Review of Accounting Studies (2021) 26:867–905
https://doi.org/10.1007/s11142-021-09602-y

Financial misconduct and employee mistreatment: Evidence from wage theft

Aneesh Raghunandan[1]

Accepted: 3 May 2021 / Published online: 15 June 2021

Abstract
I examine the relation between firms' financial conduct and wage theft. Wage theft represents the single largest form of theft committed in the United States and primarily affects firms' most vulnerable employees. I show that wage theft is more prevalent (i) when firms just meet or beat earnings targets and (ii) when executives' personal liability for wage theft decreases. Wage theft precedes financial misconduct while the theft is undetected, but once firms are caught engaging in wage theft they are more likely to shift to engaging in financial misconduct. My findings highlight an economically meaningful yet previously undocumented way in which firms' financial incentives relate to employee treatment.

Keywords Wage theft · Real earnings management · Financial misconduct · Labor practices

JEL Classification J31 · J83 · K31 · M14 · M41

1 Introduction

In response to financial pressures, firms frequently engage in real activities management (Dechow et al. 2010) or outright misconduct (Chu et al. 2019). Prior studies also document a substitute relation between real activities management and accruals management (e.g., Roychowdhury 2006; Zang 2012). In this paper, I examine an economically meaningful but previously unexplored form of real activities management: corporate wage theft. I ask two main questions: (i) does wage theft arise from similar financial incentives to existing real activities management measures and, if so, (ii) are firms less likely to engage in financial misconduct when they engage in higher levels of wage theft?

✉ Aneesh Raghunandan
a.raghunandan@lse.ac.uk

1 London School of Economics, London, UK

The term "wage theft" encompasses several actions that firms take to deny employees their rightful pay or benefits. Common violations include not paying employees for working overtime or forcing them to underreport the number of hours worked. Although the violations that encompass wage theft typically affect lower-paid and non-salaried workers, the aggregate economic effects of wage theft are material. For example, in 2017 the Economic Policy Institute (EPI) estimated that workers lose more than $8 billion per year to wage theft, representing nearly one-quarter of affected workers' earned wages; this $8 billion represents a direct boost to employers' bottom lines. These figures are unlikely to reflect the actions of a handful of "bad actors," as the EPI estimated that 17% of low-wage employees suffer from wage theft by their employers.[1] Yet, despite being the single largest form of theft committed in the United States (Bobo 2011), wage theft has received little prior attention in relation to other aspects of corporate conduct.

Although it is illegal, wage theft may be financially attractive to employers because of its relatively low direct costs. Unlike the Securities and Exchange Commission (SEC) in the securities fraud setting, federal law caps the fines that the US Department of Labor's Wage & Hour Division (WHD) can charge for wage violations. The indirect firm-level costs of wage theft are also constrained in many cases because of contract provisions that disallow certain employees from bringing wage theft lawsuits against their employer and instead force the employees into confidential arbitration.[2] The expected gains from wage theft may therefore substantially exceed the expected costs for large public firms. In addition, wage theft typically affects low-wage workers, who may be less aware of their rights in the workplace and therefore less likely to complain. Because wage theft is only observable to management and to directly affected employees before detection (but not to third parties), employees' lower levels of awareness further reduce the expected cost of noncompliance for firms.[3] In contrast, many other forms of operational misconduct are externally observable. For example, environmental violations may be detected by concerned citizens near a facility, and unusual levels of production or advertising may raise questions from institutional investors or analysts. Given the immediate savings from and low direct costs of wage theft, I expect wage theft to be higher when firms' or managers' incentives for managing earnings are higher.

I obtain wage violation data from Violation Tracker, compiled by the non-profit organization Good Jobs First. Although Violation Tracker contains comprehensive

---

[1] See https://www.epi.org/publication/employers-steal-billions-from-workers-paychecks-each-year/

[2] There is no comprehensive publicly available data on which firms impose arbitration clauses on their employees or on arbitration settlements, so it is not possible to measure the extent to which lawsuit risk is mitigated, either in total or in terms of cross-sectional differences. However, prior studies that have obtained private access to arbitration data (e.g., Estlund 2018) show that these clauses are prevalent across a wide variety of industries and that arbitration settlements are substantially lower than wage theft lawsuit settlements. In addition, the confidential nature of arbitration can prevent reputational costs from arising in the same way that a lawsuit may damage a firm's reputation.

[3] For example, if a firm is engaging in wage theft against some employee A, neither customers nor some other employee B can observe this.

information on fines paid to other federal agencies as well, I focus on wage theft because WHD provides start and end dates for each violation. In contrast, most other federal agencies only disclose violation detection or settlement dates but not the dates during which violations actually occurred, making it infeasible to identify the relative timing of financial and non-financial misconduct. Moreover, wage theft is typically penalized immediately upon detection, i.e., there is no lag time between when a violation ends and when it is detected and penalized. Focusing on WHD violations therefore allows for tighter measurement, as I can examine the years in which misconduct actually occurred.

To test whether wage theft is related to firms' motivations for managing earnings, I follow prior literature (e.g., Caskey and Ozel 2017) and consider benchmark beating. If wage theft represents a form of real earnings management, it should be higher in just-meet-or-beat years. Conversely, if wage theft represents a longer-term strategy (Ashenfelter and Smith 1979), then it should be orthogonal to benchmark-beating. I find evidence that firms engage in wage theft more frequently in years in which they just meet or beat analyst forecasts. These results are economically significant: firms that just meet or beat analyst forecasts are 13.7% more likely to engage in wage theft, and face penalties that are 9.8% – 10.7% higher. This result is consistent with the argument that wage theft is financially motivated.

I next examine the link between managers' personal incentives and wage theft. I measure personal incentives using both compensation incentives and personal litigation risk. As a proxy for compensation incentives, I use CEO vega (Core and Guay 2002), which captures the sensitivity of executives' compensation to stock volatility. Prior literature argues that CEOs with higher vega are more willing to engage in risky behavior (Coles et al. 2006). Tolerance for risky behavior, resulting from strong compensation incentives, may lead executives to exert greater top-down pressure to cut labor costs.[4] Consistent with a concurrent working paper by Chircop et al. (2020) that finds a positive relation between vega and broader workplace misconduct, I find that firms whose CEOs have greater risk-related incentives (i.e., higher vega) are more likely to engage in wage theft.

Prior literature argues that while *individual managers* face significant litigation risk for accrual-based actions (Chung and Wynn 2008; Amiram et al. 2020), they face minimal litigation risk for real activities management (Cohen and Zarowin 2010; Khan and Wald 2015). Underlying the latter argument is the assumption that it is difficult to separate real activities management from firms' competitive strategies, which makes it difficult to build a successful legal case against management. This assumption is unlikely to hold for wage theft, given its illegality. Moreover, while *firm*-level

[4]For example, in a 2016 cost-cutting effort, Boeing significantly limited employees' abilities to claim overtime pay. In the internal memo sent to employees regarding this move, company executives stated that "employees are expected to get their work done without paid overtime", however, several workers expressed concern that the new policy effectively amounted to a pay cut for providing the same labor (source: Seattle Times article, available at https://www.seattletimes.com/business/boeing-aerospace/boeing-curtails-overtime-for-80000-white-collar-workers-in-cost-cutting-move/).

litigation risk for wage theft is constrained by mandatory arbitration clauses, these clauses typically do not cover litigation against individuals associated with the firm.[5]

To test the effect of managers' litigation risk, I exploit a staggered series of US circuit court cases that focused specifically on executives' personal liability for wage theft. Two cases (in 2008 in the Eleventh Circuit and 2012 in the Fifth Circuit) reduced personal liability for executives of firms in those respective circuits, while three cases (in 2009 in the Ninth Circuit and 2013 in both the First and Second Circuits) increased personal liability for executives of firms in those circuits. I find that firms headquartered in the Fifth and Eleventh Circuits engaged in higher levels of wage theft after the respective court cases in those circuits, while firms headquartered in the First, Second, and Ninth Circuits engaged in lower levels of wage theft after the respective court cases in those circuits. My findings highlight a form of real activities management that is subject to significant (managerial) litigation risk. I also find that the effects documented in this paragraph and in the preceding two paragraphs reinforce each other; firms are more likely to engage in wage theft in response to meet-or-beat incentives when their managers have higher compensation incentives or face lower personal litigation risk.

Having established that wage theft arises from similar firm-level and managerial motivations to financial misconduct, I next directly test whether there is a link between wage theft and financial misconduct. Prior literature (Cohen and Zarowin 2010; Badertscher 2011; Zang 2012) suggests a substitutive relation between legal forms of real and accrual-based earnings management. These studies' findings suggest that firms with more opportunities to commit wage theft will do so and, as a result, have lower incentive to engage in financial misconduct. However, prior studies assume, whether explicitly (as in Ewert and Wagenhofer 2005) or implicitly (as in the empirical studies above) that the costs of the two forms of earnings management are distinct. In contrast, there are several costs associated with illegal activity (e.g., litigation risk) that may be common to both forms of misconduct. In the presence of common costs, the findings from prior literature may not generalize.

Using SEC Accounting and Auditing Enforcement Releases (AAERs) and shareholder lawsuits as a proxy for financial misconduct, I find that firms are *more* likely to engage in financial misconduct after they are caught engaging in wage theft, but *less* likely to engage in financial misconduct before wage theft is detected. This result suggests that wage theft precedes financial misconduct. Before a firm is caught engaging in wage theft, it may not need to manage earnings in other ways. After a firm is caught, the costs of future wage theft increase because WHD penalties are higher for repeat violators and because repeat violators are more likely to be investigated in subsequent years (Weil 2010). I test the latter assertion by showing that subsequent wage theft is lower in the years following its detection; this suggests that the deterrence effects of heightened regulatory scrutiny (the "regulatory observer" effect) play a role in firms' decisions to shift between forms of misconduct. Collectively, my findings can therefore be interpreted as evidence of a substitute relation between wage theft and financial misconduct.

[5] While most executives have directors and officers (D&O) insurance, such insurance typically does not cover wage theft (Gilhuly and Dillman 2010).

In additional analyses, I examine the role of corporate culture in explaining my findings. Culture could affect the likelihood that a firm engages in wage theft, the firm's substitution between forms of misconduct, or both. Firms with weak compliance cultures may be more willing to engage in wage theft in general in response to financial incentives. Moreover, when a firm is caught engaging in wage theft, the likelihood that it subsequently engages in another form of misconduct (rather than shifting to a legal method of boosting earnings) could be stronger if its compliance culture is weaker. To test these possibilities I construct two measures of corporate culture, based on internal control weaknesses as in Altamuro et al. (2017) and on compliance history as in Kedia et al. (2019). I find that the internal control-based culture measure does not explain any of my findings. However, I do find that firms with worse compliance cultures are more likely to engage in wage theft. The compliance culture measure, however, does not affect substitution between misconduct types. These results suggest that (i) measurement of culture is important, and (ii) compliance culture affects the initial decision to engage in wage theft but not the substitution between wage theft and other forms of misconduct.

The paper contributes to the earnings management literature in three ways. First, I identify wage theft as an economically meaningful yet previously unstudied form of real activities management that directly harms firms' most vulnerable employees. Moreover, prior studies assume that real earnings management is subject to low litigation risk; I document a setting where this is not the case. Second, prior studies (e.g., Badertscher 2011) find that legal forms of real activities management precede financial misconduct; I extend these findings to the case of illegal forms of real activities management. Third, because of my focus on illegal actions I provide direct empirical evidence of the role that realized, rather than hypothetical, misconduct detection plays in facilitating the tradeoff from real to accrual-based actions.

2 Background on wage theft

2.1 Definition of wage theft

Wage theft arises from actions taken by firms to deny employees their legally-mandated pay or benefits. Most wage theft represents violations of the Fair Labor Standards Act (FLSA). Common examples of such actions include forcing employees to underreport their hours worked, not paying for overtime work by misclassifying workers as exempt from overtime requirements, forcing employees to work through legally-mandated paid breaks, and paying workers less than the minimum wage. These actions represent direct financial savings to the firm, and in many cases these savings exceed the direct amount of underpaid wages. For instance, employees at large firms who work 30 or more hours per week are entitled to several employer-provided benefits (e.g., healthcare). If a firm were to force an employee to report that she worked 29 hours per week rather than 30, the firm would save both an hour's wages and the cost of these benefits.

2.2 Enforcement

The Wage & Hour Division (WHD), an office within the US Department of Labor, enforces compliance with laws governing employees' pay and benefits.[6] WHD was formed as part of the enactment of the original Fair Labor Standards Act (FLSA) of 1938. The last major change in US federal wage and hour laws was in 2004, when the FLSA was overhauled to reclassify the types of workers subject to overtime pay requirements.[7] Although WHD, like other Department of Labor agencies (e.g., the Occupational Safety & Health Administration), is not a Cabinet-level office, it operates with a high degree of autonomy.

