Three Essays on Misconduct and Reporting Outcomes

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Denny Kutter, M.Sc.

Lehrstuhl für Rechnungswesen und Wirtschaftsprüfung

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1. Gutachter und Betreuer:
Prof. Dr. Ulfert Gronewold
Lehrstuhl für Rechnungswesen und Wirtschaftsprüfung im privaten und öffentlichen Sektor
Universität Potsdam

2. Gutachter:
Prof. Dr. Joachim Gassen
Professur für Rechnungslegung und Wirtschaftsprüfung
Humboldt-Universität zu Berlin

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Abstract

English: This cumulative doctoral thesis consists of three empirical studies that examine the role of top-level executives in shaping adverse financial reporting outcomes and other forms of corporate misconduct. The first study examines CEO effects on a wide range of offenses. Using data from enforcement actions by more than 50 U.S. federal agencies, regression results show CEO effects on the likelihood, frequency, and severity of corporate misconduct. The findings hold for financial, labor-related, and environmental offenses; however, CEO effects are more pronounced for non-financial misconduct. Further results show a positive relation between CEO ability and non-financial misconduct, but no relation with financial misconduct, suggesting that higher CEO ability can have adverse consequences for employee welfare and society and public health. The second study focuses on CEO and CFO effects on financial misreporting. Using data on restatements and public enforcement actions, regression results show that the incremental effect of CFOs is economically larger than that of CEOs. This greater economic impact of CFOs is particularly pronounced for fraudulent misreporting. The findings remain consistent across different samples, methods, misreporting measures, and specification choices for the underlying conceptual mechanism, highlighting the important role of the CFO as a key player in the beyond-GAAP setting. The third study reexamines the relation between equity incentives and different reporting outcomes. The literature review reveals large variation in the empirical measures for firm size as standard control variable, equity incentives as key explanatory variables, and the reporting outcome of interest. Regression results show that these design choices have a direct bearing on empirical results, with changes in t-statistics that often exceed typical thresholds for statistical significance. The findings hold for aggressive accrual management, earnings management through discretionary accruals, and material misstatements, suggesting that common design choices can have a large impact on whether equity incentives effects are considered significant or not.

Abstract

Deutsch: Die vorliegende kumulative Doktorarbeit umfasst drei empirische Studien, die den Einfluss von Führungskräften auf nachteilige Rechnungslegungspraktiken und anderes Fehlverhalten von Unternehmen untersuchen. Die erste Studie untersucht den Einfluss von CEOs auf ein breites Spektrum an Verstößen. Dazu werden Daten zu Durchsetzungsmaßnahmen von mehr als 50 US-Regulierungsbehörden ausgewertet. Die Ergebnisse der Regressionsanalyse zeigen, dass CEOs die Wahrscheinlichkeit, Häufigkeit und Schwere des Fehlverhalten beeinflussen, sowohl bei finanziellen Verstößen als auch bei Verstößen gegen Arbeitnehmerrechte und Umweltvorschriften, wobei der Einfluss von CEOs bei den nicht-finanziellen Verstößen durchgehend stärker ausgeprägt ist. Weitere Ergebnisse zeigen einen positiven (keinen) Zusammenhang zwischen der Managementbegabung des CEOs und nicht-finanziellen (finanziellen) Verstößen, was darauf hindeutet, dass höhere Managementbegabung negative Konsequenzen für das Wohlergehen von Mitarbeitern und die Gesellschaft als Ganzes haben kann. Die zweite Studie geht verstärket auf den Einfluss von CEOs und CFOs auf Falschberichterstattung ein. Dazu werden Daten zu nachträglichen Anpassungen bereits veröffentlichter Rechnungslegungswerke sowie Durchsetzungsmaßnahmen ausgewertet. Die Ergebnisse der Regressionsanalyse zeigen, dass CFOs, im Vergleich zu CEOs, einen ökonomisch bedeutsameren inkrementellen Einfluss haben. Der größere Einfluss des CFOs ist bei betrügerischer Falschberichterstattung noch stärker ausgeprägt. Die Ergebnisse bleiben konsistent, wenn verschiedene Stichproben analysiert, verschiedene Analysemethoden herangezogen und verschieden Operationalisierungen für das Konstrukt Falschberichterstattung verwendete werden. Insgesamt deuten die Ergebnisse darauf hin, dass CFOs eine zentrale Rolle bei Verstößen gegen Rechnungslegungsgrundsätze zukommt. Die dritte Studie wirft einen genaueren Blick auf den Zusammenhang zwischen Anreizen aus aktienbasierter Vergütung und Rechnungslegungspraktiken. Der Literaturüberblick zeigt große Unterschiede bei der Operationalisierung von Firmengröße als Standardkontrollvariable, Anreizen aus aktienbasierter Vergütung als zentrale erklärenden Variable und der zu untersuchenden Rechnungslegungspraktik. Die Ergebnisse der Regressionsanalyse zeigen, dass sich diese Unterschiede stark auf die empirischen Ergebnisse auswirken können, wobei die daraus resultierenden Veränderungen der t-Werte oftmals typische Grenzwerte für statistische Signifikanz übersteigen. Dies gilt für den Zusammenhang zwischen Anreizen aus aktienbasierter Vergütung und verschiedenartigen Rechnungslegungspraktiken, z.B. die Einflussnahme auf die insgesamt ausgewiesenen Posten zur Periodenabgrenzung, Ergebnissteuerung mittels solcher Posten zur Periodenabgrenzung, die stärkerem Ermessen des Managements unterliegen und wesentliche Falschdarstellungen in veröffentlichten Rechnungslegungswerken. Insgesamt deuten die Ergebnisse darauf hin, dass kleine Unterschiede im Studienaufbau einen großen Einfluss darauf haben können, ob Ergebnisse zu den Anreizen aus aktienbasierter Vergütung signifikant sind.

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Introductory Summary

This cumulative doctoral thesis consists of three empirical studies that tie in with the emerging literature on executive effects in observed firm behavior, particularly adverse financial reporting outcomes and other forms of corporate misconduct (e.g., Armstrong, Larcker, Ormazabal, and Taylor 2013; Ham, Lang, Seybert, and Wang 2017; Heese and Pérez-Cavazos 2020). For example, accounting frauds such as Enron, WorldCom, and Tyco destroyed billions of dollars of equity market value. Successful companies like, for example, Amazon or Tesla have received an ever-growing amount of negative press for unsafe working conditions and employee exploitation. BP's 2010 Deepwater Horizon disaster caused severe environmental damage that ended up costing the company and its partners tens of billions of dollars. Beyond high-profile cases, such events are by no means the exception and it appears that misconduct has become pervasive (e.g., Egan, Matvos, and Seru 2019; Soltes 2019; Dyck, Morse, and Zingales 2021), with harmful consequences for investors, employees, and society and public health. While the firm and its institutional environment have been studied extensively in the literature, this thesis seeks to shed more light on the role of top-level executives in shaping such firm behavior.

The first study examines CEO effects on broader corporate misconduct. Therefore, I obtain data on corporate misconduct from Violation Tracker, produced by the Corporate Research Project of Good Jobs First. The dataset covers a wide range of offense types collected from enforcement actions by more than 50 U.S. federal agencies, including financial, workplace safety, fair employment, and environmental offenses, among others. Using a sample of publicly traded firms that includes 28,590 violations with $57,308.31 million in total penalties for the period 2000-2018, I first investigate CEO effects on corporate misconduct in a set of non-directional tests. In a related study, Davidson, Dey, and Smith (2019) find that CEO fixed effects explain most of the variation in firms' self-reported Corporate Social Responsibility

(CSR) performance. I assume and find that CEO effects map into actual firm behavior reflected in enforcement records, documenting statistically significant CEO fixed effects on the likelihood, frequency, and severity of corporate misconduct. The documented CEO effects are also significant in economic terms and, at the minimum, account for 55.79 percent of the explained variation in the number of violations.

In separate analyses, I find significant CEO fixed effects on financial, workplace safety, fair employment, and environmental offenses, respectively. The results hold for the likelihood, frequency, and severity of the misconduct. However, the documented CEO effects are generally more (less) pronounced for non-financial (financial) misconduct. By correlating the CEO fixed effect estimates across offense types, I provide some evidence of overarching patterns in the observed violation behavior. For example, I find positive correlations among workplace safety, fair employment, and environmental offenses, suggesting that CEOs engage in non-financial misconduct across offense types. On the other hand, I find negative or insignificant correlations with financial offenses, which indicates differential CEO effects on financial and non-financial misconduct.

To consider this possibility, I next investigate CEO effects in a set of directional tests. In view of the fact that CEO fixed effects are interpreted as their intrinsic ability (e.g., Graham, Li, and Qiu 2012; Demerjian, Lev, Lewis, and McVay 2013; Gormley and Matsa 2014), I focus on the relation between CEO ability and corporate misconduct. Consistent with the idea that CEOs with higher ability are faced with higher net benefits of engaging in corporate misconduct, on average, I find a positive relation between CEO ability and the likelihood, frequency, and severity of corporate misconduct. The results hold for different measures of CEO ability, either based on the generation of revenues (Demerjian, Lev, and McVay 2012) or investor perceptions reflected in stock market valuations (Daniel, Li, and Naveen 2020), and in within-firm analysis using a sample of at-risk firms (i.e., firms with at least one violation during the sample period). The documented CEO effects are again both statistically significant and economically meaningful. For example, I find that, for the at-risk sample, a one-standard-deviation increase in CEO ability is associated with an increase of 2.1 percent in the number of violations and 9.4 percent in the dollar amount of penalties, respectively.

