the former effect is more likely to prevail - only in very extreme cases a customer buys more than 10% of its input from one specific supplier.[14]

We construct two variables to measure whether a customer is a dependent customer. We first calculate a customer's input dependence on a supplier using the supplier-customer pair-level sales volume divided by the customer's cost of goods sold (COGS). Next, we define *High Trade Volume Customer* as a dummy variable that equals one if the input dependence is above the sample median, and zero otherwise. We also adopt a stricter proxy *High Trade Volume Customer1* where an additional requirement is imposed: the supplier needs to be one of the top 3 suppliers to the customer regarding the input dependence in our sample.

We present the results in Table 3.5. Columns 1-2 focus on a small sample where only major customers are included. We find that among all major customers, only the ones that buy a large share of inputs from the short-termist supplier exit the relationship. We then re-perform the baseline analysis in columns 3-4 using the full sample, where the major customer dummy is replaced with the high trade volume customer dummy. Our findings suggest that our results are mainly driven by customers that buy a large share of inputs from short-termist suppliers and that the concern over supply stability serves as a motive behind customer monitoring.

3.3.3.2 Outside Options, Product Specificity, and Switching Costs

Are all customers equally likely to exit the supplier-customer relationship when their suppliers have short-term EPS incentives? Would outside options and switching costs deter customer exit and thus mitigate the impact of customer governance? In this section, we aim to answer these questions from two perspectives: product market competition and

[14]As Compustat Segment only discloses major customers instead of major suppliers, we reverse the data on sales proportion towards each major customer to obtain the data on how much input a customer purchases from a supplier. This inevitably leads to a restricted sample where suppliers in our sample may not be the major suppliers.

product specificity.

Product Market Competition We first explore the heterogeneous effects of short-term EPS incentives on customer exit from the angle of suppliers' product market competition. When suppliers operate in a competitive product market, their customers have more outside options and are thus more likely to terminate the contract when suppliers engage in EPS manipulation, such as conducting EPS-boosting repurchases. We construct two proxies for suppliers' product market competition. The first proxy counts the number of competitors each supplier has in each year disclosed by FactSet Revere. The second proxy is suppliers' industry HHI developed by Hoberg and Phillips (2016). We then define two dummy variables using the above-mentioned proxies based on the sample medians and perform a cross-sectional analysis. Columns 1-2 in Table 3.6 report the regression results.

We find that major customer losses are more severe for suppliers operating in competitive markets. This suggests that when customers have outside options, they are more prone to exit the relationship when suppliers have short-term EPS incentives. Our findings complement the conventional view in the corporate governance literature that product market competition enforces an important governance discipline on firms. However, existing studies do not explicitly show how this discipline is exerted. By showing that customers exit the supply chain relationships with short-termist suppliers, we propose a plausible mechanism on how product market competition performs governance functions.

Product Specificity Suppliers' product specificity may also affect customers' decisions to exit given its positive relation with the switching cost. Although customers may be more motivated to monitor their specific input providers, it is also more difficult for them to find alternative suppliers they can switch to (e.g., Barrot and Sauvagnat (2016); Custódio, Ferreira, and Garcia-Appendini (2022)) and may thus be less likely to exit. To study this, we follow the literature and use firms' R&D expenditure as our first proxy for product specificity. We define firms with innovation expenditure higher than the sample median as the ones that produce specific inputs. We perform a cross-sectional analysis exploiting this heterogeneity and report the results in column 3 of Table 3.6. We find that firms with higher R&D expenditure suffer less from major customer losses, which suggests that high switching costs can mitigate the impact of customer governance.

Our second proxy for product specificity is an indicator for a firm's product duration.

As argued by Banerjee, Dasgupta, and Kim (2008), firms in durable goods sectors produce differentiated products that are more specific to each individual customer, especially to major customers. Short-termist firms producing durable goods would thus be less likely to lose their major customers as there are limited outside options. However, durable goods typically require warranty or post-purchase service compared with non-durable goods, which imposes a higher requirement on firms' financial capacity. Customers of durable-goods producers may thus be more prone to exit when these suppliers have short-term EPS incentives due to the financial concerns.

To test which effect prevails, we follow the literature and categorize firms whose SIC codes are between 3400 and 4000 as durable goods producers, and those whose SIC codes are between 2000 and 3400 as non-durable goods producers (e.g., Titman and Wessels (1988)). Column 4 in Table 3.6 shows that major customer losses are less severe in firms operating in durable goods sectors.

Our results differ from that of Custódio, Ferreira, and Garcia-Appendini (2022) where they find that the indirect costs of financial distress are more pronounced if firms produce durable goods. This may be due to the difference between the two settings. We examine how firms' incentive to engage in EPS manipulation, such as EPS-boosting repurchases, affects the stability of firms' supplier-customer relationships. Although EPS-boosting repurchases can worsen firms' financial capacity, the fact that firms may self-select into engaging in this type of costly repurchases serves as a buffer to customers' concern over their financial conditions. By contrast, Custódio, Ferreira, and Garcia-Appendini (2022) focuses on the financial distress caused by real estate price shocks, which may have a deeper and wider impact on firms' financing resources and may therefore result in different dynamics.

Our third measure for product specificity relies on the product similarity score developed by Hoberg and Phillips (2016). We use FactSet Revere to identify a firm's competitors and calculate its overall product similarity score compared with these competitors. The overall product similarity score is defined as the sum of the product similarity scores between each firm and all its competitors. If a firm's overall product similarity score is above the sample median, we categorize it as a firm with low product specificity. As the measures constructed by Hoberg and Phillips (2016) only include US firms, the scope of this test focuses solely on US firms and their domestic competitors. Column 5 in Table 3.6

reports consistent results compared with previous tests and shows that short-termist firms with high product similarity scores are more likely to lose their major customers.

Arguably, customers that buy specific inputs or that have limited alternative outside options should have a higher incentive to monitor. It is however important to note that our findings do not infer the intensity of customers' monitoring incentive. Indeed, customers with a high dependence on suppliers may have a stronger incentive to monitor, but they may be less inclined to exit. Instead, they may exploit the *voice* strategy or even serve as a direct rescuer when suppliers experience difficulties. For instance, in 2019, Apple agreed to shorten payment periods and put up $200 million to help its distressed liquid crystal display (LCD) maker, Japan Display Inc.[15]

3.3.4 Ex-Ante Effects of Customer Governance

Having established that major downstream firms discipline their short-termist upstream suppliers via exiting the supply chain relationship, we next turn to explore whether there is an ex-ante effect of customer governance. In particular, suppliers with major customers should anticipate the disciplinary customer exits and may therefore be less inclined to use repurchases to boost EPS when they have negative earnings surprises.

Consistent with this conjecture, we find that the presence and importance of major customers effectively deter firms' use of share repurchases as a way to manipulate EPS. To demonstrate this, we construct three proxies. The first proxy captures the existence of major customers, which is a dummy variable that takes the value of one if a firm has at least one major customer, and zero otherwise. The second proxy measures the potential influence of major customers, which is calculated as the log value of one plus the number of major customers a firm has in that year. The underlying assumption is that the more major customers a firm has, the more customer monitoring and threat of exiting it would face. The definition of our third proxy follows Patatoukas (2012) and Campello and Gao (2017), where we calculate a firm's sales concentration towards its major customers. A high sales concentration of a firm indicates a high sales dependence on major customers, leading to stronger customer governance power.

We perform a firm-level regression analysis and provide evidence on the deterring role of major customers with respect to suppliers' EPS-boosting share repurchases. To con-

[15] See `https://www.reuters.com/article/us-japan-display-funding-idUSKBN1YF2VE` for more details.

trol for the governance role played by shareholders, we additionally include institutional investors' ownership and their ownership concentration (HHI) in the regression analyses. Table 3.7 presents the results. Across all specifications, we observe a significant decrease in short-termist firms' probability of conducting accretive share repurchases when they have major customers. The effect is economically important: for instance, the existence of at least one major customer reduces suppliers' propensity to carry out EPS-boosting repurchases by 38.4% compared with the sample average, and by 29.4% compared with the sample average of firms with short-term EPS incentives. This suggests that major corporate customers, by monitoring and potentially threatening to exit, offer an effective source of corporate governance.

3.4 How Do Firms Respond to Customer Governance?

3.4.1 Major Customer *versus* Largest Customer

Firms' decision to conduct EPS-boosting repurchases when having short-term incentives depends on various factors. Although major customers play a deterrence role, EPS-boosting repurchases still exist in equilibrium, as the benefits of meeting analysts' EPS forecast may outweigh the cost of customer exits for some firms. In such cases, how do firms react to mitigate the consequences brought by customer exits? Do they prioritize customers that they have higher sales dependence on, i.e., the *largest customer*?

To answer these questions, we rank each firm's major customers based on the proportion of a firm's sales to these customers and identify the largest customer of each supplier. We then classify all the rest of the major customers (customers that do not take up the largest sales proportion of a particular supplier) as the major (excluding the largest) customers. We replace the major customer dummy in Equation 3.2.3 with the newly defined largest/major (excluding the largest) customer dummies and re-perform the baseline analysis. If firms see their major customers as equally important, one would expect to see no difference in the likelihood of relationship breaks between the *largest customer* and other major customers.

Our evidence suggests that firms are prone to retain their largest customer whilst relinquishing other major customers. Columns 1-4 of Panel B in Table 3.8 report the results. In both the OLS and Cox Hazard specifications, we find no increase in the probability of a short-termist supplier losing its *largest customer*. However, we find consistent evidence that firms experience a significant increase in losing other major customers when they have short-term EPS incentives. Furthermore, the magnitude of coefficients is larger than that in Table 3.3, indicating that the baseline results are mainly driven by the losses of other major customers, not the losses of the *largest customer*.

If the retention of the *largest customer* is due to firms' consideration over future profitability, we should observe a more distinct contrast between the likelihood of losing the largest customer compared with other major customers if a firm has high sales dependence on its largest customer. According to the descriptive statistics presented in Panel A of Table 3.8, the average sales proportion taken up by the largest customer in our sample is 20.8%, which is significantly higher than that of other major customers (12.5%). For instance, the largest customer of Amgen, a biotechnology company, represents about 41% of its total sales in 2007, whilst the other two major customer represent 18% and 16% of its total sales respectively. One would expect that firms like Amgen have a higher incentive to preserve their relationships with the largest customer, even at the expense of sacrificing other major customers.

To test this, we focus on a sample including only suppliers whose pair-level sales volume to both the *largest customer* and other major customers are identifiable. We then split the sample into two subsamples based on the ratio of a supplier's sales proportion to its largest customer divided by the average sales proportion to its other major customers. If the ratio is above the sample median, we categorize the suppliers as the ones with a high dependence on their largest customer, and vice versa. Columns 5-8 of Panel B in Table 3.8 report the regression results. Consistent with our prediction, firms only tend to prioritize their largest customer at the expense of other major customers if their sales rely heavily on their largest customer.

3.4.2 Financial Concessions: Trade Credit

How do firms preserve their *largest customer*? Do they offer financial concessions to do so? One tool that firms often use when financially contracting with customers is trade credit. We explore in this section whether firms strategically employ trade credit as a means to manage their supply chain relationships.

It is *a priori* unclear how firms change their trade credit provision when manipulating EPS. On the one hand, firms may cut their trade credit extension as engaging in manipulation such as EPS-boosting repurchases is costly, which may potentially lead to financial difficulties. On the other hand, they may offer more trade credit to customers as a way to preserve the relationship (Ersahin, Giannetti, and Huang (2021)). We examine how firms' trade credit days are affected by their short-term incentives and report the results in Table 3.E.6. We find no effect of such incentives on either firms' accounts receivable days or accounts payable days. This suggests that the two effects mentioned above cancel each other out in our setting.

3.4.2.1 Customer-Level Evidence

Although we do not observe changes in trade credit extension at the firm level, firms may have different priorities among customers and thus manipulate trade credit provision at the relationship level. If firms cut their extension of trade credit towards other major customers to satisfy the trade credit needs of their largest customer, we should expect to see a drop in the accounts payable days of the major - but not the largest - customers. To test this, we calculate each customer's accounts payable days at the firm level and examine whether they are affected by suppliers' short-term EPS incentives. As we can only observe a customer's total accounts payable, the payable days we measure are the required period of payment averaged across all its suppliers. Under the assumption that the supplier with short-term EPS incentives is the only one cutting trade credit extension, our analysis provides an estimated effect of suppliers' short-term EPS incentives on customers' accounts payable days.

We follow the methodology in Ersahin, Giannetti, and Huang (2021) and collapse data to the customer level and perform the analysis using the regression model below:

$$\begin{aligned} Change\,in\,AP\,Days_{j,(t-1,t+1)} = \alpha &+ \beta_1 Neg\_Sue\_Pct_{j,t} + \beta_2 Customer\,Importance_{j,t} \\ &+ \beta_3 Neg\_Sue\_Pct_{j,t} * Customer\,Importance_{j,t} \\ &+ \beta_4 X_{j,t} + \theta_j + \gamma_{ind_j,t} + \epsilon_{j,t}, \end{aligned} \tag{3.4.1}$$

where j denotes the customer, and t indicates the quarter. $Neg\_Sue\_Pct_{j,t}$ measures a customer's exposure to its suppliers' short-term EPS incentives, which is calculated as the number of short-termist suppliers normalized by the total number of suppliers a customer has in that quarter. We include $Customer\,Importance$ and its interaction term with $Neg\_Sue\_Pct_{j,t}$ to capture the heterogeneity across different customers with respect to their importance to suppliers. Specifically, $Customer\,Importance$ indicates three different variables that measure the importance based on whether a customer is seen as a major/largest/major (excl. largest) customer by its suppliers.[16] To mitigate the confounding impact of suppliers' short-termism on the trade credit customers receive in the concurrent quarter, we measure the change in customers' *AP Days* as the ratio between the *AP Days* one quarter after and before the treatment quarter. The quarter-level data also allows us to examine the instant change in customers' trade debt subsequent to suppliers' short-term EPS incentives.

Consistent with our predictions, Table 3.9 shows that major - but not the largest - customers are required to make faster payment when their suppliers have short-term EPS incentives. This suggests that short-termist firms cut the trade credit provision towards other major customers, but do not cut the trade credit extended to the *largest customer*. Our findings provide suggestive evidence that that trade credit extension is exploited by short-termist suppliers to retain customers.