As part of the enforcement process WHD conducts numerous on-site visits to firms' establishments to conduct inspections. Many of these inspections are in response to workers' complaints. Because not all victims of wage theft complain (or even know that they have been victimized), however, WHD also conducts a number of random audits each year. Although WHD does not disclose what share of its investigations arise from random audits versus employee complaints, in a 2011 Congressional hearing WHD indicated that the majority of its investigations arose from random audits.[8] Visits are typically not preannounced to the firm, to minimize the likelihood that a firm targeted for inspection can proactively cover up evidence of wrongdoing before the inspector arrives. Firms' uncertainty about whether they will be investigated also serves as a mechanism to deter wage theft (Weil 2010). This uncertainty is enabled by the relative frequency with which WHD conducts investigations (more than 20,000 per year on average). As with all forms of misconduct, I cannot observe wage theft that WHD did not catch; nonetheless, because of the higher frequency and lower costs of conducting investigations, it is unlikely that a firm that engages in a nontrivial amount of wage theft will have no detected violations.

2.3 Penalties

Based on WHD's investigations, if a firm is found to be noncompliant, it will be required to provide back pay to employees as well as possible civil penalties to WHD. These penalties vary in magnitude, depending on both the severity of the violation and the firm's compliance history (i.e., conditional on committing the same type of violation, repeat offenders will pay more than first-time offenders). WHD publicly discloses enforcement actions on its website. Disclosure creates an indirect cost of noncompliance by enabling investors and news outlets to learn of corporate wage theft.[9] Although difficult to precisely measure, this cost can be nontrivial. For

[6] While WHD has jurisdiction over a handful of other forms of labor-related misconduct, these other violations – e.g., using child labor – do not reflect wage theft. To that end, I focus on offenses directly related to the underpayment of employees.

[7] See, for example https://money.cnn.com/2004/08/23/news/economy/overtime/.

[8] A full transcript of the hearing is available at https://www.govinfo.gov/content/pkg/CHRG-112hhrg70971/pdf/CHRG-112hhrg70971.pdf.

[9] Traditional definitions of earnings management rely on reversals, i.e., earnings management in one period imposes costs in the next. In the wage theft setting, the analogous constructs are the direct and indirect costs of misconduct outlined here.

example, Johnson (2020) finds that the decision by another Department of Labor office, the Occupational Safety and Health Administration, to publicly name-and-shame violators in certain states led to a decrease in subsequent workplace safety violations in those states. Johnson (2020) identifies the media as a key channel that drives his results; two-thirds of OSHA press releases in his sample were picked up by at least one newspaper. In addition, public disclosure of enforcement actions can provide employees with information that enables them to bring class-action lawsuits for wage theft (where this is not prohibited by arbitration clauses). These lawsuits serve as another significant potential cost of misconduct, as the average employee class action lawsuit settlement exceeds $10 million on average (Li and Raghunandan 2021). Thus, although the direct penalties for wage theft are small, indirect costs can serve as a deterrent.

3 Related literature and hypotheses

3.1 Employee consequences of employers' financial incentives

Several existing studies (e.g., Kedia and Philippon 2009; Call et al. 2017) relate firms' financial incentives and misconduct to compensation practices, finding that employees command a wage premium and are awarded more options during the misconduct period. As a result of these findings, I would expect that firms are less likely to engage in wage theft when they engage in financial misconduct. However, these studies focus on executive compensation or employee stock options, due to the availability of data on these measures; as a result, they consider only salaried employees, who are exempt from most wage and hour regulations. An important reason to overcompensate employees during fraud periods is to deter whistleblowing, because employees who hold equity know that they will suffer if, due to fraud being detected, the value of the company's stock decreases. Firms are unlikely to have concerns about hourly-wage workers blowing the whistle on financial misconduct, however. Thus, although my study relates to existing research on the employment consequences of financial fraud, the mechanism underlying the conclusions of prior studies is unlikely to apply in the wage theft setting.

Recent research documents direct consequences, to employees, of their employers' financial incentives. These studies focus on workplace safety. For example, employees suffer injuries attributable to unsafe working conditions more frequently when their employers are unexpectedly cash-constrained (Cohn and Wardlaw 2016) or are under pressure to meet earnings benchmarks (Caskey and Ozel 2017). While workplace injuries do not necessarily reflect outright misconduct, the underlying actions (e.g., reducing maintenance) that result in higher injury rates are similar to those that, if taken further, result in labor law violations.[10] I contribute to this

[10]For example, suppose that a company currently performs maintenance on a machine six times a year, and that it is legally required to perform maintenance at least four times a year. If, in an effort to cut short-term expenses, the company cuts maintenance from six times a year to four times a year, that decision constitutes real earnings management (and may result in more injuries) but not misconduct. If the company

literature by focusing on another key labor-related consequence of firms' financial reporting incentives: rank-and-file employees' compensation. To do so, I focus on meet-or-beat situations as a measure of financial incentives. Based on these findings from prior literature, I hypothesize the following:

Hypothesis 1 *Firms with meet-or-beat incentives engage in wage theft more frequently.*

3.2 Accrual and real actions

Hypothesis 1 does not directly imply a relation between wage theft and financial misconduct. An extensive body of literature documents the financial incentives that underpin financial misconduct but, depending on the way in which the costs of wage theft and financial misconduct are related, firms may either view the two as complements or substitutes. To develop predictions of this relation I draw on prior literature that links real and accrual-based decisions. This literature finds a negative and/or substitutive relation between the two (e.g., Roychowdhury 2006; Cohen et al. 2008; Zang 2012), especially in the period subsequent to the 2002 Sarbanes-Oxley Act. Given that wage theft represents a form of real earnings management gone too far while financial misconduct represents accrual earnings management gone too far, I expect the following:

Hypothesis 2 *Firms that engage in wage theft are less likely to also engage in financial misconduct.*

There are reasons why Hypothesis 2 might not hold. Prior studies on real earnings management rely on the assumption that all firms in the same industry have the same cost structure and competitive strategy. Srivastava (2019) argues that this assumption means that proxies for real earnings management used in prior literature are misspecified and hence it is difficult to draw conclusions from measures of manipulative (but legal) behavior that rely on differences from industry peers; heterogeneity in such measures often reflects heterogeneous competitive strategies. For example, while a firm may cut discretionary costs in response to earnings targets, it could just as easily have done so as part of a longer-term cost-cutting strategy if it believed itself to be bloated relative to peers. I sidestep this limitation because wage theft is illegal and, as a result, cannot be the basis of a firm's (legally permissible) competitive strategy. However, the illegality of wage theft also provides a reason why Hypothesis 2 might not hold. Certain costs may realize only if a firm engages in illegal activity (but not in legal manipulation). These costs include litigation risk and the potential for reputational damage. For example, Li and Raghunandan (2021) show that firms caught engaging in labor-related misconduct are more likely to subsequently face

---

were instead to cut maintenance from six times a year to three times a year, that decision constitutes misconduct – and reflects real earnings management gone too far.

labor lawsuits.[11] If such costs are nontrivial, then the overall costs of the two types of misconduct will be positively correlated and, as a result, the two types of misconduct may instead be positively associated.

4 Empirical approach

4.1 Financial incentives and wage theft

I begin my empirical analyses by first testing whether wage theft is financially motivated. To do so I follow prior literature on the operational consequences of financial incentives and study the effect of meet-or-beat incentives on the likelihood that firms engage in wage theft. Because my underlying data primarily consists of large firms, I focus on meet-or-beat behavior with respect to analyst forecasts. Moreover, Gilliam et al. (2015) show that the zero-earnings discontinuity and the discontinuity around prior-year earnings have largely disappeared since the 2002 Sarbanes-Oxley Act. Following Caskey and Ozel (2017), I define an indicator, $SUSPECT_{it}$, that equals one for firm-years that just meet or beat the consensus analyst forecast by between zero and two cents per share. I then estimate the following linear probability model of wage theft:

$$WageTheft_{it} = \beta_0 + \beta_1 SUSPECT_{it} + \beta_2 Controls_{it} + \gamma_i + \theta_t + \varepsilon_{it} \quad (1)$$

Because violations may systematically vary as a function of cross-sectional characteristics, I include firm fixed effects, γ_i. I also include year fixed effects, θ_t, to control for time-variant changes in either the incentives for wage theft or the strength of enforcement. In this and all subsequent estimations, I cluster standard errors by firm.

In Eq. 1, $WageTheft_{it}$ is an indicator for whether wage theft occurred in year t (and was ultimately detected, whether in year t or in a later year). I use the indicator as my main wage theft measure because Weil (2010) highlights that while WHD compliance action data provide a relatively accurate depiction of which firms are engaging in wage theft as well as the ability to assess firms relative to one another, the inability to inspect every establishment makes it difficult to quantify the exact amount of wage theft that a given firm is engaging in.

One limitation of the indicator-based approach to constructing $WageTheft_{it}$ is that an indicator may not properly account for one-off incidents that may not reflect the firm's behavior more generally. I therefore construct three alternative measures of $WageTheft_{it}$ to account for the severity and frequency of wage theft. These are (i) the natural logarithm of the dollar value of penalties assessed for wage theft that occurred in year t; (ii) the natural logarithm of the number of distinct sites at which violations occurred during year t, irrespective of the severity in each location; and (iii)

[11] I do not consider labor lawsuits in my analyses, because I only observe the dates on which these lawsuits were initially filed and ultimately settled but not the period during which the misconduct underlying the lawsuits actually occurred.

the natural logarithm of the per-affected-employee dollar value of penalties. Underlying proxies (i) and (ii) is the assumption that more severe forms of wage theft are less likely to be random.[12] Proxy (iii) accounts for the potential concern that proxies (i) and (ii) may systematically vary with firm size. While this concern is mitigated by the fact that my sample comprises only large public firms and by my use of firm fixed effects, I nonetheless measure wage theft using this alternative measure to ensure that my results are not driven by other firm characteristics. For each of these continuous variables, I use the natural logarithm of the underlying construct to mitigate the potential effects of large outliers.

While I observe the start and end dates of wage theft, I do not observe a year-by-year breakdown of the violation amount. The mean (median) wage violation in my sample lasts 817 (729) days, so in constructing proxies (i) and (iii) above, it is necessary to allocate the penalty amount over time to gauge the severity of the wage theft the firm committed in year t. For example, a firm that pays \$50, 000 for a single violation that lasts two years has likely engaged in less overall wage theft than a firm that pays \$30, 000 twice (i.e., \$60, 000 total) for two separate one-year violations. In addition, the underlying WHD data are at the violation level. If a firm engages in multiple instances of wage theft in a given year, I must aggregate these instances to the firm-year level to conduct my analyses. However, violation dates might not perfectly overlap. For example, a firm might engage in wage theft in one location in 2007 and 2008, and in a second location in 2008 alone. To account for these scenarios, I first attribute the total penalty for each individual violation evenly across the years in which that violation was committed, then aggregate to the firm-year level. For example, if a firm engages in wage theft in 2007 and 2008 and ultimately has to pay \$60, 000 in back pay and civil penalties, I allocate $\frac{\$60{,}000}{2} = \$30,000$ to each of 2007 and 2008. If that firm also engaged in a separate instance of wage theft in 2008, for which it ultimately had to pay \$20, 000, then the total penalty allocated to 2007 would be \$30, 000 and the total penalty allocated to 2008 would be \$50, 000.

I obtain wage theft data from Good Jobs First's Violation Tracker database.[13] Although Violation Tracker does not contain exact violation start and end dates, I am able to obtain these dates directly from WHD's WHISARD database. I also obtain, from WHISARD, the number of employees affected by each violation. Good Jobs First provides parent-subsidiary matching for roughly the largest 3,000 firms in the United States by size, although some of these are private companies or subsidiaries of parent companies traded abroad. To ensure that I do not erroneously mislabel unmatched violation firms as non-violation firms, I limit the sample to firms with at least one observation in either Violation Tracker (regardless of whether the firm has

[12]For example, a firm that withholds overtime pay is required to pay \$2, 050 plus back pay per affected employee; while one case of over-withholding may simply be random error, numerous cases are less likely to be (source for amounts: https://www.dol.gov/whd/resources/cmp.htm).