In terms of effect heterogeneity, consistent with the notion that managers prefer taking real actions, I find a significantly positive relation between CEO ability and non-financial misconduct, while I fail to find a relation with financial misconduct. This is an important finding given that prior studies suggest that CEO ability has a positive effect on CSR performance (e.g., Davidson et al. 2019; Yuan, Tian, Lu, and Yu 2019). To mitigate selection issues, I exploit variation in the underlying reasons for CEO departures and exclude CEO-firm matches resulting from forced turnover events, where firms choose to replace a CEO to meet their time-varying needs. I continue to find a significantly positive relation between CEO ability and non-financial misconduct, suggesting that my findings are unlikely to be driven by endogenous matching of CEOs and firms.

The study contributes to the growing literature on the determinants of corporate misconduct (e.g., Cohn and Wardlaw 2016; Caskey and Ozel 2017; Heese and Pérez-Cavazos 2020), providing consistent evidence that CEOs matter for corporate misconduct and non-financial misconduct increases with CEO ability, which highlights some of the tensions and tradeoffs between financial performance and awareness of social and environmental stewardship in the C-suite. Thus, the study also adds to the literature on the effects of CEO ability (e.g., Baik, Farber, and Lee 2011; Kaplan, Klebanov, and Sorensen 2012; Yuan et al. 2019), showing that managerial ability in generating excess revenues or stock returns, which increases shareholder value, also has a dark side from endangering or harming other stakeholders.

The second study, which is co-authored with Katharina Weiß, focuses on CEO and CFO effects on firms' financial misreporting. Whether CEOs or CFOs play a more relevant role in financial misreporting matters for corporate governance and is at the heart of recent legislative changes. However, based on incentive effects provided by their equity portfolios, empirical evidence on the relevance of the CEO versus the CFO is mixed (e.g., Feng, Ge, Luo, and Shevlin 2011; Armstrong et al. 2013). We argue that, beyond those from equity incentives, there are many potential confounding effects in the CEO-CFO relationship. For example, recent studies find systematic differences between CEOs and CFOs in terms of ability and risk attitudes (e.g., Graham, Harvey, and Puri 2013; Kaplan and Sorensen 2021), where those unobservables most likely drive both their exposure (and response) to equity incentives and (mis)reporting behavior (e.g., Soltes 2016; Page 2018). In this study, we leverage the concept of "manager style" (e.g., Bertrand and Schoar 2003; Graham et al. 2012) to compare the incremental effects of CEOs and CFOs, thereby making many of the unobserved confounding factors from prior studies the variable of interest.

Empirically, we construct a manager-firm matched panel dataset that tracks CEOs and CFOs across firms over the period 1995-2016. We measure financial misreporting by using restatements covered in the Audit Analytics database, including a large number of Form 8-K Item 4.02 material event filings, which we complement with Accounting and Auditing Enforcement Releases (AAERs) issued by the Securities and Exchange Commission (SEC). To identify style effects, based on a series of fixed effects estimations, we combine three different methods: the manager mobility method, the connectedness method, and the spell method.

First, we apply the mobility method (Bertrand and Schoar 2003). For the CEO sample, we find that adding CEO fixed effects to a base model increases the (adjusted) R^2 by 3.77 (3.62) percentage points. This CEO-specific effect remains statistically significant after controlling for the incremental effect of CFOs, using a combination of the spell method and the mobility method. For the CFO sample, adding CFO fixed effects increases the (adjusted) R^2 by 12.17 (9.37) percentage points. Similarly, this CFO-specific effect is statistically significant, regardless of whether we control for the incremental effect of CEOs. To analyze the economic meaning of the fixed effect estimates, following Graham et al. (2012), we decompose the model R^2 and examine their distribution. We find that CEOs versus CFOs account for a significant proportion of 26.93 and 42.94 percent of the explained variation in financial misreporting and that the difference in the interquartile ranges of the CEO and CFO fixed effect estimates corresponds to an increase in the misreporting probability of 14.32 and 22.97 percentage points, respectively. While CEOs and CFOs incrementally affect financial misreporting, these results support the argument that CFOs have a greater economic impact.

The mobility method provides the cleanest research design for investigating managers' incremental effect, but it imposes strict conditions on the studied population of managers and firms. To illuminate the generalizability of our findings, our second and third methods are the connectedness and spell methods (Abowd, Kramarz, and Margolis 1999). Using the connectedness method, we continue to find significant CEO- and CFO-specific effects on financial misreporting. Results from a detailed analysis again indicate that CFOs play an economically more important role. Similarly, using the spell method, we find that CEOs and CFOs show a significant effect on financial misreporting that is incremental to the impact of the firm, where the marginal effect of including CFOs again is economically larger than that of CEOs. In sum, these results suggest that our inferences hold in larger samples of managers and firms.

In a second set of tests, we examine whether the documented difference in the incremental effects of CEOs and CFOs carries over to fraudulent misreporting. First, we focus on irregularities, that is, restatements of more material and presumably intentional misstatements. For the CEO sample, we fail to find a statistically significant CEO-specific effect after controlling for the incremental effect of CFOs. For the CFO sample, on the other hand, we find that adding CFO fixed effects to the base model increases the (adjusted) R^2 by 11.03 (8.38) percentage points, which exceeds the marginal effect of including CEOs by 3.13 (3.14) percentage points. This CFO-specific effect is also statistically significant, regardless of whether we control for the incremental effect of CEOs. In terms of economic meaning, we find that CFOs account for 43.20 percent of the explained variation in irregularities across firms. Further, we find that the difference in the interquartile range of the CFO fixed effect estimates corresponds to an increase of 2.04 percentage points in the probability for irregularities, which is economically significant, given an observed mean probability of 1.52 percent. These results suggest that the greater economic impact of CFOs increases in cases of fraudulent misreporting. Further analysis based on AAERs to address potential type-I errors in irregularities reinforces this notion.

In additional analyses, we first show that our findings are not driven by equity incentives or the mediating effect of firm characteristics, that is, accrual-based and off-balance sheet activities, that are themselves potential outcomes of managers' reporting behavior. Second, we replicate our analysis using a setting of plausibly exogenous management turnover and find consistent results. Finally, we test the construct validity of our dependent variables by addressing potential type-II errors in the restatement data, using the F-Score for a firm's predicted misreporting risk. Our findings remain consistent.

The study provides new insights into the economic importance of CEOs and CFOs in the beyond-GAAP setting. Prior studies focusing on incentive effects provided by equity portfolios find inconsistent results (e.g., Feng et al. 2011; Armstrong et al. 2013). Addressing unobserved confounding factors and challenges to the separation of firm- and manager-specific effects, we investigate their total effects and find that CFOs have a greater economic impact than CEOs. While the findings of Feng et al. (2011) link CFO involvement in financial misreporting to CEO pressure, the documented CFO effect in our study, based on commonalities across different firms and CEO-firm regimes, cannot be considered a manifestation of the CEO's influence. Findings from an additional analysis where we incorporate CEO incentives (to pressure the CFO) reinforce this notion. The evidence is consistent with a broader literature highlighting the important role of the CFO in the within-GAAP setting (e.g., Geiger and North 2006; Chava and Purnanandam 2010; Jiang, Petroni, and Wang 2010).

Besides the contributions to the literature, the findings can inform regulators when designing and implementing corporate governance reforms. While regulations have focused on strengthening management responsibilities (e.g., Sarbanes-Oxley Act), Feng et al. (2011) suggest that reform efforts should focus on improving CFOs' independence from CEOs. Our findings complement these suggestions by showing that CFO independence should not be viewed as panacea. Given that CFOs have significant influence over financial misreporting that cannot be assigned to CEOs, our findings indicate that, in addition to reduced CEO pressure, regulators should also consider changing the legal basis for CFO liability and shifting corporate governance toward a stricter monitoring of CFOs.

The third study reexamines the relation between executive equity incentives and financial reporting outcomes. Despite considerable academic interest, there is little consistency in the empirical evidence in the literature, with prior studies documenting positive, negative, or no associations (e.g., Armstrong et al. 2013; Davidson 2021; Mayberry, Park, and Xu 2021). While those findings have important implications for corporate governance, the sources of the lack of consistent evidence in the literature are not well understood. In this study, I focus on the role of research design choices as a possible explanation for inconsistent findings.

Some prior studies recognize that differences in research designs such as analyzing unmatched versus matched samples most likely contribute to the mixed empirical results (e.g., Armstrong, Jagolinzer, and Larcker 2010). To consider this possibility, I first explore variation in research design choices and findings in the literature. My survey of the literature reveals that, for studies using both unmatched and matched samples, regression and matched-pair designs yield similar results within each study. On the other hand, similarly designed studies, either based on unmatched-sample or matched-sample tests, still find inconsistent results. Across the different studies, I find little differences for other influential research design choices like, for example, controlling for unobserved heterogeneity across firms (e.g., Gormley and Matsa 2014).