3.4.2.2 Relationship-Level Evidence

Do firms extend more trade credit to their largest customer as a way to make financial concessions? A plausible test aiming to answer this question requires detailed relationship-level trade credit data. To collect this data, we follow Freeman (2023) and manually identify the accounts receivable extended to individual major customers disclosed by firms' 10-K filings. Due to the voluntary nature of firms' disclosure, our sample size is inevitably restricted to include not just major customers, but also the ones towards whom the accounts receivable extended by the supplier is available. This, together with our demanding empirical setting (e.g., we only include firms whose earnings surprises locate in a small window), yield a regression panel with 330 observations.[17]

Our main variable of interest is *Trade Credit*, which is defined as the ratio of a firm's accounts receivable extended to each individual customer normalized by its sales to that specific customer. A higher value of *Trade Credit* indicates a better credit term relative to the customer's sales importance. To capture how the credit terms differ across the *largest customer* and other major customers, we include a dummy variable *Largest Customer* and its interaction term with the *Neg_Sue* indicator. Considering that firms may have

[16]To quantify the customer importance, we count the number of relationships that a customer is viewed as a major/largest/major (excl. largest) customer in that quarter and calculate the log value of one plus this number.

[17]The inclusion of fixed effects also requires that the supplier-customer pairs appear more than once with available trade credit data in our sample, which further reduces our sample size.

varying incentives to keep their largest customers, we define *Largest Customer* as one if a customer is the largest customer in at least one year of the following three years. This definition captures two sets of major customers: first, customers that are the *largest* ones both before and after the shock; second, customers that were *major* customers before the shock but become the *largest* ones after the shock. It also excludes the case where a customer was the *largest* customer before the shock but is no longer the largest one afterwards. This definition allows us to mitigate the potential bias in cases where firms are not willing to preserve the previous *largest customer* - for instance, when firms plan to switch to new product lines and target a new group of customers.[18]

Table 3.10 presents the results. The dependent variable *Trade Credit* measures the credit term each customer receives from the supplier in the year following the supplier's earnings surprise. We find that suppliers with short-term incentives offer better trade credit terms to their largest customer if they wish to preserve the corresponding relationships. By contrast, short-termist suppliers provide worsened credit terms to their other major customers. This cut in trade credit may be used as a way to finance the increased trade credit extended to the largest customer.[19]

In sum, our findings suggest that short-termist suppliers strategically re-allocate their trade credit extension towards different customers. Expecting the threat of customer exits, firms with short-term EPS incentives offer better trade credit terms to their largest customer at the expense of other major customers. This helps these firms retain their *largest customer*, which in turn mitigates the adverse impact of short-termism on their future profitability.

3.4.3 Financial Concessions: Product Prices

Another financial concession firms may offer is price reduction. As we do not have data on product prices, we use firms' gross margin as a proxy and provide indicative evidence on whether and how short-termist firms change the prices to reduce the impact of customer governance.

A firm's gross margin is affected by both its product price and cost of production.

[18] When defining *largest customer* as only those that are the largest both before and after the earnings surprises, our results are both qualitatively and quantitatively similar.

[19] Due to the limited sample size, we cannot afford to include the same levels of fixed effects or interaction terms as our baseline regressions. However, adopting the same empirical setting as our baseline analysis yields qualitatively similar results.

Using the gross margin as a proxy for product price rests on the assumption that firms' production cost remains unchanged. One potential violation to this assumption is that firms experience a cut in R&D investment when they have short-term EPS incentives (Almeida, Fos, and Kronlund (2016)). This could slow down firms' technology development and potentially increase the cost of production. However, as our analysis is at the quarterly level, the likelihood that a firm suffers from a sudden deterioration in technology and thus a sudden increase in production cost is low. Despite this, the results in this section should be interpreted with caution.

We report the results from the regression analyses in Table 3.E.7 and present the graphical evidence in Figure 3.F.5. We find that there is a drop in firms' gross margin when they have short-term EPS incentives. This evidence suggests that firms may offer their products at a reduced price to preserve their customers. Due to the data limitation on product prices, we cannot capture whether there is any price discrimination across different customers. However, we find some pre-trends in firms' gross margin one and two quarters before having the short-term EPS incentive. It is therefore unclear whether firms offer price discounts as a financial concession.

3.5 Additional Analyses and Robustness Checks

3.5.1 Additional Analyses: Validity of Identification Strategy

In this section, we probe into the potential concerns over the validity of our results and perform a set of additional analyses to mitigate these concerns.

First, firms with large negative or positive earnings surprises may be intrinsically different and can thus bias our results. To alleviate this concern, we conduct all the previous analyses using observations located in a small window ($-0.003 \leq Sue \leq 0.003$). Restricting our sample to this window to some extent rules out firms whose earnings surprises are driven by real positive or negative shocks. To test whether our results are sensitive to the choice of window bandwidth, we expand, as well as narrow down, the window width, allowing it to range from 0.001 to 0.005. Table 3.E.9 presents the results and confirms that our findings are not sensitive to the choice of bandwidth for the small window.

Second, limiting the sample to a small window may not guarantee that firms' negative earnings surprises are not triggered by real negative shocks. If this is the case, our results may suffer from omitted variable bias as the observed customer losses may also be driven by these real negative shocks instead of short-term EPS incentives. To address this concern, we examine whether firms whose pre-repurchase earnings surprises locate nearer to the left of the zero pre-repurchase earnings-surprise threshold suffer more from customer losses compared with those that locate further away but still within a reasonable distance to the left side of the threshold. The intuition is that it is more likely for firms whose earnings surprises are only slightly negative to boost EPS via repurchasing shares. A higher likelihood of relationship breaks for these firms would thus suggest that such breaks are more likely to be triggered by EPS-boosting repurchases. We limit our sample to firms whose pre-repurchase earnings surprises are within the range of $(-0.005, 0)$ and define a dummy variable *Neg_Sue_Small* that equals one if $-0.0025 \leq Sue \leq 0$. Results in Table 3.E.10 show that among all firms that have small negative pre-repurchase earnings surprises, those that locate nearer to the left of the zero threshold experience more major customer losses.

However, the presence of an incentive to conduct EPS-driven repurchases does not always ensure that repurchases will be carried out. One may argue that firms conducting EPS-boosting repurchases to meet their analysts' forecasts are to some extent different from those that do not. While our 2SLS analysis in Table 3.E.3 shows the Local Average Treatment Effect (LATE) of accretive repurchases on supply chain relationship stability, we follow Almeida, Fos, and Kronlund (2016) and limit our treated sample to include only firms that would have had actual negative earnings surprises in the absence of share repurchases (i.e., firms whose post-repurchase EPS have met their analysts' forecasts). Table 3.E.11 documents similar results to our baseline findings.

Additionally, we perform a more stringent test asking whether firms that conduct repurchases as opposed to those that do not are more likely to lose customers when faced with negative pre-repurchase EPS surprises. We include only firms with small negative surprises as these firms would be more prone to actually use repurchases to boost EPS and that their economic fundamentals are likely to be similar. We define a dummy variable *EPS-Boosting Repurchases* that equals one if a firm with a negative pre-repurchase earnings surprise eventually met the analysts' forecast and use it in replacement of *Neg_Sue* in our regressions. Table 3.E.12 shows that firms conducting EPS-boosting share repurchases suffer from a higher probability of major customer exits, which further alleviates our concern.

We next exploit the heterogeneity over whether a supplier has an open market repurchases program or not. Firms need to have a repurchase program approved by the board of directors before initiating share repurchases. Therefore, suppliers with an ongoing open market repurchase program would be more likely to conduct EPS-boosting repurchases, and thus more prone to lose major customers. We rely on the announcement of open market share repurchases programs documented by SDC and find supporting results in Table 3.E.13.

Finally, firms may use other earnings management methods instead of share repurchases to boost their EPS, which may lead to omitted variable bias. To alleviate this concern, we exploit total accruals as well as discretionary accruals as proxies for other earnings management activities. We explicitly control for these two proxies in our analyses and report the results in Table 3.E.14. The estimates of our negative pre-repurchase earnings surprise remain significantly positive, suggesting that our findings are not affected by other potential forms of earnings management.

Firms may also file earnings restatement *ex post* as a way to manage earnings. Although firms with earnings restatement are more likely to be targeted by short sellers and experience higher management turnovers (Desai, Krishnamurthy, and Venkataraman (2006), Desai, Hogan, and Wilkins (2006)), filing earnings restatement does not consume financial resources compared with the costly EPS-boosting repurchases. As a result, we would expect major customers to react to EPS-boosting repurchases instead of restatement.[20] Table 3.E.15 shows supporting results. Interesting, we find that suppliers that file earnings restatement lose minor customers. These results are consistent with our previous argument - when suppliers have to relinquish some customers, they first let go minor customers as opposed to the major ones.

3.5.2 Additional Evidence on Customer Monitoring

As data limitation poses an empirical obstacle to capture customer monitoring directly, our evidence on customer monitoring so far is indirect and drawn from two facts: first, major customers have an information advantage of detecting the financial cost of suppliers' EPS manipulation; second, suppliers have a higher incentive to preserve major customers

[20] We look at the earnings period in which the earnings restatement refers to instead of the period when the restatement is filed.

out of the concern over future profitability. Our findings on the exit of major but not minor customers can therefore be interpreted as customer monitoring.

As shown in Section 3.3.2, customers monitor their suppliers due to concerns over suppliers' financial conditions. In the case of suppliers manipulating EPS using share repurchases, we argue that what customers truly care about when monitoring is the consequences of suppliers' share repurchases, not their decision to buy back shares. This provides a useful setting to test customer monitoring: if customers actively monitor their suppliers, they should react differently to different types of repurchases. For instance, when firms engage in repurchases aiming to return excess cash to investors, customers may see it as a sign of having strong financial muscles and may thus be more likely to stay in the relationship.

To study this, we perform a test examining how the stability of supplier-customer relationships is affected by other types of repurchases. We define a dummy variable *Other Repurchases* that equals one if a firm conducts repurchases not aiming to boost its EPS to meet the analysts' forecast. As shown in Table 3.E.16, when doing the regression on the full sample, customers are less likely to exit when suppliers conduct repurchases, as these repurchases may signal a good financial status. When limiting our sample to the small window as in previous analyses where firms barely meet or miss the earnings forecast, we do not observe any changes to the stability of supply chain relationships.

Overall, our findings in this section provide additional evidence on customer monitoring. We show that what customers care about is not the suppliers' decision to do share repurchases, but the potential consequences of these repurchases.

3.5.3 Robustness Checks

In this section, we conduct a series of robustness tests and discuss the results.

We first examine whether our results are driven by other corporate activities that could take place simultaneously with short-termist repurchases, such as M&As. Boehm and Sonntag (2020) document that when suppliers vertically integrate with one of their customers' competitors, they are more likely to experience a breakdown of their existing supply chain relationships. To rule out the probability of having confounding results, we exclude all the observations where a firm engaged in an M&A deal as an acquiror with a transaction value of more than 50 million dollars in that year. Similarly, we argue that if a firm is fully acquired in an M&A deal, it may undertake some major adjustments in its supply chain components, which would also influence our results. We therefore omit all the firm-year observations if it is the last year a firm appears in Compustat. Columns 1 and 2 of Table 3.11 report similar results compared with our baseline findings, suggesting that our findings are not driven by M&A deals.

The stability of supply chain relationships can be influenced by abnormal changes in firms' economic conditions, which may also affect firms' repurchase decisions. For instance, in periods when economic growth is stagnant, firms that are about to miss their EPS forecasts may conduct EPS-boosting share repurchases to boost market confidence; in the meantime, their customer may seek new (probably more efficient) suppliers not due to the repurchases, but to survive the economic downturns. To mitigate this concern, we first exclude the years where the 2008 financial crisis took place to account for sudden changes in the overall economic environment.[21] To capture the idiosyncratic changes of firms' conditions, we next exclude all the firm-year observations where a firm experiences a more than 50% increase/decrease in its PPE. Columns 3 and 4 of Table 3.11 report the results. We continue to find a significantly positive effect of suppliers' short-term EPS incentives on major customer losses.

We next proceed to test to what extent the inclusion of polynomials affects our results. In columns 5 and 6 of Table 3.11, we include the second- and third-order polynomials of pre-repurchase earnings surprises as well as their interaction terms with *Neg_Sue* in our baseline model. Our results are not sensitive to the order of polynomials.

Critics may argue that in our small window sample, firms with slightly negative pre-repurchase earnings surprises may be different from those with positive surprises. These differences, instead of the short-term EPS incentives, could be driving our results. To alleviate this concern, we perform a Mahalanobis matching based on firms' size, ROA, dividends, and stock returns. We assign one control firm (firms with positive *Sue*) to each treated firm (firms with negative *Sue*) and repeat our analysis on this matched sample. As is shown in column 7, our results remain significantly positive, suggesting that our findings are not driven by the intrinsic differences between treated firms and their controls.

Last but not least, we explore a new measure of supply chain relationship breaks and re-examine the effect of suppliers' short-term EPS incentives using this measure. To be specific, we count the number of customers each firm has and the number of customers

[21] Following the literature, we exclude all the observations between 2007 and 2009.

and major customers each firm loses in that year, and then calculate the break ratio. We replace our relationship break dummy with the variable *Break Ratio* and *Break Ratio (Major)* and re-run the regression at the supplier level. The results are shown in columns 8 and 9 of Table 3.11 - short-termist firms are more likely to lose their major customers, which is in line with our baseline findings.

3.6 Conclusion

This paper provides novel evidence that downstream firms in supply chains play a governance role with respect to their upstream firms. We look at a specific governance issue, EPS manipulation, with a particular focus on when firms are prone to conduct EPS-boosting share repurchases to meet or beat analysts' forecasts. Such repurchases consume suppliers' financial resources, potentially affecting product quality and supply stability, which may induce customer exits.

We show that major, as opposed to minor, customers exit the relationship when suppliers have the incentive to manipulate EPS. As suppliers are inclined to preserve major customers given their importance to business (Costello (2020)), our findings suggest that the relationship breakdown is initiated by customers and triggered by customer monitoring. Anticipating the possibility of customer exits, suppliers with major customers appear less likely to conduct EPS-boosting repurchases when they are about to miss their EPS forecasts, indicating an ex-ante effect of customer governance.

Although customer governance deters EPS-boosting share repurchases, suppliers' decision on earnings manipulation is also affected by other factors and therefore such repurchases still exist in equilibrium. To tackle the threat of customer exit, short-termist firms try to mitigate the consequences on future performance by prioritizing their *largest customer*. They do so by providing more favorable trade credit terms to their largest customer while reducing such terms for other major customers. This suggests that the effectiveness of customer governance can be affected by the trade credit arrangements between trading partners.

Finally, this paper shows that short-termism imposes costs similar to the indirect costs of financial distress. By revealing the destabilizing effect of short-term EPS incentives on supply chains, our findings suggest that the consequences of corporate short-termism extend beyond firm boundaries. As a result, firm boundaries should not serve as a confinement when estimating the economic costs of short-termism. Indeed, the spillover on firms' supply chain components should also be considered.