[13]WHD violations pertain to six main areas: (i) child labor; (ii) work visa violations; (iii) minimum wage violations; (iv) overtime pay, including potentially misclassifying employees in order to avoid having to pay them overtime; (v) compliance with wage requirements written into government contracts that may be higher than the prevailing minimum wage; and (vi) forcing employees to work "off the clock" or denying them benefits. I define wage theft as encompassing areas (iii)-(vi) and retain WHD violation data accordingly.

any incidences of wage theft specifically) or Good Jobs First's other main firm-level database, Subsidy Tracker.[14] Approximately 27% of the sample reflects firms that do not have any observations in Violation Tracker (whether for wage theft or for one of the numerous other types of violations covered in Violation Tracker) but are covered in Subsidy Tracker. Because Good Jobs First has performed parent-subsidiary matching, these firms are likely "true zeros" in the sense that their exclusion from Violation Tracker means that they genuinely do not have any violations for any agencies covered by Violation Tracker from 2000 onward. All results in the paper are robust to excluding these firms. Because the combined Violation Tracker - Subsidy Tracker universe primarily comprises large firms, imposing this sample restriction may limit external validity to the extent that the relation between financial reporting incentives and wage theft may differ in small firms. However, the inclusion of firms that appear in Subsidy Tracker mitigates potential biases in coefficients on variables of interest within the set of large firms, thereby improving internal validity with respect to understanding the behavior of large public firms.

I include two proxies for the expected costs of wage theft in Eq. 1. The first is the percentage of employees in a firm's 2-digit NAICS industry that are unionized, obtained from UnionStats.[15] The second is the industry-year violation rate, which I measure as the fraction of firms in the same 2-digit NAICS industry that received sanctions for wage theft in year t (exclusive of the firm itself). This variable picks up changes in both industry practices and enforcement intensity over time.

Other firm-specific controls include the sales per employee ratio, as a proxy for how labor-intensive the firm's production process is, as well as several financial variables based on prior literature (Cohn and Wardlaw 2016; Caskey and Ozel 2017). These include the natural logarithm of the firm's total number of employees, leverage, return on assets (ROA), change in ROA, and sales growth rate. I measure ROA as the ratio of net income to lagged assets. I do not include total assets as a control variable because this would introduce multicollinearity: the correlation between the log of assets and the log of the number of employees is 0.77.

4.2 Financial misconduct

Similar to Eq. 1 above, I estimate a linear probability model of fraud:

$$FinMisconduct_{it} = \beta_0 + \beta_1 WageTheft_{it} + \beta_2 Controls_{it} + \gamma_i + \theta_t + \varepsilon_{it} \quad (2)$$

where $WageTheft_{it}$ represents one of the four wage theft measures outlined in Section 4.1. Following prior literature (e.g., Donelson et al. 2020), the indicator variable $FinMisconduct_{it}$ equals one if firm i received an SEC Accounting and

[14] For both databases, Good Jobs First provides a list of *current* parent-subsidiary linkages. I manually inspect and correct all such matches to ensure that subsidiary companies are matched to the parent company as of the time of violation.

[15] I use industry-year membership because, as far as I am aware, there is no firm-year-level measure of union membership that is publicly available.

Auditing Enforcement Release (AAER) or paid a settlement in an investor lawsuit pertaining to actions taken in fiscal year t or $t + 1$, regardless of when the detection/settlement occurs.[16] Note that one lawsuit or AAER can cover multiple fiscal years and multiple financial reporting violations. Lawsuit data come from the Stanford Securities Class Action Clearinghouse (SSCAC); I follow Karpoff et al. (2017) and exclude lawsuits that do not pertain to financial reporting or violations of SEC Rule 10(b)-5.[17] I construct the dependent variable using both year t and year $t + 1$ to avoid potential measurement issues arising from uncertainty over whether wage theft occurs earlier in the year than financial misconduct.[18]

I begin my sample in 2004 for two reasons: (i) to avoid potential confounding effects of including both pre- and immediately post- Sarbanes-Oxley observations on the dependent variable, and (ii) to begin my sample after the overhaul of the Fair Labor Standards Act discussed in Section 2. I end the sample in 2015 because financial misconduct can take several years to detect and, subsequent to initial detection, class-action lawsuits can take multiple years to resolve. As with Eq. 1, I include firm and year fixed effects.

Although it is the limitations of the wage theft data that restrict my sample to larger companies, my focus on larger companies has an additional benefit for the financial misconduct sample: law firms that file class-action lawsuits prefer to sue larger companies, to attain more "bang for the buck." Dyck et al. (2010) argue that the eagerness of law firms to file class-action lawsuits also mitigates the likelihood of undetected frauds perpetrated by large companies. Thus, by focusing on large firms, $FinMisconduct_{it}$ is a reasonable representation of the set of actual financial misconduct that occurs. In addition, I omit financial services firms from the sample due to the different structure of their financial statements and regulations relative to other industries.

I select control variables based on prior fraud research (e.g., Dechow et al. 2011). These include firm size (measured as the log of firm assets), the percentage of soft assets, firm R&D, leverage, previous-year ROA as a measure of firm performance, year-over-year changes in inventories plus receivables, and the level of PP&E (scaled by assets). When R&D expenditure is missing in Compustat, I code this as zero and include an additional indicator to correct for the issues highlighted in Koh and Reeb (2015). Following Brazel et al. (2009), I include abnormal changes in the number of employees (measured as the percentage change in the number of employees minus the percentage change in the level of sales). I also include a proxy for the firm's external financing dependence, as a measure of the firm's incentive to raise additional capital (Rajan and Zingales 1998). This is an indicator variable that takes the value

[16]For example, if firm i received an AAER in 2010 for misconduct it committed in fiscal years 2007 and 2008, I would set $FinMisconduct_{it}$ for $t \in \{2006, 2007, 2008\}$ but $FinMisconduct_{it} = 0$ for $t = 2010$.

[17]To maximize the statistical power of my tests, I follow Karpoff et al. (2017) in not imposing a minimum threshold on the lawsuit amount required. My results are robust to imposing Dyck et al. (2010) screen that excludes lawsuits with settlements under $3 million.

[18]My results are robust to using only the incidence of financial misconduct in year t to construct the dependent variable.

Table 1 Sample selection

Start: All Compustat firm-years, 2004-2015	95,823	
Less: Financial services firms (SIC codes 60-69)		(19,020)
Less: Missing Compustat employee data		(15,836)
Less: Missing other Compustat financial data		(9,220)
Less: Missing IBES analyst forecast data		(20,731)
Less: Firms not matched in Violation Tracker or Subsidy Tracker data		(14,324)
Final Sample Size	16,692	

of one if $\frac{\text{CFO}-\text{CAPX}}{\text{CAPX}} < -0.5$, where CAPX represents capital expenditures and CFO represents cash flow from operations. The greater a firm's dependence on external financing, the higher its incentive to convince capital markets of its financial health. I provide details on the construction of the final estimation sample in Table 1.

5 Results

5.1 Descriptive statistics

Firm-year average levels of wage theft by 2-digit NAICS industry and year are presented in Panels A and B of Table 2. In both panels, column (1) considers the proportion of sample firm-years in which wage theft occurred (regardless of whether or not a violation was detected by WHD in those years). Columns (2), (3), and (4) provide descriptive statistics for the other three wage theft proxies used in my analyses. The incidence of (detected) wage theft is relatively stable over time, although it exhibits a decline after 2013. In untabulated analyses, I verify that my results are robust to excluding firm-year observations from 2014 and 2015 from the sample. Panel C presents descriptive statistics for the three non-indicator measures of $WageTheft_{it}$ for the conditional sample of 1,573 firm-years where wage theft occurred. Panel C highlights the limitations of federal laws that set low maximum penalties; the average violation, scaled by the number of associated violation-years, results in \$57, 640.19 in penalties.

Summary statistics for all regression variables are presented in Table 3. Continuous variables are winsorized at the 1% and 99% levels. On average 11.6% of employees in firms' industries are unionized, although this ranges from 2.0% to 51.3% within individual industry-years. The sample consists of large firms with average leverage ratios of 25.2%, which is low relative to the Compustat universe. The majority of firms (82%) are profitable in that they have positive ROA, reflecting the fact that my sample comprises larger firms. Approximately 2.9% of firm-years correspond to financial misconduct; because $FinMisconduct_{it} = 1$ when *either* the current or subsequent year is a misconduct year, this gives a slightly higher sample mean value of $FinMisconduct_{it}$ of 4.0%.

Table 2 Summary statistics on wage theft

Panel A: Wage theft by year

	Measure of $WageTheft_{it}$			
Year	Indicator	Log Pen. $	Log # Viol. Sites	Log Per-Capita Pen. $
2004	0.113	1.083	0.099	0.803
2005	0.109	1.057	0.097	0.786
2006	0.098	0.954	0.087	0.697
2007	0.099	0.959	0.084	0.684
2008	0.102	0.983	0.085	0.714
2009	0.097	0.928	0.084	0.692
2010	0.111	1.058	0.096	0.810
2011	0.104	0.983	0.090	0.755
2012	0.100	0.963	0.090	0.719
2013	0.088	0.845	0.079	0.643
2014	0.068	0.652	0.058	0.505
2015	0.045	0.419	0.034	0.338
Overall	0.094	0.904	0.082	0.677

Panel B: Wage theft by NAICS 2-digit industry

Industry	Indicator	Log Pen. $	Log # Viol. Sites	Log Per-Capita Pen. $
Mining and Oil & Gas	0.053	0.533	0.046	0.402
Utilities	0.054	0.552	0.043	0.369
Construction	0.232	2.302	0.199	1.753
Manufacturing (NAICS code 31)	0.080	0.748	0.063	0.542
Manufacturing (NAICS code 32)	0.055	0.541	0.043	0.393
Manufacturing (NAICS code 33)	0.066	0.627	0.056	0.486
Wholesale Trade	0.099	0.898	0.077	0.712
Retail Trade (NAICS code 44)	0.141	1.244	0.107	1.046
Retail Trade (NAICS code 45)	0.164	1.506	0.153	1.212
Transportation	0.088	0.826	0.076	0.678
Couriers, Warehousing and Storage	0.333	3.121	0.241	2.926
Information	0.077	0.76	0.069	0.515
Finance and Insurance	0.059	0.593	0.076	0.444
Real Estate	0.093	0.800	0.065	0.718
Professional Services	0.146	1.572	0.147	1.088
Admin/Support/Waste Management	0.290	2.832	0.295	1.979
Educational Services	0.020	0.173	0.014	0.148
Healthcare	0.382	3.584	0.341	2.671
Arts, Entertainment, and Recreation	0.147	1.491	0.117	0.894
Accommodation and Food Services	0.198	1.808	0.185	1.339
Other	0.232	2.133	0.232	1.591
Overall	0.094	0.904	0.082	0.677

Table 2 (continued)

Panel C: Wage theft severity within wage theft incidence years ($n = 1,573$)					
Variable	Mean	Std. Dev.	10 %ile	Median	90 %ile
Log Penalty $	9.597	1.488	7.832	9.422	11.685
Log # Violation Sites	0.868	0.333	0.693	0.693	1.386
Log Per-Capita Penalty $	7.188	1.395	5.235	7.295	8.864
Penalty $	57,640.19	174,318.60	2,518.67	12,360.67	118,811.00
# Violation Sites	1.559	1.3	1	1	3
Per-Capita Penalty $	3,261.09	6,009.06	186.75	1472.14	7073.00

This table presents summary statistics pertaining to the incidence and magnitude of penalties assessed for wage theft in my sample. Panel A presents a breakdown by year, while Panel B presents a breakdown by two-digit NAICS industry, both with respect to the full sample. In Panels A and B, the summary statistics presented reflect means (which are equivalent to proportions of firm-years with wage theft for the indicator-based measure). Panel C presents descriptive statistics on the magnitude of penalties assessed for wage theft conditional on wage theft occurring, i.e., only for firm-years for which wage theft occurs

5.2 Meet-or-beat incentives and wage theft

Results from estimating (1) are in Table 4. I present results separately for each of the four wage theft measures outlined in Section 4.1. In all four cases, I find a positive and significant coefficient on $SUSPECT_{it}$, meaning that firms are more likely to engage in wage theft when they have incentives to meet or beat analyst forecasts. These effects are economically significant; the coefficient in column (1) suggests that the mean firm is 13.7% more likely to engage in wage theft relative to the case where it does not have meet-or-beat incentives and faces penalties that are $9.8\% - 10.7\%$ higher (based on the coefficients in columns (2) and (4)). These results are consistent with Hypothesis 1. To ensure that the results in Table 3 are unlikely to be driven by an omitted correlated variable, I follow Larcker and Rusticus (2010) and calculate the impact threshold of a confounding variable (ITCV). In untabulated tests I find that a potential omitted correlated variable would have to have an impact at least 7.6, 2.7, 4.3, and 9.4 times that of the most impactful control variable in columns (1)-(4), respectively, in order to invalidate my results. The results in Table 4 reflect meet-or-beat incentives driving the incidence, as well as the severity, of corporate wage theft: in untabulated analyses, I find that, within the set of wage theft firm-years observed in my sample, firms with meet-or-beat incentives face higher penalties per affected employee.