Mitton (2022) highlights that more subtle research design choices, such as dependent variable selection, variable transformation, and outlier treatment, significantly affect inferences in corporate finance research, and that even minor changes in the measurement of routine control variables can have a direct bearing on the statistical significance of empirical results. My literature review shows that, across the studies that examine the relation between equity incentives and financial reporting outcomes, there is little variation in the treatment of outliers. However, I document large variation in the measurement of firm size as most commonly used control variable, the definition of equity incentives as key explanatory variables, and variable selection for a given dependent construct. While this variation could be driven by theoretical considerations, the literature review suggests that, in general, prior studies use an ad hoc approach when choosing among reasonable alternatives.

In the next step, I examine the extent to which variation in these research design choices affects inferences about the relation between equity incentives and financial reporting outcomes, including aggressive accrual management, earnings management through discretionary accruals, and material misstatements (e.g., Bergstresser and Philippon 2006; Armstrong et al. 2013; Davidson 2021). Therefore, I estimate a series of regressions and examine the absolute change in the t-statistics for the coefficients on equity incentives associated with changes in the empirical measures for firm size, equity incentives, and the reporting outcome of interest, while keeping all other design-specific factors constant.

My findings can be summarized as follows. First, for a given set of research design choices, I am able to replicate the results documented in the literature. Second, these results can change significantly with common design choices. I find that reasonable alternatives for firm size and equity incentives measures result in an average change in the t-statistics for the equity incentives coefficients of 0.55 and 1.46, with individual changes in the amount of up to 4.97 and 7.81, respectively. On the other hand, selecting an alternative dependent variable for the same conceptual outcome changes the t-statistics by 2.42 on average, where individual changes reach up to 11.25. Given typical thresholds of 2.58, 1.96, and 1.64 for statistical significance at the 1%, 5%, and 10% levels, these findings suggest that common design choices have a large impact on whether equity incentives effects are considered significant or not. The findings hold for CEO and CFO equity incentives from both delta (i.e., the sensitivity of the executive's equity portfolio to stock price) and vega (i.e., the sensitivity of the executive's equity portfolio to stock price volatility). Inferences about the relation between equity incentives and accrual-based outcomes are generally more sensitive to design-specific factors.

The study contributes to the literature on the relation between equity incentives and financial reporting outcomes in two ways. First, despite the lack of consistent evidence, I am able to replicate most results from prior studies, where the positive effect of risk-taking incentives provided by vega on material misstatements is the exception (e.g., Armstrong et al. 2013). This is in line with recent archival evidence presented in Mayberry et al. (2021) and the survey evidence in Graham, Harvey, and Rajgopal (2005) and Dichev, Graham, Harvey, and Rajgopal (2013), where executives respond that they manage earnings to increase stock prices and reduce stock price volatility, suggesting that delta is more important in providing reporting incentives. Second, the study highlights how subtle research design choices related to the measurement of firm size, definition of equity incentives, and dependent variable selection contribute to the mixed empirical results documented in the literature, which can be problematic given their possible implications for corporate governance.

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CEO Effects on Corporate Misconduct: The Better, the Worse?

Denny Kutter
University of Potsdam
kutter@uni-potsdam.de

Abstract

This study examines CEO effects on corporate misconduct. In a set of non-directional tests, I first investigate whether and to what extent corporate misconduct varies with individual CEOs. I find significant CEO fixed effects on the likelihood, frequency, and severity of corporate misconduct. These findings hold for different offense types; however, CEO effects are more (less) pronounced for firms' non-financial (financial) misconduct. Next, in a set of directional tests, I examine the relation between CEO ability and corporate misconduct. Consistent with the idea that more able CEOs face higher net benefits of engaging in corporate misconduct, I find that, on average, corporate misconduct increases with CEO ability. In terms of effect heterogeneity, I find consistent evidence of a positive relation between CEO ability and non-financial misconduct, while I find no evidence of a relation between CEO ability and financial misconduct. My findings suggest that CEOs matter for corporate misconduct and higher CEO ability can have adverse consequences for employee welfare and society and public health.

Data Availability: All data are publicly available from sources indicated in the text.

Keywords: CEO Ability; Corporate Misconduct; Employee Welfare; Environment; Manager Fixed Effects.

JEL Classification: G38; K31; K32; M41; M48.

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1. Introduction

What drives corporate misconduct? Anecdotal evidence suggests that addressing this question is of critical importance to the welfare of a variety of corporate stakeholders, e.g., shareholders, rank-and-file employees, and society at large. For example, accounting frauds such as Enron, WorldCom, and Tyco in the early 2000s destroyed billions of dollars of equity market value, leading to the enactment of the Sarbanes-Oxley Act (SOX). In recent years, large employers like Amazon or Tesla have received an ever-growing amount of negative press for unsafe working conditions and abnormally high injury rates, underreporting of such incidents, or underpayment and wage theft.[1] High-profile cases include the Upper Big Branch disaster in 2010, where 29 miners were killed due to workplace safety issues at Massey Energy, triggering Forms 10-K and 10-Q mine-safety disclosure mandates under section 1503 of the Dodd-Frank Act.[2] Similarly, BP's 2010 Deepwater Horizon disaster in the Gulf of Mexico not only resulted in the death of 11 workers, where 17 others were injured, but also caused substantial environmental damage due to the largest spill of oil in the history of marine oil drilling that ended up costing the company and its partners tens of billions of dollars.[3] Other examples include a series of fatal accidents at BP's Texas City refinery in 2005, DuPont's toxic releases from the production of Teflon, and Volkswagen's emissions fraud in 2015, to name a few.

Beyond the support from anecdotal evidence, such events are by no means the exception and prior studies suggest that corporate misconduct is pervasive (e.g., Egan, Matvos, and Seru 2019; Soltes 2019; Dyck, Morse, and Zingales 2021). For example, Leigh (2011) estimates that more than 3.5 million workplace injuries and illnesses at a total cost of $250 billion occur in the U.S. each year, where even minor injuries can have significant costs for employees, e.g., due to losses in wealth and future earnings (e.g., Boden and Galizzi 1999; Galizzi

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[1] For example, https://www.latimes.com/business/story/2020-03-06/tesla-left-injuries-out-of-reports-california-safety-regulator-says and https://www.washingtonpost.com/technology/2021/06/01/amazon-osha-injury-rate/.
[2] See, e.g., https://www.congress.gov/congressional-report/115th-congress/house-report/571.
[3] See, e.g., https://www.epa.gov/enforcement/deepwater-horizon-bp-gulf-mexico-oil-spill.

and Zagorsky 2009). In addition to injuries and illnesses, Mattera (2018) and the Economic Policy Institute[4] report that employees lose billions of dollars annually to wage theft, while, for example, Mattera and Baggaley (2021) document over 50,000 violations of clean air, clean water, and other environmental laws over the past two decades, which can have adverse economic and social welfare consequences, e.g., in the form of pollution, infant mortality, or reduced labor market participation (e.g., Chay and Greenstone 2003; Graff Zivin and Neidell 2012). In light of these figures, I argue that understanding the extent to which executives shape broader corporate misconduct is important. While firm characteristics and firms' institutional environment have been studied extensively in the literature, the role of executives within misconduct firms remains largely unexplored. Therefore, given that the CEO is the most powerful top-level executive and ultimately in charge of major firm policies, this study examines CEO effects on corporate misconduct.

I obtain data on corporate misconduct from Violation Tracker, which is compiled by the Corporate Research Project of Good Jobs First. The dataset covers a broad range of offense types, including financial, workplace safety, fair employment, and environmental offenses, among others, collected from enforcement actions brought by more than 50 federal agencies. Violation Tracker provides establishment-level information on violations and the resulting penalties. Empirically, I aggregate that data to the firm-level to investigate CEO effects on the likelihood that a firm engages in corporate misconduct, as well as the frequency (i.e., the number of violations) and severity (i.e., the dollar amount of penalties) of the misconduct, using a sample of publicly traded firms that includes 28,590 violations for the period 2000-2018, amounting to $57,308.31 million in total penalties.

First, in a set of non-directional tests, I investigate whether and to what extent corporate misconduct varies with individual CEOs. Using the connectedness method (Abowd,

[4] For example, https://www.epi.org/publication/employers-steal-billions-from-workers-paychecks-each-year/ and https://www.epi.org/publication/wage-theft-2021/.

Kramarz, and Margolis 1999), conditioning on observed firm characteristics, firm fixed effects, and year fixed effects, I find statistically significant CEO fixed effects on the likelihood, frequency, and severity of corporate misconduct. To analyze the effect size, following Graham, Li, and Qiu (2012), I decompose the model R^2 to quantify the relative importance of CEO fixed effects in explaining corporate misconduct. I find that the documented CEO effects are also significant in economic terms and, at the minimum, account for 55.79 percent of the explained variation in the number of violations.

Looking at different offense types, in separate analyses, I find significant CEO fixed effects on financial, workplace safety, fair employment, and environmental offenses, respectively. The results hold for the likelihood, frequency, and severity of the misconduct; however, the documented CEO effects are generally more (less) pronounced for firms' non-financial (financial) misconduct. Further analysis of the correlation structure between the fixed effect estimates provides descriptive evidence of specific patterns in the violation behavior. For example, I find positive correlations among workplace safety, fair employment, and environmental offenses, suggesting that CEOs engage in non-financial misconduct across offense types. On the other hand, I find negative or insignificant correlations with financial offenses, which indicates differential CEO effects for financial and non-financial misconduct.

