

Institutional ownership and labor-related misconduct: Evidence from U.S. federal violations*

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Abstract

Using a novel, comprehensive dataset on penalties assessed by U.S. federal agencies for labor-related misconduct, we examine the effect of institutional investors on firms' labor practices. We find that ownership by institutions, especially those with a long-term investment horizon and monitoring incentives, is associated with a lower likelihood of firms receiving federal penalties for labor-related violations. Although the direct penalty amounts are typically immaterial for violating firms, we find that firms receiving such penalties are more likely to face employee lawsuits in subsequent years. This finding is consistent with the view that institutional investors view labor-related misconduct as value-destructive in the long run – potentially due to heightened litigation risk and associated reputational damage. Finally, to understand potential channels through which institutional investors can exert influence to reduce labor-related misconduct, we consider potential influence via boards of directors and voting on shareholder proposals. This paper adds to the literature examining the role of institutional investors beyond capital markets.

Keywords: Institutional Ownership, Corporate Social Responsibility, Labor Practice, Violation, Misconduct

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“Society is demanding that companies, both public and private, serve a social purpose. To prosper over time, every company must not only deliver financial performance, but also show how it makes a positive contribution to society. Companies must benefit all of their stakeholders, including shareholders, employees, customers, and the communities in which they operate.”

– “*A Sense of Purpose*”, BlackRock CEO Larry Fink’s 2018 Letter to CEOs

1. Introduction

A large literature examines institutional investors’ role in capital markets. Prior research documents a link between institutional ownership and numerous aspects of firms’ financial behavior, including mandatory and voluntary disclosure, corporate governance, tax planning, and dividend policy (e.g., Ramalingegowda and Yu, 2012; Boone and White, 2015; Appel, Gormley, and Keim, 2016; Chen, Huang, Li, and Shevlin, 2018). The central finding of this literature is that institutional investors improve shareholder value by exerting influence over managers’ decision-making processes.

However, there is little prior research on institutional investors’ impact on parties other than capital market participants. In light of a growing push by investors and regulators for additional disclosures pertaining to Corporate Social Responsibility (CSR) – and for a more stakeholder-centric, rather than shareholder-centric approach to business – our paper addresses the following key question: how do institutional investors influence firms’ treatment of employees? We answer this question by taking advantage of a unique dataset called *Violation Tracker* that contains comprehensive information on penalties assessed by U.S. federal agencies. Of these agencies, we focus on enforcement actions assessed by the nine federal agencies that primarily write and enforce labor laws. We examine whether institutional ownership affects firms’ tendency to commit various forms of labor-related misconduct (e.g., failing to maintain a safe workplace, underreporting workers’ hours or mis-classifying workers as exempt to either avoid paying them overtime or providing them with benefits, or forcing employees to work during mandatory paid rest time). We focus on labor-related misconduct for two

main reasons. First, among all penalties for non-financial misconduct assessed by U.S. federal agencies, labor-related misconduct is the most prevalent across our sample period and across different industries. Over 41% of distinct firms in our sample have at least one labor-related violation, corresponding to roughly one in every five firm-years examined in our sample. Other types of non-financial misconduct are either relatively rare (e.g., significant product safety violations) or are concentrated in specific industries (e.g., violations of environmental law). Second, employees are arguably the most important non-shareholder stakeholder of a firm and, in recent years, U.S. regulators have increased enforcement of labor-related laws. For example, David Weil, the head of the Department of Labor’s Wage and Hour Division (WHD) from 2014 to 2017, stated in a recent interview about his time running the WHD: ¹

“We had not historically used all of our enforcement tools. We had tools in the tool chest we had neglected to use. So we started to aggressively recover liquidated damages along with back wages. We started to use civil monetary penalties much more aggressively.”

The increased focus by regulators parallels increasing demand for information on firms’ labor practices by institutional investors. In 2017 the Human Capital Management Coalition – a coalition of institutional owners with over \$2.8 trillion in assets under management – formally petitioned the SEC to require additional corporate disclosures about labor practices and the workplace environment more generally.²

Despite this recent attention, it is not ex-ante obvious what the effect of institutional ownership on firms’ labor-related misconduct would be. On one hand, institutional shareholders may view employee satisfaction as a long-run value-enhancing attribute. Consistent with this view, prior studies have documented evidence suggesting a positive association between employee satisfaction and shareholder value (e.g. Edmans, 2011). If this reflects institutional

¹ “A Conversation with David Weil” (Heather Bouchey, *Washington Center for Equitable Growth*, June 2017); full interview available at <https://equitablegrowth.org/equitable-growth-in-conversation-david-weil/>

² The full text of the petition can be found on the SEC’s webpage at <https://www.sec.gov/rules/petitions/2017/petn4-711.pdf>

shareholders' view, then we should find a negative association between institutional ownership and the likelihood of firms' labor-related misconduct. On the other hand, institutional investors may also view generous employee compensation or benefit packages as wasteful and socially responsible activities as reflecting agency problems (Cheng, Hong, and Shue, 2013; Masulis and Reza, 2015). While tightening their monitoring over management, these investors may at the same time reduce portfolio firms' employee-related expenses to divert more money back to shareholders. Moreover, unlike in the case of financial misconduct (e.g. violating securities laws or committing accounting fraud), most federal agencies lack discretion in setting penalty amounts when it comes to non-financial misconduct. While penalties are higher for more severe violations, the direct monetary penalties for violations typically do not come close to offsetting the economic impact of these violations in the way that the penalties for financial misconduct can. Thus, in contrast to the case of financial misconduct, there is unlikely to be a major direct financial impact if a firm is caught committing labor-related misconduct. Institutional shareholders may therefore encourage firms to take employee-unfriendly activities if, net of the additional expected fines, doing so would increase shareholder value. This view predicts a positive association between institutional ownership and the likelihood of firms' labor-related misconduct.

Using a sample of 20,499 firm-years (2,107 distinct firms) from 2004 to 2016, we find that institutional ownership is negatively associated with firms' proclivity to engage in labor-related misconduct, measured by the likelihood and size of federal penalties for violations of labor laws, after controlling for firm characteristics as well as firm and year fixed effects. A one-standard-deviation increase in institutional ownership is associated with an 8.6% decrease in the likelihood of receiving a labor violation in the subsequent year, relative to the sample mean labor violation rate. We next explore heterogeneity in the effect of institutional ownership

on firms' labor-related misconduct. Consistent with the view that investors with long-term investment horizons care more about firms' social performance and exert influence over firms' management (e.g. Chen, Harford, and Li 2007; Starks, Venkat, and Zhu 2018), we find that our results are driven by long-term focused institutional investors and those with monitoring incentives. The above findings are consistent with the view that institutional investors regard labor violations as detrimental to long-run shareholder value. We do not, however, find a link between quasi-indexers and labor-related misconduct, in contrast with a wide body of prior research on quasi-indexers' effect on corporate behavior (e.g., Appel, Gormley, and Keim, 2016). This difference highlights the fact that we study an aspect of firm behavior that, while relevant to firm value, is distinct from prior research on institutional owners' influence on their portfolio firms' financial reporting and conduct.

Investors with a shorter-term focus may not care about labor misconduct due to one notable feature of most federal sanctions for labor-related misconduct; the relatively small size of fine amounts. The median penalty assessed in our sample is only \$21,996, representing a median of 0.005% of firm sales. The direct monetary cost of committing non-financial misconduct is therefore immaterial. Our results are thus unlikely to be driven by institutional investors' financial concern for the direct monetary costs associated with labor-related misconduct. Consistent with this view, we find that *conditional on* violating federal laws, institutional ownership is *not* associated with the size of a penalty. However, there may be indirect costs; it is possible that violations of labor laws may increase the likelihood of future lawsuits, which could be material and costly to shareholders. We directly test this assertion by examining the association between labor violations and employee lawsuits. We find that firms that commit labor violations are more likely to face employee lawsuits in subsequent years – with the average settlement in excess of \$10 million and some settlements as high as 2.5% of a firm's annual sales. This finding provides support to the view that indirect costs, such as employee lawsuits

or associated reputational damage, might be a reason why institutional shareholders care about labor violations.

Finally, we explore channels through which institutional investors could reduce labor violations. Prior literature suggests shareholder voice as a dominant mechanism to drive corporate governance, financial, and non-financial changes in the firm (Appel, Gormley, and Keim, 2016; Crane, Michenaud, Weston, 2015; Dyck, Lins, Roth, and Wagner, 2018). Institutional investors may exercise voice via three channels: (1) influencing the board of directors, (2) voting at annual general meetings (AGM), and (3) private engagements with the target firm. While (3) is not publicly observable, we are able to empirically test (1) and (2). To test the first channel, we explore the cross-sectional variations in state-level legislation of corporate constituency statutes. Prior to 2004, 33 U.S. states enacted laws that allow directors of public firms to consider the interests of non-shareholder stakeholders (Orts, 1992; Flammer and Kacperczyk, 2016). Boards of companies incorporated in states with constituency statutes are therefore more “stakeholder-friendly”, as directors are more likely to incorporate other stakeholders’ interests into their decision-making process. Consistent with this argument, we find that the effect of institutional ownership on labor-related misconduct is stronger among firms incorporated in states with constituency statutes. This finding is consistent with the idea that institutional investors may exercise their influence over corporate decision-making via the board of directors. We next examine the second channel of shareholder voice via voting at AGMs. We find that institutional ownership is positively associated with the number of shareholder proposals in socially responsible investment (SRI) topics, as well as the percentage of votes in favor of such proposals once they are put to vote at AGMs. Although SRI shareholder proposals generally do not pass in AGMs (only 25 out of 2,954, or less than 1%, of SRI proposals in our sample receive more than the 50% votes required to pass), we find that they are more likely to be withdrawn and receive a higher percentage of votes when institutional ownership is higher.

Withdrawal may indicate that an agreement was reached between the parties before a proposal advanced to the voting stage. These results therefore suggest shareholders' dissent on SRI issues may contribute to the reduction in labor-related legal violations.