With respect to control variables, I find that larger firms (based on the number of employees) are more likely to engage in wage theft. This result may simply reflect the fact that firms with more opportunities to engage in wage theft are more likely to do so. Interestingly, I do not find statistically significant results for union coverage or the industry-year violation rate. These results are likely driven by the fixed effects design

Table 3 Summary statistics of regression variables

Variable	N	Mean	Std. Dev.	10 %ile	Median	90 %ile
$FinMisconduct_{it}$	16,692	0.040	0.196	0	0	0
$WageTheft_{it}$ (indicator)	16,692	0.094	0.292	0	0	0
$WageTheft_{it}$ Log Pen. $)	16,692	0.904	2.841	0	0	0
$WageTheft_{it}$ (Log # Viol. Sites)	16,692	0.082	0.273	0	0	0
$WageTheft_{it}$ (Log Per-Capita Pen. $)	16,692	0.677	2.143	0	0	0
Suspect firm	16,692	0.235	0.424	0	0	1
Union coverage	16,692	11.614	6.5	3.949	10.92	15.438
Log sales per employee ratio	16,692	5.733	0.917	4.747	5.675	6.846
Industry-year violation rate	16,692	0.043	0.044	0.014	0.03	0.098
Post Legal Liability Increase	16,692	0.104	0.306	0	0	1
Post Legal Liability Decrease	16,692	0.057	0.232	0	0	0
Log CEO vega	11,495	3.787	1.969	0.000	4.166	6.021
Log CEO delta	11,422	5.470	1.500	3.680	5.466	7.290
Habitual meet-or-beat	13,527	0.279	0.449	0	0	1
Log employees	16,692	2.130	1.353	0.495	1.946	4.062
Log assets	16,692	7.678	1.726	5.501	7.601	10.029
% Soft assets	16,692	0.599	0.218	0.263	0.64	0.858
Sales growth rate	16,692	0.107	0.256	-0.117	0.072	0.343
Abnormal employee change	16,692	-0.051	0.228	-0.235	-0.031	0.139
Leverage	16,692	0.206	0.185	0.000	0.185	0.443
ROA	16,692	0.042	0.122	-0.058	0.054	0.149
Change in ROA	16,692	0.000	0.103	-0.081	0.001	0.077
External financing need	16,692	0.122	0.327	0	0	1
Change in inv. + rec.	16,692	0.017	0.054	-0.032	0.011	0.078
Log R&D	16,692	2.157	2.473	0	1.065	5.707
Missing R&D	16,692	0.377	0.485	0	0	1
PP&E	16,692	0.286	0.231	0.049	0.212	0.665

This table presents summary statistics for the variables used in my regression specificationsSummary statistics are taken over the final estimation sample used in Table 4. All continuous variables are winsorized at the 1% level

and minimal within-firm, between-year variation in these measures; in untabulated analyses that use 2-digit NAICS industry fixed effects in lieu of firm fixed effects, I find a significant negative relation between union coverage and the likelihood of wage theft. In these analyses I also find that ROA is negatively associated with the likelihood of wage theft; i.e., better-performing firms have less of a need to engage in actions that harm their employees.

Table 4 Meet-or-beat incentives and wage theft

Dependent Var.:	Indicator (1)	Log Pen. $ (2)	Log # Viol. Sites (3)	Log Per-Capita Pen. $ (4)
Suspect firm	0.013**	0.107**	0.010**	0.098**
	[2.48]	[2.13]	[2.27]	[2.55]
Union coverage	−0.003	−0.031	−0.002	−0.018
	[−0.95]	[−0.98]	[−0.79]	[−0.75]
Industry-year violation rate	0.051	0.520	0.116	−0.041
	[0.46]	[0.50]	[1.14]	[−0.05]
Log employees	0.051***	0.517***	0.053***	0.333***
	[3.32]	[3.49]	[3.91]	[2.89]
Sales growth	−0.006	−0.018	−0.002	−0.046
	[−0.75]	[−0.22]	[−0.27]	[−0.71]
Log sales/employee ratio	0.012	0.106	0.011	0.096
	[1.51]	[1.41]	[1.48]	[1.58]
Abnormal change in emps.	−0.000	0.020	0.001	−0.017
	[−0.05]	[0.26]	[0.09]	[−0.30]
Leverage	−0.011	−0.085	−0.016	−0.131
	[−0.41]	[−0.34]	[−0.69]	[−0.66]
ROA	0.039	0.411	0.027	0.252
	[1.24]	[1.41]	[1.06]	[1.07]
Change in ROA	−0.018	−0.211	−0.016	−0.122
	[−1.01]	[−1.27]	[−1.11]	[−0.91]
Observations	16,692	16,692	16,692	16,692
Adjusted R^2	0.382	0.401	0.464	0.374

This table presents results from estimating (1). Observations are at the firm-year level, and the sample period ranges from 2004 to 2015. In column (1) the dependent variable is an indicator for whether firm i engaged in wage theft in year t (and was ultimately caught, whether in year t or later); in column (2) the dependent variable is the natural logarithm of the scaled dollar value of penalties assessed for wage theft undertaken in year t; in column (3) the dependent variable is the number of distinct locations in which the firm committed wage theft in year t; and in column (4) the dependent variable is the natural logarithm of per-capita penalties assessed for wage theft that occurred in year t. See Appendix A for variable definitions. The primary independent variable of interest is an indicator labeled *Suspect firm*, which equals 1 if a firm just meets or beats the analyst consensus forecast. All specifications include firm and year fixed effects. Estimated t-statistics are in brackets. * denotes significance at the 10% level; ** denotes significance at the 5% level; and *** denotes significance at the 1% level

5.2.1 Extreme earnings observations

Prior research (Siriviriyakul 2014; Srivastava 2019) highlights that extreme-earnings firms may have unusual characteristics that could drive inferences related to real earnings management. Underlying this criticism is the assertion that financial statement-based measures of real earnings management (e.g., those constructed using disclosed production costs or SG&A) are likely to suffer from an omitted variables problem for extreme observations. This is unlikely to be a concern in my setting because, unlike prior studies such as Roychowdhury (2006) or Zang (2012), I observe wage theft directly rather than inferring it from firms' financial statements. Nonetheless, to verify that my results are not driven by comparing just-meet-or-beat firms against extreme earnings observations, I restrict the sample by excluding extreme earnings observations. I construct three different subsamples based on excluded extreme observations.[19] Subsample 1 directly follows Siriviriyakul (2014) and Roychowdhury (2006) and excludes observations with earnings before extraordinary items (scaled by assets) that lie outside the interval $(-0.075, 0.075)$. Results from estimating (1) on Subsample 1 are presented in column (1) of Table 5; the results in Table 4 continue to hold.

Prior literature focuses on extreme intervals with respect to the zero-earnings benchmark. However, as an additional robustness test, I consider extreme intervals with respect to prior-year earnings and analyst forecasts as well. Because of differences in scaling, I do not use endpoints of $(-0.075, 0.075)$ to construct these intervals; a cutoff of 0.075 will exclude substantially more firms on the basis of scaled earnings than on the basis of the change in scaled earnings. To ensure that my definitions of extreme earnings are as similar as possible across the three measures, I therefore define earnings interval endpoints on the basis of the number of observations included in these subsamples. Subsample 1, described above, contains 9,105 observations, relative to a total of 16,692 used in Table 4. By defining Subsample 2 as observations with the change in income before extraordinary items (scaled by assets) lying in the interval $(-0.03, 0.03)$ and Subsample 3 as observations with analyst forecast error lying in the interval (-0.06, 0.06), I obtain 9,388 and 9,604 observations, respectively. Results from estimating Equation (1) on Subsamples 2 and 3 are presented in columns (2) and (3) of Table 5; my results continue to hold. In the analyses that follow, I use the full sample from Table 4 to maximize statistical power.

5.3 Managerial incentives and wage theft

Meet-or-beat behavior represents a response to firm-level financial incentives. However, managers' personal incentives are also frequently cited as a cause of firms' decisions to engage in misconduct (e.g., Armstrong et al. 2010). I therefore test whether managerial incentives play a role in firms' decisions to engage in wage theft. I consider both positive and negative managerial incentives for wage theft. Following Coles et al. (2006) and Armstrong et al. (2013), I measure positive managerial incentives using CEO delta and vega. Delta measures the the sensitivity of executive compensation with respect to stock prices, while vega measures the sensitivity of executive compensation with respect to risk-related incentives. I obtain data on delta and vega from the authors of Coles et al. (2006) and include the natural logarithms of these quantities as additional variables in a modified version of Eq. 1.

[19] For brevity, I tabulate results using only the wage theft indicator variable as a dependent variable,results in this section are robust to using any of the other three wage theft measures.

Table 5 Meet-or-beat incentives and wage theft, excluding extreme intervals

Dependent Variable:	$WageTheft_{it}$ (indicator)		
Extreme obs. based on:	Zero benchmark (1)	Prior-year benchmark (2)	Analyst benchmark (3)
Suspect firm	0.016**	0.017**	0.012**
	[1.96]	[2.24]	[2.18]
Union coverage	−0.001	−0.005	−0.007
	[−0.28]	[−1.61]	[−1.54]
Industry-year violation rate	−0.024	0.190	0.072
	[−0.15]	[1.17]	[0.54]
Log employees	0.058***	0.050**	0.055**
	[2.86]	[2.36]	[2.42]
Sales growth	0.002	−0.034	−0.012
	[0.10]	[−1.57]	[−0.90]
Log sales/employee ratio	0.016	0.022	0.013
	[0.82]	[1.11]	[1.03]
Abnormal change in emps.	−0.017	−0.004	0.007
	[−1.00]	[−0.20]	[0.65]
Leverage	−0.046	−0.031	0.034
	[−1.13]	[−0.68]	[0.91]
ROA	0.162	0.078	0.067
	[1.57]	[0.82]	[1.42]
Change in ROA	−0.030	0.003	−0.034
	[−0.81]	[0.03]	[−1.32]
Constant	−0.105	−0.042	−0.014
	[−0.71]	[−0.28]	[−0.12]
Observations	9,105	9,388	9,604
Adjusted R^2	0.405	0.388	0.377

This table presents results from re-estimating (1) but excluding extreme earnings intervals. Observations are at the firm-year level, and the sample period ranges from 2004 to 2015. In all three columns the dependent variable is an indicator for whether firm i engaged in wage theft in year t (and was ultimately caught, whether in year t or later). See Appendix A for variable definitions. The primary independent variable of interest is an indicator labeled *Suspect firm*, which equals 1 if a firm just meets or beats the analyst consensus forecast. Column (1) excludes observations for which income before extraordinary items, scaled by total assets, lies outside the interval $(-0.075, 0.075)$; column (2) excludes observations for which the change in scaled income before extraordinary items lies outside the interval $(-0.03, 0.03)$; and column (3) excludes observations for which the analyst forecast error lies outside the interval (-0.06, 0.06). All specifications include firm and year fixed effects. Standard errors are clustered by firm and are in parentheses. * denotes significance at the 10% level; ** denotes significance at the 5% level; and *** denotes significance at the 1% level

To measure managerial disincentives I exploit five circuit court cases in the United States that shifted managerial liability for wage theft for subsets of my sample. Under the FLSA, certain employees can be held liable for wage theft. However, in order to be held personally liable for wage theft, an employee must have "significant control" over the establishment.[20] Courts across the United States have interpreted "significant control" differently and, as a result, the set of employees who are considered personally liable for wage theft has varied in a staggered fashion across geography and time. During my sample period, two notable circuit court cases had the effect of decreasing individual executives' liability for firms headquartered in those circuits, and three other circuit court cases had the effect of increasing individual executives' liability for firms headquartered in those circuits.

In 2008, the Eleventh Circuit Court of Appeals ruled in *Alvarez Perez v. Sanford-Orlando Kennel Club* that executives who were not regularly on site at a specific workplace did not have significant control and thus could not be held liable for wage theft at that location. Under similar logic, in 2012 the Fifth Circuit Court of Appeals handed down a similar ruling in *Gray v. Powers*. In contrast, in 2009 the Ninth Circuit Court of Appeals ruled in *Boucher v. Shaw* that executives exercised significant control over all company locations and could, as such, be held personally liable for wage theft. Rulings similar to *Boucher v. Shaw* were handed down in 2013 by both the First and Second Circuits, in *Manning v. Boston Medical Center Corp.* and *Irizarry v. Catsimatidis*, respectively. Finally, in 2014 the Supreme Court resolved the uncertainty created by these five circuit court rulings by affirming the decision in *Irizarry v. Catsimatidis*; in doing so, the Supreme Court established that individuals with "general control over corporate affairs" may be held personally liable for wage theft under the FLSA.