Table 3.1. Descriptive statistics

This table reports the descriptive statistics of our small window sample where $-0.003 \leq Sue_{adj} \leq 0.003$. The sample period spans from 2003 to 2019. *Treated Firms* are defined as the ones with slightly negative pre-repurchase earnings surprises ($-0.003 \leq Sue_{adj} \leq 0$); *Control Firms* are the ones with slightly positive pre-repurchase earnings surprises ($0 \leq Sue_{adj} \leq 0.003$). The detailed variable definitions are described in Table 3.A.1.

	Full Sample				Treated Firms				Control Firms				Difference	
	Mean	Median	SD	N	Mean	Median	SD	N	Mean	Median	SD	N	Difference	P-Value
Relationship-Level Variables:														
All customers														
Relationship Break	0.128	0.000	0.334	47871	0.127	0.000	0.334	16055	0.128	0.000	0.334	31816	-0.000	0.950
When the customer is a major customer														
Relationship Break	0.027	0.000	0.163	4716	0.033	0.000	0.179	1567	0.024	0.000	0.153	3149	0.009*	0.087
Firm-Level Variables														
Repurchases	187.476	0.000	1094.391	6272	289.958	0.000	1520.926	2123	135.036	0.000	786.882	4149	154.922***	0.000
Repurchases/Assets	0.016	0.000	0.037	6272	0.020	0.000	0.043	2123	0.014	0.000	0.033	4149	0.006***	0.000
Accretive Repurchases	0.138	0.000	0.345	6272	0.180	0.000	0.385	2123	0.117	0.000	0.321	4149	0.064***	0.000
Size	7.643	7.566	1.803	6272	7.618	7.530	1.915	2123	7.655	7.592	1.743	4149	-0.038	0.445
Sales	7.403	7.373	1.808	6260	7.389	7.373	1.905	2117	7.409	7.373	1.756	4143	-0.020	0.680
ROA	0.046	0.056	0.101	6272	0.041	0.054	0.110	2123	0.048	0.058	0.096	4149	-0.007**	0.014
Cash	0.144	0.107	0.131	6200	0.137	0.096	0.131	2106	0.148	0.111	0.131	4094	-0.012***	0.001
Dividend	0.490	0.000	0.500	6272	0.523	1.000	0.500	2123	0.473	0.000	0.499	4149	0.050***	0.000
Number of Customers	7.632	4.000	11.890	6272	7.562	3.000	13.220	2123	7.668	4.000	11.150	4149	-0.106	0.752
Number of Major Customers	0.752	0.000	1.478	6272	0.738	0.000	1.448	2123	0.759	0.000	1.493	4149	-0.021	0.593
When there is at least one major customer														
Number of Major Customers	1.926	1.000	1.826	2449	1.897	1.000	1.787	826	1.940	1.000	1.847	1623	-0.043	0.577

Table 3.2. Impact of short-term EPS incentives on firm outcomes

This table reports the firm-level estimates of the impact of short-term EPS incentives on firm outcomes. *Cash* is calculated as $(Cash_{(t+1,t+4)} - Cash_{(t-4,t-1)})/Assets_{(t-4,t-1)}$, where t is the earnings surprise quarter. *Interest Coverage* is at the annual level and is calculated as EBIT/interest expenses in year $t + 1$. We measure *Investment* as $(CAPEX_{(t+1,t+4)} - CAPEX_{(t-4,t-1)})/Assets_{(t-4,t-1)}$, and measure *Sales Growth* as $(Sales_{(t+1,t+4)} - Sales_{(t-4,t-1)})/Sales_{(t-4,t-1)}$. *Cash* and *Investment* are adjusted by multiplying with 100. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. The detailed variable definitions are described in Table 3.A.1. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. We control for firm fixed effects, time × industry fixed effects and firm-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the firm level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Cash	Interest Coverage	Investment	Sales Growth
	(1)	(2)	(3)	(4)
Neg_Sue	-0.6324***	-0.0842***	-0.0341***	-0.0213***
	(0.191)	(0.032)	(0.013)	(0.005)
Observations	51678	9716	51107	51302
R^2	0.400	0.759	0.384	0.445
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Time*Industry FE	Yes	Yes	Yes	Yes

Table 3.3. Impact of short-term EPS incentives on supplier-customer relationships

This table reports estimates of the impact of short-term EPS incentives on the stability of supplier-customer relationships. Columns (1)-(3) report the OLS regression results, whilst columns (4)-(7) report results estimated by the Cox Hazard model. The outcome variable *Relationship Break* measures whether a supplier-customer relationship is active in year t but is no longer active in year $t+1$. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise, where the pre-repurchase EPS surprise is calculated as the difference between the repurchase-adjusted EPS and the median end-of-quarter EPS forecast, scaled by the end-of-quarter stock price. *Major Customer* is a dummy variable that equals one if a supplier-customer relationship is covered by the Compustat Segment data (i.e., if a customer represents more than 10% of the supplier's sales). We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. All variables are defined in Table 3.A.1. We control for the supplier-level characteristics (size, ROA, dividend, and stock returns) in certain specifications. In OLS regressions, we include the supplier × customer fixed effects, customer × year fixed effects, supplier industry × customer industry × year fixed effects. In the Cox Hazard model, we control for year strata, supplier industry strata, customer industry strata, and supplier industry × customer industry strata across different specifications. All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break						
	OLS			Cox Hazard Model			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Neg_Sue	0.005	-0.001	-0.001	-0.001	0.005	-0.001	0.005
	(0.007)	(0.008)	(0.008)	(0.030)	(0.030)	(0.030)	(0.030)
Neg_Sue × Major Customer		0.047***	0.049***	0.593***	0.591***	0.593***	0.591***
		(0.015)	(0.015)	(0.201)	(0.200)	(0.201)	(0.200)
Size			0.033***		0.006		0.006
			(0.009)		(0.006)		(0.006)
ROA			-0.013		-0.161		-0.161
			(0.038)		(0.106)		(0.106)
Dividend			-0.004		-0.048**		-0.048**
			(0.012)		(0.022)		(0.022)
Stock Return			0.062***		0.125**		0.125**
			(0.014)		(0.058)		(0.058)
Observations	47915	47915	47871	81592	81507	81592	81507
R^2	0.663	0.664	0.664				
Controls	No	No	Yes	No	Yes	No	Yes
Supplier*Customer FE	Yes	Yes	Yes				
Customer*Year FE	Yes	Yes	Yes				
S.Industry*C.Industry*Year FE	Yes	Yes	Yes				
Year Strata				Yes	Yes	Yes	Yes
S.Industry Strata				Yes	Yes	No	No
C.Industry Strata				Yes	Yes	No	No
S.Industry*C.Industry Strata				No	No	Yes	Yes

Table 3.4. Mechanism: suppliers' financial conditions

This table reports how the impact of short-term EPS incentives on the stability of supplier-customer relationships differs according to suppliers' financial conditions. We use the Hadlock-Pierce Index and Whited-Wu Index to proxy for financial constraints. The outcome variable *Relationship Break* measures whether a supplier-customer relationship breaks in year $t+1$. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. *Major Customer* is a dummy variable that equals one if a supplier-customer relationship is covered by the Compustat Segment data (i.e., if a customer represents more than 10% of the supplier's sales). We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. All variables are defined in Table 3.A.1. We include the supplier × customer fixed effects, customer × year fixed effects, supplier industry × customer industry × year fixed effects and supplier-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break			
	Hadlock-Pierce Index		Whited-Wu Index	
	(1)	(2)	(3)	(4)
Neg_Sue	-0.000	-0.007	-0.010	0.002
	(0.014)	(0.011)	(0.013)	(0.012)
Neg_Sue × Major Customer	0.094***	0.022	0.093***	0.027
	(0.028)	(0.025)	(0.024)	(0.030)
Observations	19753	17350	20821	14924
R^2	0.691	0.763	0.684	0.783
Difference	0.071*		0.066*	
Sample	Fin Constrained	Fin Un-Constrained	Fin Constrained	Fin Un-Constrained
Controls	Yes	Yes	Yes	Yes
Supplier*Customer FE	Yes	Yes	Yes	Yes
Customer*Year FE	Yes	Yes	Yes	Yes
S.Industry*C.Industry*Year FE	Yes	Yes	Yes	Yes

Table 3.5. Important suppliers regarding trade volume

This table examines how the destabilizing effect of short-term EPS incentives on supply chains varies according to the trade volume between suppliers and customers. Columns (1)-(2) use the sample where only relationships between suppliers and their major customers are included, whilst columns (3)-(4) also include relationships with minor customers. The outcome variable *Relationship Break* measures whether a supplier-customer relationship is active in year t but is no longer active in year $t + 1$. *High Trade Volume Customer* is a dummy variable that equals one if the ratio of the sales volume between a supplier-customer pair divided by the customer's COGS is above sample median, and zero otherwise. *High Trade Volume Customer1* builds on the definition of *High Trade Volume Customer* and adds an additional condition requiring the supplier to be one of the top 3 suppliers to a customer in the sample. We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. All variables are defined in Table 3.A.1. We control for the supplier-level characteristics (size, ROA, dividend, and stock returns) in all specifications. We include the supplier × customer fixed effects, customer × year fixed effects, and supplier industry × customer industry × year fixed effects. All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break			
	(1)	(2)	(3)	(4)
Neg_Sue	-0.002	0.014	0.003	0.004
	(0.017)	(0.015)	(0.007)	(0.007)
Neg_Sue × Dependent Customer	0.058**		0.039*	
	(0.028)		(0.021)	
Neg_Sue × Dependent Customer1		0.059*		0.056**
		(0.034)		(0.026)
Sample	Major Customers	Major Customers	Full Sample	Full Sample
Observations	2845	2845	47871	47871
R^2	0.658	0.659	0.664	0.664
Controls	Yes	Yes	Yes	Yes
Supplier*Customer FE	Yes	Yes	Yes	Yes
Customer*Year FE	Yes	Yes	Yes	Yes
S.Industry*C.Industry*Year FE	Yes	Yes	Yes	Yes

Table 3.6. Outside options, product specificity, and switching costs

This table reports the results of cross-sectional analyses according to suppliers' outside options, product specificity, and switching costs. We conduct two sets of tests from the perspective of product market competition and product specificity. The outcome variable *Relationship Break* measures whether a supplier-customer relationship breaks in year $t+1$. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. *Major Customer* is a dummy variable that equals one if a supplier-customer relationship is covered by the Compustat Segment data (i.e., if a customer represents more than 10% of the supplier's sales). We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. All variables are defined in Table 3.A.1. We include the supplier × customer fixed effects, customer × year fixed effects, supplier industry × customer industry × year fixed effects and supplier-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break				
	Competition		Product Specificity		
	(1)	(2)	(3)	(4)	(5)
Neg_Sue	0.026**	0.003	-0.005	-0.029*	0.024*
	(0.013)	(0.012)	(0.009)	(0.016)	(0.015)
Neg_Sue × Major Customer	-0.024	0.088***	0.072***	0.120***	-0.022
	(0.022)	(0.030)	(0.019)	(0.025)	(0.028)
Neg_Sue × Major Customer × High # of Competitors	0.096***				
	(0.029)				
Neg_Sue × Major Customer × High HHI		-0.064*			
		(0.035)			
Neg_Sue × Major Customer × High R&D			-0.064**		
			(0.032)		
Neg_Sue × Major Customer × Durable				-0.095***	
				(0.034)	
Neg_Sue × Major Customer × High Product Similarity					0.062*
					(0.037)
Observations	46211	47063	47871	28320	32453
R^2	0.668	0.665	0.665	0.653	0.681
Controls	Yes	Yes	Yes	Yes	Yes
Supplier*Customer FE	Yes	Yes	Yes	Yes	Yes
Customer*Year FE	Yes	Yes	Yes	Yes	Yes
S.Industry*C.Industry*Year FE	Yes	Yes	Yes	Yes	Yes

Table 3.7. Ex-ante effect of customer governance

This table reports the estimates of how major customers deter accretive repurchases. The dependent variable *Accretive Repurchases* is an indicator that equals one if the repurchases increase EPS by at least one cent. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. We use three proxies to measure the presence and influence of major customers. *w/ Major Customer* is a dummy variable that equals one if a firm has at least one major customer in that year. *# Major Customer* is the log value of one plus the number of major customers a firm has in that year. *Major Customer HHI* measures the Herfindahl-Hirschman Index of major customers, which is calculated using the pair-level sales volume between each supplier and its major customers (Campello and Gao (2017)). To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. All variables are defined in Table 3.A.1. We control for firm fixed effects, industry × year fixed effects, and firm-level characteristics (size, ROA, dividend, and stock returns). We additionally control for shareholder governance by including firms' institutional investor ownership and institutional investor ownership concentration (HHI) in the regression analyses. All standard errors are clustered at the firm level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Accretive Repurchases					
	(1)	(2)	(3)	(4)	(5)	(6)
Neg_Sue	0.083***	0.079***	0.081***	0.077***	0.072***	0.067***
	(0.016)	(0.016)	(0.016)	(0.015)	(0.014)	(0.013)
Neg_Sue × w/ Major Customer	-0.052**	-0.053**				
	(0.024)	(0.024)				
Neg_Sue × # Major Customer			-0.047**	-0.049**		
			(0.023)	(0.023)		
Neg_Sue × Major Customer HHI					-0.394***	-0.369***
					(0.143)	(0.138)
Observations	7223	7209	7223	7209	7223	7209
R^2	0.520	0.526	0.519	0.526	0.519	0.526
Controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year*Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.8. Customer prioritization - largest customer v.s. other major customers

This table reports the heterogeneous impact of short-term EPS incentives on the stability of supplier-customer relationships across the largest and other major customers. Panel A splits the major customers into the largest one and other major ones and reports the descriptive statistics for these customers. Panel B reports the regression results. The outcome variable *Relationship Break* measures whether a supplier-customer relationship breaks in year $t + 1$. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. *Largest Customer* is a dummy variable indicating whether a customer is the firm's largest customer. *Major Customer (excl. Largest)* is a dummy variable indicating whether a customer is a major but not the largest customer. Columns (1)-(4) in Panel B perform the baseline regression where both the largest and other major customer dummies are included. Columns (5)-(8) split the sample into firms that rely heavily on their largest customer as opposed to other major customers and firms that do not, where the dependence is measured by sales volume. We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. All variables are defined in Table 3.A.1. We include firm-level controls (size, ROA, dividend, and stock returns) and different levels of fixed effects or strata across specifications. All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Descriptive Statistics							
Panel A	Largest Customers			Major Customers (excl Largest)			Difference	
	Mean	Median	N	Mean	Median	N	Difference	P-Value
Sales Proportion	0.208	0.167	2416	0.125	0.115	1423	0.083***	0.000
Size	10.571	10.761	2396	10.368	10.450	3568	0.203***	0.000
ROA	0.062	0.062	2396	0.050	0.049	3557	0.012***	0.000
Dividend	0.768	1.000	2396	0.764	1.000	3568	0.004	0.697
Stock Return	0.034	0.042	2390	0.031	0.036	3551	0.003	0.407