This paper makes two key contributions to the literature. First, it adds to the literature on the determinants of firms' investment in workplace safety and labor-related misconduct (e.g., Filer and Golbe, 2003; Cohn and Wardlaw, 2016; Caskey and Ozel, 2017; Raghunandan, 2019). These papers identify firms' short-term financial incentives, such as operating margins, financing constraints, and meeting-or-beating earnings targets, as driving factors for their underinvestment in workplace safety and decisions to commit labor violations. Our study suggests that the presence of institutional investors, especially those with a long-term investment horizon and monitoring incentive, could mitigate these short-term financial incentives. Second, our study adds to the literature on the role of institutional shareholders in shaping firm behavior, especially with respect to non-financial activities. Our findings suggest that institutional shareholders can use their influence over firms' management and shareholder proposals to avoid poor employee practices.

The paper proceeds as follows. Section 2 outlines relevant prior literature and develops the paper's main hypotheses. Section 3 outlines our empirical approach. Section 4 discusses results. Section 5 concludes.

2. Literature Review and Hypothesis Development

A large literature in accounting and finance examines the effect of institutional investors on capital market participants and firms' financial behavior. Ajinkya, Bhojraj, and Sengupta (2005) examine the influence of institutional investors on voluntary disclosure and find that higher institutional ownership is associated with more management forecasts. Rama-

lingegowda and Yu (2012) examine the influence of institutional ownership on accounting attributes and find that higher ownership by institutions, especially those that tend to monitor managers, is associated with more conservative financial reporting. Boone and White (2015) and Bird and Karolyi (2016) examine the effect of institutional ownership on firms' information and trading environment. They find that institutional ownership is positively associated with higher quantity and quality of management disclosure and more analyst following. Appel, Gormley, and Keim (2016) examine the effect of institutional ownership on corporate governance. They find that passive institutional investors influence firms' governance choices by appointing more independent directors, removing anti-takeover provisions, and requiring for more equal voting rights. Crane, Michenaud, and Weston (2015) find that institutional investors influence firms' payout policy by promoting higher dividends. Chen, Huang, Li, and Shevlin (2018) and Khan, Srinivasan, and Tan (2017) find that institutional investors improve firms' tax planning and increase tax avoidance.

All of the studies described above find that institutional investors have positive effects on firms' capital market behavior, which, in turn, increases institutional investors' return on investment. One notable exception is Burns, Kedia, and Lipson (2010), who find that firms with higher levels of institutional ownership are more likely to engage in financial misreporting. Their results are mainly driven by institutions with short investment horizons, as these institutions lack incentives to monitor portfolio firms' behavior; these institutions may also introduce additional short-term pressures to portfolio firms. One goal of the current paper is therefore to test whether these findings extend to the case of *non*-financial misconduct.

A growing literature has moved beyond capital market effects to study how institutional shareholders can affect firms' non-financial performance. Using firms' environmental and social (E&S) ratings as measures for firms' CSR performance, Dyck, Lins, Roth, and Wagner (2018) and Chen, Dong, and Lin (2019) find that institutional ownership leads to better CSR

performance. Dimson, Karakas, and Li (2015, 2018) provide evidence suggesting that institutional investors actively engage with firms to promote better ESG practices. Kim et al. (2019) find that shareholdings by local institutional investors, especially by local SRI funds and public pension funds, are negatively associated with the amount of toxic release by nearby facilities. Our study extends this emerging literature by focusing on the welfare of perhaps the most crucial non-investor stakeholders in a firm: its employees. There are advantages of using labor violations assessed by federal agencies as a measure for employee welfare relative to E&S ratings or employee surveys. Commercially available E&S ratings are often computed using data vendors' proprietary algorithms using public information extracted from firms' own filings and disclosures. To that end, these ratings likely reflect researchers' and data vendors' subjective and diverse views on what matters in CSR – and are often backward-looking rather than capturing *existing* corporate issues (*Financial Times*, 2018).³ The analysis conducted by CSR rating providers to produce these ratings “can appear arbitrary as it may treat similar situated companies differently and may even treat the same company differently over time for no clear reason”, as recently remarked by SEC Commissioner Hester M. Peirce.⁴ Employee surveys have their own problems as well; most notably, they are subject to voluntary response bias (i.e., the notion that dissatisfied employees are more likely to leave reviews than satisfied ones).⁵ In contrast to these approaches, we focus on explicit negative actions taken by firms, i.e., violations of federal labor laws. Our approach thus provides a more transparent, objective, and timely measure of a firm's corporate social (ir)responsibility towards its employees.

³ “Strong ESG policies are no protection against scandal” (Chris Flood, *FT*, February 2018; see <https://www.ft.com/content/08a3420e-05bf-11e8-9650-9c0ad2d7c5b5>)

⁴ Hester M. Peirce, “Scarlet Letters: Remarks before the American Enterprise Institute”, 18 June 2019. <https://www.sec.gov/news/speech/speech-peirce-061819>.

⁵ Voluntary response bias is a common issue in survey-based research. For an application to the workplace environment setting, see Ji et al. (2017) for academic evidence; or a January 2018 *New Yorker* article (Widcombe 2018; <https://www.newyorker.com/magazine/2018/01/22/improving-workplace-culture-one-review-at-a-time>) that details (among other things) the steps taken by a major employee surveyor – Glassdoor.com – to combat such bias, with varying levels of success.

Existing theory provides countervailing arguments on the potential effect of institutional ownership on firms' labor-related misconduct. On one hand, institutional shareholders, especially those with a long-term focus, may view other employees as part of the business. They believe that having satisfied employees enhances long-run shareholder value. Consistent with this view, Edmans (2011) finds that employee satisfaction is positively associated with long-term stock returns. Albuquerque, Koskinen, and Zhang (2018) find that firms' CSR activities increases firm value by reducing firms' systematic risk, especially among firms with high product differentiation. Moreover, although the monetary value of direct federal penalties for non-financial misconduct is relatively small, federal violations may also trigger lawsuits from employees, which could result in more material monetary and reputational damages. The above views thus predict a negative association between institutional ownership and the likelihood of labor-related misconduct.

On the other hand, institutional investors may view generous employee compensation packages as reflective of inefficiencies. If institutional investors are primarily focused on shareholders, they may promote practices to "trim the fat" and return cash to shareholders. Consistent with this view, Brav, Jiang, and Kim (2015) find that employees at firms subject to activist hedge fund interventions experience significant subsequent deteriorations in their overall compensation packages, including stagnation in work hours and wages as well as a decrease in fringe benefits. Popular press portrays institutional investors as working in "a microcosm of the struggle between a financial sector fixated on short-term returns and corporate managements who are trying to run profitable business while sharing some of the gains with their workers and communities" (Wall Street Journal, 2014).⁶ Furthermore, institutional investors

⁶ A vivid example, illustrated in the article, is Timken Corp. Timken was forced by Relational Investors, an investment firm that manages pension funds, to split into two firms, in the name of "unlocking value". Before being targeted by Relational Investors, workers at Timken Corp. were paid \$23 an hour, a rate that is "higher than at any of Timken's specially-steel rivals," and "workers receive the equivalent of another \$20 an hour in benefits." However, in the aftermath of the split, the new bearing firm's pension contributions dropped from nearly a third of cash flows to near zero, while its share of cash flows dedicated to share buybacks were quadrupled.

may also view CSR activities as a reflection of managerial agency problems or insider-initiated corporate philanthropy. Under this view, CSR reflects managers' own desire to engage in philanthropy rather than being motivated by stakeholders' demands (Benabou and Tirole, 2010). Consistent with this view, Masulis and Reza (2015) find that corporate donations are positively associated with CEOs' personal charity preferences and negatively associated with corporate governance quality. Cheng, Hong, and Shue (2016) find that higher incentive alignment between managers and shareholders and better corporate governance are both negatively associated with firms' CSR performance. As a result, while institutional investors may positively affect a firm's corporate governance, they may at the same time reduce its CSR activities if they regard the latter as value-destroying. As previously mentioned, since penalties for non-financial misconduct are immaterial relative to firm size, if institutional shareholders believe cutting investment in workplace safety or reducing employee benefits could increase firm value, they may force the firm to take such actions even if such practices violate federal regulations. This view thus predicts a positive association between institutional ownership and the likelihood of non-financial misconduct.

3. Research Design

3.1 Data

3.1.1 Labor-Related Misconduct

We obtain data on labor-related violations (for example, creating an unsafe workplace or underpaying workers) from a new, comprehensive database on federal agency enforcement actions called Violation Tracker published by the non-profit organization Good Jobs First. As of this writing, Violation Tracker is comprised of data on penalties for over 300,000 unique

violations of federal laws assessed by 47 U.S. federal agencies.⁷ We focus on labor-related violations, which primarily fall under the purview of nine distinct federal agencies. These nine agencies are the Employee Benefits Security Administration (EBSA); Equal Employment Opportunity Commission (EEOC); Federal Motor Carrier Safety Administration (FMCSA); Federal Railroad Administration (FRA); Department of Health & Human Services (HHS); Mine Safety & Health Administration (MSHA); National Labor Relations Board (NLRB); Occupational Safety & Health Administration (OSHA); Office of Workers' Compensation Programs (OWCP); and Wage & Hour Division (WHD). The majority of violations in our sample are assessed by OSHA, NLRB, and WHD; we refer the reader to Appendix B for further details.

The most common type of labor violation pertains to workplace safety; more than one-third of Violation Tracker is comprised of fines assessed by OSHA, and more than 13% of firm-years in our sample received fines from OSHA. Other common types of violations include labor and wage-related misconduct – reflected in enforcement actions by agencies such as the NLRB, WHD, and EEOC. Wage-related misconduct typically pertains to overtime pay or denying employees benefits they are entitled to. For example, hourly workers are required by law to be paid 1.5 times their normal hourly rate for overtime work; firms frequently either force employees to underreport such hours or do not follow the 1.5 multiplier even when the number of hours reported is accurate. Similarly, employees working more than 30 hours a week (at large firms) are classified by law as full-time and are thus required to be provided with access to healthcare and other benefits. Were a firm not to provide healthcare to such an employee (by, e.g., forcing that employee to claim (s)he had worked only 29 hours a week), it would be a clear violation of labor law assessed by the WHD. Another common example of wage theft is forced off-the-clock work. For example, retail workers are required by law to be

⁷ We refer the reader to Raghunandan (2019) for a more detailed description of the data available in the Violation Tracker database.

given 30 minutes of breaks during a shift of 5 or more hours; however, these workers may be forced to work through their legally-mandated breaks.