Using the court cases above, I conduct staggered difference-in-differences tests to examine whether these shifts in executives' liability affect firms' levels of wage theft. I construct two variables for use in these tests. The first, $LiabDecrease_{it}$, equals 1 for firms headquartered in the Eleventh Circuit from 2008-2014 or the Fifth Circuit from 2012-2014. The second, $LiabIncrease_{it}$, equals 1 for firms headquartered in the Ninth Circuit from 2009-2014 or the First or Second Circuits from 2013-2014. These two variables represent the product of the "treatment" and "post" variables in the differences-in-differences specifications. The main effect of the "post" variable is subsumed by year fixed effects, while, to account for the main effect of the "treatment" variable, I include headquarters state fixed effects.[21] I obtain headquarters information data from the Loughran and McDonald Augmented 10-X Header file.

I present results from tests of managerial incentives in Table 6. For brevity, I only tabulate results using the wage theft indicator. Results using the other three wage theft proxies outlined in Section 4.1 are qualitatively similar in terms of both directional effect and statistical significance. In column (1) I consider CEO compensation incentives alone; in columns (2) and (3) I consider the effects of liability-decreasing court cases and liability-increasing court cases, respectively; and in column (4) I consider all three measures. The sample is significantly smaller in columns (1) and (4) because of limited data coverage in ExecuComp that underlies the calculations of vega and

[20] For a brief overview of personal liability and wage theft, see https://shawlawgroup.com/2017/03/wage-violations-personal-liability/

[21] Despite the presence of firm fixed effects in the model, state fixed effects are identified by firms that shift headquarters states during the sample period.

Table 6 Managerial incentives and wage theft

Dependent Variable:	$WageTheft_{it}$ (indicator)			
Managerial Incentive:	CEO vega	Legal liability		Both
	(1)	(2)	(3)	(4)
Suspect firm	0.013**	0.013***	0.013***	0.014**
	[2.12]	[2.68]	[2.66]	[2.27]
CEO vega	0.009**			0.008***
	[2.57]			[3.20]
CEO delta	−0.001			−0.002
	[−0.30]			[−0.46]
$LiabDecrease_{it}$		0.040***		0.029**
		[3.21]		[2.01]
$LiabIncrease_{it}$			−0.021*	−0.023*
			[−1.79]	[−1.73]
Union coverage	−0.003	−0.003	−0.003	−0.002
	[−0.60]	[−1.21]	[−1.19]	[−0.77]
Industry-year violation rate	−0.003	0.047	0.049	−0.005
	[−0.02]	[0.52]	[0.54]	[−0.05]
Log employees	0.034	0.049***	0.050***	0.036*
	[1.64]	[3.57]	[3.59]	[1.95]
Sales growth	−0.009	−0.006	−0.007	−0.013
	[−0.72]	[−0.75]	[−0.78]	[−1.28]
Log sales/employee ratio	0.019	0.012	0.013	0.024
	[1.21]	[1.43]	[1.57]	[1.61]
Abnormal change in emps.	−0.000	0.001	0.001	0.001
	[−0.04]	[0.15]	[0.10]	[0.05]
Leverage	−0.034	−0.003	−0.002	−0.023
	[−0.94]	[−0.08]	[−0.05]	[−0.60]
ROA	−0.009	0.043	0.042	0.002
	[−0.18]	[1.48]	[1.47]	[0.03]
Change in ROA	−0.007	−0.017	−0.017	−0.014
	[−0.26]	[−1.13]	[−1.11]	[−0.75]
Observations	11,405	16,558	16,558	11,366
Adjusted R^2	0.390	0.385	0.385	0.392

This table presents results from estimating a modified version of Eq. 1, incorporating measures of managerial incentives and liability. Observations are at the firm-year level, and the sample period ranges from 2004 to 2015. In all cases, the dependent variable is an indicator, $WageTheft_{it}$, that equals one for years in which the firm engaged in wage theft (and was ultimately caught, whether in year t or later). See Appendix A for variable definitions.The primary independent variables of interest are the log of CEO delta and vega in column (1); a variable $LiabDecrease_{it}$ that equals one for firm-years affected by liability-decreasing circuit court cases in column (2); a variable $LiabIncrease_{it}$ that equals one for firm-years affected by liability-increasing circuit court cases in column (3); and all three measures in column (4). All specifications include firm and year fixed effects. Standard errors are clustered by firm and are in parentheses. * denotes significance at the 10% level; ** denotes significance at the 5% level; and *** denotes significance at the 1% level delta. My findings in column (1) are consistent with 2020) results for labor violations more broadly: I find that CEO vega is positively associated with wage theft although, unlike in their study, I do not find that CEO delta has an effect on wage theft. This result implies that executives with greater incentives for risk-taking are more likely to engage in wage theft. The effect of the circuit court cases is also consistent with my expectation that wage theft is higher (lower) after court cases that decrease (increase) executives' personal liability for wage theft.

5.4 Wage theft and financial misconduct

Having documented that wage theft arises from similar financial incentives to other forms of real activities management, I next turn to the relation between wage theft and financial misconduct. Table 7 presents results from estimating (2) for each of the four wage theft proxies (indicator, total penalties assessed, number of sites at which wage theft occurred, and per capita penalties). In all cases, the dependent variable is $FinMisconduct_{it}$, an indicator for whether firm-year t or $t+1$ resulted in either an AAER or a securities lawsuit.

Control variable results are relatively standard with respect to prior literature. For example, firms that see year-over-year increases in ROA are less likely to be caught engaging in fraud. Higher R&D, which may signal more opaque financial statements, is also associated with higher levels of financial misconduct. Firms with a greater need for external financing are more likely to engage in financial misconduct, possibly due to the heightened incentive to obtain favorable financing terms.

Across all specifications in Table 7, the coefficient on the wage theft indicator is not statistically significant. However, this indicator does not differentiate *undetected* wage theft from detected wage theft and, accordingly, the results above could reflect countervailing forces. When wage theft is ongoing and undetected, firms may have less of a need to engage in financial misconduct. Conversely, after wage theft is caught, the costs of further wage theft increase due to WHD's dynamic enforcement model, in which the severity of the penalty and future WHD scrutiny depend on both the severity of the underlying violation and the firm's compliance history. Repeat violators (i) are more likely to be investigated by WHD in subsequent years and (ii) receive higher penalties than first-time offenders for the same types of violations. In addition, repeat violations may incur significant incremental litigation or arbitration risk. If the firm deems the increase in the costs of future wage theft to be material, it may shift from wage theft to other forms of misconduct *subsequent* to the detection of wage theft.

I am able to directly test this possibility because wage theft typically lasts for multiple years and, unlike in the case of financial misconduct, WHD typically catches wage theft while it is still ongoing and imposes sanctions immediately.[22] As a result, for many of the violations in my sample, there are two distinct periods: (i) a period in which the violation was occurring and undetected, and (ii) a period in which the

[22] Firms under investigation by the SEC or facing securities class-action lawsuits can wait several years to figure out if they will receive an AAER or reach the point of settling a lawsuit.

Table 7 Wage theft and financial misconduct

Dependent Variable:	$FinMisconduct_{it}$			
Wage Theft Variable	Indicator (1)	Log Pen. $ (2)	Log # Viol. Sites (3)	Log Per-Capita Pen. $ (4)
$WageTheft_{it}$ (indicator)	−0.009			
	[−1.04]			
$WageTheft_{it}$ (log $ value)		−0.001		
		[−0.74]		
$WageTheft_{it}$ (log # sites)			0.000	
			[0.01]	
$WageTheft_{it}$ (log per-capita pen. $)				−0.001
				[−0.98]
Habitual Meet-or-Beat	0.011**	0.011**	0.011**	0.011**
	[2.03]	[2.02]	[2.01]	[2.03]
Log assets	0.044***	0.044***	0.044***	0.044***
	[3.84]	[3.83]	[3.81]	[3.83]
% Soft assets	0.054	0.054	0.054	0.054
	[1.36]	[1.36]	[1.36]	[1.36]
Abnormal change in emps.	−0.001	−0.001	−0.000	−0.001
	[−0.06]	[−0.05]	[−0.04]	[−0.06]
Leverage	−0.009	−0.009	−0.009	−0.009
	[−0.29]	[−0.29]	[−0.28]	[−0.29]
ROA	−0.016	−0.016	−0.016	−0.016
	[−0.39]	[−0.39]	[−0.39]	[−0.39]
Change in ROA	−0.020	−0.020	−0.020	−0.020
	[−0.83]	[−0.83]	[−0.83]	[−0.83]
Sales growth	0.008	0.008	0.008	0.008
	[0.77]	[0.77]	[0.77]	[0.77]
External financing need	0.001	0.001	0.001	0.001
	[0.07]	[0.07]	[0.08]	[0.07]
Change in inventories+receivables	0.011	0.011	0.011	0.011
	[0.26]	[0.27]	[0.27]	[0.27]
Log R&D	−0.003	−0.003	−0.002	−0.003
	[−0.31]	[−0.31]	[−0.29]	[−0.31]
Missing R&D	0.012	0.012	0.013	0.012
	[0.46]	[0.46]	[0.48]	[0.46]
PP&E	0.139**	0.139**	0.139**	0.139**
	[2.38]	[2.38]	[2.39]	[2.38]

Table 7 (continued)

Dependent Variable:	$FinMisconduct_{it}$			
Wage Theft Variable	Indicator	Log Pen. $	Log # Viol. Sites	Log Per-Capita Pen. $
	(1)	(2)	(3)	(4)
Observations	12,071	12,071	12,071	12,071
Adjusted R^2	0.282	0.282	0.282	0.282

This table presents results from estimating (2). Observations are at the firm-year level, and the sample period ranges from 2004 to 2015. The dependent variable in all columns is $FinMisconduct_{it}$, an indicator for whether firm i either received an SEC AAER or settled a securities lawsuit pertaining to fiscal year t or $t+1$. The primary independent variable of interest in each column measures the incidence of wage theft. In column (1) wage theft is measured as an indicator for whether firm i engaged in wage theft in year t (and was ultimately caught, whether in year t or later); in column (2) wage theft is measured as the natural logarithm of one plus the scaled dollar value of penalties assessed for wage theft undertaken in year t; in column (3) wage theft is measured as the natural logarithm of one plus the number of distinct locations in which the firm committed wage theft in year t; and in column (4) wage theft is measured as the natural logarithm of one plus per-capita penalties assessed for wage theft that occurred in year t. See Appendix A for variable definitions. All specifications include firm and year fixed effects. Standard errors are clustered by firm and are in parentheses. * denotes significance at the 10% level; ** denotes significance at the 5% level; and *** denotes significance at the 1% level

violation was occurring and detected. Because I can identify these periods, I can separate the effect on financial misconduct of firms' decisions to engage in wage theft from the effect of firms' wage theft being detected.

To implement this empirically, I create a variable, $WageTheftCaught_{it}$, measured analogously to the main wage theft measures. Specifically, for each of the four measures of $WageTheft_{it}$, the corresponding measure of $WageTheftCaught_{it}$ considers only the portion of $WageTheft_{it}$ that is allocable to detection years. For example, if a firm engages in wage theft in 2008 and 2009, and is caught in 2009, then the indicator form of $WageTheft_{it}$ equals 1 for both 2008 and 2009 while the indicator form of $WageTheftCaught_{it}$ is equal to 0 in 2008 but 1 in 2009. I then estimate the following modified version of Eq. 2:

$$\begin{aligned} FinMisconduct_{it} = & \beta_0 + \beta_1 WageTheft_{it} + \beta_2 WageTheftCaught_{it} \\ & + \beta_3 Controls_{it} + \gamma_i + \theta_t + \varepsilon_{it}. \end{aligned} \tag{3}$$

Results from estimating (3) are in Table 8. Each of columns (1)-(4) corresponds to one of the four wage theft measures. In all four cases, the coefficient on $WageTheftCaught_{it}$ is positive and significant. Conversely, the coefficient on $WageTheft_{it}$ is negative, and statistically significant in all but column (3).[23] These results suggest that while wage theft is undetected, firms have *less* incentive to engage in financial misconduct; however, once firms have been caught engaging in wage theft, they shift toward engaging in financial misconduct. The coefficient of

[23] Variance inflation factors for $WageTheft_{it}$ and $WageTheftCaught_{it}$ are, respectively, 2.00 and 1.84 in column (1); 2.06 and 1.90 in column (2); 2.28 and 2.12 in column (3); and 1.97 and 1.81 in column (4). These values suggests that the results in Table 8 are not driven by multicollinearity.