Panel B	*Relationship Break*							
	OLS		Cox Hazard Model		OLS		Cox Hazard Model	
Neg_Sue	-0.001	-0.001	0.005	0.005	-0.003	0.101*	-0.227	0.582***
	(0.008)	(0.008)	(0.030)	(0.030)	(0.039)	(0.052)	(0.142)	(0.206)
Neg_Sue × Largest Customer	0.033	0.037*	-0.039	-0.039	0.054	-0.065	0.045	-0.092
	(0.021)	(0.021)	(0.291)	(0.291)	(0.045)	(0.065)	(0.449)	(0.850)
Neg_Sue × Major Customer (excl Largest)	0.059***	0.061***	1.198***	1.198***	0.186**	-0.064	1.873***	-0.211
	(0.020)	(0.020)	(0.271)	(0.271)	(0.081)	(0.064)	(0.672)	(0.574)
Observations	47915	47871	81507	81507	2510	947	9372	4422
R^2	0.664	0.664			0.773	0.751		
Dep. on Largest Customer					High	Low	High	Low
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Supplier*Customer FE	Yes	Yes			Yes	Yes		
Customer*Year FE	Yes	Yes			Yes	Yes		
S.Industry*C.Industry*Year FE	Yes	Yes			Yes	Yes		
Year Strata			Yes	Yes			Yes	Yes
S.Industry Strata			Yes	No				
C.Industry Strata			Yes	No				
S.Industry*C.Industry Strata			No	Yes			Yes	Yes

Table 3.9. Trade credit reallocation: customer-level evidence

This table reports how customers' accounts payable is affected by suppliers' short-term EPS incentives. We collapse the relationship-level data to customer-level and define the main independent variable *Neg_Sue_Pct* as the ratio of the number of short-termist suppliers divided by the total number of suppliers a customer has in that quarter. *#.Major Customer* is measured as the log number of supplier-customer relationships a customer has in which it is a major customer in that quarter. *#.Largest Customer* and *#.Major Customer (excl. Largest)* are defined in a smilar way. To capture the immediate effect of supplier's short-term EPS incentives on customers' accounts payable days, *Change in AP Days* is calculated as $Accounts\,Payable\,Days_{t+1}/Accounts\,Payable\,Days_{t-1}$, where t is the supplier's earnings surprise quarter and accounts payable is normalized by customer's COGS in the corresponding quarter. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. We control for customer firm fixed effects, year-quarter × industry fixed effects and customer-level characteristics (size, ROA, dividend, leverage, and cash). All standard errors are clustered at the customer level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Change in Accounts Payable			
	(1)	(2)	(3)	(4)
Neg_Sue_Pct	-0.007	-0.004	-0.006	-0.004
	(0.007)	(0.008)	(0.007)	(0.008)
Neg_Sue_Pct × #.Major Customer		-0.050**		
		(0.024)		
Neg_Sue_Pct × #.Largest Customer			-0.043	
			(0.037)	
Neg_Sue_Pct × #.Major Customer (excl Largest)				-0.064**
				(0.029)
Observations	42748	42748	42748	42748
R^2	0.200	0.200	0.200	0.200
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year-Quarter*Industry FE	Yes	Yes	Yes	Yes

Table 3.10. Trade credit reallocation: supplier-customer pair-level evidence

This table reports the estimates of how suppliers' short-term EPS incentives affect supplier-customer pair-level trade credit. The dependent variable *Trade Credit* is defined as the ratio of a firm's trade receivable balance with a customer to its annual sales to that customer. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. *Largest Customer* is a dummy variable that equals one if a customer is the largest customer in at least one of the three following years. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. All variables are defined in Table 3.A.1. We control for supplier × customer fixed effects, customer × year fixed effects, and supplier-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Trade Credit	
	(1)	(2)
Neg_Sue	-0.037*	-0.043**
	(0.020)	(0.020)
Neg_Sue × Largest Customer	0.054**	0.057**
	(0.024)	(0.024)
Observations	331	330
R^2	0.891	0.894
Controls	No	Yes
S.Firm*Customer FE	Yes	Yes
Customer*Year FE	Yes	Yes

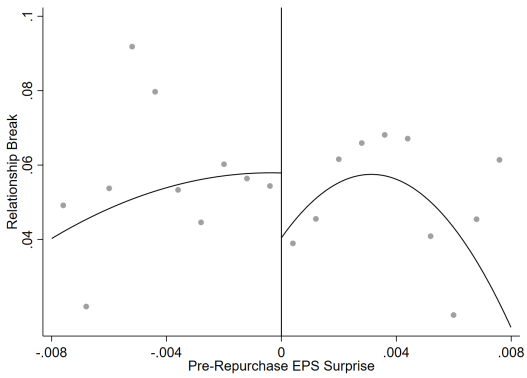
Table 3.11. Robustness check

This table reports the results of our robustness tests. In the first two columns, we exclude suppliers that have engaged in M&A deals in that year. Specifically, in column (1), We exclude all the suppliers that have acquired another firm in that year; and in column (2), we exclude the ones that were acquired and removed from Compustat. In column (3), we exclude the observations during the financial crisis (2007-2009). In column (4), we exclude firms that experience a more than 50% increase/decrease in PPE to rule out the possibility of abnormal expansion or downsizing. In columns (5) and (6), we further control for the second-/third-order polynomial and its interaction terms with the sign of the pre-repurchase surprise. In column (7), we use a matched sample where each treated firm with a negative *Sue* is matched with one control firm that has a positive *Sue* based on firms' size, ROA, dividends, and stock returns. In columns (8) and (9), we define two new outcome variables as a way to measure the intensity of relationship breaks and perform the analysis at the supplier-year level. The outcome variable *Break Ratio* is measured as the number of customer relationship breaks divided by the total number of customers a supplier has in that year. Similarly, *Break Ratio (Major)* is measured as the number of major customer losses divided by the total number of customers a supplier has in that year. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. All variables are defined in Table 3.A.1. We control for different levels of fixed effects and supplier-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the supplier-customer pair level in columns (1)-(7) and are clustered at the supplier firm level in columns (8)-(9) and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

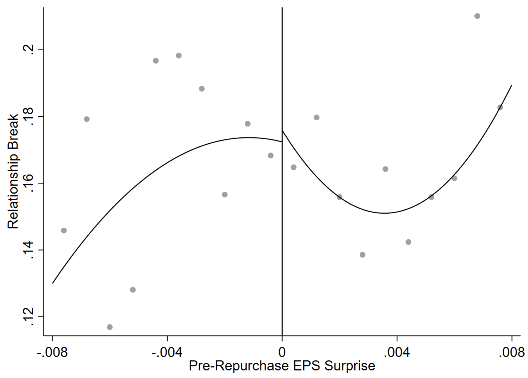
	Relationship Break								
	Excl M&A		Excl Fin. Crisis	Excl $\|\Delta PPE\| \geq 50\%$	Poly2	Poly3	Matched Sample	Break Ratio	Break Ratio(Major)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Neg_Sue	-0.010	-0.016*	-0.005	-0.011	-0.010	-0.005	0.009	0.012	0.006*
	(0.011)	(0.009)	(0.008)	(0.008)	(0.009)	(0.010)	(0.010)	(0.009)	(0.003)
Neg_Sue × Major Customer	0.047**	0.038**	0.047***	0.072***	0.048***	0.049***	0.049***		
	(0.021)	(0.015)	(0.017)	(0.016)	(0.015)	(0.015)	(0.018)		
Observations	27927	36889	42436	39792	47871	47871	31852	9851	9851
R^2	0.693	0.662	0.679	0.677	0.665	0.665	0.683	0.265	0.399
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Supplier*Customer FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Customer*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
S.Industry*C.Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Year FE								Yes	Yes
Supplier FE								Yes	Yes

Figure 3.1: Negative pre-repurchase EPS surprises and supplier-customer relationship stability

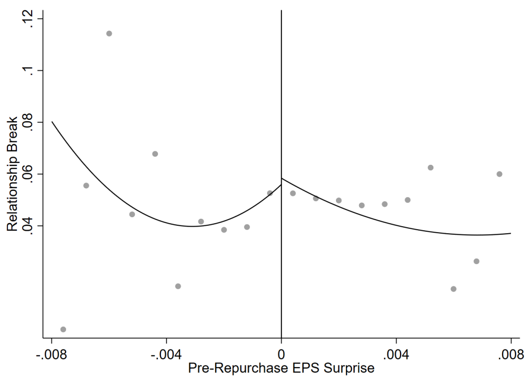
This figure depicts the probability of relationship breaks between firms and their customers. For every earnings surprise bin, the dots represent the probability of customer relationship breaks. The lines are second-order polynomials fitted through the estimated relationship break probabilities on each side of the zero pre-repurchase earnings surprise. Figure (a) uses the sample including only major customers; Figure (b) uses the sample including only minor customers; Figure (c) includes only the largest customer of each supplier; and Figure (d) uses the sample with all the major customers excluding the largest ones.



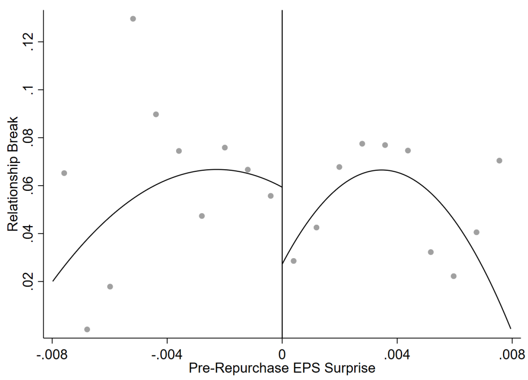
(a) Major Customer



(b) Minor Customer



(c) Largest Customer



(d) Major Customer (excl. Largest Customer)

Appendix

3.A Variable Definitions and Descriptive Statistics

Table 3.A.1. Variable definitions

This table shows the definitions of all the variables used in this paper.

Variable	Definition	Source
Relationship-Level Variables		
Relationship Break	A dummy variable that equals one if and only if the supplier-customer relationship is active in year t but not active in year $t+1$.	FactSet Revere
Major Customer	An indicator variable that equals one if a customer is a major customer of the supplier (if the supplier-customer relationship appears in the Compustat Segment data), zero otherwise.	Compustat Segment
Largest Customer	An indicator variable that equals one if a customer is the largest customer of the supplier (the customer that takes up the supplier's highest sales proportion), zero otherwise.	Compustat Segment
Major Customer (excl Largest)	An indicator variable that equals one if a customer is a major but not the largest customer of the supplier, zero otherwise.	Compustat Segment
High Trade Volume Customer	An indicator variable that equals one if the ratio of the sales volume between a supplier-customer pair divided by the customer's COGS is above the sample median, and zero otherwise.	Compustat Segment Compustat
High Trade Volume Customer1	An indicator variable that builds on *High Trade Volume Customer* with a further restriction that the supplier is one of the top 3 suppliers to the customer regarding sales volume in the sample.	Compustat Segment Compustat
Break Ratio	The number of customer relationship breaks divided by the total number of customers in that year.	FactSet Revere
Trade Credit	This variable measures trade credit at the supplier-customer level. We calculate it as the supplier's receivables to a specific customer normalized by its sales to that customer.	Compustat Segment 10-K
Firm-Level Variables		
Sue_adj	Pre-repurchase EPS surprise calculated as the difference between the repurchase-adjusted EPS and the median end-of-quarter EPS forecast, normalized by the end-of-quarter stock price. The repurchase-adjusted EPS is calculated as $EPS\_adj = \frac{E\_adj}{S\_adj} = \frac{(E+I)}{(S+\Delta S)}$, where E is the reported earnings, I is the estimated forgone interest incurred by the repurchase (calculated as the after-tax return a firm would have obtained if it invested the repurchase stock in a 3-month T-bill), S is the end-of-quarter number of shares, and ΔS is the estimated number of shares repurchased (the amount of repurchase divided by the average daily share price in that quarter).	CRSP Compustat I/B/E/S
		continue...

...continued		
Firm-Level Variables		
Neg_Sue	An indicator that equals one if *Sue_adj* is negative, zero otherwise.	Same as above
Accretive_Rep	An indicator that equals one if the repurchase is accretive (if the repurchase increases EPS by at least one cent).	Same as above
Repurchase	Measured as the net repurchase scaled by assets. Net repurchase is calculated as the increase in common Treasury stock if Treasury stock if not zero or missing. If Treasury stock is zero in the current and prior quarter, net repurchase is measured as the difference between stock repurchases and stock issuances. If either of these two amounts is negative, net repurchase is set to zero.	Compustat
AP Days	A firm's accounts payable normalized by its cost of goods sold.	Compustat
AR Days	A firm's accounts receivables scaled by its sales.	Compustat
Durable	A firm is categorized as a durable goods producer if its SIC code is between 3400 and 4000, and is categorized as a non-durable goods producer if its SIC code is between 2000 and 3400.	Compustat
Whited-Wu Index	Whited-Wu index $= -0.091 \times Cash\,flow + 0.062 \times Dividend\,dummy + 0.021 \times Long-term\,debt - 0.044 \times Size + 0.102 \times Industry\,sales\,growth - 0.035 \times Sales\,growth$.	Compustat
Hadlock-Pierce Index	Hadlock-Pierce index $= -0.737 \times Size + 0.043 \times Size^2 - 0.04 \times Age$.	Compustat
Interest Coverage	Interest coverage ratio is defined as EBIT/interest expenses.	Compustat
Gross Margin	Firms' gross margin is defined as (Revenue - COGS)/Revenue.	Compustat
Size	Log value of a firm's total assets.	Compustat
ROA	A firm's return on asset.	Compustat
Dividend	A dummy variable that equals one if a firm pays positive dividend in that year, zero otherwise.	Compustat
q_ret	Firms' quarterly stock returns.	CRSP
w/ Major Customer	A dummy variable that equals one if a firm has at least one major customer in that year.	Compustat Segment
# Major Customer	The log value of one plus the number of major customers a firm has in that year.	Compustat Segment
Major Customer HHI	The Herfindahl-Hirschman Index of major customers calculated using the pair-level sales volume between each supplier and its major customers.	Compustat Segment Compustat

Table 3.A.2. Industry and country distributions

This table shows the distributions of firms and their customers in the Fama-French 17 industries and in countries/regions. All the included firms locate in the small window where $-0.003 \leq Sue_{adj} \leq 0.003$.