Prior literature often uses the number of workplace injuries (Cohn and Wardlaw, 2016; Caskey and Ozel, 2017) or OSHA violations (Filer and Golbe, 2003) to measure the quality of firms' labor practices. We expand the scope of this paper beyond workplace safety as a non-trivial share of labor-related misconduct comes in other forms (e.g., wage theft or the denial of collective bargaining rights where legally required). However, we are aware that not all poor labor practices necessarily trigger violations of federal labor laws.⁸ By focusing on these violations, we are likely capturing a lower bound on the extent of firms' misconduct toward their employees.

3.1.2 Other Data and Sample Selection

We obtain financial data (firm size, return on assets, leverage, and sales) from Compustat North America. We then hand-match Violation Tracker to Compustat at the parent company-year level by comparing firm names provided in Compustat with parent company names provided in Violation Tracker, using information on industry and location to resolve any ambiguity.⁹ We merge this data (by CUSIP) with institutional ownership data from Thomson Reuters' 13-F Holdings database as well as analyst forecast data from I/B/E/S. As our measure of analyst coverage, we use the log of the number of unique analysts providing earnings forecasts for the given fiscal year.

⁸ For example, firms may cut investments in workplace safety in ways that avoid explicit OSHA sanctions but nonetheless put their employees at risk (Cohn and Wardlaw, 2016).

⁹ Of note is the fact that Violation Tracker provides incorrect parent-subsidary matches for companies that have been involved in an acquisition at some point. Specifically, Violation Tracker lists the *current* (i.e., as of the last update) parent rather than the parent at the time of violation. For example, suppose that Subsidiary X committed a violation in 2010, and was owned by Parent A at the time. If Parent B were to acquire Parent A (and, as a result, Subsidiary X) in 2015, Violation Tracker would list Subsidiary X's parent company for the 2010 violation as Parent B – even though the parent company *at the time of violation* was actually Parent A. We manually correct all such instances (accounting for roughly 12% of the portion of Violation Tracker for which parent-subsidary linkages are available), merging Compustat data with violation data based on the parent company at the time a violation was committed.

3.1.3 Sample Composition and Descriptive Statistics

There are several data limitations that restrict the size of our sample. Foremost among these is a limitation on the scope of coverage in Violation Tracker that restricts our analysis to larger firms. In Violation Tracker, data are at the subsidiary company level; Good Jobs First only provides parent-subsidiary matches for roughly the largest 2,500 firms operating in the United States by market value. We therefore limit the sample to those firms that were among the largest 2,500 by market capitalization at the end of 2015. We use 2015 market capitalization to reflect the time at which Good Jobs First carried out the majority of its parent-subsidiary matching.

Beyond limiting our sample to large firms, we impose three additional restrictions that further reduce our sample size. First, we limit the sample to firms with available data from Compustat for at least two years of the sample period to allow for the inclusion of firm fixed effects. Second, we omit firms for which data on institutional ownership is not available through the Thomson Reuters 13F database during our sample period. Third, we omit firm-years that are backfilled by Compustat. This primarily occurs for firms that undergo a significant merger, as it is common for such firms to receive new security identifiers post-merger. Compustat often backfills observations pertaining to these new security identifiers; these backfilled financial data often do not reflect the predecessor firm, which Compustat often concurrently reports data for. This creates issues when merging with security and ownership data; to avoid contaminating our final sample, we drop such observations. Imposing these restrictions ultimately restricts our analysis to 2,107 unique firms representing 20,499 firm-years over the time period 2004-2016. We provide an overview of our sample construction in Table 1.

We provide descriptive statistics for our sample in Table 2. Panel A indicates that firms in our sample are large, and the fact that even the 10th percentile of ROA (Tobin's q) is positive (is greater than 1) indicates that our sample firms are generally profitable. Moreover, perhaps

because we focus on large and generally stable firms, the median firm in our sample has a sales growth rate of 11.7% and relatively low leverage, i.e., a debt-to-equity ratio of 25.1%. The mean (median) firm in our sample is covered by 9.4 (7) distinct analysts making earnings forecasts, indicating high capital market interest in the firm's financial performance. This panel also indicates that 19.0% of firm-years in our sample incur at least one violation of federal labor laws.

Table 2, Panel B reports the sample distribution by industry of the violating firm. Labor penalties occur relatively frequently in all industries except for financial services. Such a distribution supports our decision to consider a broad set of firms rather than limiting our analysis to a handful of industries; this, in turn, improves our study's external validity.

Table 2, Panel C presents average labor violation levels and institutional ownership over time. There does not appear to be a strong time trend for violations. There also does not appear to be a significant increase in the number of violations assessed subsequent to 2009. The latter result is somewhat surprising in light of the fact that the political party in power in the U.S. changed from Republican to Democrat in early 2009 with the election of Barack Obama to the presidency. As the U.S. President is tasked with appointing the heads of federal agencies, it might be expected that the more pro-regulation Democratic party would introduce regulators and regulations resulting in more frequent sanctions against corporate misconduct; however, it does not appear that this is the case. The similarity in violation frequency across the two presidential administrations also suggests that politically-driven changes in enforcement intensity are unlikely to explain our results. From this panel, we also see that institutional ownership is nearly monotonically increasing throughout our sample period. The year-over-year change in institutional ownership is positive for all years except during the 2007-09 financial crisis, and typically between 1 and 2 percent. This result is consistent with prior literature

that documents the increasing role that institutional investors are playing over time in publicly traded firms and underscores the importance of including time effects in our estimations.

3.2 Empirical Approach

One challenge we face is that institutional ownership is endogenous. To that end, it may be difficult to draw causal inferences from simply regressing an outcome variable on the percentage of a company’s shares held by institutional owners, because any results could be driven by inherent firm characteristics. Several recent papers on institutional ownership in the U.S. (e.g., Appel et al., 2016; Khan et al., 2017) get around this empirical limitation by using Russell index reconstitution as an identification method. Briefly, these papers rely on the fact that Russell reconstitutes its Russell 1000 and Russell 2000 indices each June based on end-of-May market capitalization. Many quasi-indexer institutional owners track the Russell index, and so reconstitution serves as a “shock” for firms just above or below the Russell 1000 and Russell 2000 index cutoffs. However, because of several changes introduced by Russell starting in 2007 (see Appel, Gormley, and Keim 2019), the sample period in all of those studies ends in 2006. Because the bulk of our violation data reflects a period subsequent to 2006, we are unable to use this approach. We therefore design our main tests using a linear probability framework with firm and year fixed effects. The basic regression that we estimate is

$$LABOR_{i,t+1} = \beta_0 + \beta_1 IO_{it} + \beta_2 CONTROLS_{it} + \gamma_i + \theta_t + \varepsilon_{it} \quad (1)$$

where $LABOR_{i,t+1}$ is an indicator that takes the value of 1 if firm i is sanctioned for violations of federal labor law (by any of the nine federal agencies described in Section 3.1.1) in year $t + 1$ and IO_{it} represents the proportion of firm’s shares held by institutional owners in year t . The variables γ_i and θ_t denote firm and year fixed effects, respectively. Our design also allows us to conduct additional analyses regarding the types of institutional ownership that may be driving our results (as in, e.g., Bena et al. (2017)). This would not be possible under an instrumental

variables specification, because an instrument for institutional ownership in general may not also be an appropriate instrument for ownership held by a specific type of investor (i.e., we would be unable to partition the IO_{it} variable). Nonetheless, as an additional robustness check, we do estimate an alternative specification using a two-stage least squares (2SLS). Following Aghion, van Reenen, and Zingales (2013), we use membership in the S&P 500 as an instrument for institutional ownership, i.e., we pair the second-stage regression above with the following first-stage regression:

$$IO_{it} = \alpha_0 + \alpha_1 S\&P500_{it} + \alpha_2 CONTROLS_{it} + \Gamma_i + \Theta_t + \varepsilon_{it} \quad (2)$$

where $S\&P500_{it}$ is an indicator for whether firm i was a member of the S&P 500 during year t . When adding a new stock to the S&P 500 is required, Standard and Poor's seeks to maintain an index that is representative of the broader cross-section of industries in the U.S. economy and one that is relatively stable; inclusion in the S&P 500 is therefore relatively orthogonal to firm performance (Aghion, van Reenen, and Zingales, 2013) and we thus view it as a valid instrument.

We include control variables based on prior studies of institutional ownership and shareholder activism (e.g., Brav et al., 2015; DeHaan et al., 2019) as well as studies on the determinants of workplace safety (e.g., Caskey and Ozel 2017; Cohn and Wardlaw 2016). These include firm size (measured using natural logarithm of total assets), return on assets (ROA), sales growth rate, leverage, level of analyst coverage (measured using the number of distinct analysts providing forecasts for that firm-year), and Tobin's q . Apart from analyst coverage, which is obtained from I/B/E/S, all control variables are constructed using data from Compustat. Our basic specification does not differentiate amongst types of institutional shareholders. However, in additional tests, we separate IO_{it} by type of institutional owner. We provide further detail on this in Section 4.2.