Table 8 Wage theft detection and financial misconduct

Dependent Variable:	$FinMisconduct_{it}$			
Wage Theft Variable:	Indicator (1)	Log Pen. $ (2)	Log # Viol. Sites (3)	Log Per-Capita Pen. $ (4)
$WageTheft_{it}$ (indicator)	−0.018*			
	[−1.91]			
$WageTheftCaught_{it}$ (indicator)	0.021**			
	[2.01]			
$WageTheft_{it}$ (log $ value)		−0.002*		
		[−1.75]		
$WageTheftCaught_{it}$ (log $ value)		0.002**		
		[2.21]		
$WageTheft_{it}$ (log # sites)			−0.015	
			[−1.28]	
$WageTheftCaught_{it}$ (log # sites)			0.034**	
			[2.28]	
$WageTheft_{it}$ (log per-capita pen. $)				−0.002*
				[−1.89]
$WageTheftCaught_{it}$ (log per-capita pen. $)				0.003*
				[1.88]
Controls	Yes	Yes	Yes	Yes
Observations	12,071	12,071	12,071	12,071
Adjusted R^2	0.282	0.282	0.282	0.282

This table presents results from estimating (3). Observations are at the firm-year level, and the sample period ranges from 2004 to 2015. The dependent variable in all columns is $FinMisconduct_{it}$, an indicator for whether firm i either received an SEC AAER or settled a securities lawsuit pertaining to fiscal year t or $t+1$. The primary independent variables of interest in each column measure the incidence of wage theft both overall and in years in which it was detected. In column (1) wage theft is measured as an indicator for whether firm i engaged in wage theft in year t (and was ultimately caught, whether in year t or later); in column (2) wage theft is measured as the natural logarithm of one plus the scaled dollar value of penalties assessed for wage theft undertaken in year t; in column (3) wage theft is measured as the natural logarithm of one plus the number of distinct locations in which the firm committed wage theft in year t; and in column (4) wage theft is measured as the natural logarithm of one plus per-capita penalties assessed for wage theft that occurred in year t. See Appendix A for variable definitions. All specifications include firm and year fixed effects. Standard errors are clustered by firm and are in parentheses. * denotes significance at the 10% level; ** denotes significance at the 5% level; and *** denotes significance at the 1% level

-0.018 on $WageTheft_{it}$ suggests that firms are nearly 45% less likely to engage in financial misconduct while engaging in undetected wage theft, relative to non-wage-theft firm-years. However, once these firms are caught, the coefficient of 0.021 on $WageTheftCaught_{it}$ implies that their likelihood of engaging in financial misconduct nearly doubles (relative to the firms concurrently engaging in undetected wage theft). Collectively, and perhaps most relevant, the total effect of the two coefficients suggests that firms caught engaging in wage theft are 7.5% more likely to subsequently engage in financial misconduct. Because $FinMisconduct_{it}$ reflects the incidence of financial misconduct in year t or $t+1$, it is unlikely that my results merely reflect companies managing earnings in the year of detection in response to the imposition of financial penalties. In sum, my findings suggest that wage theft precedes financial misconduct. This result is consistent with Choi and Gipper (2019), who find that lower-paid employees' wages decline significantly in the years immediately preceding financial fraud; the results in Table 8 suggest one mechanism (wage theft) by which this decline occurs.

A potential alternate explanation for the result in Table 8 is that it reflects complementarities in the *detection* of misconduct rather than in the incidence of misconduct, i.e., that when one agency detects misconduct at a firm, other agencies also investigate that firm. However, this is unlikely with respect to financial and labor-related misconduct. The SEC's enforcement manual[24] references collaboration with and referrals from agencies such as Treasury, the DOJ, and state securities regulators, but does not mention wage theft or the Department of Labor. Further, while securities lawsuits can arise from a variety of trigger events, I manually investigate all securities lawsuits in my sample, and none mention wage-related enforcement actions or lawsuits.

6 Additional tests

In this section I examine cross-sectional variation in the results underlying the findings in Tables 4–8.

6.1 Do meet-or-beat incentives interact with other incentives?

In this section I test whether the documented results in Sections 5.2 and 5.3 interact; that is, do increased personal incentives (or reduced disincentives) increase the likelihood that firms engage in wage theft in response to meet-or-beat incentives? To address this question I estimate modified versions of Eq. 1 that include interaction terms between the meet-or-beat indicator and the two incentive-increasing measures outlined above (CEO vega and liability-decreasing court cases). I also explore the role of firm-level incentives and opportunities in facilitating the relation between wage theft and meet-or-beat incentives. To do this, I consider habitual meet-or-beat

[24] https://www.sec.gov/divisions/enforce/enforcementmanual.pdf

firms (as a proxy for firms with heightened incentives for meet-or-beat behavior) and non-R&D firms (as a proxy for firms with greater reliance on hourly, rather than salaried, employees). I tabulate results in this section using only the indicator variable version of $WageTheft_{it}$; my findings are qualitatively unchanged if I instead use any of the other three proxies.

Prior literature (e.g., McVay et al. 2006) documents a link between managers' personal incentives and earnings management behavior around analyst forecast benchmarks. If the results presented in Table 4 reflect wage theft as a form of real earnings management, I should therefore observe that stronger managerial incentive corresponds to a stronger relation between meet-or-beat incentives and wage theft.

The findings in columns (1) and (2) of Table 9 are consistent with this assertion. I find in column (1) that the interaction between $SUSPECT_{it}$ and $LiabDecrease_{it}$ is positive and significant. This result suggests that a decrease in managers' personal disincentives for wage theft makes wage theft a more attractive form of real earnings management. In column (2), I interact $SUSPECT_{it}$ with CEO vega and find a positive and significant coefficient on the interaction term. Although CEO vega does not correspond to wage theft incentives specifically, higher risk-reward sensitivity creates a higher *overall* incentive to manage earnings to meet benchmarks. Consistent with this prediction, I find that the relation between wage theft and meet-or-beat incentives is stronger when CEO vega is higher (and, hence, when there may be more top-down pressure to cut wage costs).

I next examine the role of firm-level incentives and opportunities in facilitating the relation between meet-or-beat incentives and wage theft. Chu et al. (2019) argue that meet-or-beat incentives are more important to firms with a reputation for beating earnings expectations. To test whether this finding applies to the wage theft setting, I consider whether firms with a reputation for beating earnings expectations are more likely to engage in wage theft when faced with meet-or-beat incentives. I construct a measure of reputation similar to that of Chu et al. (2019) and define a firm as a habitual meet-or-beat firm if its actual earnings exceeded the analyst consensus (regardless of the margin) in each of the previous three years. Results from this specification are presented in column (3) of Table 9. Consistent with the argument that habitual meet-or-beat firms have stronger incentives to engage in real earnings management to continue meeting earnings targets, I find that habitual meet-or-beat firms are more likely to engage in wage theft.

In addition to firm-level incentives, I consider heterogeneity in the opportunities for wage theft that may arise as a result of a firm's business model. Because it is not possible to directly observe the proportion of a firm's employees that are salaried, I measure opportunities for wage theft using an indicator variable for whether firms do not disclose R&D expenditures. Firms that rely upon R&D are likely to be less reliant on hourly (rather than salaried) labor. Column (4) of Table 9 presents results using this interaction. Surprisingly, I find that non-R&D firms are *less* likely than R&D firms to engage in wage theft in response to meet-or-beat incentives. This result may reflect differences in the signalling value of observed wage theft as a form of real earnings management. Specifically, when firms' business models are naturally prone to wage violations, such violations may be less likely to indicate deliberate

Table 9 Incentives, meet-or-beat behavior, and wage theft

Dependent Variable:	$WageTheft_{it}$ (indicator)			
Cross Section:	Manager-Level		Firm-Level	
	(1)	(2)	(3)	(4)
Suspect firm × $LiabDecrease_{it}$	0.041***			
	[2.81]			
Suspect firm × Log CEO vega		0.006*		
		[1.94]		
Suspect firm × Habitual Meet-or-Beat			0.023*	
			[1.79]	
Suspect firm × Missing R&D				−0.021*
				[−1.83]
Suspect firm	0.011**	−0.011	0.003	0.020***
	[2.07]	[−0.79]	[0.38]	[3.22]
$LiabDecrease_{it}$	0.031***			
	[2.95]			
Log CEO vega		0.007*		
		[1.96]		
Log CEO delta		−0.002		
		[−0.38]		
Habitual Meet-or-Beat			0.003	
			[0.31]	
Missing R&D				−0.017
				[−0.72]
Controls	Yes	Yes	Yes	Yes
Observations	16,558	11,366	13,440	16,692
Adjusted R^2	0.385	0.392	0.396	0.382

This table presents results from estimating a modified version of Eq. 1 that interacts firms' meet-or-beat incentives with other firm- and manager- level incentives and opportunities for wage theft. Observations are at the firm-year level, and the sample period ranges from 2004 to 2015. In all cases the dependent variable is an indicator, $WageTheft_{it}$, that equals one for years in which the firm engaged in wage theft (and was ultimately caught, whether in year t or later). See Appendix A for variable definitions. Column (1) considers the interaction of meet-or-beat incentives with circuit court case-driven decreases in liability; column (2) interacts meet-or-beat incentives with CEO vega; column (3) interacts meet-or-beat incentives with firms' status as a habitual meet-or-beat firm; and column (4) interacts meet-or-beat incentives with an indicator variable that equals 1 when firms do not disclose R&D expenditures. All specifications include firm and year fixed effects. Control variables are the same as in Table 6, but not tabulated for brevity. Standard errors are clustered by firm and are in parentheses. * denotes significance at the 10% level; ** denotes significance at the 5% level; and *** denotes significance at the 1% level

actions. Conversely, when firms' business models are less prone to wage violations, the incidence of such violations is more likely to indicate a deliberate strategy to cut costs, even when it may not be feasible to do so legally.

6.2 Repeat instances of wage theft

Several firms have multiple instances of detected wage theft during my sample period. If firms that get caught continue to engage in wage theft, then my results may not actually reflect substitution away from wage theft (i.e., additional monitoring by WHD may have no effect). On the other hand, this may simply reflect cross-sectional characteristics. For example, suppose Firm A is systematically more likely to engage in wage theft than Firm B (because of, e.g., the industry Firm A operates in). If Firm A gets caught engaging in wage theft, it may reduce its likelihood of future wage theft. This does not mean that it will *never* engage in wage theft again, just that it is less likely to do so. If this is the case, then observing repeat instances of wage theft is still consistent with my interpretation of the results in Section 5.4. To distinguish between these alternative explanations, I estimate the following model:

$$\begin{aligned} FutureWageTheft_{it} = {} & \beta_0 + \beta_1 WageTheftCaught_{it} + \beta_2 Controls_{it} \\ & + + \gamma_i + \theta_t + \varepsilon_{it} \end{aligned} \tag{4}$$

where $FutureWageTheft_{it}$ is an indicator variable that equals 1 if firm i engaged in wage theft in at least one of years $t+1$, $t+2$, or $t+3$. A negative value of β_1 in Eq. 4 implies that after being caught engaging in wage theft in year t, firm i is less likely to engage in it over the next three years. Results from estimating (4) are in Table 10. The significant negative coefficient on $WageTheftCaught_{it}$ in column (1) suggests that firms are less likely to engage in future wage theft subsequent to having been caught.

As an additional mitigating factor to the concern of repeat violations, I consider the geography of violations within firms over time. Raghunandan and Ruchti (2021) show that after firms face sanctions from the Occupational Safety and Health Administration (OSHA), they are less likely to continue to engage in violations in the state they were sanctioned in. This may be because OSHA is more likely to flag a future violation as repeat if it occurs in the same state as the initial violation; as a result, the increase in the costs of future misconduct subsequent to detection may not be uniform across locations in which the firm operates. Building on these findings, I re-estimate (4) accounting for geographic dispersion in violations. I construct four new variables: $FutureWageTheftHQ_{it}$ and $FutureWageTheftNonHQ_{it}$, which are analogous to $FutureWageTheft_{it}$ but for a firm's headquarters and non-headquarters states, respectively; and $WageTheftCaughtHQ_{it}$ and $WageTheftCaughtNonHQ_{it}$, which are analogous to $WageTheftCaught_{it}$ but for a firm's headquarters and non-headquarters states, respectively.