Second, given that fixed effects can be interpreted as CEOs' intrinsic ability (e.g., Graham et al. 2012; Demerjian, Lev, Lewis, and McVay 2013; Gormley and Matsa 2014), I investigate the relation between CEO ability and corporate misconduct in a set of directional tests. From a theoretical standpoint, the effect of CEO ability on corporate misconduct is not clear. On the one hand, in a reputation context, better and more reputable managers could have more to lose and thus be less likely to take actions that damage their reputation (e.g., Graham, Harvey, and Rajgopal 2005; Francis, Huang, Rajgopal, and Zang 2008). Under this view, I would expect that corporate misconduct decreases with CEO ability. Alternatively, in the spirit of Becker's (1968) model, managers carefully weigh the present value of expected benefits of non-compliance with regulations against the present value of expected costs, engaging in corporate misconduct if the expected benefits exceed the expected costs. In this context, more able CEOs may be faced with higher net benefits given, for example, that they can draw from a deeper pool of positive net present value (NPV) projects than their less able peers (e.g., Chemmanur and Paeglis 2005; Kaplan, Klebanov, and Sorensen 2012). Under this alternative view, I would expect that corporate misconduct increases with CEO ability.

Consistent with the idea that CEOs with higher ability face higher net benefits of engaging in corporate misconduct, on average, I find a positive relation between CEO ability and the likelihood, frequency, and severity of corporate misconduct. The results hold for different measures of CEO ability, based on Demerjian, Lev, and McVay (2012) and Daniel, Li, and Naveen (2020), and in within-firm analysis using a sample of at-risk firms (i.e., firms with at least one violation during the sample period), where firm-level and environmental factors are held constant (e.g., Heese and Pérez-Cavazos 2020; Heese, Pérez-Cavazos, and Peter 2021). The documented CEO effects are both statistically significant and economically meaningful; for example, I find that, for the at-risk sample, a one-standard-deviation increase in CEO ability is associated with an increase of 2.1 percent in the number of violations and 9.4 percent in the dollar amount of penalties, respectively.

In terms of effect heterogeneity, consistent with the notion that managers prefer taking real actions (e.g., Graham et al. 2005), I find a significantly positive relation between CEO ability and non-financial misconduct, while I fail to find a significant relation with firms' financial misconduct. This is an important finding given that prior studies suggest that CEOs and their intrinsic ability improve Corporate Social Responsibility (CSR) performance (e.g., Davidson, Dey, and Smith 2019; Yuan, Tian, Lu, and Yu 2019). A potential concern is that my findings could be driven by endogenous matching of CEOs and firms (e.g., Fee, Hadlock, and Pierce 2013; Pan 2017). Given that there is no truly exogenous variation in CEO-firm matches, as well as no instrument or natural experiment, I exploit variation in the underlying reasons for CEO departures to mitigate selection issues. I continue to find a significantly positive relation between CEO ability and non-financial misconduct when I exclude CEO-firm matches resulting from forced turnover events, where firms choose to replace a CEO to meet their time-varying needs, suggesting that my findings are unlikely to be driven by endogenous matching of CEOs and firms as they hold in a setting where the matching issue is arguably less prevalent.

My study contributes to the growing literature on the determinants of broader corporate misconduct, providing consistent evidence that CEOs matter for corporate misconduct and non-financial misconduct increases with CEO ability. While prior studies examine firm characteristics and firms' institutional environment as potential drivers of non-financial misconduct (e.g., Cohn and Wardlaw 2016; Caskey and Ozel 2017; Xu and Kim 2022), my study focuses on the important role of CEOs and their intrinsic ability, highlighting some of the tensions and tradeoffs between firms' financial performance and awareness of social and environmental stewardship in the C-suite (e.g., Admati 2017; Shapira and Zingales 2017). Thus, my study also adds to the literature on the effects of CEO ability (e.g., Baik, Farber, and Lee 2011; Kaplan et al. 2012; Yuan et al. 2019), showing that managerial ability in generating revenues (Demerjian et al. 2012) or stock returns (Daniel et al. 2020), which increases shareholder value, also has a dark side from endangering or harming other stakeholders.

Besides the contributions to the literature, given that higher CEO ability can have negative externalities for employee welfare and society and public health, these findings can inform nomination and compensation committees involved in CEO hiring decisions, management training, and the adoption of non-financial clauses in compensation contracts. Similarly, the findings can inform regulators when designing and implementing mechanisms to internalize the social costs of corporate misconduct, e.g., by means of disclosure mandates and SOX-like certification requirements of compliance with non-financial regulations or similar policies that raise the personal stake of C-suite executives.

The study is organized as follows. Section 2 outlines prior research and develops my hypotheses. Section 3 describes the research design for hypotheses testing. I present and discuss the empirical results in section 4. Finally, section 5 concludes.

2. Prior Research and Hypotheses

2.1. Managers and Firm Behavior

Generally, the theoretical literature provides two opposing views on the role of individual managers in shaping observed firm behavior. On the one hand, the neoclassical view of the firm (e.g., Veblen 1900) assumes firms to be governed by the economic forces that determine supply and demand, where, for example, firm fundamentals drive future performance and managers are simply selfless and homogenous input factors to the firm. Thus, different managers are regarded as perfect substitutes for one another, making the same rational choices when facing identical economic situations. In a similar vein, agency theory takes a nexus of contracts view (e.g., Jensen and Meckling 1976), where managers are expected to make similar choices when responding rationally to a firm's economic environment, monitoring mechanisms, and contractual incentives in place. Hence, variation in firm behavior is attributed to heterogeneity in governance structures across firms rather than differences across managers. Collectively, neoclassical economic and agency theories, focusing on economic tradeoffs and incentives, leave little room for an incremental effect of individual managers.

On the other hand, from a behavioral economics perspective (e.g., Rosen 1981; Hambrick and Mason 1984), heterogeneities across managers should become reflected in observed firm behavior. The underlying rationale is that it is people, not firms, who make decisions.

Based on the premise of bounded rationality (e.g., Simon 1955), managers are assumed to differ in the way they respond to apparently similar economic situations. Under this perspective, managers have heterogenous talents or qualities, which affect their decision outcomes beyond economic tradeoffs and incentives and ultimately map into corporate policies. An important takeaway from the behavioral view is that firm behavior will therefore vary with individual managers, and prior studies provide empirical evidence consistent with this conjecture. For example, in their seminal study, Betrand and Schoar (2003) show that individual managers matter for a wide variety of corporate policies, where the authors document manager fixed effects on firm performance, investment and financing, and cost-cutting policies. Using a similar approach, other studies find that managers affect their firms' accounting quality and earnings properties, tax avoidance, and voluntary disclosure choices, among others (e.g., Dyreng, Hanlon, and Maydew 2010; Davis, Ge, Matsumoto, and Zhang 2015; Wells 2020).

In a related study, Davidson et al. (2019) find that CEO fixed effects explain most of the variation in firms' reported CSR performance, which, in principle, has a direct link to the type of firm behavior I examine. However, the study infers actual firm behavior from CSR ratings provided by commercial vendors such as Morgan Stanley Capital International (MSCI), formerly Kinder, Lydenberg, Domini Research & Analytics (KLD). Prior research suggests that these ratings have low construct validity and are not borne out by firms' underlying practices, i.e., firms claim to be socially responsible as a means of voluntary disclosure but do not walk the talk (e.g., Cai, Jo, and Pan 2012; Chatterji, Durand, Levine, and Toubal 2016; Raghunandan and Rajgopal 2021). In comparison, I focus on firms' compliance with regulations reflected in enforcement actions from several regulatory agencies, which is unlikely to be driven by voluntary disclosure choices. To sum up, however, the literature generally supports the behavioral view, where individual managers matter for major firm policies. Given that the CEO is the most powerful top-level executive and ultimately in charge of such policies, I predict that heterogeneities across CEOs also have a share in explaining corporate misconduct, e.g., through CEOs' direct impact on corporate strategy, budgeting, and cost-cutting policies or when setting the tone at the top. This leads to my first hypothesis:

H1: *CEOs affect corporate misconduct.*

2.2. Determinants of Corporate Misconduct

Consistent with neoclassical economic and agency theories, prior studies focus on firm characteristics and firms' institutional environment as determinants of corporate misconduct. For example, there is a large literature that examines the relation between financial misconduct and firm size, profitability, leverage and financing activities, capital market incentives, and statutory audits, among others (e.g., Beneish 1999; Lennox and Pittman 2010; Dechow, Ge, Larson, and Sloan 2011). In terms of non-financial misconduct, using data from inspections by the Occupational Safety and Health Administration (OSHA), Cohn and Wardlaw (2016) and Chatterjee, Hass, Hribar, and Kalogirou (2021) show that firms' workplace safety declines with financing constraints and cost-cutting pressure from debt covenant violations, where injuries and violations of safety regulations are associated with increases in leverage, negative cash flow shocks, and proximity to covenant thresholds. Similarly, using data on toxic releases administered by the Environmental Protection Agency (EPA), Xu and Kim (2022) find that emissions and violations of environmental regulations increase with financial constraints. Using a mining setting, Christensen, Floyd, Liu, and Maffett (2017) show that including safety records in firms' financial reports reduces injuries and citations for violations from inspections by the Mine Safety and Health Administration (MSHA), suggesting that increased investor awareness improves workplace safety.[5] Consistent with the monitoring role of investors, Shive and Forster (2020) find that ownership structure affects firms' toxic re-

[5] Chen, Hung, and Wang (2018) and Downar, Ernstberger, Reichelstein, Schwenen, and Zaklan (2021) find that similar disclosure mandates decrease wastewater, sulfur dioxide, and greenhouse gas emissions using samples of Chinese and UK firms, respectively.

leases and compliance with environmental regulations enforced by the EPA, where, for example, greenhouse gas emissions decrease with mutual fund ownership. In line with this, the findings in Cohn, Nestoriak, and Wardlaw (2021) and Li and Raghunandan (2021) suggest that private equity (PE) buyouts and holdings of institutional investors reduce injuries and violations of workplace safety regulations and other labor rights, whereas others highlight the disciplinary role of analysts and the press (e.g., Johnson 2020; Bradley, Mao, and Zhang 2021; Heese et al. 2021).