One notable feature of the violation data is that, by and large, penalties are statutory. Although most federal agencies lack the discretion to set penalty amounts for individual instances of misconduct, we still observe substantial variation in the size of penalties in our sample. This is because penalty amounts are primarily formulaic and capped. For example, when a company commits minimum wage over overtime pay violations, it is fined (as of 2017) \$1,925 per underpaid employee by the WHD (i.e. the punitive damages) regardless of whether the company underpaid its employees by \$0.10 per hour or by \$1.00 per hour. Similarly, OSHA classifies penalties as either “Other-than-Serious”, “Serious”, or “Repeat or Willful”. The penalty for a “Repeat or Willful” violation is set at 10 times the penalty for a “Serious” violation, even though the difference in actual impact for a “Serious” violation could be far greater. This contrasts with the case of financial misconduct, where securities regulators’ substantial discretion over the size of fines enables them to assess the impact of a misconduct on a case-by-case basis.¹⁰ Securities regulators also can (and do) take the extent of damages into account when choosing penalties; this was made explicit in the 2006 *Statement of the Securities and Exchange Commission Concerning Financial Penalties*.¹¹

Violation Tracker also contains some inconsistencies with respect to how additional requirements bundled with penalties are treated. For example, sanctions for both NLRB and WHD violations require employers to provide back pay to affected employees in addition to any punitive damages. However, while back pay is included in the total penalty amount reported in Violation Tracker (in addition to any fines) for NLRB violations, it is not included in the computation of WHD penalty amounts. The size of penalties in Violation Tracker is therefore a somewhat noisy measure of the economic impact of non-financial misconduct. Despite these limitations, however, the penalty amount reported in Violation Tracker is nonetheless a

¹⁰ The large discretion afforded to by securities regulators may sometimes result in their playing favorites; for example, Correia (2014) finds that politically connected firms are subject to less severe penalties for similar types of financial misconduct.

¹¹ See <https://www.sec.gov/news/press/2006-4.htm>

reasonable proxy for the severity of a violation; it is, for example, possible to use these reported amounts to argue that one NLRB violation is more serious than another. We therefore estimate whether institutional ownership can affect the size of penalties using the following regression:

$$\log(1 + PENALTIES_{i,t+1}) = \alpha_0 + \alpha_1 IO_{it} + \alpha_2 CONTROLS_{it} + \gamma_i + \theta_t + \varepsilon_{it} \quad (3)$$

where $PENALTIES_{i,t+1}$ denotes the dollar value of all penalties assessed at the firm-year level. Because the distribution of $PENALTIES_{i,t+1}$ (conditional on a penalty having occurred) is skewed, we use the log rather than the raw value. We estimate Equation (2) in two ways: (i) for the full sample, and (ii) on the sub-set of observations where a violation occurred (the “conditional sample”). The former approach pools violation observations with non-violation observations to directly estimate whether institutional ownership influences the expected fines portfolio companies pay. The latter approach tests whether, conditional on a violation having occurred, institutional ownership helps the firm pay lower fines.

4. Results

4.1 Baseline Specification

Results from estimating the baseline specification in Equations (1) and (2) are presented in Panels A and B of Table 5. Panel A presents result from our reduced-form fixed-effects specification, while Panel B presents results using S&P 500 index inclusion as an instrument for institutional ownership. Our key finding is that firms with higher levels of institutional ownership are less likely to engage in non-financial misconduct. This effect is economically meaningful: the coefficient of -0.0522 in Column (1) of Table 5 suggests that a one-standard-deviation increase in institutional ownership of 0.313 is associated with a 1.6 percentage point decrease in the likelihood of a labor violation, i.e., an 8.6% decrease with respect to the sample mean of 19%. In light of recent literature documenting that labor-related misconduct can reflect responses to short-term financial pressures (Caskey and Ozel, 2017; Raghunandan, 2019), our

main result suggests institutional investors may reduce these pressures. We find similar results in Column (2), where we instead use penalty size (measured as the log of the dollar value of total fines assessed at the firm-year level, across agencies) as our dependent variable.

Our results using the instrumental variables specification in Panel B are substantially larger; these results imply that a one percentage point increase in institutional ownership would yield a 0.64 percentage point decrease in the likelihood of non-financial misconduct. Similarly, the coefficient estimate in Column 2 of Panel B is approximately 11 times larger than that in Column 2 of Panel A. While imperfect, these estimates provide (in our view) lower and upper bounds on the impact that institutional investors may have. The estimates from our instrumental variables specification may also reflect the fact that we do not limit the bandwidth around the cutoff point for inclusion in the S&P 500, in part because (unlike the Russell indices described in detail by prior research) S&P does not use a formal cutoff for inclusion in the index.

In terms of control variables, we find that firm size is positive and significant, implying that larger firms are more likely to commit labor violations and to face higher penalty amounts. The former finding could be a result of higher enforcement levels faced by larger firms: federal agencies with a limited budget may be more likely to target larger firms with more market power. For example, OSHA states on its webpage:

“OSHA cannot inspect all 7 million workplaces it covers each year. The agency seeks to focus its inspection resources on the most hazardous workplaces [...]”¹²

The relation between penalty size and firm size is consistent with the formulaic way in which federal agencies often determine the size of a penalty. If a large firm and a small firm both commit labor violations of equal severity, for example, the violation committed by the large firm is likely to affect more employees even if the proportion of employees affected in both firms is similar; to that end, the dollar value of a penalty amount may partially reflect firm

¹² This quotation is taken from the OSHA Inspection Fact Sheet, available online at https://www.osha.gov/Osh-Data/data_General_Facts/factsheet-inspections.pdf

size rather than the severity of misconduct. In untabulated additional analyses, we use the total number of employees rather than total assets as a proxy for firm size. Our results are qualitatively unchanged in all respects.

We find a positive association between labor violations and both sales growth and Tobin's Q, suggesting that high growth firms are more likely to commit labor violations, potentially to save costs during the high-growth phase. In contrast to prior studies (e.g., Cohn and Wardlaw, 2016; Filer and Golbe, 2003) we do not find a significant relation between leverage and the likelihood of labor-related misconduct. There are several reasons that this could be the case. First, our sample composition differs from theirs; because we limit our sample to large firms (for the reasons given in Section 3.1.3), the firms in our sample are relatively stable and have relatively low levels of leverage. It may, therefore, be the case that we do not have enough variation in the cross-section of firms that we consider to document a link between leverage and labor-related misconduct. Second, our lack of results concerning leverage could reflect the fact that, like Filer and Golbe (2003) but unlike Cohn and Wardlaw (2016), we consider violations rather than injuries. As Cohn and Wardlaw (2016) point out, leverage-constrained firms may cut spending in ways that are likely to increase injuries but do not increase the likelihood of workplace safety violations (e.g., cutting voluntary supplemental maintenance and only performing legal minimum). Third, financing constraints cause firms to take *reactive*, rather than *proactive* measures (i.e., a firm is not "forced" to meet a certain earnings threshold in the way that it may actively be forced to cut costs in response to financing constraints). Our results may simply reflect this distinction.

In Column (3) of both panels of Table 5, we again use the log dollar value of penalties as the dependent variable but instead use the subsample of firm-years in which at least one violation occurred (i.e., a conditional sample). In this specification, the coefficient on institu-

tional ownership is statistically insignificant. This result suggests that *conditional* on committing misconduct, institutional ownership appears to have no effect on penalty amounts. This again highlights the fact that penalty size is a noisy measure for the economic impact of non-financial misconduct, due to the legally-imposed disconnect between the severity of a violation and the associated penalty amount.

4.2 Types of Institutional Ownership

Our next tests consider differences across various types of institutional owners. We first classify institutional investors based on their time horizons, which we define based on their portfolio turnover. Relying on the logic that short-term institutions trade frequently, while long-term institutions hold trading positions unchanged for a considerable length of time, we identify long-term institutions as those having a low portfolio turnover (Gaspar, Massa, and Matos, 2005; Starks, Venkat, and Zhu, 2017). Specifically, we calculate each institution i 's churn ratio at quarter t as

$$CR_{it} = \frac{\sum_{j \in Q} |N_{j,i,t} P_{j,t} - N_{j,i,t-1} P_{j,t-1} - N_{j,i,t-1} \Delta P_{j,t}|}{\sum_{j \in Q} \frac{N_{j,i,t} P_{j,t} + N_{j,i,t-1} P_{j,t-1}}{2}}$$

where $P_{j,t}$ is the stock price of firm j at the end of quarter t , $\Delta P_{j,t}$ is the change of stock price from quarter $t-1$ to t for firm j , and $N_{j,i,t}$ is the number of firm j 's shares held by institution i at the end of quarter t . We then use each institution's average churn ratio over the past four consecutive quarters as its current churn ratio. Finally, we rank all institutions' average churn ratio at each quarter and separate institutions into terciles. An institution with bottom-tercile churn ratio is considered to be long-term focused (which approximately matches the 30th percentile cutoff used by Starks, Venkat, and Zhu (2017)). We define an institution with middle-tercile churn ratio institution as medium-term focus; and top-tercile churn ratio as short-term focused. Our sample firms have an average long-term-focused IO of 21.6%; an average medium-term

focused IO of 29.7%; and an average short-term focused IO of 14.8%. Table 6, Column (1) presents our results using this partition. We find that our main result in Table 5 appears to be driven by both long-term and short-term investors, but not by medium-term investors. While this result may be puzzling on the surface, it is consistent with the findings in Aghion, van Reenen, and Zingales (2013) and reflects the different ways in which investors can discipline a firm; either by having a significant voice in the firm (as a long-term or dedicated institution) or by having a strong exit option. In further support of this argument is Table 6, Column (2) in which we use the Bushee (1998) institutional ownership classification. We find that dedicated and transient ownership are associated with a lower likelihood of subsequent labor-related misconduct, while there is no significant relation between ownership by quasi-indexers and labor-related misconduct.