I present results from this modified specification in columns (2) and (3) of Table 10. The negative and significant coefficients on $WageTheftCaughtHQ_{it}$ in column (2) and $WageTheftCaughtNonHQ_{it}$ in column (3) are consistent with Raghunandan and Ruchti (2021) finding that the deterrence effect of violations is strongest within rather than between geographic jurisdictions. In conjunction with

Table 10 Repeat wage theft violations

Dependent Variable:	$Future\ Wage\ Theft_{it}$	$Future\ Wage\ TheftHQ_{it}$	$Future\ Wage\ TheftNonHQ_{it}$
	(1)	(2)	(3)
$WageTheft\ Caught_{it}$	−0.161***		
	[-10.64]		
$WageTheft\ CaughtHQ_{it}$		−0.248***	0.012
		[-9.82]	[0.47]
$Wage\ Theft\ Caught\ NonHQ_{it}$		−0.004	−0.166***
		[−0.34]	[-9.68]
Union coverage	−0.004*	−0.001	−0.004*
	[−1.71]	[−0.73]	[−2.04]
Industry-year violation rate	0.264***	0.088*	0.241**
	[2.61]	[1.67]	[2.51]
Log employees	0.026*	0.004	0.026*
	[1.73]	[0.42]	[1.88]
Sales growth	−0.005	−0.001	−0.005
	[−0.62]	[−0.29]	[−0.75]
Log sales/employee ratio	0.009	0.001	0.007
	[1.07]	[0.09]	[1.00]
Abnormal change in emps.	0.012*	0.002	0.010
	[1.72]	[0.38]	[1.62]
Leverage	−0.022	−0.023	−0.021
	[−0.89]	[−1.46]	[−0.96]
ROA	0.072**	0.034*	0.036
	[2.56]	[1.70]	[1.56]
Change in ROA	−0.035**	−0.015	−0.025*
	[−2.31]	[−1.46]	[−1.90]
Observations	19,768	19,768	19,768
Adjusted R^2	0.494	0.378	0.487

This table presents results from estimating the models described in Section 6.2. Observations are at the firm-year level, and the sample period ranges from 2004 to 2015. The dependent variable in column (1) is $FutureWageTheft_{it}$, an indicator for whether the firm engaged in wage theft in years $t+1$, $t+2$, or $t+3$ (and was ultimately caught, whether in these years or later); the dependent variable in column (2) is $FutureWageTheftHQ_{it}$, an indicator for whether the firm engaged in wage theft in its headquarters state in years $t+1$, $t+2$, or $t+3$; and the dependent variable in column (3) is $FutureWageTheftNonHQ_{it}$, an indicator for whether the firm engaged in wage theft anywhere other than its headquarters state in years $t+1$, $t+2$, or $t+3$. See Appendix A for variable definitions. In column (1) the main independent variable of interest is the indicator variable form of $WageTheftCaught_{it}$, i.e., whether wage theft was detected in year t. In columns (2) and (3) the main independent variables of interest are $WageTheftCaughtHQ_{it}$ and $WageTheftCaughtNonHQ_{it}$, which are indicator variables for whether the firm was caught engaging in wage theft inside and outside of its headquarters state, respectively. All specifications include firm and year fixed effects. Estimated t-statistics are in brackets. * denotes significance at the 10% level; ** denotes significance at the 5% level; and *** denotes significance at the 1% level the results in Section 5.4, Table 10 suggests that after getting caught engaging in wage theft, firms are subsequently (i) less likely to engage in future wage theft and (ii) more likely to engage in financial misconduct.

6.3 Corporate culture

Recent literature finds that firms with worse corporate culture are more likely to engage in financial misconduct, suggesting that operational culture extends to financial reporting practices. For example, Kim et al. (2012) find that more "socially responsible" firms engage in earnings management less frequently and are less likely to receive SEC enforcement actions. Building on this literature, in this section I examine the role of corporate culture in driving the relation between firms' financial reporting incentives and wage theft.

To test whether either the paper's meet-or-beat results or the results on financial misconduct are driven by corporate culture, I construct two measures of corporate culture based on recent studies by Altamuro et al. (2017, hereafter AGZ) and Kedia et al. (2019, hereafter KLR). AGZ measure culture as firms' internal control environments and show that, within the pharmaceutical industry, firms with internal control weaknesses (ICWs) are more likely to both restate financial statements and have Food & Drug Administration inspection failures. KLR use firms' compliance history across five federal agencies to measure recent compliance culture, and document that past compliance behavior predicts the likelihood of receiving an AAER from the SEC.

I construct the internal control-based measure by first building a probit model of the likelihood of an internal control weakness. I then follow AGZ and label a firm as having a weak compliance culture ($WeakICW_{it} = 1$) if the firm reports ineffective internal controls despite its predicted probability of an ICW being below the median of the sample predicted ICW probability. To construct the compliance history-based measure, I draw upon the remainder of the Violation Tracker database, which contains compliance information for over 50 federal agencies. I define a firm as having a weak compliance culture ($WeakViol_{it} = 1$) if the firm had at least one non-wage theft violation in fiscal years t, $t - 1$, and $t - 2$.[25]

Using these measures of corporate culture, I estimate modified versions of Eqs. 1 and 3 that include one of the corporate culture proxies as well as an interaction term between the culture proxy and the main independent variable of interest. I present results in Table 11. For brevity, I do not tabulate the first-stage ICW estimation model

[25] KLR's main culture measure is based on a decile ranking of firms, based on their penalties paid to each agency, then taking an average of these within-year decile rankings. However, because I use a broader sample, in which more than half of sample firm-years do not have any associated violations, it is not possible to directly reconstruct this measure; I therefore use KLR's alternative culture measure, which is an indicator variable for the incidence of any misconduct in the preceding years.

Table 11 Corporate culture

Dependent Variable:	$WageTheft_{it}$ (indicator)		$FinMisconduct_{it}$	
Culture Measure:	ICW	Compliance	ICW	Compliance
	(1)	(2)	(3)	(4)
Suspect firm $\times WeakICW_{it}$	−0.002			
	[−0.05]			
Suspect firm $\times WeakViol_{it}$		0.032***		
		[2.72]		
$WageTheftCaught_{it} \times WeakICW_{it}$			−0.039	
			[−1.23]	
$WageTheftCaught_{it} \times WeakViol_{it}$				−0.011
				[−0.58]
Suspect firm	0.013**	0.001		
	[2.41]	[0.16]		
$WeakICW_{it}$	0.019		0.028	
	[0.98]		[1.00]	
$WeakViol_{it}$		−0.017**		−0.010*
		[−2.11]		[−1.74]
$WageTheft_{it}$			−0.018*	−0.019**
			[−1.87]	[−1.96]
$WageTheftCaught_{it}$			0.020*	0.028*
			[1.71]	[1.87]
Observations	14,040	16,692	10,214	12,071
Adjusted R^2	0.379	0.382	0.287	0.282

This table presents results from estimating the models described in Section 6.3. Observations are at the firm-year level, and the sample period ranges from 2004 to 2015. In columns (1) and (2) the dependent variable is $WageTheft_{it}$, an indicator that equals 1 for years in which the firm engaged in wage theft (and was ultimately caught, whether in year t or later). In columns (3) and (4) the dependent variable is $FinMisconduct_{it}$, an indicator for whether the firm either received an SEC AAER or settled a securities lawsuit pertaining to fiscal year t or $t + 1$. The other primary independent variable of interest, $WageTheftCaught_{it}$, is equal to 1 only for wage theft detection years. See Appendix A for variable definitions. Columns (1) and (3) include an indicator variable that equals 1 if the firm is deemed to have a weak corporate culture based on the ICW measure outlined in Section 6.3, while columns (2) and (4) include an indicator variable for whether the firm is deemed to have a weak compliance culture based on its compliance history with other federal agencies. Control variables for columns (1) and (2) are the same as in Table 4, while control variables for columns (3) and (4) are the same as in Table 8; however, these are not tabulated for brevity. All specifications include firm and year fixed effects. Estimated t-statistics are in brackets. * denotes significance at the 10% level; ** denotes significance at the 5% level; and *** denotes significance at the 1% level

or control variable coefficients. I also only tabulate results using the indicator form of $WageTheft_{it}$; results using the other three measures are similar. The sample size is smaller in the ICW-based tests because of data unavailability in constructing

$WeakICW_{it}$. In columns (1) and (3) I consider $WeakICW_{it}$ for the meet-or-beat and financial misconduct tests, respectively, but find that the coefficient on the interaction term is insignificant in both cases. In columns (2) and (4) I consider $WeakViol_{it}$. The positive and statistically significant interaction term in column (2) suggests that firms with a weaker compliance culture are more likely to respond to meet-or-beat incentives by engaging in wage theft. However, I find no such relation between compliance culture and financial misconduct in column (4), suggesting that inasmuch as compliance culture may have an effect on financial misconduct, any such effect flows through the decision to first engage in nonfinancial misconduct.

6.4 Does other nonfinancial misconduct predict financial misconduct?

The results in Section 5.4 suggest a potential broader implication, beyond the wage theft setting, for researchers and practitioners interested in financial misconduct. The fact that financial misconduct becomes more likely after the detection of wage theft rather than after the decision to initially engage in it suggests that using data on violation detection dates alone could be useful in predicting firms' financial reporting quality. However, this possibility may not hold; as discussed in the introduction, a unique feature of wage theft is that it is not generally observable to outsiders until detected, while other forms of operational misconduct are.

To directly test whether the detection of other forms of non-financial misconduct predicts financial misconduct, I draw upon the entirety of the Violation Tracker database. I classify violations into one of four types: labor-related (other than wage theft), environmental, consumer protection-related, and other.[26] I then re-estimate a modified version of Eq. 3 incorporating indicators for whether firm i was sanctioned during fiscal year t for each of these types of violations.[27] For brevity, I do not tabulate this specification. While my results on wage theft continue to hold, I do not find a statistically significant relation between financial misconduct and the detection of non-wage theft labor misconduct (e.g., workplace safety violations), consumer protection violations, or environmental violations. These results suggest that the detection of misconduct may be most informative in settings where it is more difficult to externally observe ongoing misconduct.

[26]Good Jobs First classifies non-financial violations into eight major groups: competition, consumer protection, employment, environment, government contracting, healthcare, workplace safety, and miscellaneous. I classify employment and workplace safety as pertaining to labor; consumer protection and environment as in the raw Violation Tracker data; and government contracting, healthcare, competition, and miscellaneous as other.

[27]Note that, unlike for wage theft, I only observe detection dates for these violations, so the indicators do not necessarily reflect the underlying misconduct years.

7 Conclusion

I study the relation between firms' financial reporting practices and wage theft. I find robust evidence that firms' short-term financial reporting incentives as well as managers' individual incentives contribute to the incidence of wage theft. While prior academic and practitioner-oriented work argues that wage theft is primarily unintentional (Crampton et al. 2003; Tarara 2013), my findings suggest that this need not always be the case, especially in cases of more severe wage theft; if wage theft is unintentional, then there should be no systematic link between firm- and manager-level incentives and the incidence or severity of wage theft, or between wage theft and financial misconduct. While it is possible that my results are driven by an omitted correlated variable, this is unlikely both because of the ITCV tests described in Section 5.2 and my use of firm fixed effects in all specifications.

In studying wage theft, I contribute to the literature on the relative timing of different forms of earnings management. I find evidence that wage theft precedes financial misconduct, and that when wage theft is detected firms shift to engaging in financial misconduct. The latter finding represents an unintended consequence of enforcement and has implications both for the financial misconduct literature and for the literature on corporate misconduct more broadly. In many settings – for example, EPA enforcement (Blundell et al. 2020) – the penalty for a given violation depends on both the severity of the underlying action and the firm's history of engaging in that type of action. In such settings, the detection of a violation increases the convexity of misconduct costs, which may induce firms that are caught in one form of misconduct to shift to another. My results thus suggest that those interested in detecting financial misconduct, such as investors and the SEC, may benefit from paying attention to firms' broader compliance histories in settings where externally observing misconduct can be difficult.

In addition, in various policy documents, WHD (and the Department of Labor more broadly) alludes to a number of factors that may trigger inspections.[28] Notably absent from these documents is any reference to firms' financial incentives. In conjunction with recent other work (e.g., Caskey et al. 2017, Cohn et al. 2016), my findings suggest potential benefits to the Department of Labor in explicitly accounting for firms' financial incentives in setting enforcement priorities.

[28] For example, at the beginning of each year, OSHA publishes an annual *Federal Agency Targeting Inspection Program* which details inspection priorities for the year (see, e.g., https://www.osha.gov/sites/default/files/enforcement/directives/FAP_01-00-008.pdf for the 2019 version). While I am not aware of a directly analogous document for WHD, former WHD director David Weil's 2010 report on strategic enforcement practices within WHD (see https://www.dol.gov/sites/dolgov/files/WHD/legacy/files/strategicEnforcement.pdf) outlines similar points.