In terms of managers' incentives, from an agency perspective, Heese and Pérez-Cavazos (2020) further show that, on average, increased monitoring through visits by headquarters' managers reduces establishment-level violations. However, these visits can also lead to greater misconduct when firms are subject to strong performance pressure, which is consistent with the findings in Caskey and Ozel (2017), suggesting that managers compromise workplace safety to meet earnings expectations.[6] Similarly, using data from investigations by the Wage and Hour Division (WHD), Raghunandan (2021) shows that firms with incentives to meet earnings expectations or strong CEO equity incentives from portfolio vega (i.e., the sensitivity of a manager's wealth to stock volatility) commit more wage theft, which is in line with Chircop, Tarsalewska, and Trzeciakiewicz (2022), who find a positive relation between CEOs' risk-taking incentives provided by vega and broader workplace misconduct.[7] Using data from EPA, Akey and Appel (2021) also find a positive relation between parent companies' limited liability and subsidiaries' toxic releases, which is concentrated in firms with convex managerial payoffs from high vega. In sum, these findings comport with the broader risk-taking literature (e.g., Coles, Daniel, and Naveen 2006; Gormley, Matsa, and Milbourn

[6] Using a Chinese setting, Liu, Shen, Welker, Zhang, and Zhao (2021) find that earnings pressure also leads to higher sulfur dioxide emissions.
[7] In a similar vein, consistent with the idea that inside debt holdings reduce managerial risk-taking, Wu, Li, and Yu (2022) find fewer workplace injuries in firms with higher CEO inside debt.

2013) and prior studies that document a positive relation between equity incentives and financial misconduct (e.g., Armstrong, Larcker, Ormazabal, and Tylor 2013; Davidson 2021).

From a behavioral perspective, recent studies provide some evidence that, for example, managerial overconfidence or narcissism can increase financial misconduct (e.g., Schrand and Zechman 2012; Ham, Lang, Seybert, and Wang 2017). In terms of non-financial misconduct, however, only a few studies examine the relation between CEOs and firms' compliance with workplace safety standards. In particular, Chen, Ofosu, Veeraraghavan, and Zolotoy (2021) show that workplace injuries increase with CEO overconfidence, based on CEOs' stock option exercising, which is closely linked to managerial risk-taking given that overconfident managers will be more likely to take risky projects because they overestimate the cash flows of a project or underestimate the risk of the payoffs (e.g., Malmendier and Tate 2005; Ben-David, Graham, and Harvey 2013). On the other hand, Haga, Huhtamäki, and Sundvik (2021) find inconsistent results for powerful CEOs, where, for example, the likelihood of the CEO being the chairman (founder) of the firm decreases (increases) workplace injuries. Taken together, the literature provides little insights into how CEOs impact broader corporate misconduct beyond the effects of equity incentives and risk-taking behavior. In comparison, consistent with theories such as Rosen (1981), I focus on managerial ability as a first-order distinction for CEOs, which has a direct link to the CEO's important role in generating firm performance and value creation.

2.3. Influence of CEO Ability

For example, using a setting of PE buyouts and venture capital transactions, Kaplan et al. (2012) show that CEOs primarily vary in terms of their intrinsic ability and find that CEO ability is positively related to firm performance. Similarly, Chemmanur and Paeglis (2005) find that firm performance and long-term stock returns following initial public offerings increase with management quality, and Demerjian et al. (2012) show that appointing a CEO with higher ability is associated with improvements in subsequent firm performance using a sample of publicly traded firms. Demerjian et al. (2013) further document a positive relation between managerial ability and accounting quality and earnings properties across firms.[8] In line with this, Krishnan and Wang (2015) show that both audit fees and the likelihood of issuing a going concern opinion are decreasing with managerial ability, and Baik et al. (2011) find that both the frequency and accuracy of management earnings forecasts increase with CEO ability, which is consistent with the explanation that more able managers seek to reduce information asymmetries through voluntary disclosure to lower firms' costs of capital (e.g., Chemmanur and Paeglis 2005; Lambert, Leuz, and Verrecchia 2007). In a similar vein, given that CSR ratings from vendors such as KLD or MSCI capture voluntary disclosure choices (e.g., Chatterji et al. 2016; Raghunandan and Rajgopal 2021), Yuan et al. (2019) find a positive relation between firms' reported CSR performance and CEO ability, while others find a negative relation with credit risk, the cost of debt capital, and corporate tax payments (e.g., Bonsall IV, Holzman, and Miller 2017; Koester, Shevlin, and Wangerin 2017).

In sum, these studies provide support for a positive view of CEO ability, where more able CEOs are expected to have a deeper understanding of their business and industry trends than their less able peers, leading to more accurate predictions and estimates, better timing of capital markets, and more timely anticipation of changes in firms' economic environment or regulatory requirements, among others. However, the link between CEO ability and corporate misconduct is ambiguous. Consistent with the positive view, in a reputation context (e.g., Graham et al. 2005; Francis et al. 2008), better and more reputable managers will refrain from taking actions that damage their reputation and instead use other means of achieving their goals. Different versions of this story suggest that CEOs with higher ability are less likely to

[8] Francis et al. (2008) find that accrual quality based on the Dechow and Dichev (2002) model varies inversely with CEO reputation, measured by media citations. However, Demerjian et al. (2013) show that managerial ability is positively related to firms' broader accounting quality and earnings properties, including accrual quality based on Dechow and Dichev (2002) as modified by Ball and Shivakumar (2006), adjusting the accrual variability measure to account for measurement error from systematic differences across firms.

engage in corporate misconduct (or at least less likely to get caught). Alternatively, under an opportunistic view, corporate misconduct may increase with CEO ability. Firms invest in complying with regulatory requirements just as they invest in other activities such as research and development (R&D), including both tangible (e.g., replacement or expansion of plant and equipment) and intangible investments (e.g., implementing and maintaining internal controls and safety procedures, employee training, or abatement activities), where, for example, the costs of compliance with financial, safety, and environmental regulations can be substantial (e.g., Iliev 2010; Greenstone, List, and Syverson 2012; Kniesner and Leeth 2014).[9] Following, given firms' budget constraints, prior studies suggest that those investments are subject to cost-benefit considerations and managers compromise regulatory compliance to meet a firm's financial needs (e.g., Heese and Pérez-Cavazos 2020; Raghunandan 2021; Xu and Kim 2022).

Consider the decision to comply with regulations within a simple NPV framework similar to Becker (1968), where managers carefully weigh the present value of expected benefits of non-compliance against the present value of expected costs, engaging in corporate misconduct if the expected benefits exceed the expected costs. All else equal, compared to their less able peers, more able CEOs are assumed to convey the intrinsic value of their firm more credibly to outsiders, attract more profitable investment opportunities, and select projects with higher NPV for any given scale (e.g., Chemmanur and Paeglis 2005; Kaplan et al. 2012), increasing the marginal benefits of corporate misconduct. On the other hand, the expected costs of engaging in corporate misconduct are a joint function of the penalties and the probability of being caught. However, Shapira and Zingales (2017) note that firms are unlikely to take those costs as given but seek to diminish them, where CEOs with higher ability are expected to work more efficiently to reduce both the size of penalties and the probability of detection, e.g., through withholding of information or regulatory capture and revolving doors (e.g., Ad-

[9] While these activities are largely implemented at the establishment-level, they are driven by firm-level decisions, e.g., budgeting and cost-cutting policies (e.g., Cohn and Wardlaw 2016).

mati 2017; Soltes 2020; Emery and Faccio 2022), which, ceteris paribus, decreases the marginal costs associated with corporate misconduct. Along these lines, other studies show how engaging in financial misconduct can be ex-ante optimal for a given firm under reasonable probabilities of getting caught (e.g., Amiram, Huang, and Rajgopal 2020; Dyck et al. 2021).