To further understand the results in Columns (1) and (2) of Table 6, we study the characteristics of institutional ownership that would be consistent with the arguments given for why long-term or dedicated institutional ownership is associated with lower subsequent labor-related misconduct. To do, we partition institutions based on the institution type. Prior studies suggest that independent institutions (relative to grey institutions) likely monitor management and exert influence over firms' decision-making processes (Chen, Harford, and Li, 2007; Ramalingegowda and Yu, 2012). Following Chen, Harford, and Li (2007), we classify investment companies, investment advisors, and public pension funds as independent institutions.¹³ Table 6, Column (3) presents results using this partition on institution type. We find that ownership by independent institutions reduces the likelihood of labor-related misconduct. Conversely, there appears to be no effect of ownership by grey institutions. Consistent with prior research,

¹³ Due to a mapping error, the institution type in Thomson Reuters' 13F dataset is inaccurate after 1998. We thus use the type definition compiled by Brian Bushee (<http://acct.wharton.upenn.edu/faculty/bushee/IIclass.html>).

this result suggests that institutions that are more likely to be actively involved in monitoring portfolio firms are able to induce greater positive changes in behavior.

4.3 Consequences of Violations: Why Do Institutional Investors Care?

We have established a link between institutional ownership and labor-related misconduct. Prior research suggests that our findings could be explained by a variety of factors. Investors may care about the revenue-related benefits of better treatment of employees, as satisfied employees may work harder and thus benefit shareholders (Edmans, 2011). Alternatively, investors may care about the costs of poor employee treatment; while the size of federal fines for labor misconduct means that such fines unlikely to serve as a monetary deterrent, mistreatment of employees may result in lawsuits – for which no such caps exist. While we cannot directly test the former explanation, we are able to test one potential cost of labor misconduct using data on employee lawsuits contained in *Violation Tracker*. We caution that there are limitations to using this data, namely (i) we only observe lawsuits filed in federal, but not state, courts; (ii) some companies force their employees to sign mandatory arbitration agreements, which mechanically prevents these employees from suing their employer (Estlund 2018); and (iii) we only observe successful lawsuits, not those that were dismissed. Nonetheless, the likelihood of lawsuit occurrence is non-trivial; we observe at least one successful employee lawsuit (i.e., a lawsuit in which damages were awarded, either by the jury or via a pretrial settlement) for approximately 13% of the firms in our sample.

Because of the nature of employee lawsuits – that they are typically brought, and won, for actions taken by the offending firm over the course of several years – our basic research design is not appropriate for assessing the relation between labor lawsuits and federal labor law violations. Moreover, several firms in our sample are subject to two or three employee class-action lawsuits, often initiated only a few years apart; it is unclear what effect, if any, the filing of the first lawsuit may have had on the likelihood of subsequent lawsuits being filed. We

therefore turn to a hazard model, treating the *first* employee lawsuit that we observe for each firm as the “failure”. We include firm-specific random effects in our hazard model, following Allison (2009) and Allison and Christakis (2006); moreover, because a hazard model relies on the proportional-odds assumption across time periods and includes a time trend variable, we cannot include year fixed effects.¹⁴ We do include industry fixed effects; this allows the base hazard rate (i.e., the baseline likelihood of a lawsuit) to vary by industry, and controls for any systematic inter-industry differences in potential lawsuit risk. The basic model that we estimate is thus a standard hazard model of the form

$$\log\left(\frac{P_{it}^{LAWSUIT}}{1 - P_{it}^{LAWSUIT}}\right) = \theta + INDUSTRY_{it} + \gamma \cdot t + \beta_1 \cdot LABOR_{it} + \beta_2 \cdot X_{it} + \varepsilon_{it}$$

where $P_{it}^{LAWSUIT}$ reflects the probability of a lawsuit against firm i being filed in year t and $LABOR_{it}$ is an indicator that takes the value of 1 if firm i commits a labor-related violation in year t . Because the behavior that can ultimately lead to lawsuits is typically financially motivated (i.e., by the desire to cut costs), we use a similar set of control variables to our baseline violation-prediction model described in Section 3.2.

Because we are focused in this section in the potential consequences of violations rather than the determinants of these violations, we apply a propensity score matching approach (with replacement) to ensure that we are properly measuring the effect of potential violations relative to non-violation control firms. We consider firms that commit at least one labor violation during our sample period as the treatment (violators) and match them to firms that never commit a violation during our sample period (non-violators). We match based on firm fundamentals in the year prior to a violator’s first observed violation and retain constant violator/non-violator

¹⁴ One concern is that the effect of a specific variable (or even across an entire industry) may be time-variant, which would violate the proportional-hazards assumption. To assess whether this is the case, in untabulated robustness checks we allow for coefficients to vary over time (i.e., we allow an interaction term between coefficients and the time variable); statistically significant interaction terms would suggest that the proportional hazards assumption is violated. However, we find no evidence that the proportional-hazards assumption is violated in our model.

matches throughout the entire sample period. In doing so, we are able to match 627 violation firms with 330 distinct non-violator firms for a total of 957 distinct firms spanning 13,812 firm-years (note that in the hazard model specification, a firm that is sued subsequently drops out of the sample).¹⁵ We match using all control variables from our main tests, including total institutional ownership. In addition, the WHD and OSHA provide on their websites lists of specific industries that they deem to have an inherently higher risk of labor violations due to the amount of low-paid labor required in those industries (retail, hospitality, etc.). We therefore include in our first-stage model an indicator variable for these industries.

We present results from our employee lawsuit analysis in Table 7. Column 1 presents results from a hazard model specification on the full sample (i.e., without any matching), using as the primary independent variable $LABOR_{it}$; Column (2) replicates Column (1), but with the propensity score matching design described above. Similarly, Column (3) present results from a hazard model specification using as the primary independent variable a modified indicator variable, $LABOR_50K_{it}$, which takes the value of 1 only if firm i receives more than \$50,000 in fines for labor violations in year t . Column (4) replicates Column (3), but on the propensity score-matched sample rather than the full sample. We find in both cases that the incidence of labor violations appears to predict an eventual labor-related lawsuit; the hazard ratios for the violation variables ranging from 1.91 to 2.19 in Column (2) suggest that firms that are penalized for violations of federal labor laws are approximately twice as likely to ultimately lose or be forced to settle an employee lawsuit, with potentially millions of dollars in damages, relative to firms that do not violate federal labor laws. This result supports the argument that a reduction in labor violations may potentially reflect institutional owners' desires to reduce exposure to potential future lawsuits against portfolio firms.

¹⁵ As an additional robustness check, we match without replacement. Our results are qualitatively unchanged.

4.4 Channels of Institutional Investor Influence

We have thus far documented that higher levels of institutional ownership appear to be related to a lower likelihood of engaging in labor-related misconduct; our evidence also suggests that one reason for this phenomenon may be the threat of significant lawsuits. In this section, we therefore attempt to identify the channel through which institutional owners may influence firms' labor-related misconduct. We focus on two channels: (i) influence over the board of directors and (ii) shareholder proposals.

4.4.1 Board Influence

Our first test, relating to institutional owners' potential influence over the board of directors, is based on cross-sectional variation in state-level corporate constituency statutes. Constituency statutes explicitly require directors of publicly traded firms to consider the interests of non-shareholder stakeholders (e.g., employees or customers) in deciding on strategic plans. Prior literature documents that the introduction of corporate constituency statutes can shift firm-level strategic priorities; for example, Flammer and Kacperczyk (2016) find an increase in corporate innovation and patenting for firms affected by such statutes.

As a result of corporate constituency statutes, boards of companies incorporated in constituency statute states have greater capability to take stakeholder-friendly strategic actions. Thus, if institutional investors care about non-financial misconduct, they are more likely to be able to convince the board to take actions to reduce non-financial misconduct – even if these actions result in lower shareholder value.¹⁶

Even though institutional ownership is associated with positive changes in corporate governance (Appel, Gormley, and Keim, 2016), we study constituency statutes rather than

¹⁶ Note that constituency statutes apply based on a company's incorporation state, not its headquarters state. For example, numerous companies are incorporated in Delaware but have their headquarters located elsewhere. Because Delaware does not have a constituency statute, these firms are not subject to constituency laws – even if the states where these firms' actual headquarters are located have constituency statutes.

more traditional measures of board effectiveness from the corporate governance literature. We do so because the link between traditional measures of governance, such as board independence, and non-financial misconduct is unclear. For example, in a financial reporting context, higher board independence is often thought of as indicative of better governance and monitoring capability (e.g., Beasley, 1996; Chen, Cheng, and Wang, 2015); the increased monitoring, in turn, deters financial misconduct. While it is possible that this deterrence effect applies to non-financial misconduct as well, there are two key reasons that the effect of board independence is ambiguous in the non-financial misconduct setting. First, directors' obligations vary; a director who only has fiduciary obligations toward shareholders may implicitly endorse harm to other stakeholders (i.e., non-financial misconduct) if the financial benefits outweigh potential penalties. Second, independent directors are not employees of the firm, and as such may have weaker incentive to ensure a positive work environment. Insider directors may therefore have stronger incentives to reduce non-financial misconduct.

We construct our stakeholder orientation tests by partitioning firms based on their state of incorporation. Prior to 2000, 33 states had introduced corporate constituency statutes. Texas introduced such a statute in 2006; no state has done so subsequent to 2006.¹⁷ We omit the 157 firm-years in our sample that are incorporated in Texas and partition the remaining firm-years based on whether their state of incorporation has corporate constituency laws. We then re-estimate Equation (1) separately for each of these two sub-samples. In addition, it could be the case that firms in constituency states and non-constituency states differ with respect to observed

¹⁷ Flammer and Kacperczyk use the staggered introduction of constituency statutes in the 1980s and 1990s to construct a staggered difference-in-differences test. Because we do not observe temporal variation in the presence of constituency statutes over our sample period (other than for a small set of firms incorporated in Texas), we cannot employ a difference-in-differences approach. Nonetheless, we view our approach as the most economically appropriate; a difference-in-differences specification assumes that firms' states of incorporation do not change subsequent to the introduction of the treatment. This assumption gives rise to a potential endogeneity concern, however; large firms can change their state of incorporation (most commonly switching from their headquarters state to Delaware) with relative ease.

covariates. To remove the possibility that this drives any potential finding regarding the presence of constituency statutes, we employ entropy balancing so that the first and second moments of all independent variables are equalized across firms with and without constituency statutes. Put another way, we obtain weights for non-constituency-state observations (which comprise roughly three-quarters of our sample) so that the weighted means and standard deviations for control variables in non-constituency statute states are equal to the unweighted means and standard deviations for control variables in constituency statute states. Further, industries frequently cluster within states. To ensure that our results are not driven by the fact that different industries may be more prevalent in different states, we also balance on firms' two-digit SIC codes. Pre-entropy balancing means for firms in states with and without constituency statutes are given in Columns (1) and (2), respectively, of Table 8; after entropy balancing, the (weighted) means for both subsamples are equal to those in Column (1).