Industry	Supplier Firm No. (%)	Customer Firm No. (%)
Food	69(4.89%)	92(4.62%)
Mining and Minerals	7(0.50%)	19(0.95%)
Oil and Petroleum Products	44(3.12%)	55(2.76%)
Textiles, Apparel, Footware	38(2.70%)	42(2.11%)
Consumer Durables	31(2.20%)	40(2.01%)
Chemicals	37(2.62%)	52(2.61%)
Drugs, Soap, Prfums, Tobacco	77(5.46%)	114(5.72%)
Construction and Construction Materials	38(2.70%)	34(1.71%)
Steel Works Etc	19(1.35%)	18(0.90%)
Fabricated Products	14(0.99%)	2(0.10%)
Machinery and Business Equipment	331(23.48%)	314(15.76%)
Automobiles	30(2.13%)	66(3.31%)
Transportation	66(4.68%)	118(5.92%)
Utilities	0(0%)	0(0%)
Retail Stores	35(2.48%)	192(9.63%)
Banks, Insurance Companies, and Other F	0(0%)	0(0%)
Other	574(40.71%)	835(41.90%)
Total	1410(100%)	1993(100%)
Country/Region	Customer Firm No.	Customer Firm%
Argentina	1	0.05
Australia	39	1.95
Austria	2	0.10
Belgium	8	0.40
Bermuda	1	0.05
Brazil	17	0.85
Bulgaria	1	0.05
Canada	2	0.10
Chile	13	0.65
China	49	2.46
Colombia	2	0.10
Croatia	3	0.15
Czech Republic	3	0.15
Denmark	8	0.40
Estonia	1	0.05
Finland	6	0.30
France	59	2.96
Germany	57	2.86
Greece	5	0.25
Hong Kong	38	1.90
Hungary	2	0.10
Iceland	1	0.05
continue...		

Country/Region	Customer Firm No.	Customer Firm%
continued. . .		
India	40	2.01
Indonesia	19	0.95
Ireland	9	0.45
Isle of Man	1	0.05
Israel	30	1.50
Italy	12	0.60
Jamaica	1	0.05
Japan	158	7.92
Jordan	4	0.20
Kuwait	2	0.10
Luxembourg	5	0.25
Macao	1	0.05
Malaysia	8	0.40
Mauritius	2	0.10
Mexico	31	1.55
Morocco	3	0.15
Netherlands	18	0.90
New Zealand	5	0.25
Norway	4	0.20
Pakistan	2	0.10
Panama	1	0.05
Peru	1	0.05
Philippines	11	0.55
Poland	8	0.40
Portugal	2	0.10
Qatar	2	0.10
Russia	7	0.35
Saudi Arabia	5	0.25
Singapore	14	0.70
Slovak Republic	1	0.05
South Africa	21	1.05
South Korea	56	2.81
Spain	11	0.55
Sri Lanka	6	0.30
Sweden	19	0.95
Switzerland	20	1.00
Taiwan	68	3.41
Thailand	17	0.85
Trinidad and Tobago	1	0.05
Turkey	9	0.45
Ukraine	1	0.05
United Arab Emirates	5	0.25
United Kingdom	84	4.21
United States	950	47.62
Vietnam	2	0.10
Total	1995	100.00

Table 3.A.3. Time series summary statistics

This table shows the annual distribution of our final sample. All firms that are included locate in a small window where $-0.003 \leq Sue_{adj} \leq 0.003$.

Year	Supplier Firm	Customer Firm	Customer Relationship	Customer Break
2003	343	281	1570	59
2004	399	334	2049	298
2005	154	182	878	147
2006	90	136	476	121
2007	367	316	1628	81
2008	293	283	1380	138
2009	375	333	1759	121
2010	431	405	2214	238
2011	107	181	727	135
2012	472	664	3592	254
2013	615	904	5213	524
2014	652	1052	5855	815
2015	604	978	5165	858
2016	174	460	1774	235
2017	122	434	1500	211
2018	520	1315	6226	580
2019	554	1258	5865	1295

3.B Cox Hazard Model

The Cox Hazard Model (Cox (1972)) is widely used within survival analysis, enabling the simultaneous evaluation of various factors' impact on survival outcomes. In a Cox proportional hazards regression model, the measure of effect is the hazard rate, which is the risk of failure/death given that the participant has survived up to a specific time. Within our framework, the hazard rate serves as a measure for estimating the probability of a supplier-customer relationship breakdown. Specifically, in our context, we are interested in examining how suppliers' incentives to engage in EPS manipulation and the importance of a customer (e.g., major customer) influence the risk of supplier-customer relationship termination.

In this paper, the Cox proportional hazards regression model is estimated as below:

$$\begin{aligned} h(t) = h_0(t)exp(&\beta_1 Neg\_Sue_{i,t} + \beta_2 Neg\_Sue_{i,t} Major\,Customer_{i,j,t} + \beta_3 Major\,Customer_{i,j,t} \\ &+ \beta_4 Sue\_adj_{i,t} + \beta_5 Neg\_Sue_{i,t} Sue\_adj_{i,t} + \beta_6 Major\,Customer_{i,j,t} Sue\_adj_{i,t} \\ &+ \beta_7 Neg\_Sue_{i,t} Sue\_adj_{i,t} Major\,Customer_{i,j,t} + \beta_8 X_{i,t}), \end{aligned} \tag{3.B.1}$$

where the relative hazard, $\frac{h(t)}{h_0(t)}$, is related to the risk factors (the exponential function). $Exp(\beta_i)$ is called the hazard ratio. We include the year strata, supplier industry strata, customer industry strata, or alternatively the industry-pair strata in replace of the fixed effects in the OLS regression model to allow for different baseline hazards for each unique stratum.

A hazard ratio of 1 indicates that the predictor in the Cox Hazard Model does not affect the stability of supply chain relationships. If the hazard ratio is less than 1, then the predictor is protective and improves relationship survival (the smaller the hazard ratio is compared with 1, the more protective the predictor is). If the hazard ratio is greater than 1, then the predictor is associated with an increased probability of relationship breaks (the larger hazard ratio is compared with 1, the more the predictor increases the likelihood of relationship severance). The hazard ratio of the covariates is the amount of change in the probability of a supply chain relationship breakdown occurring for each unit change in the covariates.

We are especially interested in the coefficient β_2 of the interaction term $Neg\_Sue_{i,t} Major\,Customer_{i,j}$ As the interaction term is dichotomous, the antilog of β_2 produces the hazard ratio comparing the risk of relationship breaks in cases where the supplier has short-term EPS incentives and the customer is a major customer to cases where the supplier has no such incentives and the customer is a minor customer.

The estimates reported in columns 4-7 of Table 3.3 are coefficients of the Cox Hazard Model. To obtain the hazard ratio, we can calculate the exponential value of the coefficients. For instance, in column 7, the coefficient is 0.591 and its exponential value is $exp(0.591) = 1.806$. This indicates that for suppliers with short-term EPS incentives, there is approximately an 80.6% increase in the risk of losing each of the major customers.

3.C Additional Results: Factors that Favor or Hinder Customer Governance

In this section, we explore several factors that may favor or hinder customer governance that are not discussed in the main part of the paper.

3.C.1 Bargaining Power

One possible factor that may have an influence on customer governance is the relative bargaining power between the supplier and customer. It is well-established in the literature that large buyers have higher bargaining power and often extract favorable contract terms from their suppliers, such as lower prices (Fee and Thomas (2004), Bhattacharyya and Nain (2011)) and higher trade credit (Murfin and Njoroge (2015), Giannetti, Serrano-Velarde, and Tarantino (2021)). The exit effect should thus be more pronounced for these customers as it is more difficult for the short-termist suppliers to make concessions that can persuade them to stay.

To examine this, we perform a cross-sectional analysis exploiting a widely-used proxy for bargaining power, the relative size of customers and suppliers (e.g. Giannetti, Serrano-Velarde, and Tarantino (2021)). Consistent with the conjecture that bargaining power is positively associated with customer governance efficacy, results in columns 1-2 of Table 3.C.1 show that customers' propensity to exit decreases as the suppliers' power increases. In columns 3-4, we define a supplier to have low bargaining power if its relative size compared with the customer is below the sample median. We then split the sample into suppliers with high/low bargaining power and re-perform the baseline analysis. Our results show that major customers only exit when they have relatively high bargaining power compared with their suppliers, suggesting that bargaining power affects the impact of customer governance.

3.C.2 Suppliers' Previous Short-Term EPS Incentives

The severity of customer exit may differ based on firms' history of engaging in EPS manipulation. On the one hand, customers buying inputs from firms that have conducted EPS-boosting repurchases before are expected to be more alerted and prone to exit as these firms should have more limited financial resources.[1] On the other hand, the fact that a firm can afford to conduct EPS-boosting repurchases reflects its financial robustness, resulting in reduced likelihood of customer exits.

To examine which effect prevails, we categorize short-termist firms into two groups depending on whether it is the first time that a firm has negative earnings surprises in the last four years.[2] We then perform our baseline regression analysis using these two subsamples where firms with slightly positive earnings surprises are used as controls. One concern of performing this analysis is that firms with multiple negative earnings surprises in the last four years may be the ones that are having some intrinsic negative shocks and our results are indeed capturing the effect of these shocks on supply chain relationships. To alleviate this concern, we count the number of negative earnings surprises a firm has in the last four years and exclude the ones with more than two negative surprises. The results are presented in Table 3.C.2.

Our results show that major customer losses are concentrated in firms that have had short-term EPS incentives before, suggesting that customers view this as a negative sign of financial strength. When excluding poor-performing firms from our regression analysis, the results still hold and the magnitude of the effect remains largely unchanged. This mitigates the concern that the customer exits for firms with a history of negative earnings surprises are triggered by firms' intrinsic negative shocks.

3.C.3 Relationship Intensity

The level of interaction between each customer and supplier is another element that can influence customer governance. Customers that have frequent and meaningful engagements with the supplier are expected to have more monitoring advantage and may thus be more likely to learn about the EPS manipulation, such as EPS-boosting repurchases. However, due to their intense interaction with the supplier, these customers may also be less inclined to terminate the relationship when the supplier has short-term EPS incentives. We perform a cross-sectional analysis to examine this issue and report the results in Table 3.C.3.

[1] An alternative way to interpret this assumption is that when customers monitor, the signal of whether a firm is actually short-term oriented/whether the manager has low ability to meet the EPS forecast is noisy and cannot be learnt in a short period (or in just one period). Instead, the information of multiple periods needs to be collected before customers can make the judgement call.

[2] The average length of a supplier-customer relationship in our sample is four years.

We first employ the number of relationships a customer has with the supplier to proxy for relationship intensity. We define a dummy variable *High Rel Intensity*, which equals one if the supplier-customer pair also has other types of relationships (e.g., partnership), and zero otherwise. As reported in column 1, supplier-customer links tend to be more resilient if the trading partners are also connected outside the supply chain relationships.

Our second proxy exploits the duration of the supplier-customer relationship, as suppliers and customers who have been in trade with each other for a long time tend to develop more intense relationships. Accordingly, we define a dummy variable *Long Relationship* that equals one if the relationship length between a specific supplier-customer pair is longer than the sample median, and zero otherwise. In column 2, we show that the supplier-customer relationships with longer duration appear to be less likely to break when suppliers have short-term EPS incentives.

Table 3.C.1. Cross-sectional analysis: bargaining power

This table reports the cross-sectional analysis regarding how bargaining power affects customer governance. The outcome variable *Relationship Break* measures whether a supplier-customer relationship is active in year t but is no longer active in year $t + 1$. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. *Major Customer* is a dummy variable that equals one if a supplier-customer relationship is covered by the Compustat Segment data (i.e., if a customer represents more than 10% of the supplier's sales). *Relative Size (S/C)* captures the supplier's relative bargaining power and is calculated as the supplier size divided by the customer size. In the first two columns, we explore whether supplier power exerts a downward pressure on customer exit. In Columns (3)-(4), we split the sample based on the median value of the relative size of supplier and customer and examine the variations in major customers' probability to exit. We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. All variables are defined in Table 3.A.1. We control for the supplier-level characteristics (size, ROA, dividend, and stock returns), and include the supplier × customer fixed effects, customer × year fixed effects, as well as supplier industry × customer industry × year fixed effects. All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break			
	(1)	(2)	(3)	(4)
Neg_Sue	0.025*	0.028*	-0.004	0.006
	(0.015)	(0.015)	(0.012)	(0.010)
Neg_Sue × Relative Size (S/C)	-0.023**	-0.026**		
	(0.011)	(0.012)		
Neg_Sue × Major Customer			0.024	0.050***
			(0.106)	(0.017)
Sample	All	All	Large Supplier	Small Supplier
Observations	46774	46729	11543	30013
R^2	0.656	0.656	0.813	0.615
Controls	No	Yes	Yes	Yes
Supplier*Customer FE	Yes	Yes	Yes	Yes
Customer*Year FE	Yes	Yes	Yes	Yes
S.Industry*C.Industry*Year FE	Yes	Yes	Yes	Yes

Table 3.C.2. Cross-sectional analysis: previous short-term EPS incentives

This table examines how the destabilizing effect of suppliers' short-term EPS incentives on supply chains varies with respect to suppliers' previous negative pre-repurchase earnings surprises. The outcome variable *Relationship Break* measures whether a supplier-customer relationship is active in year t but is no longer active in year $t+1$. *Neg_Sue (1st time)* is a dummy variable indicating whether it is the first time for a firm to have negative pre-repurchase EPS surprises in the last four years. Similarly, *Neg_Sue (>1st)* is a dummy variable indicating whether it is *not* the first time for a firm to have negative pre-repurchase EPS surprises in the last four years. The first two columns use the full sample in our analysis, whilst the last two columns impose a restriction where firms that have had negative EPs surprises for more than twice in the last four years are excluded from the sample. *Major Customer* is a dummy variable that equals one if a supplier-customer relationship is covered by the Compustat Segment data (i.e., if a customer represents more than 10% of the supplier's sales). We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. All variables are defined in Table 3.A.1. We control for the supplier-level characteristics (size, ROA, dividend, and stock returns) in all specifications. We include the supplier × customer fixed effects, customer × year fixed effects, and supplier industry × customer industry × year fixed effects. All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break			
	Full Sample		Excl. Poor-Performing Firms	
	(1)	(2)	(3)	(4)
Neg_Sue (1st time)	0.020*		0.011	
	(0.011)		(0.012)	
Neg_Sue (1st time) × Major Customer	0.002		0.017	
	(0.021)		(0.022)	
Neg_Sue (>1st)		-0.011		-0.012
		(0.010)		(0.016)
Neg_Sue (>1st) × Major Customer		0.071***		0.083***
		(0.021)		(0.031)
Observations	32731	37092	30405	27707
R^2	0.677	0.683	0.683	0.698
Controls	Yes	Yes	Yes	Yes
Supplier*Customer FE	Yes	Yes	Yes	Yes
Customer*Year FE	Yes	Yes	Yes	Yes
S.Industry*C.Industry*Year FE	Yes	Yes	Yes	Yes

Table 3.C.3. Cross-sectional analysis: relationship intensity

This table reports how the impact of suppliers' short-term EPS incentives on the stability of supplier-customer relationships varies according to relationship intensity. The outcome variable *Relationship Break* measures whether a supplier-customer relationship is active in year t but is no longer active in year $t+1$. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. *High Rel Intensity* is a dummy variable indicating whether a customer has other relationships with the supplier (e.g., partnership). *Long Relationship* is a dummy variable indicating whether the length of a supplier-customer relationship is above the sample median. We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. We control for the supplier-level characteristics (size, ROA, dividend, and stock returns) in all specifications. We include the supplier × customer fixed effects, customer × year fixed effects, and supplier industry × customer industry × year fixed effects. All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break	
	(1)	(2)
Neg_Sue	-0.000	-0.033***
	(0.008)	(0.011)
Neg_Sue × Major Customer	0.065***	0.150***
	(0.018)	(0.050)
Neg_Sue × Major Customer × High Rel Intensity	-0.071**	
	(0.033)	
Neg_Sue × Major Customer × Long Relationship		-0.138***
		(0.052)
Observations	47871	47871
R^2	0.665	0.665
Controls	Yes	Yes
Supplier*Customer FE	Yes	Yes
Customer*Year FE	Yes	Yes
S.Industry*C.Industry*Year FE	Yes	Yes

3.D Additional Evidence on Customer Monitoring: Repurchases Driven by Misvaluation

In this section, we provide additional evidence on customer monitoring by examining the effect of a different type of repurchases on customer exit: repurchases driven by undervaluation.