That said, on average, some offense types might have more net benefits than others. For example, under the current regulatory regime, the effect of CEO ability on financial misconduct may differ from that on non-financial misconduct for at least two reasons. First, enforcement of compliance with non-financial regulations generally relies on rigid schedules with frequent investigations and low sanctions, where federal laws cap the penalties that regulatory agencies like, for example, the OSHA, MSHA, and WHD can charge for violations of safety regulations and other labor rights (e.g., Li and Raghunandan 2021; Raghunandan 2021). Similarly, sanctioning by EPA follows regulated patterns, depending on both the severity of the violation and the firm's compliance history (e.g., Blundell, Gowrisankaran, and Langer 2020; Xu and Kim 2022).[10] On the other hand, in the securities laws setting, enforcement actions are less common; however, regulatory agencies such as the Securities and Exchange Commission (SEC) or the U.S. Department of Justice (DOJ) have substantial discretion over the size of penalties that can be in the hundreds of millions or billions of dollars, making the expected costs of financial misconduct less predictable (e.g., Soltes 2019; Blackburne, Bozanic, Johnson, and Roulstone 2021).[11]

Second, while the certification requirements in section 302 of SOX strengthen management responsibilities for firms' compliance with financial regulations, requiring that CEOs and CFOs review all 10-K and 10-Q reports filed with the SEC, there is no such legal act that

[10] Note that penalties imposed by EPA can be much higher; however, violations of environmental regulations may be easier to hide, given that firms control most of the information and its release (e.g., Shapira and Zingales 2017).
[11] For example, OSHA, WHD, and EPA conduct more than 80,000, 20,000, and 7,000 investigations per year, respectively (e.g., Li and Raghunandan 2021; Raghunandan 2021; Xu and Kim 2022), whereas Blackburne et al. (2021) document that the SEC completes approximately 1,000 investigations per year, on average.

ensures the C-suite's personal accountability for non-financial misconduct, making it a "face-less crime" with diffused responsibilities (e.g., Admati 2017; Shapira and Zingales 2017). In line with this, the survey evidence in Graham et al. (2005) suggests that managers prefer taking real economic actions (e.g., cut R&D and maintenance spending or change the timing and scale of new projects) over manipulating accruals to achieve earnings benchmarks, and other studies document a shift from accrual-based earnings management to real activities manipulation in the post-SOX period (e.g., Cohen, Dey, and Lys 2008; Zang 2012). Taken together, whether and how CEO ability affects corporate misconduct is an empirical question. Under the positive (opportunistic) view, I would expect that more able CEOs are generally less (more) likely to engage in corporate misconduct. Formally:

H2a: *Corporate misconduct decreases with CEO ability.*

H2b: *Corporate misconduct increases with CEO ability.*

3. Research Design

3.1. Data and Sample

The data used in my study come from the intersection of the ExecuComp, Compustat, and Violation Tracker databases, supplemented with equity incentive and ability data obtained from Coles et al. (2006) and Demerjian et al. (2012). The sample spans the period 2000-2018 since Good Jobs First's Violation Tracker includes enforcement actions for corporate misconduct brought against firms from 2000 onward, with 2018 being the most recent year with available data. I begin my sample selection by collecting all firm-years with available CEO data from ExecuComp to construct a manager-firm matched panel dataset that tracks CEOs across firms over time. I correct this dataset for ExecuComp errors identified by Gentry, Harrison, Quigley, and Boivie (2021).[12] Next, I add annual financial data and firm fundamentals from Compustat, where firms must have available data for each variable used in the analysis for a given firm-year to be included in the sample. Following prior studies (e.g., Cohn and Wardlaw 2016; Caskey and Ozel 2017), I exclude financial firms and regulated utilities (SIC codes 6000-6999 and 4900-4999). For the remaining sample firms, I add CEO equity incentives (Coles et al. 2006) and ability data (Demerjian et al. 2012). Finally, I merge this ExecuComp/Compustat dataset with data on corporate misconduct from Violation Tracker.

Violation Tracker comprises over 408,000 establishment-level violations during the sample period with a minimum penalty of $5,000 each, where 90,048 violations can be linked to 3,297 parent companies.[13] From that data, I keep 58,016 violations that pertain to 1,560 publicly traded firms with non-missing CIK. I further exclude enforcement actions from local and state regulatory agencies to account for regional differences in enforcement and geographic shifts in violation behavior, where, for example, firms caught violating in one state subsequently are less likely to have violations in that state but more likely to violate regulations in other states (e.g., Mattera and Baggaley 2021; Li and Raghunandan 2021; Raghunandan and Ruchti 2022). The remaining Violation Tracker data consist of 52,905 violations perpetrated by 1,521 firms and include enforcement actions brought by more than 50 federal agencies, covering a wide variety of financial, workplace safety, fair employment, and environmental offenses, among others. After aggregating that data to the firm-level and restricting the sample to firms with available data from the ExecuComp/Compustat intersection, my final dataset comprises 28,590 violations by 772 unique firms with a total penalty amount of $57,308.31 million, which, in the cross-section, corresponds to approximately one-fifth of

[12] I use the ExecuComp variables *AnnualCEOFlag* (flag variable indicating that a manager served as CEO for a given firm for all or most of the indicated fiscal year) and *AnnualTitle* (string variable that includes the title(s) of a manager as listed in the historical proxy statement for the indicated fiscal year), reported tenure, and information provided by Gentry et al. (2021) to identify managers as CEO in a given firm-year.

[13] Establishments include a firm’s distinct physical locations, e.g., branch offices, stores, factories, or mines.

these firms' annual mean profits over the sample period.[14] Details on the sample selection are provided in Table 1.

Throughout my study, I use two different samples of firms to investigate corporate misconduct. The full sample covers all 2,305 firms from the ExecuComp/Compustat intersection with available CEO equity incentive and ability data, including a large number of firms without violations. Given that coverage in Violation Tracker (e.g., Russell 3000 and S&P 500 firms) is greater than in ExecuComp (S&P 1500 firms), non-violation firms are likely "true zeros" (e.g., Raghunandan 2021), i.e., their omission from Violation Tracker suggests that they did not perpetrate any misconduct over the sample period rather than representing false negatives, and I run my primary analysis using the full sample. However, to address the possibility of limited scope in Violation Tracker and mitigate the concern that systematic differences between violation and non-violation firms drive my results, I also perform within-firm analysis using the at-risk sample, which is restricted to include those 772 firms with at least one violation during the sample period (e.g., Heese and Pérez-Cavazos 2020; Heese et al. 2021).[15] As seen in Table 1, the unconditional probability for corporate misconduct in the full sample is 22.61 percent, where the average firm engages in 1.2 violations per year, with a mean penalty amount of $2.33 million. Consequently, the sample averages increase for at-risk firms, where the observed mean probability of corporate misconduct is 47.10 percent, and the average firm engages in 2.4 violations per year, with a mean penalty amount of $4.86 million.

[Table 1 about here]

[14] The evident gap between the Violation Tracker data and my final dataset mainly stems from differences in each database's scope and the sampling procedure. ExecuComp covers larger S&P 1500 firms, whereas Violation Tracker includes enforcement actions brought against firms in the Fortune 1000, Fortune Global 500, S&P 500, and Russell 3000 universe, among others. In addition, in line with prior literature, I exclude financial and regulated firms, which stand out both in terms of number of violations and dollar amount of penalties (e.g., Mattera 2016).

[15] Note that Cadman, Klasa, and Matsunaga (2010) document systematic differences between firms included in the ExecuComp database and non-ExecuComp firms, which can substantially affect empirical results. Hence, I acknowledge that my findings may not generalize to corporate misconduct in a broader sample of firms.

3.2. Empirical Models

I examine CEO effects on corporate misconduct in an OLS regression framework. I begin my analysis with a set of non-directional tests, investigating to what extent corporate misconduct varies with individual CEOs. Following recent studies (e.g., Graham et al. 2012; Davidson et al. 2019; Wells 2020), I apply the connectedness method (Abowd et al. 1999) to test whether CEO fixed effects help explain variation in misconduct across firms after controlling for known determinants, firm fixed effects, and year fixed effects. Like the mobility method (Betrand and Schoar 2003), the connectedness method exploits the movement of managers across firms over time. However, it extends the mobility method by leveraging managerial mobility to back out information about non-moving managers employed by firms with at least one moving manager. Thus, while the population of firms is the same as for the mobility method, the population of managers that can be studied is extended to include both moving and non-moving managers.[16] Using the connectedness method, I estimate the following three-way fixed effects model for 149 moving and 477 non-moving CEOs in 272 firms from the full sample (firm, year, and CEO subscripts suppressed):

Equation (1): $y = \varphi + \mu + \theta + \beta_k X_k + \varepsilon$

where the dependent variable (y) substitutes for the different measures for corporate misconduct used in my study. I start with a linear probability model to test CEO effects on the likelihood that firms engage in corporate misconduct, using an indicator that takes the value of one for firm-years with at least one violation in Violation Tracker (*Violation*) as the dependent variable. To investigate the frequency and severity of corporate misconduct, I also use the natural logarithm of one plus the number of violations per firm-year (*#Violations*) and the

[16] Abowd et al. (1999) formally prove that connectedness is necessary and sufficient for the separate identification of manager and firm fixed effects, while mobility is sufficient but not a necessary condition. Graham et al. (2012) provide a more detailed discussion of the connectedness method and its econometric properties.

natural logarithm of one plus total penalties for violations per firm-year (*$Penalties*) as dependent variables (e.g., Heese and Pérez-Cavazos 2020; Raghunandan 2021).

In equation (1), there are three sets of fixed effects: firm fixed effects (φ), year fixed effects (μ), and CEO fixed effects (θ). Firm fixed effects are estimated using within-transformation to account for unobserved time-invariant or slow-moving firm characteristics that might affect corporate misconduct (e.g., corporate culture, differences in economic environment, or industry-specific regulations). Year fixed effects are added by including a dummy variable for each fiscal year throughout the sample period to account for time-varying cross-sectional effects that are common to all firms (e.g., regulatory changes, strength of enforcement, or changes in macroeconomic conditions). CEO fixed effects are estimated using the least squares dummy variable approach within the framework of the connectedness method, capturing commonalities in corporate misconduct associated with a given CEO that are distinct from (1) the average misconduct over time for a given firm, (2) the average misconduct across firms for a given year, and (3) misconduct driven by observed firm characteristics.