Columns (3) and (4) present estimation results for the entropy-balanced sample. Because this test focuses on the potential influence of institutions via monitoring, we separate institutional ownership into independent (i.e., monitoring) and grey institutions. We find a link between independent institutional ownership and a reduction in subsequent labor violations only in states with constituency statutes, i.e., only in states where the board must by law consider the impact of company actions on non-shareholder stakeholders. The results in Table 8 thus support the argument that institutional investors affect firms' non-financial conduct via influencing the board of directors.

4.4.2 Shareholder Proposals

As another potential channel through which institutional investors may influence firms' labor practices, we consider shareholder proposals related to socially responsible investing (SRI) issues. It is difficult to draw a direct link from SRI proposals to subsequent violations (or

the lack thereof), particularly because very few SRI proposals actually pass. To that end, our main goal in this section is not to document a direct link between SRI proposals and violations. Rather, we seek to provide further evidence of the channel through which institutional investors may influence firms' labor practices. We cannot observe direct engagements between institutional investors and portfolio firms; nonetheless, in light of the fact that institutional investors increase support for governance-related shareholder proposals (Appel, Gormley, and Keim, 2016), if institutional investors care about firms' overall social performance, then we should see an increase in support for SRI-related shareholder proposals as well.

Table 9 presents results from three tests of institutional investors' influence on SRI related shareholder proposals. In Column (1), we estimate the number of SRI proposals being tabled at the annual general meeting (regardless of whether it passes, fails, or is withdrawn). We find that firms with higher institutional ownership have more SRI proposals, consistent with the idea that institutional investors (i) care about social issues and (ii) exert influence over social issues via shareholder proposals.

Columns (2) and (3) of Table 9 further support the idea that institutional investors influence social issues via shareholder proposals. In Column (2), we limit the sample to firm-years with SRI proposals and test whether institutional ownership affects the likelihood of a proposal being withdrawn. While we cannot observe the reasons for each withdrawal, withdrawals frequently reflect a settlement between the parties being reached prior to a vote taking place. We find that withdrawals are more likely to occur for higher levels of institutional ownership. Finally, in Column (3), we limit the sample to proposals that were actually voted on (i.e., not withdrawn or, at the firm's request, successfully omitted). While most proposals fail, we find weak evidence that institutional ownership is associated with an increase in the percentage of votes received by these proposals; we are limited in this test by a small sample

size (less than 1,000 firm-years have SRI proposals that are voted on). On the whole, the results in Table 9 suggest that institutional ownership is positively associated with both the number of SRI proposals being tabled and better outcomes when these proposals are tabled. This, in turn, supports the broader idea that institutional investors care about, and may take active steps to improve, their portfolio firms' labor practices.

5. Conclusion

Using a novel dataset that tracks penalties assessed by nine U.S. federal agencies for labor-related misconduct, we examine the effect of institutional investors on firms' propensities to engage in poor labor practices. We find that firms with higher levels of institutional ownership have a lower likelihood of receiving fines for violations of federal labor laws; to better understand why institutional investors would care, we show that while the penalties for labor-related misconduct itself are quite small, firms that violate labor laws are also more likely to face employee lawsuits in subsequent years. We further find that the results are stronger among institutional investors with long-term investment horizons and monitoring incentives; these institutions are more likely to care about their portfolio firms' long-term performance and exert influence over management. Our findings suggest that institutional investors view labor-related misconduct as value-destroying in the long run.

In additional analyses, we study potential channels through which institutional investors may influence firms' labor practices. Although our setting precludes using typical proxies for corporate governance, by exploiting differences in stakeholder orientation laws across states we find evidence consistent with institutional investors exerting influence over the board of directors. We also document that institutional ownership is positively associated with outcomes of SRI shareholder proposals, suggesting further that institutional investors appear to care about – and take steps to influence – their portfolio firms' non-financial performance.

Our findings expand the literature on institutional investors. Prior work typically focuses on the role of institutional investors in the capital markets; we instead provide evidence that institutional investors may have an impact on firms' labor practices as well. Moreover, prior studies that link ownership structure with CSR measures typically use black-box CSR or E&S ratings from data providers such as MSCI or RepRisk; it is difficult to ascertain how various aspects of a firm's behavior may contribute to these ratings. Our study avoids this issue by using an objective measure of non-financial performance that reflects specific firm actions; we are therefore able to better identify potential effects of institutional ownership.

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Appendix A: Variable Definitions

We define below each of the variables used in our regression specifications.

<i>Variable name</i>	<i>Definition</i>
<i>Non-Financial Misconduct Variables</i>	
Labor violation	Indicator for whether the firm was fined for violations of federal labor laws. This variable is equal to 1 if the firm was fined by any of the following US federal agencies: <ul style="list-style-type: none"> • Employee Benefits Security Administration • Equal Employment Opportunity Commission • Federal Motor Carrier Safety Administration • Federal Railroad Administration • Department of Health & Human Services • Mine Safety & Health Administration • National Labor Relations Board • Occupational Safety & Health Administration • Office of Workers' Compensation Programs • Wage & Hour Division
Employee lawsuit	Indicator for whether an employee lawsuit, that ultimately resulted in either damages being awarded or a monetary settlement, was initiated against the firm. These lawsuits are typically, but not always, class-action lawsuits. We use the initiation date rather than the ultimate settlement date (e.g., if a lawsuit was filed in 2010 and resolved in 2013, the employee lawsuit indicator would reflect the 2010 lawsuit filing).
<i>Institutional Ownership Variables</i>	
Institutional Ownership (IO)	Proportion of shares held by institutional owners
Long-Term IO	Proportion of shares held by institutional investors with lower-than-median portfolio turnover, as measured by churn ratio
Short-Term IO	Proportion of shares held by institutional investors with higher-than-median portfolio turnover, as measured by churn ratio
Independent IO	Proportion of shares held by mutual fund managers and investment advisers
Grey IO	Proportion of shares held by institutions other than mutual fund managers or investment advisers
Independent & Long-Term IO	Proportion of shares held by independent, low-churn institutions
Other IO	Proportion of shares held by high-churn <i>or</i> non-independent institutions
<i>Control Variables</i>	
Log assets	Log of firm's fiscal year-end assets
Sales growth rate	Percentage change in year-over-year sales
ROA	Return on assets
Leverage	Debt to equity ratio
Analyst Coverage	Log of 1 + the number of unique analysts providing forecasts for the firm
Tobin's q	$(\text{Total Assets} + \text{Market Value} - \text{Book Value}) / \text{Total Assets}$
<i>Channel Variables</i>	
Constituency State	Indicator for whether the firm's state of incorporation has constituency laws (laws which explicitly require firms to consider stakeholders other than shareholders)
Number of SRI Proposals	Number of SRI ("socially responsible investing") shareholder proposals tabled in a given year, regardless of whether passed/failed/omitted/withdrawn.
SRI Proposal Withdrawn	Indicator for whether a shareholder proposal was withdrawn before voting
SRI Proposal Vote %	Percentage of votes in favor of a shareholder proposal
S&P 500	Indicator for whether firm was a member of the S&P 500 in the given year

Appendix B: List of federal agencies that oversee labor practices

We construct our main labor violation variable based on the agency issuing those violations. We provide below a list of nine federal agencies assessing labor-related misconduct for which Violation Tracker data is available.

Agency Code	Agency Full Name	Frequency (% of firm-years with at least one violation assessed by agency)	Median penalty amount (in thousands of dollars)
EBSA	Employee Benefits Security Administration	0.17%	10
EEOC	Equal Employment Opportunity Commission	0.90%	120
FMCSA	Federal Motor Carrier Safety Administration	0.43%	10
FRA	Federal Railroad Administration	2.18%	8
HHSOIG	Department of Health & Human Services Office of Inspector General	0.05%	353
MSHA	Mine Safety & Health Administration	1.34%	8
NLRB	National Labor Relations Board	2.50%	23
OSHA	Occupational Safety & Health Administration	12.52%	9
WHD	Department of Labor Wage and Hour Division	3.39%	19

TABLES

TABLE 1: Sample Construction

This table details how we construct our sample. We note that our tests focus on large firms; this is because the Violation Tracker database provides data at the subsidiary company level. Violation Tracker provides parent-subsidiary matching only for (roughly) the largest 2500-3000 firms. A large portion of the parent-subsidiary matching was done in 2016, using (among other sources) a ranking of firm size by relative market value based on 2015 year-end rankings. We therefore consider only firms that were ranked in the top 2500 by market capitalization in 2015.

	Firm-Years	
	Obs. Dropped	Obs. Remaining
Start: All firm-years for 2,500 largest firms by market value as of 2016, with at least 2 years of data available between 2004 and 2016		27,600
Less: Firms with backfilled/inconsistent Compustat data	(3,439)	24,161
Less: Firms missing from Thomson Reuters 13F Holdings data	(1,070)	23,091
Less: Firms missing ROA data	(1,908)	21,183
Less: Firms missing sales data	(475)	20,708
Less: Firms missing other Compustat data	(209)	20,499

TABLE 2: Descriptive Statistics

This table presents descriptive statistics (mean, median, standard deviation, 10th and 90th percentiles) for all variables in our final estimation sample. Panel A presents descriptive statistics for the overall sample. Panel B presents year-by-year descriptive statistics for our main non-financial misconduct variables and for institutional ownership; in Panel B, the violation variables are one-year ahead to mirror our main regression specifications (i.e., the row pertaining to 2004 reflects 2004 institutional ownership and 2005 non-financial misconduct). Although we use two-digit SIC industry-by-year fixed effects in our changes specifications, for brevity we tabulate this panel by major SIC industry grouping rather than by individual two-digit SIC codes. Panel C presents descriptive statistics on violations and institutional ownership by industry. For consistency with our main regressions, we use one-year-ahead incidences of penalties.