Our test is built on the literature related to misvaluation, which posits that companies with undervalued equity are more inclined to undertake share repurchases to gain strategic advantage (e.g., Stephens and Weisbach (1998); Bonaimé, Öztekin, and Warr (2014)). This alternative motivator for share repurchases provides us with an opportunity to employ a new instrumental variable, namely, the degree of misvaluation. This allows us to measure how customers respond to suppliers' share repurchases that are prompted by perceived misvaluation. If customers' primary concern revolves around whether the share repurchases are value-enhancing or not, we should anticipate a strengthened stability of the supplier-customer relationship when buybacks are driven by undervaluation. Nevertheless, considering the potential endogeneity issues of this setting, the results in this section should be interpreted with caution.

To measure firms' misvaluation, we follow Rhodes-Kropf, Robinson, and Viswanathan (2005) and decompose the market-to-book (MTB) ratios into three components: firm-specific error (FSE), time-series sector error (TSSE), and long-run value to book (LRVTB). We use the sector-level misvaluation, TSSE, instead of firm-level misvaluation to mitigate the endogeneity concerns. TSSE captures the disparities in valuation when the prevailing accounting multiples of a given sector deviate from the long-run sector multiples. This element serves as a measure to assess whether a specific sector is overvalued. We discuss the details of the decomposition and calculation of misvaluation in Appendix 3.D.1.

We present the estimates of customers' response towards suppliers' repurchases driven by misvaluation in Table 3.D.1. Columns 1-2 report the first-stage firm-level OLS results of our instrumental variable. Firms that are overvalued (with a high TSSE) are less likely to engage in share repurchases due to the relatively high costs. Columns 3-4 report the estimates from the 2SLS regressions where firms' level of repurchases (measured as the repurchase amount normalized by total assets) are instrumented by TSSE. In contrast to our baseline findings, major customers are less likely to terminate the supplier-customer relationship when suppliers' repurchases are motivated by misvaluation. When firms increase their amount of repurchases relative to total assets by one standard deviation, there is a 29.7% decrease in the probability of having a relationship break with each of the major customers. This suggests that what customers monitor are the reasons for and the consequences of repurchases - customers are less prone to exit if suppliers' share repurchases are value-enhancing.

3.D.1 Measuring Misvaluation

In this section, we describe in detail how the sector-level misvaluation is calculated. Rhodes-Kropf, Robinson, and Viswanathan (2005) (RKRV) decomposes the MTB ratios into three components: firm-specific error (FSE), time-series sector error (TSSE), and long-run value to book (LRVTB). When writing in logarithm terms, the decomposition can be written as,

$$m_{it} - b_{it} = m_{it} - v(\theta_{it}; \alpha_{jt}) + v(\theta_{it}; \alpha_{jt}) - v(\theta_{it}; \alpha_j) + v(\theta_{it}; \alpha_j) - b_{it}, \tag{3.D.1}$$

where $m_{it} - v(\theta_{it}; \alpha_{jt})$ is FSE, $v(\theta_{it}; \alpha_{jt}) - v(\theta_{it}; \alpha_j)$ is TSSE, and $v(\theta_{it}; \alpha_j) - b_{it}$ is LRVTB.

FSE measures the difference between a firm's market value and its fundamental value estimated using its firm-specific accounting data θ_{it} and its contemporaneous sector multiples α_{jt}. TSSE captures the deviation of a firm's fundamental value when using the contemporaneous sector multiples α_{jt} from that when using the long-run sector multiples α_j. Finally, LRVTB measures the difference between a firm's value estimated using the long-run multiples and its book value.

The decomposition of MTB developed by Rhodes-Kropf, Robinson, and Viswanathan (2005) has been widely used in the literature (e.g., Hertzel and Li (2010), Bonaimé, Öztekin, and Warr (2014)), we closely follow the RKRV model and use their third model to estimate $v(\theta_{it}; \alpha_{jt})$ and $v(\theta_{it}; \alpha_j)$. Specifically, we run the regression model below for each Fama-French 12 industry in each year to estimate firms' fundamental value,

$$m_{it} = \alpha_{0jt} + \alpha_{1jt} b_{it} + \alpha_{2jt} ln(NI)^+_{it} + \alpha_{3jt} I_{<0} ln(NI)^+_{it} + \alpha_{4jt} Leverage_{it} + \epsilon_{it}, \tag{3.D.2}$$

where NI^+ is the absolute value of net income, $I_{<0}$ is an indicator regarding whether net income is negative.

A firm's estimated $v(\theta_{it};\alpha_{jt})$ is the fitted value from Equation 3.D.2:

$$\begin{aligned} &v(b_{it}, NI_{it}, Leverage_{it}; \hat{\alpha}_{0jt}, \hat{\alpha}_{1jt}, \hat{\alpha}_{2jt}, \hat{\alpha}_{3jt}, \hat{\alpha}_{4jt}) \\ &\qquad = \hat{\alpha}_{0jt} + \hat{\alpha}_{1jt} b_{it} + \hat{\alpha}_{2jt} ln(NI)^{+}_{it} + \hat{\alpha}_{3jt} I_{<0} ln(NI)^{+}_{it} + \hat{\alpha}_{4jt} Leverage_{it}. \end{aligned} \tag{3.D.3}$$

To estimate $v(\theta_{it};\alpha_j)$, we first calculate the long-run multiples α_j by averaging α_{jt} over the sample period ($\alpha_j = \frac{1}{T}\alpha_{jt}$). We then replace $\hat{\alpha}_{jt}$ in Equation 3.D.3 with the estimated α_j (denoted as $\bar{\alpha}_{jt}$),

$$\begin{aligned} &v(b_{it}, NI_{it}, Leverage_{it}; \bar{\alpha}_{0j}, \bar{\alpha}_{1j}, \bar{\alpha}_{2j}, \bar{\alpha}_{3j}, \bar{\alpha}_{4j}) \\ &\qquad = \bar{\alpha}_{0j} + \bar{\alpha}_{1j} b_{it} + \bar{\alpha}_{2j} ln(NI)^{+}_{it} + \bar{\alpha}_{3j} I_{<0} ln(NI)^{+}_{it} + \bar{\alpha}_{4j} Leverage_{it}. \end{aligned} \tag{3.D.4}$$

We use TSSE instead of FSE in this paper to capture misvaluation, and further use it as an instrument for share repurchases. This is because TSSE measures the sector misvaluation which is common to all firms in this sector and thus alleviates the concern that firms' share repurchases and misvaluation are simultaneously determined by some unobservable firm-specific factors.

Table 3.D.1. Repurchases driven by misvaluation

This table reports how repurchases driven by misvaluation affect the stability of supplier-customer relationships. Columns (1) and (2) show the firm-level first-stage OLS results on the effect of misvaluation on corporate repurchases. Columns (3) and (4) report the IV regression results regarding the impact of misvaluation-driven repurchases on the stability of supplier-customer relationships. Our instrumental variable *TSSE* measures a firm's misvaluation that is attributed to the deviation of time-varying sector multiples from the long-run value. Details of the calculation of *TSSE* is described in Section 3.D.1. We control for various fixed effects and firm-level characteristics (size, ROA, dividend, and stock returns). The standard errors for firm-level regressions are clustered at the firm level and those for the relationship-level regressions are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Repurchases		*Relationship Break*	
	OLS		IV	
	(1)	(2)	(3)	(4)
TSSE	-0.006**	-0.006**		
	(0.002)	(0.002)		
Repurchase			-2.992	-3.010
			(2.116)	(2.117)
Repurchase*Major Customer			-8.197***	-8.016***
			(3.088)	(3.082)
Observations	20029	19999	65597	65553
SW-F (Repurchase)			45.06	45.22
SW-F (Repurchase*Major Customer)			37.04	35.86
Controls	No	Yes	No	Yes
Firm FE	Yes	Yes		
Year FE	Yes	Yes		
Supplier*Customer FE			Yes	Yes
Customer*Year FE			Yes	Yes
S.Industry*C.Industry*Year FE			Yes	Yes

3.E Ancillary Tables

Table 3.E.1. Impact of short-term EPS incentives on share repurchases

This table reports the relation between having negative pre-repurchase EPS surprises and firms' probability of doing share repurchases. The outcome variable *Accretive_Rep* is an indicator that equals one if a firm conducts repurchases that increase its EPS by at least one cent. *Repurchase* is measured as the net repurchase amount scaled by total assets. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. We use the full sample as well as a small-window sample where $-0.003 \leq Sue_{adj} \leq 0.003$. We control for firm fixed effects and year-quarter $\times$ industry fixed effects. All standard errors are clustered at the firm level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Accretive_Rep		*Repurchase*	
	(1)	(2)	(3)	(4)
Neg_Sue	0.024***	0.049***	0.004***	0.004***
	(0.002)	(0.004)	(0.000)	(0.001)
Observations	94195	55779	94195	55779
R^2	0.317	0.360	0.291	0.330
Small Window	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Year-Quarter*Industry FE	Yes	Yes	Yes	Yes

Table 3.E.2. Impact of EPS-boosting repurchases on firm outcomes: IV

This table reports the firm-level estimates of the impact of EPS-boosting repurchases on firm outcomes using the IV regressions. *Cash* is calculated as $(Cash_{(t+1,t+4)} - Cash_{(t-4,t-1)})/Assets_{(t-4,t-1)}$, where t is the earnings surprise quarter. *Interest Coverage* is at the annual level and is calculated as EBIT/interest expenses in year $t+1$. We measure *Investment* as $(CAPEX_{(t+1,t+4)} - CAPEX_{(t-4,t-1)})/Assets_{(t-4,t-1)}$, and measure *Sales Growth* as $(Sales_{(t+1,t+4)} - Sales_{(t-4,t-1)})/Sales_{(t-4,t-1)}$. *Cash* and *Investment* are adjusted by multiplying with 100. *Repurchase* is measured as the net repurchase amount scaled by total assets when firms conduct accretive share repurchases, zero otherwise. We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. The detailed variable definitions are described in Table 3.A.1. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. We control for firm fixed effects, time $\times$ industry fixed effects and firm-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the firm level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Cash	Interest Coverage	Investment	Sales Growth
	(1)	(2)	(3)	(4)
Repurchase	-176.5140***	-14.9725**	-9.4650**	-5.9211***
	(54.734)	(6.149)	(3.713)	(1.365)
Observations	51678	9716	51107	51302
Kleibergen-Paap F	109.962	39.918	109.640	109.446
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Time*Industry FE	Yes	Yes	Yes	Yes

Table 3.E.3. Impact of EPS-boosting repurchases on supplier-customer relationships: IV

This table reports estimates of the impact of EPS-boosting repurchases on the stability of supplier-customer relationships using the IV regressions. The outcome variable *Relationship Break* measures whether a supplier-customer relationship is active in year t but is no longer active in year $t+1$. *Repurchase* is measured as the net repurchase amount scaled by total assets when firms conduct accretive share repurchases, zero otherwise. *Major Customer* is a dummy variable that equals one if a supplier-customer relationship is covered by the Compustat Segment data (i.e., if a customer represents more than 10% of the supplier's sales). We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise and the major customer dummy as defined in our baseline regression model. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. All variables are defined in Table 3.A.1. We control for the supplier-level characteristics (size, ROA, dividend, and stock returns) in Column (2). In both columns, we include the supplier $\times$ customer fixed effects, customer $\times$ year fixed effects and supplier industry $\times$ customer industry $\times$ year fixed effects. All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break	
	(1)	(2)
Repurchase	-0.672	-0.781
	(1.366)	(1.485)
Repurchase*Major Customer	11.194**	12.022**
	(5.646)	(5.921)
Observations	47915	47871
Controls	No	Yes
Supplier*Customer FE	Yes	Yes
Customer*Year FE	Yes	Yes
S.Industry*C.Industry*Year FE	Yes	Yes

Table 3.E.4. Impact of short-term EPS incentives on supplier-customer relationships: pre-trends

This table reports the pre-trend analysis of the impact of short-term EPS incentives on the stability of supplier-customer relationships. The column name *Break_i* indicates that we are looking at the relationship breaks i year ($i = 1, 2, 3$) before firms' negative earnings surprises. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. *Major Customer* is a dummy variable that equals one if a supplier-customer relationship is covered by the Compustat Segment data (i.e., if a customer represents more than 10% of the supplier's sales). We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. All variables are defined in Table 3.A.1. We control for supplier × customer fixed effects, customer × year fixed effects, supplier industry × customer industry × year fixed effects and supplier-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Break_1	*Break_2*	*Break_3*
	(1)	(2)	(3)
Neg_Sue	-0.011	0.018	-0.009
	(0.010)	(0.015)	(0.016)
Neg_Sue × Major Customer	-0.010	-0.029	-0.001
	(0.018)	(0.024)	(0.025)
Observations	29752	12686	9765
R^2	0.713	0.740	0.718
S.Firm*T.Firm FE	Yes	Yes	Yes
T.Firm*Year FE	Yes	Yes	Yes
S.Industry*T.Industry*Year FE	Yes	Yes	Yes