Following related studies (e.g., Cohn and Wardlaw 2016; Caskey and Ozel 2017; Xu and Kim 2022), the vector of time-varying firm characteristics (X_k) includes the following controls: the natural logarithm of one plus total assets (*Size*) to account for greater complexity and regulatory scrutiny of larger firms; the ratio of book value of total debt to total assets (*Leverage*) to capture the effects of financing constraints and debt covenants; the ratio of cash and cash equivalents to total assets (*Cash/Assets*) to control for firms' internal capacity to finance investments; the ratio of net property, plant, and equipment to total assets (*PPE/Assets*) to account for firms' tangible investments and capital intensity of production; current period sales divided by beginning total assets (*Turnover*) to account for increased workloads and excess production in firms with strong sales (growth); current period capital expenditures divided by beginning total assets (*Capex/Assets*) as these activities maintain or increment firms' tangible assets by definition; the ratio of market value of common equity to book value of common equity (*M/B*) to capture optimism in firms' growth expectations reflected in stock market valuations, where firms may cut investments and withhold cash to support growth; and current period income before extraordinary items divided by beginning total assets (*ROA*) as firms may also cut spending on, for example, employee treatment or environmental issues with deteriorating firm performance. I winsorize these controls at the 1st/99th percentiles to reduce the possible influence of outliers. Note that firm fixed effects capture the baseline of misconduct at each firm. Thus, the estimated coefficients on observed firm characteristics pick up their incremental effect on corporate misconduct from within-firm variation, relative to firms' typical level of misconduct. Standard errors are clustered at the firm-level to account for serial correlation, and all right-hand side variables (including the CEO dummy variables) are lagged by one year. Details on the definition of all variables are provided in the Appendix.

In the first part of my analysis, estimating equation (1) picks up much of the relevant manager-specific variation in the firm behavior of interest. However, this analysis does not speak to the signs of CEO effects (e.g., Fee et al. 2013). Next, I examine whether certain CEOs are associated with an increase or decrease in corporate misconduct. Note that CEO fixed effects capture fairly sticky, i.e., time-invariant or slow-moving, differences across CEOs and are often interpreted as their intrinsic ability (e.g., Graham et al. 2012; Demerjian et al. 2013; Gormley and Matsa 2014). Therefore, in a set of directional tests, I investigate the relation between CEO ability and corporate misconduct. Using both the full and the at-risk samples of firms, I estimate the following model applying standard fixed effects approaches (firm, year, and CEO subscripts suppressed):

Equation (2): $y = \varphi + \mu + \beta_1 CEO\_Ability + \beta_2 CEO\_Delta + \beta_3 CEO\_Vega + \beta_k X_k + \varepsilon$

where I replace CEO fixed effects by the managerial ability measure (*CEO_Ability*) developed by Demerjian et al. (2012), based on CEOs' efficiency in generating revenues from corporate resources.[17] In a first-stage estimation, the authors apply data envelopment analysis (DEA) to generate a measure of total firm efficiency within each industry, using several revenue-generating inputs (e.g., purchased assets, operating leases, R&D expenditures, and cost of goods sold) to form an efficient frontier. The most efficient firms operating on the frontier are assigned a value of one. The closer the value to zero, the further a firm operates from the frontier, suggesting that it would need to reduce costs or increase revenues to achieve efficiency. Following, managerial ability is calculated as the residual from a second-stage regression of total firm efficiency on a number of firm characteristics driving efficiency (e.g., size, market share, free cash flow, and business complexity), estimated by industry, with higher values indicating more able managers.[18]

Demerjian et al. (2012) perform various validity tests and provide evidence that their measure outperforms alternative ability measures, e.g., media citations or historical stock returns, and recent studies use the measure from Demerjian et al. (2012) to examine the effect of managerial ability on firms' accounting quality and earnings properties, credit ratings, and tax avoidance, among others (e.g., Demerjian et al. 2013; Bonsall IV et al. 2017; Koester et al. 2017). However, to address concerns about construct validity and measurement error bias in the fixed effects estimation (e.g., Whited, Swanquist, Shipman, and Moon 2021; Jennings, Kim, Lee, and Taylor 2022), I also report results using the skill measure (*CEO_Skill*) from Daniel et al. (2020) for the period 2000-2015, based on CEOs' success in generating excess

[17] Demerjian et al. (2012) point out that their ability measure likely captures the effect of the top management team but focus on the CEO throughout their tests, given that the CEO is the most powerful top-level executive and thus the most likely to affect firm behavior. Therefore, following prior studies (e.g., Baik et al. 2011; Bonsall IV et al. 2017; Yuan et al. 2019), I attribute managerial ability to the CEO.

[18] Details on DEA's optimization procedure and the calculation of the managerial ability measure are provided in Demerjian et al. (2012). Throughout my study, I use the residuals from this second-stage regression as an independent variable when estimating equation (2). While CEO ability is still subject to measurement error, this approach is less problematic than using the residual from one regression as the dependent variable in another regression (e.g., Chen, Hribar, and Melessa 2018).

stock returns, relative to their industry peers. Following prior research (e.g., Akey and Appel 2021; Raghunandan 2021), to disentangle the effects of managerial ability and risk-taking, I augment these measures with variables capturing CEO pay-performance sensitivity from equity portfolio delta (*CEO_Delta*) and risk-taking incentives provided by vega (*CEO_Vega*) as calculated by Core and Guay (2002), obtained from Coles et al. (2006). I winsorize the CEO characteristics at the 1st/99th percentiles. Again, standard errors are clustered at the firm-level, and all right-hand side variables are measured one year prior to the measure of corporate misconduct.

3.3. Descriptive Statistics

Figure 1 plots the time series of the number of violations and penalties over the sample period. The number of violations in my sample is scattered through time, with 2007-2012 being the period with the highest number of violations (39.06 percent of total). In terms of penalties, violations are also scattered through time, however, 2009 and 2015 are the years with the highest amounts of penalties (10.70 and 16.58 percent of total, respectively). These peaks are driven by two high-profile Food and Drug Administration (FDA) enforcement actions against off-label or unapproved promotion of medical products and one enforcement action by the EPA for environmental cleanups and claims, accounting for 60.76 and 54.19 percent of annual penalties.[19] Throughout my study, I use a log-specification to estimate CEO effects on the frequency and severity of corporate misconduct. However, to further mitigate the impact of extreme observations in the distribution of my dependent variables, I winsorize *#Violations* and *$Penalties* at the 99th percentile in all tests.

[Figure 1 about here]

[19] In 2009, Pfizer had to pay $2.3 billion and Eli Lilly $1.4 billion for marketing drugs for uses not approved as safe. Anadarko Petroleum, which was acquired by Occidental Petroleum in 2019, had to pay $5.15 billion for toxic dumping in 2015.

Table 2 presents a breakdown of the number of violations and penalties by offense, where Panel A provides a summary of the different offense types used in my study. In terms of non-financial misconduct, my sample comprises a variety of workplace safety, environmental, and fair employment offenses. Workplace safety offenses affect the welfare of the most vulnerable group of rank-and-file employees through lack of safe and healthy working conditions that can cause injuries and illnesses, income losses, or, in extreme cases, permanent disabilities and fatalities, including enforcement actions by the OSHA, MSHA, and the Federal Railroad Administration (FRA), among others. Environmental offenses impose negative externalities on society and public health (e.g., Shive and Forster 2020; Akey and Appel 2021; Xu and Kim 2022), e.g., in the form of wastewater discharge, toxic releases, and greenhouse gas emissions, where approximately 95 percent of enforcement actions are brought by EPA. Fair employment offenses refer to more egregious cases of employee mistreatment and labor rights violations, e.g., underpayment and wage theft, illegal actions to discourage unionization, or discrimination, enforced by the WHD, the National Labor Relations Board (NLRB), and the Equal Employment Opportunity Commission (EEOC). On the other hand, financial misconduct affects the welfare of strong stakeholders, e.g., shareholders, creditors, or the government, and comprises enforcement actions by the SEC, DOJ as second federal securities laws enforcer, and U.S. Attorneys' Offices (USAO) as chief federal law enforcement officers within a particular jurisdiction, where offenses include, for example, accounting fraud, investor protection, tax evasion, and cases filed under the False Claims Act.

In terms of the number of violations, workplace safety offenses represent the most common offense type (68.78 percent of total). However, in terms of penalties, this offense type is much less prevalent (5.22 percent of total). Conversely, financial misconduct occurs less often (1.91 percent of total) but accounts for most penalties (39.47 percent of total). Other offenses cover a number of healthcare and consumer protection violations, where penalties are driven by a few enforcement actions against off-label or unapproved promotion of medical products brought by the FDA. On a more granular level, Panel B of Table 2 provides a summary of the primary offenses prosecuted by the different federal agencies as described in Violation Tracker. Similarly, in terms of the number of violations, workplace safety or health violations are most prevalent (32.06 percent of total), whereas environmental and False Claims Act violations stand out in terms of penalty amount (21.71 and 25.84 percent of total, respectively).