Appendix A: Variable definitions

The table below presents definitions of variables used in the paper's empirical analyses.

Variable	Definition
$FinMisconduct_{it}$	Indicator variable that equals 1 if firm i received an AAER or was subject to a securities class-action lawsuit pertaining to year t or $t+1$.
$WageTheft_{it}$ (indicator)	Indicator variable that equals 1 if firm i engaged in wage theft during year t and was ultimately caught (whether in year t or later).
$WageTheft_{it}$ (log pen. $ value)	Natural logarithm of one plus total amount of wage theft penalties attributable to firm i in year t. I compute wage theft penalties attributable to firm i in year t by first evenly allocating penalty amounts for multi-year violations across the violation years for each individual violation and then aggregating to the firm-year level. For example, if firm i engages in one violation from 2007-2008 resulting in $30, 000 in penalties and another violation from 2008-2009 resulting in $20, 000 in penalties, I allocate $15, 000 to 2007, $25, 000 to 2008, and $10, 000 to 2009.
$WageTheft_{it}$ (log # sites)	Natural logarithm of one plus number of distinct locations in which firm i engaged in wage theft in year t and was ultimately caught (whether in year t or later).
$WageTheft_{it}$ (log per-capita pen. $)	Natural logarithm of one plus per-capita wage theft penalties associated with firm i and year t. The numerator of per-capita wage theft penalties is computed as above; the denominator is the total number of affected employees associated with violations in year t.
$WageTheftCaught_{it}$	Corresponds to one of the four $WageTheft_{it}$ variables above, but considers only the amount of wage theft that occurred during years in which wage theft was detected. In the example given above, the dollar value of penalties attributable to $WageTheftCaught_{it}$ would be $0 in 2007, $15, 000 in 2008, and $10, 000 in 2009.

Variable	Definition
Suspect firm	Indicator for whether firm just meets or beats median consensus analyst forecast by two cents per share or less.
Industry union coverage %	Percent of employees in firm's two-digit NAICS industry covered by a union.
Industry-year violation rate	Fraction of firm's industry-year peers that received sanctions from WHD.
Log employees	Natural logarithm of the number of employees.
Log sales per employee ratio	Natural logarithm of ratio of sales to number of employees.
Log assets	Natural logarithm of firm's total assets
Soft assets	Ratio of total assets less cash holdings and property, plant, and equipment to total assets.
Sales growth rate	Year-over-year change in sales divided by lagged sales.
Abnormal employee change	Year-over-year employee growth rate minus year-over-year total assets growth rate.
Leverage	Ratio of long-term debt to assets.
ROA	Ratio of net income to lagged assets.
Change in ROA	Year-over-year change in ROA.
External financing need	Indicator for whether ratio of (operating cash flow minus capex) to capex is less than -0.5.
Change in inv. + rec.	Year-over-year change in the ratio of (inventory plus receivables) to assets.
Log R&D	Natural logarithm of research and development expenditures if disclosed; 0 if missing.
Missing R&D	Indicator that equals 1 if firm does not disclose R&D expenditures.
$LiabDecrease_{it}$	Indicator that equals 1 for firms headquartered in states under the jurisdiction of the Ninth Circuit Court of Appeals between 2009-2014 or the First or Second Circuit Courts of Appeals between 2013-2014.
$LiabIncrease_{it}$	Indicator that equals 1 for firms headquartered in states under the jurisdiction of the Eleventh Circuit Court of Appeals between 2008-2014 or the Fifth Circuit Court of Appeals between 2012-2014.
Log CEO vega	Natural logarithm of one plus CEO vega, where vega is measured as in Coles et al. (2006).

Variable	Definition
Log CEO delta	Natural logarithm of one plus CEO delta, where delta is measured as in Coles et al. (2006).
Habitual meet-or-beat firm	Indicator that equals 1 if firm's actual earnings exceeded analyst consensus in each of years $t-1$, $t-2$, and $t-3$.
PP&E	Ratio of property, plant, and equipment to assets.

Acknowledgements The Institute for Fraud Prevention (IFP) provided generous financial support that enabled this project. Special thanks goes to Patricia Dechow (editor), Aiyesha Dey (RAST conference discussant), and two anonymous referees for comments that improved this paper, as well as to Phil Mattera of Good Jobs First for sharing Violation Tracker data. For helpful comments and suggestions I also thank Joy Begley, David Farber (discussant), Ian Gow (discussant), Nadja Guenster (discussant), Justin Hopkins, Jonathan Jona, Zach Kaplan, Simi Kedia, Yongtae Kim, Xi Li, Mike Power, Shiva Rajgopal (discussant), Mario Schabus, Anup Srivastava, Thomas Ruchti, Felix Vetter, Rajesh Vijayaraghavan, Christopher Yust, and Xiu-Ye Zhang; and seminar and conference participants at University of British Columbia, London School of Economics, University of Melbourne, Santa Clara University, the 2020 *Review of Accounting Studies* Conference, the 2020 AAA FARS Meeting, the 2019 AAA Annual Meeting, the 2019 Bolzano-Padova Accounting Summer Camp, the 2019 Conference on CSR, the Economy, and Financial Markets, the 2019 EAA Annual Congress, the 2019 UTS Australian Summer Accounting Conference, and the 2019 UN PRI Conference. Simran Sethi and Anjali Srinivas provided research assistance in cleaning *Violation Tracker*. Any errors are my own.

References

Altamuro, J, J Gray, and H Zhang. 2017. Corporate culture and compliance: A study of the pharmaceutical industry. Working paper, available at https://ssrn.com/abstract=2658225.

Amiram, D, S Huang, and S Rajgopal. 2020. Does financial reporting misconduct pay off even when discovered? *Rev Account Stud* 25: 811–854.

Armstrong, C, A Jagolinzer, and D Larcker. 2010. Chief executive officer equity incentives and accounting irregularities. *J Account Res* 48(2): 225–271.

Armstrong, C, D Larcker, G Ormazabal, and D Taylor. 2013. The relation between equity incentives and misreporting: The role of risk-taking incentives. *J Financ Econ* 109: 327–350.

Ashenfelter, O, and R Smith. 1979. Compliance with the minimum wage law. *J Polit Econ* 87(2): 333–350.

Badertscher, B. 2011. Overvaluation and the choice of alternative earnings management mechanisms. *Account Rev* 86(5): 1491–1518.

Bobo, K. 2011. *Wage theft in America*. New York: The New Press.

Blundell, W, G Gowrisankaran, and A Langer. 2020. Escalation of scrutiny: The gains from dynamic enforcement of environmental regulations. *Amer Econ Rev* 110(8): 2558–2585.

Brazel, J, K Jones, and M Zimbelman. 2009. Using nonfinancial measures to assess fraud risk. *J Account Res* 47(5): 1135–1166.

Call, A, S Kedia, and S Rajgopal. 2017. Rank and file employees and the discovery of misreporting: The role of stock options. *J Account Econ* 62(2-3): 277–300.

Caskey, J, and N Ozel. 2017. Earnings expectations and employee safety. *J Account Econ* 63: 121–141.

Chircop, J, M Tarsalewska, and A Trzeciakiewicz. 2020. CEO risk taking equity incentives and workplace misconduct. Working paper, available at https://ssrn.com/abstract=3511638.

Choi, J, and B Gipper. 2019. Fraudulent financial reporting and the consequences for employees. Working paper, available at https://ssrn.com/abstract=3346759.

Chu, J, P Dechow, K Hui, and A Wang. 2019. Maintaining a reputation for consistently beating earnings expectations and the slippery slope to earnings manipulation. *Contemp Account Res* 36(4): 1966–1998.

Chung, H, and J Wynn. 2008. Managerial legal liability coverage and earnings conservatism. *J Account Econ* 46(1): 135–153.

Cohen, D, A Dey, and T Lys. 2008. Real and accrual-based earnings management in the pre- and post-Sarbanes-Oxley periods. *Account Rev* 83(3): 757–787.

Cohen, D, and P Zarowin. 2010. Accrual-based and real earnings management activities around seasoned equity offerings. *J Account Econ* 50(1): 2–19.

Cohn, J, and M Wardlaw. 2016. Financing constraints and workplace safety. *J Financ* 71(5): 2017–2058.

Coles, J, N Daniel, and L Naveen. 2006. Managerial incentives and risk-taking. *J Financ Econ* 79: 431–468.

Core, J, and W Guay. 2002. Estimating the value of employee stock option portfolios and their sensitivities to price and volatility. *J Account Res* 40: 613–630.

Crampton, S, J Hodge, and J Mishra. 2003. The FLSA and overtime pay. *Publ Person Manag* 32(3): 331–354.

Dechow, P, W Ge, and C Schrand. 2010. Understanding earnings quality: a review of the proxies, their determinants, and their consequences. *J Account Econ* 50(2-3): 344–401.

Dechow, P, W Ge, C Larson, and R Sloan. 2011. Predicting material accounting misstatements. *Contemp Account Res* 28(1): 17–82.

Donelson, D, A Kartapanis, J McInnis, and C Yust. 2020. Measuring accounting fraud and irregularities using public and private enforcement. Working paper.

Dyck, A, A Morse, and L Zingales. 2010. Who blows the whistle on corporate fraud? *J Financ* 65(6): 2213–2253.

Estlund, C. 2018. The black hole of mandatory arbitration. *North Carolina Law Rev* 96: 679–709.

Ewert, R, and A Wagenhofer. 2005. Economic effects of tightening accounting standards to restrict earnings management. *Account Rev* 80(4): 1101–1124.

Gilhuly, P, and T Dillman. 2010. Officers and directors' personal liability for wages: The impact and limits of Boucher v. Shaw. *Americal Bankruptcy Inst* J 29(1).

Gilliam, T, F Heflin, and J Paterson. 2015. Evidence that the zero-earnings discontinuity has disappeared. *J Account Econ* 60(1): 117–132.

Johnson, M. 2020. Regulation by shaming: Deterrence effects of publicizing violations of workplace safety and health laws. *Amer Econ Rev* 110(6): 1866–1904.

Karpoff, J, A Koester, D Lee, and G Martin. 2017. Proxies and Databases in Financial Misconduct Research. *The Accounting Review* 92(6): 129–163.

Kedia, S, S Luo, and S Rajgopal. 2019. The association between compliance history and financial misreporting. Working paper.

Kedia, S, and T Philippon. 2009. The economics of fraudulent accounting. *Rev Financ Stud* 22(6): 2169–2199.

Khan, S, and J Wald. 2015. Director liability protection, earnings management, and audit pricing. *J Empir Legal Stud* 12(4): 781–814.

Kim, Y, M Park, and B Wier. 2012. Is earnings quality associated with corporate social responsibility? *Account Rev* 87(3): 761–796.

Koh, P, and D Reeb. 2015. Missing R&D. *J Account Econ* 60(1): 73–94.

Larcker, D, and T Rusticus. 2010. On the use of instrumental variables in accounting research. *J Account Econ* 49(3): 186–205.

Li, X, and A Raghunandan. 2021. Institutional ownership and workplace misconduct: Evidence from U.S. federal violations. Working paper, available at https://ssrn.com/abstract=3460126.

McVay, S, V Nagar, and V Tang. 2006. Trading incentives to meet the analyst forecast. *Rev Account Stud* 11: 575–598.

Raghunandan, A, and T Ruchti. 2021. Real effects of information frictions within regulators: Evidence from workplace safety violations. Working paper.

Rajan, R, and L Zingales. 1998. Financial dependence and growth. *Amer Econ Rev* 88(3): 559–586.

Roychowdhury, S. 2006. Earnings management through real activities manipulation. *J Account Econ* 42(3): 335–370.

Siriviriyakul, S. 2014. Re-examining real earnings management to avoid losses. PhD dissertation, available at https://digitalassets.lib.berkeley.edu/etd/ucb/text/Siriviriyakul_berkeley_0028E_14177.pdf.

Srivastava, A. 2019. Improving the measures of real earnings management. *Rev Account Stud* 24: 1277–1316.

Tarara, C. 2013. The FLSA: The law that almost every employer is violating. *The Federal Lawyer*: 15–21.

Weil, D. 2010. Improving workplace conditions through strategic enforcement. Report to the Wage & Hour Division of the US Department of Labor, available online at https://www.dol.gov/sites/dolgov/files/WHD/legacy/files/strategicEnforcement.pdf.

Zang, A. 2012. Evidence on the trade-off between real activities manipulation and accrual-based earnings management. *Account Rev* 87(2): 675–703.

Publisher's note Springer Nature remains neutral with regard to jurisdictional claims in published maps and institutional affiliations.