Table 3.E.5. Effect of financial constraints for firms with positive actual EPS surprises

This table reports how the impact of EPS-boosting repurchases on the stability of supplier-customer relationships differs according to suppliers' financial constraints. We use the Hadlock-Pierce Index and Whited-Wu Index to proxy for financial constraints. For the observations that are to the left of the zero pre-repurchase earnings surprise, we only include firm-years that would have missed the EPS forecast if share repurchases had not taken place. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. Same as in the previous tables, we also include the size of the pre-repurchase EPS surprise, as well as its interaction term with *Neg_Sue*. We control for supplier × customer fixed effects, customer × year fixed effects, supplier industry × customer industry × year fixed effects and supplier-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break			
	Hadlock-Pierce Index		Whited-Wu Index	
	(1)	(2)	(3)	(4)
Neg_Sue	0.029	-0.060***	-0.076***	-0.033*
	(0.026)	(0.018)	(0.025)	(0.018)
Neg_Sue × Major Customer	0.104**	0.066*	0.205***	0.034
	(0.052)	(0.034)	(0.057)	(0.040)
Observations	11485	11435	11804	10037
R^2	0.712	0.785	0.715	0.798
Sample	Fin Constrained	Fin Un-Constrained	Fin Constrained	Fin Un-Constrained
Controls	Yes	Yes	Yes	Yes
Supplier*Customer FE	Yes	Yes	Yes	Yes
Customer*Year FE	Yes	Yes	Yes	Yes
S.Industry*C.Industry*Year FE	Yes	Yes	Yes	Yes

Table 3.E.6. Impact of short-term EPS incentives on trade credit

This table reports the firm-level estimates of the impact of short-term EPS incentives on trade credit. We measure *AP Days* as $Accounts\,Payable\,Days_{(t+1,t+4)}$, and measure *AR Days* as $Accounts\,Receivable\,Days_{(t+1,t+4)}$, where t is the earnings surprise quarter. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. The detailed variable definitions are described in Table 3.A.1. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. We control for firm fixed effects, year-quarter × industry fixed effects and supplier-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the firm level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	AP Days		AR Days	
	(1)	(2)	(3)	(4)
Neg_Sue	-0.001	-0.001	0.004	0.002
	(0.005)	(0.005)	(0.003)	(0.003)
Observations	53064	51766	53032	51675
R^2	0.823	0.824	0.855	0.858
Controls	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Year-Quarter*Industry FE	Yes	Yes	Yes	Yes

Table 3.E.7. Impact of short-term EPS incentives on gross margin

This table reports the firm-level estimates of the impact of short-term EPS incentives on gross margin. *Gross Margin* is calculated as $(GrossMargin_{(t+1,t+4)} - GrossMargin_{(t-4,t-1)})/GrossMargin_{(t-4,t-1)}$, where t is the earnings surprise quarter. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. The detailed variable definitions are described in Table 3.A.1. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. We control for firm fixed effects, year-quarter $\times$ industry fixed effects and firm-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the firm level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Gross Margin	
	(1)	(2)
Neg_Sue	-0.004**	-0.004**
	(0.002)	(0.002)
Observations	49626	48577
R^2	0.305	0.318
Controls	No	Yes
Firm FE	Yes	Yes
Time*Industry FE	Yes	Yes

Table 3.E.8. Impact of short-term EPS incentives on firm outcomes: pre-trend analysis

This table reports the pre-trend analysis of how short-term EPS incentives affects firm outcomes. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. Same as in the previous tables, we also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. We control for firm fixed effects, year-quarter × industry fixed effects and firm-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the firm level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	t-1	t-2	t-3
	(1)	(2)	(3)
Panel A: Cash			
Neg_Sue	-0.300	-0.479*	-0.281
	(0.240)	(0.247)	(0.250)
Observations	31873	30946	28135
R^2	0.457	0.437	0.438
Panel B: Interest Coverage			
Neg_Sue	0.015	0.033	-0.001
	(0.043)	(0.052)	(0.055)
Observations	6153	3864	4011
R^2	0.762	0.805	0.778
Panel C: Investment			
Neg_Sue	0.001	0.002	0.010
	(0.016)	(0.016)	(0.017)
Observations	31448	30558	27813
R^2	0.406	0.395	0.408
Panel D: Sales Growth			
Neg_Sue	-0.007	-0.003	0.000
	(0.006)	(0.006)	(0.006)
Observations	31695	30784	27991
R^2	0.522	0.501	0.507
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Time*Industry FE	Yes	Yes	Yes

Table 3.E.9. Impact of short-term EPS incentives on supplier-customer relationships: sensitivity test

This table reports the sensitivity test of the impact of short-term EPS incentives on the stability of supplier-customer relationships. We adjust the pre-repurchase EPS surprise window and allow it to range from 0.001 to 0.005. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise, where the pre-repurchase EPS surprise is calculated as the difference between the repurchase-adjusted EPS and the median end-of-quarter EPS forecast, scaled by the end-of-quarter stock price. We also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. All variables are defined in Table 3.A.1. We control for supplier $\times$ customer fixed effects, customer $\times$ year fixed effects, supplier industry $\times$ customer industry $\times$ year fixed effects and supplier-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

Dependent Variable is Relationship Break:	*Bandwidth*				
	0.001	0.002	0.003	0.004	0.005
	(1)	(2)	(3)	(4)	(5)
Neg_Sue	-0.005	-0.011	-0.001	-0.001	0.002
	(0.012)	(0.009)	(0.008)	(0.007)	(0.006)
Neg_Sue × Major Customer	0.058**	0.055***	0.049***	0.039***	0.027**
	(0.025)	(0.018)	(0.015)	(0.014)	(0.013)
Observations	22851	38530	47871	53400	57613
R^2	0.726	0.680	0.664	0.653	0.648
Supplier*Customer FE	Yes	Yes	Yes	Yes	Yes
Customer*Year FE	Yes	Yes	Yes	Yes	Yes
S.Industry*T.Industry*Year FE	Yes	Yes	Yes	Yes	Yes

Table 3.E.10. Impact of short-term EPS incentives on supplier-customer relationships: degree of EPS surprises

This table estimates whether firms with less negative pre-repurchase EPS surprises are more likely to lose customers compared with those with more negative pre-repurchase EPS surprises. Firms included in this sample all have negative pre-repurchase EPS surprises where $-0.005 \leq Sue \leq 0$. *Neg_Sue_Small* is defined as an indicator variable that equals one if a firm's *Sue* is within the range of $(-0.0025, 0)$, and zero if $-0.005 \leq Sue \leq 0.0025$. Same as in previous tables, we also include the size of the pre-repurchase EPS surprise, as well as its interaction term with *Neg_Sue_Small*. We control for supplier × customer fixed effects, customer × year fixed effects, supplier industry × customer industry × year fixed effects and supplier-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break	
	(1)	(2)
Neg_Sue_Small	-0.204*	-0.206*
	(0.115)	(0.116)
Neg_Sue_Small × Major Customer	0.702***	0.689***
	(0.195)	(0.203)
Observations	7043	7039
R^2	0.814	0.815
Controls	No	Yes
S.Firm*Customer FE	Yes	Yes
Customer*Year FE	Yes	Yes
S.Industry*C.Industry*Year FE	Yes	Yes

Table 3.E.11. Impact of EPS-boosting repurchases on supplier-customer relationships: positive actual EPS surprises

This table reports estimates of the impact of EPS-boosting repurchases on the stability of supplier-customer relationships. For the observations that are to the left of the zero pre-repurchase earnings surprise, we only include firm-years that would have missed the EPS forecast if share repurchases had not taken place. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. Same as in previous tables, we also include the size of the pre-repurchase EPS surprise, as well as its interaction term with *Neg_Sue*. We control for supplier × customer fixed effects, customer × year fixed effects, supplier industry × customer industry × year fixed effects and supplier-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break	
	(1)	(2)
Neg_Sue	-0.025**	-0.024**
	(0.012)	(0.012)
Neg_Sue × Major Customer	0.050*	0.051*
	(0.028)	(0.028)
Observations	30959	30922
R^2	0.692	0.692
Controls	No	Yes
S.Firm*Customer FE	Yes	Yes
Customer*Year FE	Yes	Yes
S.Industry*C.Industry*Year FE	Yes	Yes

Table 3.E.12. Impact of EPS-boosting repurchases on supplier-customer relationships: repurchases v.s. no repurchases

This table reports estimates of the impact of EPS-boosting repurchases on the stability of supplier-customer relationships. We only include firms with negative pre-repurchase EPS surprises and our main independent variable *EPS-Boosting Repurchases* equals one if a firm's post-repurchase EPS eventually meets analysts' forecast. Similar to previous tables, we also include the size of the pre-repurchase EPS surprise, as well as its interaction term with *EPS-Boosting Repurchases*. To ensure that firm characteristics (e.g., financial conditions) are similar across firms that conduct repurchases and those that do not, we limit the lower bound of earnings surprise to -0.0005. We control for supplier × customer fixed effects, customer × year fixed effects, and supplier-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break	
	(1)	(2)
EPS-Boosting Repurchases	-0.115*	-0.101
	(0.062)	(0.066)
EPS-Boosting Repurchases × Major Customer	0.243**	0.246**
	(0.098)	(0.100)
Observations	1077	1077
R^2	0.754	0.756
Controls	No	Yes
S.Firm*Customer FE	Yes	Yes
Customer*Year FE	Yes	Yes

Table 3.E.13. Impact of short-term EPS incentives on supplier-customer relationships: open market repurchases program

This table reports estimates of the impact of short-term EPS incentives on the stability of supplier-customer relationships, exploiting the heterogeneity of open market repurchases programs. We define a dummy variable *Open Market* that equals one if a supplier has an open market share repurchases program in that year. To ensure that firm characteristics (e.g., financial conditions) are similar across firms that conduct repurchases and those that do not, we limit the lower bound of earnings surprise to -0.0005. We control for supplier × customer fixed effects, customer × year fixed effects, and supplier-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break	
	(1)	(2)
Neg_Sue	0.017**	0.017**
	(0.008)	(0.008)
Neg_Sue × Major Customer	0.034*	0.037**
	(0.017)	(0.017)
Neg_Sue × Major Customer × Open Market	0.074**	0.071**
	(0.036)	(0.036)
Observations	47915	47871
R^2	0.664	0.665
Controls	No	Yes
Supplier*Customer FE	Yes	Yes
Customer*Year FE	Yes	Yes
S.Industry*C.Industry*Year FE	Yes	Yes

Table 3.E.14. Impact of short-term EPS incentives on supplier-customer relationships: controlling for other types of earnings management

This table reports estimates of the impact of suppliers' short-term EPS incentives on the stability of supplier-customer relationships, controlling additionally for other types of earnings management. In columns (1) and (2), we include firms' total accruals in our baseline regression; whilst in columns (3) and (4), we control for the discretionary accruals. Our main independent variable *Neg_Sue* is a dummy variable indicating whether a firm has a negative pre-repurchase EPS surprise. To measure total accruals, we follow the method in Dechow, Sloan, and Sweeney (1995) and normalize total accruals by lagged assets. As for discretionary accruals, we adopt the modified Jones model in Dechow, Sloan, and Sweeney (1995) to obtain the estimates. Same as in previous tables, we also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise. To mitigate the concern of systematic differences between firms that fall on either side of the zero pre-repurchase EPS surprise threshold, we limit our analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. We control for supplier $\times$ customer fixed effects, customer $\times$ year fixed effects, supplier industry $\times$ customer industry $\times$ year fixed effects and supplier-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break			
	(1)	(2)	(3)	(4)
Neg_Sue	-0.001	-0.001	-0.001	-0.001
	(0.008)	(0.008)	(0.008)	(0.008)
Neg_Sue × Major Customer	0.046***	0.049***	0.046***	0.049***
	(0.015)	(0.015)	(0.015)	(0.015)
Total Accruals	0.166***	0.188***		
	(0.056)	(0.056)		
Discretionary Accruals			0.133***	0.149***
			(0.050)	(0.050)
Observations	47062	47018	47041	46997
R^2	0.662	0.663	0.662	0.663
Controls	No	Yes	No	Yes
S.Firm*Customer FE	Yes	Yes	Yes	Yes
Customer*Year FE	Yes	Yes	Yes	Yes
S.Industry*C.Industry*Year FE	Yes	Yes	Yes	Yes

Table 3.E.15. Impact of restatement on supplier-customer relationships

This table reports estimates of the impact of suppliers' restatement on the stability of supplier-customer relationships. Columns (1) and (2) use the full sample whilst columns (3) and (4) limit the analysis to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$. Our main independent variable *Restatement* is a dummy variable indicating whether a firm makes an earnings restatement for that year. Same as in previous tables, we also include the size of the pre-repurchase EPS surprise, as well as its interaction term with the sign of the surprise in column (4). We control for supplier × customer fixed effects, customer × year fixed effects, supplier industry × customer industry × year fixed effects and supplier-level characteristics (size, ROA, dividend, and stock returns). All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break			
	(1)	(2)	(3)	(4)
Restatement	-0.009	-0.013	0.077***	0.077***
	(0.017)	(0.017)	(0.028)	(0.028)
Restatement × Major Customer	-0.006	-0.008	-0.132*	-0.134*
	(0.032)	(0.032)	(0.077)	(0.077)
Neg_Sue				-0.001
				(0.008)
Neg_Sue × Major Customer				0.050***
				(0.015)
Observations	85450	85158	47871	47871
R^2	0.608	0.609	0.664	0.665
Small Window	No	No	Yes	Yes
Controls	No	Yes	Yes	Yes
Supplier*Customer FE	Yes	Yes	Yes	Yes
Customer*Year FE	Yes	Yes	Yes	Yes
S.Industry*C.Industry*Year FE	Yes	Yes	Yes	Yes

Table 3.E.16. Other types of repurchases

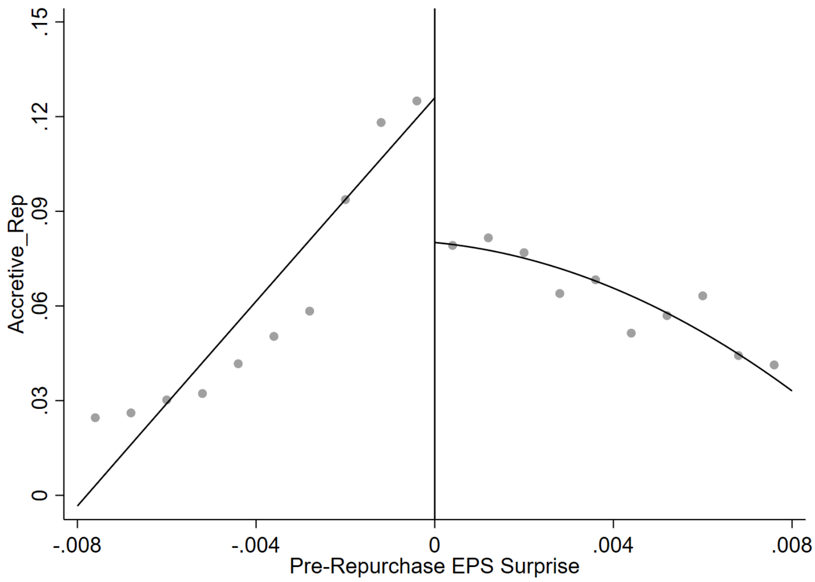
This table reports the estimated impact of other types of repurchases on supplier-customer relationships. Column (1) uses the full sample and column (2) limits the sample to a small window where $-0.003 \leq Sue_{adj} \leq 0.003$ as in previous analyses. *Other Repurchases* is defined as a dummy variable that equals one if a firm conducts repurchases that are not used to meet analysts' EPS forecast. We control for supplier × customer fixed effects, customer × year fixed effects, supplier industry × customer industry × year fixed effects and supplier-level characteristics (size, ROA, dividend, and stock return). All standard errors are clustered at the supplier-customer pair level and are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1% level, respectively.