Panel A: Descriptive statistics

Variable	<i>N</i>	Mean	Median	Std. Dev	10 th %ile	90 th %ile
<i>Non-Financial Misconduct</i>						
Labor violation indicator (t+1)	20,499	0.190	0.000	0.392	0.000	1.000
Log labor violation \$ amount (t+1), full sample	20,499	1.955	0	4.090	0	9.896
Log labor violation \$ amount (t+1), conditional on labor violation occurring	3,304	10.300	9.999	1.472	8.732	12.197
<i>Institutional Ownership</i>						
Institutional Ownership (IO)	20,499	0.660	0.763	0.313	0.080	0.993
Long-term IO	20,499	0.351	0.376	0.197	0.029	0.591
Short-term IO	20,499	0.304	0.308	0.180	0.030	0.537
Independent IO	20,499	0.482	0.537	0.244	0.060	0.758
Grey IO	20,499	0.149	0.151	0.098	0.009	0.269
Independent & Long-term IO	20,499	0.243	0.239	0.155	0.019	0.445
Other IO	20,499	0.387	0.408	0.211	0.041	0.649
<i>Control Variables</i>						
Log assets	20,499	8.244	8.002	1.662	6.392	10.454
Sales growth rate	20,499	0.117	0.071	0.290	-0.109	0.360
ROA	20,499	0.133	0.124	0.106	0.022	0.262
Leverage	20,499	0.249	0.221	0.199	0.005	0.515
Analyst Coverage	20,499	2.034	2.079	0.849	0.693	3.091
Tobin's Q	20,499	1.830	1.450	1.124	0.998	3.128
<i>Channel Variables</i>						
Labor Lawsuit Initiated	20,499	0.024	0	0.154	0	0
Constituency State	20,331	0.259	0	0.438	0	1
Number of SRI Shareholder Proposals	20,499	0.145	0	0.632	0	0
SRI Proposal Withdrawn	1,719	0.478	0	0.500	0	1
SRI Proposal Vote Fraction*	968	0.210	0.218	0.155	0.039	0.398
S&P 500 Indicator	20,499	0.220	0	0.415	0	1

Panel B: Violation frequency by industry

Industry	Labor penalties (t+1)	% Institutional Ownership (t)
Agriculture (SIC codes 1-9)	0.157	0.736
Mining (SIC codes 10-14)	0.387	0.830
Construction (SIC codes 15-17)	0.036	0.564
Manufacturing (SIC codes 20-39)	0.232	0.700
Transportation/Utilities (SIC codes 40-49)	0.259	0.615
Wholesale Trade (SIC codes 50-51)	0.337	0.751
Retail Trade (SIC codes 52-59)	0.119	0.727
Finance (SIC codes 60-69)	0.223	0.544
Services (SIC codes 70-89)	0.224	0.760
Overall	0.190	0.660

Panel C: Violation frequency and institutional ownership by year

Year	Labor penalties (t+1)	% Institutional Ownership (t)
2004	0.167	0.555
2005	0.174	0.591
2006	0.183	0.624
2007	0.165	0.661
2008	0.184	0.642
2009	0.200	0.641
2010	0.202	0.648
2011	0.211	0.670
2012	0.188	0.675
2013	0.186	0.692
2014	0.184	0.698
2015	0.194	0.706
2016	0.216	0.713
Overall	0.190	0.660

TABLE 3: Correlations

This table presents correlations for the variables used in our main regression specifications. Pearson correlations are above the diagonal, while Spearman correlations are below the diagonal.

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]	[13]	[14]	[15]	[16]
[1] Labor violation (t+1)		0.10	0.12	0.07	0.03	0.04	0.04	0.10	0.07	0.14	0.18	-0.06	0.07	0.09	0.15	-0.05
[2] Institutional Ownership (IO)	0.07		0.73	0.86	0.66	0.66	0.31	0.87	0.96	0.67	-0.20	-0.05	0.07	-0.03	0.48	0.12
[3] Long-term IO	0.13	0.67		0.42	0.20	0.31	0.49	0.60	0.67	0.52	-0.04	-0.16	0.00	-0.02	0.33	0.00
[4] Medium-term IO	0.06	0.82	0.41		0.43	0.49	0.16	0.83	0.84	0.55	-0.18	0.00	0.08	-0.04	0.45	0.15
[5] Short-term IO	0.05	0.68	0.28	0.49		0.77	0.03	0.50	0.64	0.44	-0.25	0.08	0.08	0.01	0.28	0.11
[6] Transient IO	0.05	0.67	0.38	0.54	0.80		0.10	0.39	0.67	0.37	-0.22	0.02	0.05	0.01	0.30	0.09
[7] Dedicated IO	0.09	0.37	0.51	0.24	0.14	0.21		-0.02	0.32	0.13	0.01	-0.04	-0.05	0.13	0.09	0.02
[8] Quasi-indexer IO	0.09	0.80	0.58	0.81	0.53	0.41	0.08		0.83	0.70	-0.17	-0.06	0.09	-0.10	0.44	0.10
[9] Independent IO	0.04	0.93	0.62	0.80	0.66	0.69	0.35	0.76		0.47	-0.25	-0.03	0.06	-0.02	0.42	0.12
[10] Grey IO	0.15	0.61	0.53	0.55	0.51	0.44	0.18	0.71	0.43		0.00	-0.08	0.08	-0.07	0.40	0.05
[11] Log assets	0.20	-0.17	0.00	-0.14	-0.21	-0.19	0.13	-0.13	-0.23	0.04		-0.14	-0.10	0.09	0.24	-0.27
[12] Sales growth rate	-0.06	0.00	-0.17	0.06	0.10	0.02	-0.10	0.01	0.01	-0.03	-0.16		0.16	-0.03	-0.01	0.20
[13] ROA	0.09	0.13	0.03	0.14	0.12	0.09	-0.01	0.15	0.10	0.12	-0.13	0.26		0.01	0.18	0.41
[14] Leverage	0.13	-0.01	0.00	-0.04	-0.01	-0.01	0.11	-0.10	-0.01	-0.06	0.17	-0.09	0.01		-0.05	-0.11
[15] Analyst Coverage	0.16	0.38	0.33	0.41	0.30	0.31	0.25	0.39	0.33	0.42	0.32	0.04	0.22	-0.03		0.26
[16] Tobin's q	0.01	0.18	0.06	0.21	0.14	0.13	0.12	0.16	0.17	0.13	-0.27	0.24	0.61	-0.11	0.32	

TABLE 4: Univariate Analysis

This table presents univariate differences for all variables used in our main regressions for violation and non-violation firm-years for our sample. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Variable	Non-Violation Firm-Year Mean	Violation Firm-Year Mean	Difference	t-statistic
Institutional Ownership (IO)	0.64	0.72	0.08	(7.58)
Long-Term IO	0.205	0.248	0.043	(10.41)
Medium-Term IO	0.29	0.318	0.028	(5.46)
Short-Term IO	0.146	0.155	0.009	(2.75)
Transient IO	0.134	0.143	0.009	(3.08)
Dedicated IO	0.042	0.051	0.009	(4.37)
Quasi-indexer IO	0.419	0.476	0.057	(7.61)
Independent IO	0.472	0.512	0.04	(4.99)
Grey IO	0.14	0.176	0.036	(10.93)
Log assets	7.998	8.98	0.982	(14.91)
Sales growth rate	0.13	0.08	-0.05	(-10.17)
ROA	0.129	0.146	0.017	(5.59)
Leverage	0.238	0.282	0.044	(7.18)
Analyst Coverage	1.941	2.311	0.37	(12.85)
Tobin's Q	1.873	1.702	-0.171	(-5.45)

TABLE 5: Main Regression Specification

This table presents results from our main regression specification, testing the effect of institutional ownership on firms' proclivities to commit labor-related misconduct. Panel A presents our base specification, while Panel B presents results from a two-stage model, using inclusion in the S&P 500 as a first-stage instrument for institutional ownership. In both panels, Column (1) uses as the dependent variable an indicator for whether the firm was fined for labor-related misconduct in year $t+1$. Column (2) instead uses as the dependent variable the natural logarithm of one plus the dollar value of penalties (again, measured in year $t+1$) as the dependent variable. The specifications in Columns (1) and (2) use firm and year fixed effects. Column (3) uses the same dependent variable as Column (2) but limits the sample to firm-years with at least one violation, providing a test of whether institutional ownership can help mitigate the effect of violations *conditional* on violations having occurred; because this test is cross-sectional in nature we employ year fixed effects but not firm fixed effects. In Panel B, Column (0) presents the first-stage regression from the instrumental variables approach. Please refer to Appendix A for more information on how we construct these classifications. All standard errors are clustered by firm. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Main Specification

<i>Dependent Variable:</i>	Labor Penalties	Labor Penalties	Labor Penalties
<i>Dep. Variable type:</i>	Indicator	\$ amount	\$ amount
<i>Subsample</i>	Full sample	Full sample	Violation obs.
Variable	(1)	(2)	(3)
Institutional ownership	-0.0522*** [-3.08]	-0.5306*** [-3.07]	-0.0621 [-0.31]
Log assets	0.0251*** [3.73]	0.2959*** [4.24]	0.2037*** [6.79]
Sales growth	0.0152** [2.26]	0.1609** [2.35]	0.1727 [1.49]
ROA	0.0342 [1.16]	0.4070 [1.36]	0.9880* [1.69]
Leverage	-0.0125 [-0.52]	-0.1008 [-0.42]	-0.3335** [-1.99]
Analyst coverage	0.0126* [1.92]	0.1238* [1.82]	0.0453 [0.67]
Tobin's q	0.0114*** [3.16]	0.1155*** [3.21]	-0.1003** [-2.26]
Firm Fixed Effects	Yes	Yes	No
Year Fixed Effects	Yes	Yes	Yes
Observations	20,499	20,499	3,892
Adj. R-squared	0.4555	0.4869	0.0621