	Relationship Break	
	(1)	(2)
Other Repurchases	0.005	0.009
	(0.004)	(0.006)
Other Repurchases × Major Customer	-0.016**	-0.017
	(0.008)	(0.012)
Observations	85158	47871
R^2	0.609	0.664
Sample	Full Sample	Small Window
Controls	Yes	Yes
S.Firm*Customer FE	Yes	Yes
Customer*Year FE	Yes	Yes
S.Industry*C.Industry*Year FE	Yes	Yes

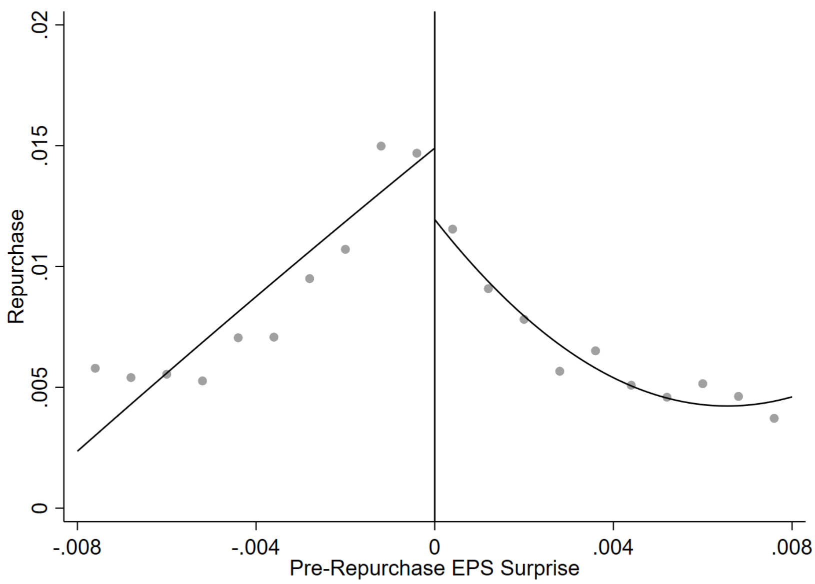
3.F Ancillary Figures

Figure 3.F.1: Negative pre-repurchase EPS surprises and repurchases

This figure depicts the probability and amount of share repurchases as a function of pre-repurchase EPS surprise. In Figure (a), for every earnings surprise bin, the dots represent the probability of accretive share repurchases. In Figure (b), the dots represent the repurchase amount for that corresponding earnings surprise bin. The lines are second-order polynomials fitted through the estimated probability and amount of repurchase on each side of the zero pre-repurchase earnings surprise.



(a)



(b)

Figure 3.F.2: Negative pre-repurchase EPS surprises and repurchases: decretive repurchases

This figure depicts the probability of decretive share repurchases as a function of pre-repurchase EPS surprise. For every earnings surprise bin, the dots represent the probability of decretive share repurchases. The lines are second-order polynomials fitted through the estimated probability of decretive repurchase on each side of the zero pre-repurchase earnings surprise.

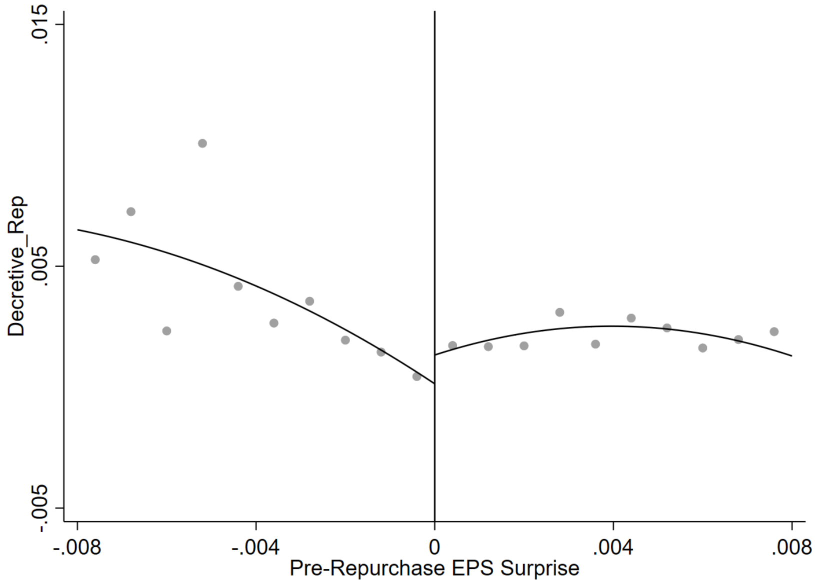
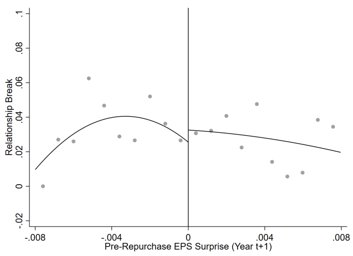
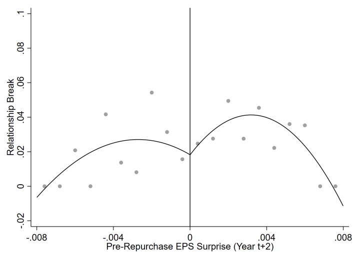


Figure 3.F.3: Negative pre-repurchase EPS surprises and supplier-customer relationship stability: pre-trend analyses

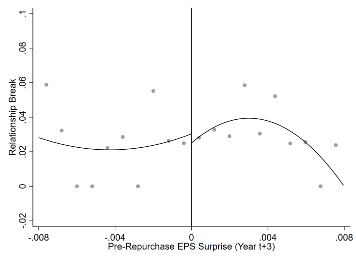
This figure plots the pre-trend analyses over the probability of relationship breaks between firms and their customers subject to the earnings surprises in year $t+i$, where $i = 1, 2, 3$. For every earnings surprise bin, the dots represent the probability of customer relationship breaks. The lines are second-order polynomials fitted through the estimated relationship break probabilities on each side of the zero pre-repurchase earnings surprise. Figure (a)-(c) use the sample with all the major customers, and Figure (d)-(e) use the sample with all customers including both the major and minor ones.



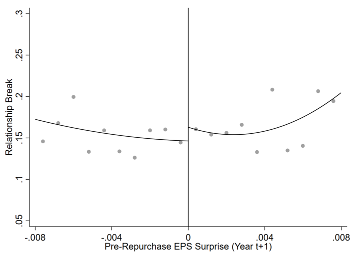
(a) Major Customer (Year t+1)



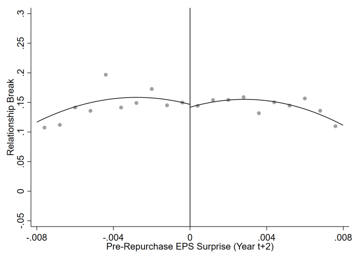
(b) Major Customer (Year $t+2$)



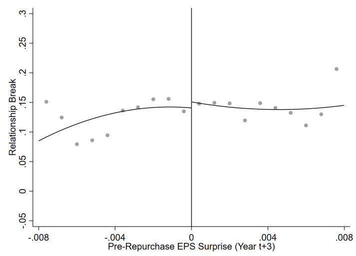
(c) Major Customer (Year t+3)



(d) Customer (Year $t+1$)



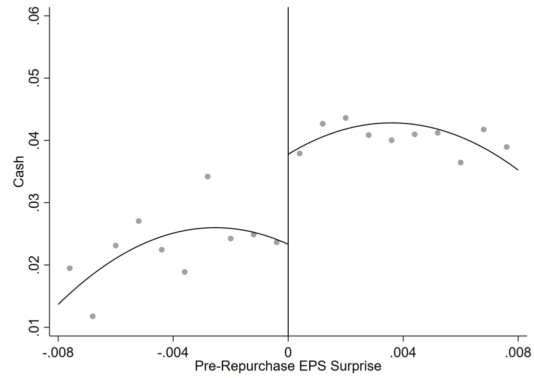
(e) Customer (Year $t+2$)



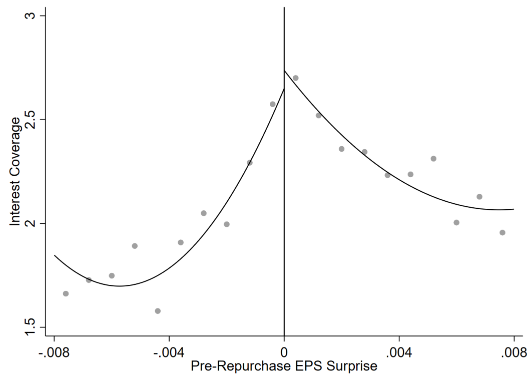
(f) Customer (Year $t+3$)

Figure 3.F.4: Negative pre-repurchase EPS surprises and firm outcomes

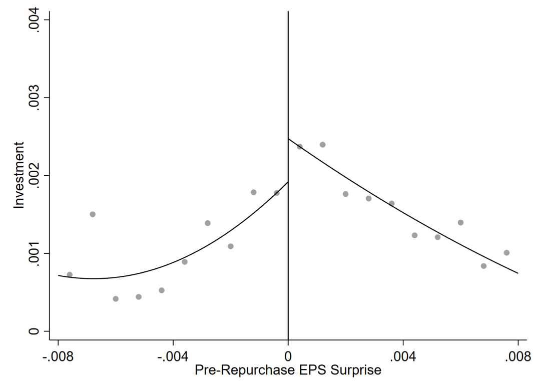
This figure plots firms' cash, interest coverage, investment, and sales growth as a function of pre-repurchase EPS surprise. The lines are second-order polynomials fitted through the estimated value of outcome variables on each side of the zero pre-repurchase earnings surprise.



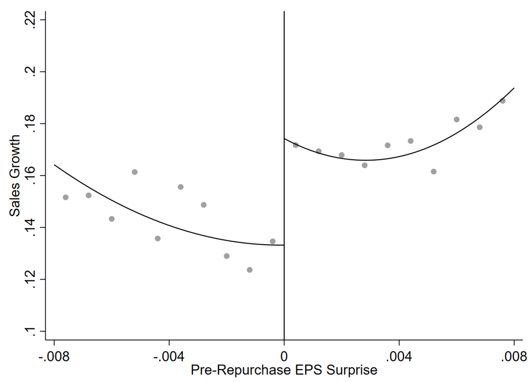
(a)



(b)



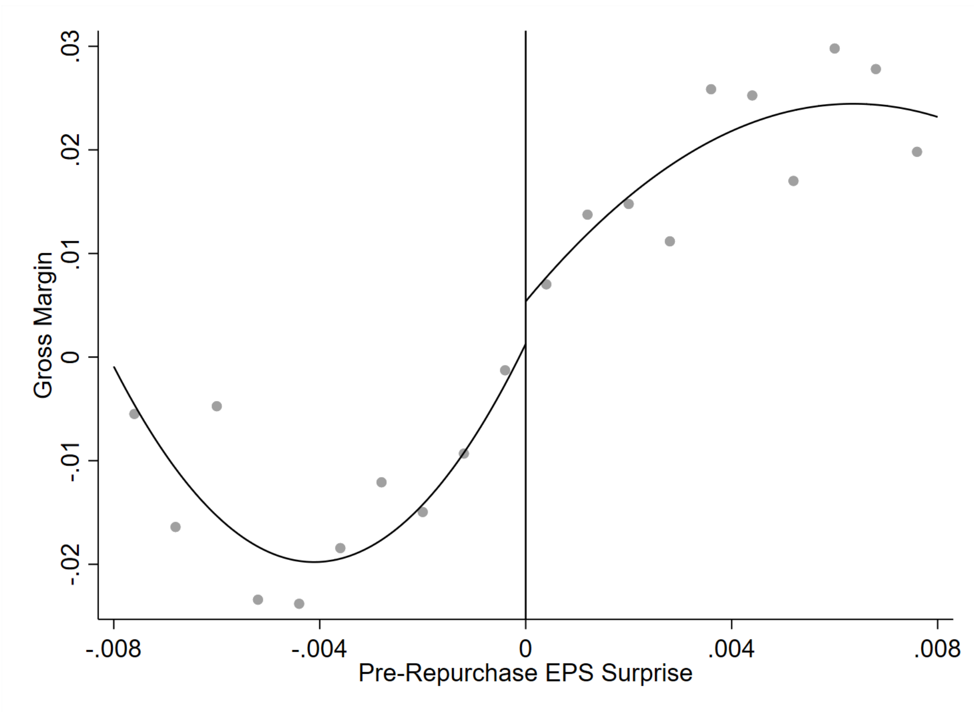
(c)



(d)

Figure 3.F.5: Negative pre-repurchase EPS surprises and gross margin

This figure plots firms' gross margin as a function of pre-repurchase EPS surprise. The lines are second-order polynomials fitted through the estimated value of outcome variables on each side of the zero pre-repurchase earnings surprise.



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