Panel B: Instrumental Variables Specification

<i>Dependent Variable:</i>	Institutional Ownership	Labor Penalties	Labor Penalties	Labor Penalties
<i>Dep. Variable type:</i>	Fraction (0 to 1)	Indicator	\$ amount	\$ amount
<i>Subsample</i>	Full sample	Full sample	Full sample	Violation obs.
<i>Variable</i>	(First Stage)	(1)	(2)	(3)
Institutional ownership		-0.6366*	-7.0615**	0.8911
		[-1.96]	[-2.06]	[0.94]
Log assets	0.0364***	0.0436***	0.5031***	0.2866***
	[5.55]	[3.28]	[3.59]	[3.78]
Sales growth	-0.0137**	0.0080	0.0804	0.1402
	[-2.49]	[0.95]	[0.93]	[1.14]
ROA	0.0247	0.0489	0.5721*	1.0562*
	[1.02]	[1.49]	[1.68]	[1.66]
Leverage	-0.0596***	-0.0452	-0.4662	-0.2291
	[-2.96]	[-1.41]	[-1.42]	[-1.03]
Analyst coverage	0.0820***	0.0605**	0.6585**	-0.1452
	[11.94]	[2.17]	[2.23]	[-0.72]
Tobin's q	0.0020	0.0120***	0.1222***	-0.0416
	[0.72]	[3.05]	[3.05]	[-0.61]
S&P 500 Indicator	-0.0576***			
	[-4.89]			
Firm Fixed Effects	Yes	Yes	Yes	No
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	20,499	20,499	20,499	3,892
F-statistic for weak instrument test	23.93			

TABLE 6: Types of Institutional Ownership

This table separates out different types of institutional ownership. Column (1) considers institutions based on their “time horizon” (long, medium or short term); these classifications are constructed using firms’ churn ratios. Column (2) instead considers institutions based on the Bushee IO classification (transient, dedicated, or quasi-indexer). Column (3) considers institutions by type: independent vs. grey. In all cases, the dependent variable is an indicator for whether a firm received at least one federal penalty for labor violations in year $t+1$. Please refer to Appendix A for more information on how we construct these classifications. We include firm and year fixed effects in both panels. All standard errors are clustered by firm. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

<i>IO variable:</i>	By time horizon (1)	By Bushee classification (2)	Independent vs. Grey (3)
Long-Term IO	-0.0802** [-2.55]		
Medium-Term IO	-0.0327 [-1.39]		
Short-Term IO	-0.0615** [-1.96]		
Transient IO		-0.0970*** [-2.79]	
Dedicated IO		-0.1119** [-2.11]	
Quasi-Indexer IO		-0.0210 [-0.98]	
Independent IO			-0.0540*** [-2.62]
Grey IO			-0.0367 [-0.75]
Log assets	0.0245*** [3.66]	0.0240*** [3.58]	0.0249*** [3.69]
Sales growth	0.0151** [2.25]	0.0160** [2.38]	0.0150** [2.23]
ROA	0.0359 [1.23]	0.0370 [1.26]	0.0340 [1.15]
Leverage	-0.0125 [-0.52]	-0.0104 [-0.44]	-0.0121 [-0.51]
Analyst coverage	0.0125* [1.90]	0.0117* [1.78]	0.0124* [1.88]
Tobin's q	0.0111*** [3.10]	0.0118*** [3.27]	0.0114*** [3.16]
Observations	20,499	20,499	20,499
Adj. R^2	0.4555	0.4556	0.4554

TABLE 7: Employee Lawsuits

This presents results from estimating the link between federal labor violations and subsequent lawsuits. For the reasons discussed in Section 4.3, we employ a Cox proportional-hazards model with industry fixed effects (i.e., allowing for the baseline hazard rate to vary by SIC two-digit industry). The outcome (“failure”) variable is an indicator that takes the value of 1 if an employee lawsuit is initiated against the firm in year t (even though that lawsuit typically does not resolve for several years). Columns (1) and (2) present results where the primary independent variable of interest is a dummy variable for all labor-related lawsuits; Column (1) uses the full sample while Column (2) uses a propensity score-matched sample. Columns (3) and (4) refine the analysis by considering only “serious” labor violation firm-years (i.e., those with more than \$50,000 in fines). We present coefficients, rather than hazard ratios, to highlight the meaning of the signs of coefficients; the hazard ratio can be obtained by exponentiating the coefficient. Please refer to Appendix A for variable definitions. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

<i>Subsample:</i>	Full (1)	Matched (2)	Full (3)	Matched (4)
Labor violation	0.9276*** [4.71]	0.9734*** [4.86]		
Serious labor violation			1.0552*** [3.80]	1.1438*** [3.89]
Log assets	0.4088*** [6.91]	0.4353*** [7.48]	0.4353*** [7.48]	0.4050*** [6.75]
Sales growth	-1.1101** [-2.36]	-1.1246** [-2.44]	-1.1246** [-2.44]	-0.3430 [-0.76]
ROA	1.6875 [1.42]	1.7413 [1.50]	1.7413 [1.50]	-1.7020 [-1.31]
Leverage	-1.9800*** [-3.50]	-2.0238*** [-3.63]	-2.0238*** [-3.63]	-1.5187*** [-2.86]
Tobin's q	-0.0390 [-0.35]	-0.0352 [-0.33]	-0.0352 [-0.33]	0.0556 [0.46]
Observations	19,530	15,604	19,530	15,604
Log likelihood	-471.8	-536.3	-476.2	-540.9

TABLE 8: By State Stakeholder Orientation Laws

This table partitions the sample based on whether firms' state of incorporation had stakeholder orientation laws in place. We omit firms incorporated in Texas, because Texas introduced a stakeholder orientation law in 2006, i.e., during our sample period; all other states in our sample either implemented a stakeholder orientation law prior to 1999 or did not have such a law as of 2016. Columns (1) and (2) present the pre-entropy balancing means of all independent variables in states with and without constituency statutes; the t-statistic of the difference is beneath. Columns (3) and (4) present regression results from partitioning the sample based on whether firms' state of incorporation has a constituency statute. The dependent variable in Columns (3) and (4) is an indicator variable for whether a firm had at least one labor violation. All specifications include firm and year fixed effects. All standard errors are clustered by firm. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. For Columns (1) and (2), the relevant number of asterisks (*) denote statistically significant differences from the values in Column (2)

<i>Output type</i>	Mean	Mean	Coefficient	Coefficient
<i>Subsample</i>	(Has Law)	(No Law)	(Has Law)	(No Law).
Variable	(1)	(2)	(3)	(4)
Independent IO	0.5012***	0.4735	-0.1022**	-0.0408
	[7.11]		[-2.05]	[-1.23]
Grey IO	0.1754***	0.1392	-0.1017	-0.1037
	[23.26]		[-0.93]	[-1.42]
Log assets	8.1351***	8.2850	0.0493***	0.0276***
	[-5.73]		[2.74]	[2.65]
Sales growth	0.0873***	0.1277	0.0068	0.0076
	[-8.71]		[0.44]	[0.63]
ROA	0.1231***	0.1366	0.0484	0.0478
	[-7.95]		[0.58]	[1.05]
Leverage	0.2300***	0.2554	-0.0527	0.0076
	[-7.97]		[-0.85]	[0.21]
Analyst coverage	1.9917***	2.0473	0.0093	0.0168
	[-4.08]		[0.72]	[1.56]
Tobin's q	1.6637***	1.8937	-0.0017	0.0136***
	[-12.81]		[-0.17]	[2.69]
Firm Fixed Effects	-	-	Y	Y
Year Fixed Effects	-	-	Y	Y
Observations	5,266	15,060	5,266	15,060
R ²	-	-	0.4277	0.4636

TABLE 9: Shareholder Proposals

This table presents tests that verify one potential channel through which institutional owners may exert influence over a firm's decision-making process: shareholder proposals. In column (1), the dependent variable is the number of shareholder proposals (no matter the outcome) the firm is subject to in year $t + 1$. This column uses firm and year fixed effects. In columns (2) and (3) we focus on subsamples in which SRI-related shareholder proposals occurred. In column (2) the dependent variable is an indicator for whether at least one proposal was withdrawn (indicating a potential settlement). In column (3) we consider only those observations where a vote occurred; our dependent variable is the highest fraction of votes received in favor of the SRI proposals voted on in that year (i.e., 0.4 reflects 40% votes in favor). Because Columns (2) and (3) reflect a cross-sectional rather than panel approach, we do not impose firm fixed effects. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

<i>Dependent Variable:</i>	Number of of SRI Proposals	SRI Proposal Withdrawn	SRI Proposal Vote Fraction
<i>Subsample</i>	(1) Full	(2) Proposal firm-years	(3) Proposal firm-years
Institutional ownership	0.1040*** [2.72]	0.2553*** [3.19]	0.0724 [1.57]
Log assets	0.0827*** [4.33]	-0.0176 [-1.64]	-0.8457** [-2.01]
Sales growth	-0.0250** [-2.46]	-0.0610 [-0.82]	2.5606 [0.96]
ROA	-0.1247* [-1.94]	0.3536** [2.19]	-6.4809 [-1.06]
Leverage	-0.0816* [-1.77]	0.0409 [0.54]	-9.7428*** [-3.75]
Analyst coverage	0.0170* [1.87]	0.0396 [1.52]	0.8391 [0.70]
Tobin's q	0.0120* [1.72]	-0.0318** [-2.09]	-2.7737*** [-5.03]
Firm Fixed Effects	Yes	No	No
Year Fixed Effects	Yes	Yes	Yes
Observations	19,195	1,719	968
Adj. R-squared	0.572	0.012	0.083