[Table 2 about here]

Table 3 provides summary statistics for firm and CEO characteristics in the full and the at-risk samples, which are consistent with those in prior studies (e.g., Cohn and Wardlaw 2016; Caskey and Ozel 2017; Bradley et al. 2021). Compared to firms without violations during the sample period, at-risk firms are, on average, larger and more financially constrained, hold less cash and cash equivalents, have higher capital intensities and turnover, and are more profitable, where CEOs in these firms have stronger equity incentives from portfolio delta and vega. On the other hand, Table 3 indicates no difference in the observed mean CEO ability between violation and non-violation firms. However, to circumvent the concern that unobserved differences affect my results, I also report results using the at-risk sample of firms.

[Table 3 about here]

4. Results

4.1. Non-Directional Tests

I start with a set of non-directional tests to examine whether corporate misconduct varies with individual CEOs. Table 4 provides regression results from estimating equation (1), applying the connectedness method to the full sample. In column (1) of Panel A, using *Violation* as the dependent variable, I find a statistically significant CEO effect on the probability of corporate misconduct conditional on observed firm characteristics, firm fixed effects, and year fixed effects. The F-statistic of 3.28 (p-value <0.001) indicates that the estimated CEO fixed effects are jointly different from zero, while the critical value at the 95th percentile of the F-distribution is around 1.11. Similarly, using *#Violations* and *$Penalties* as dependent variables, I find statistically significant CEO fixed effects on the frequency and severity of corporate misconduct in columns (2) and (3) of Panel A, with F-statistics of 7.73 and 3.51 (p-values <0.001), respectively. Across the different specifications, CEO fixed effects also provide substantial explanatory power in terms of (adjusted) R^2, ranging from 63.82 (51.28) percent in column (3) to 80.93 (74.32) percent in column (2).

Panel B further examines the economic magnitude of the estimated CEO effects. Following Graham et al. (2012), I decompose the model R^2 to separate out the relative importance of CEO fixed effects in explaining corporate misconduct, using the estimation results from columns (1) to (3) in Panel A. Note that the model R^2 can be calculated from the variance-covariance matrix as follows (firm, year, and CEO subscripts suppressed):

Equation (3): $$R^2 = \frac{cov\,(\varphi,y)}{var\,(y)} + \frac{cov\,(\mu,y)}{var\,(y)} + \frac{cov\,(\theta,y)}{var\,(y)} + \frac{cov\,(\beta_k X_k,y)}{var\,(y)}$$

where y represents the dependent variable, and the normalized covariances for firm fixed effects (φ), year fixed effects (μ), CEO fixed effects (θ), and the vector of time-varying firm characteristics (X_k) indicate how much each set of variables contributes to the variation in the dependent variable. Following, $\frac{cov\,(\theta,y)}{var\,(y)}$ can be interpreted as partial explanatory power of CEO fixed effects for the variation in corporate misconduct. Column (1) of Panel B shows that CEO fixed effects account for 57.43 percent of the explained variation in the probability of corporate misconduct, whereas observed firm characteristics and year fixed effects account for 8.44 percent, and firm fixed effects explain 34.13 percent. Again, the results hold for the frequency and severity of corporate misconduct in columns (2) and (3) of Panel B, where

CEO fixed effects contribute 55.79 and 65.02 percent to the explained variation in *#Violations* and *$Penalties*, respectively. Taken together, the results in Table 4 suggest statistically and economically significant CEO effects on corporate misconduct, which provides support for H1.

[Table 4 about here]

Next, I examine how CEOs affect different offense types. Table 5 provides regression results from estimating equation (1) separately for financial, workplace safety, fair employment, and environmental offenses, using *Violation* as the dependent variable. In column (1) of Panel A, I find a statistically significant CEO effect on the probability of financial offenses as indicated by the F-statistic of 1.61 (p-value <0.001). However, the (adjusted) R^2 of 32.65 (9.32) percent indicates a relatively low explanatory power in the fixed effects estimation, suggesting that CEO effects are less pronounced for firms' financial misconduct. Untabulated tests reinforce this notion, where I fail to find statistically and economically significant CEO effects when financial offenses are restricted to accounting fraud and other securities laws violations prosecuted by the SEC or DOJ. On the other hand, in terms of non-financial misconduct, I find statistically significant CEO fixed effects on the probability of workplace safety, fair employment, and environmental offenses in columns (2) to (4) of Panel A, with F-statistics of 3.34, 1.90, and 4.12 (p-values <0.001), respectively. In addition, the estimated CEO fixed effects are accompanied by considerable explanatory power across the different specifications, where the (adjusted) R^2 ranges from 43.14 (24.00) percent in column (3) to 63.57 (50.94) percent in column (4).

Panel B investigates specific patterns in the observed violation behavior of CEOs. Therefore, I correlate the CEO fixed effects estimated from the regressions in columns (1) to (4) of Panel A across the different offense types. Note that the CEO fixed effects are estimated relative to a benchmark and normalized to mean zero. However, they are noisy coefficient estimates, which makes them susceptible to outliers. To account for measurement error and non-linearities between the CEO fixed effects, I use Spearman rank correlations (bottom) as my primary measure in the correlation analysis. However, I also report Pearson correlations (top), where I winsorize the CEO fixed effect estimates at the 1st/99th percentiles to reduce the influence of extreme values in the tails of their distribution.[20] The results in column (1) of Panel B indicate negative Spearman correlations in the probability of financial and non-financial misconduct, where the correlations between financial and workplace safety offenses, as well as financial and environmental offenses, are statistically significant. Using Pearson correlations, environmental offenses also show a significantly negative correlation with financial misconduct. On the other hand, I find significantly positive Spearman correlations among workplace safety, fair employment, and environmental offenses in columns (2) and (3) of Panel B, suggesting that some CEOs accelerate non-financial misconduct across offense types, where the same holds for Pearson correlations.

[Table 5 about here]

Tables 6 and 7 replicate the analysis for the frequency and severity of the different offense types, using either *#Violations* or *$Penalties* as the dependent variable. Overall, the results comport with those reported in Table 5. For example, I find statistically significant CEO effects on the frequency and severity of financial offenses in column (1) of Panel A in each table, where the relatively low explanatory power in the fixed effects estimations again indicates that CEOs play an economically less relevant role in firms' financial misconduct. However, in terms of non-financial misconduct, I find statistically significant and economically meaningful CEO fixed effects on the frequency and severity of workplace safety, fair employment, and environmental offenses in columns (2) to (4) of Panel A in each table. For the number of violations, Panel B of Table 6 again indicates significantly positive Spearman cor-

[20] The results are robust to using raw CEO fixed effect estimates, winsorizing at different levels, and bootstrapping (with 1,000 iterations) to account for non-normality in the distribution of the correlations (untabulated).

relations for firms' non-financial misconduct, whereas financial and workplace safety offenses exhibit a significantly negative yet small correlation. Similarly, for the dollar amount of penalties, I find positive and statistically significant Spearman correlations among workplace safety, fair employment, and environmental offenses in Panel B of Table 7, while the correlation between financial and workplace safety (environmental) offenses again is significantly negative.

[Tables 6 and 7 about here]

Taken together, the results in Tables 5 to 7 suggest statistically and economically significant CEO effects on different offense types, where the documented CEO fixed effects appear to be especially important in firms' non-financial misconduct. Thus, I interpret the collective evidence in this section as providing additional support for H1. Further analysis of the correlation structure between the estimated fixed effects provides descriptive evidence of overarching patterns in CEOs' violation behavior. I find significantly positive associations among workplace safety, fair employment, and environmental offenses, suggesting that CEOs, at least to some extent, perpetrate non-financial misconduct across offense types. On the other hand, I find negative and often insignificant associations with financial offenses, which indicates differential CEO effects for firms' financial and non-financial misconduct.

4.2. Directional Tests

Having established that CEOs impact corporate misconduct, I next examine the relation between CEO ability and corporate misconduct in a set of directional tests. Table 8 provides regression results from estimating equation (2), where columns (1) to (3) report results using the ability measure (*CEO_Ability*) from Demerjian et al. (2012) in the full sample. Column (1) shows that, on average, CEO ability is positively related to the probability of corporate misconduct, using *Violation* as the dependent variable. In terms of effect size, the coefficient of 0.0677 (p-value 0.004) implies that a one-standard-deviation increase in CEO ability (0.1447; Table 3) is associated with a one-percentage-point increase in the probability of corporate misconduct for the average firm, which is one-fifth of the documented firm size effect. Thus, when CEO ability increases by one standard deviation, the probability of corporate misconduct increases from the average level of 22.61 to 23.61 percent, which is equal to a relative change of 4.4 percent.

Similarly, using *#Violations* and *$Penalties* as dependent variables, columns (2) and (3) show positive relations between CEO ability and the frequency and severity of corporate misconduct, respectively. In the log-level specifications, the coefficient of 0.0787 (p-value 0.009) in column (2) indicates that a one-standard-deviation increase in CEO ability results in an average increase of 1.2 percent in the number of violations, while the coefficient of 0.3382 (p-value 0.022) in column (3) implies a 5 percent increase in the dollar amount of penalties, calculated as ($e^{\beta_1 \, x \, 0.1447} - 1$). In terms of dollar magnitude, given that the mean penalty per firm-year is $2.33 million, this 5 percent increase translates into $116,500 penalties per firm-year for the average firm, amounting to $2.21 million penalties over the sample period.

[Table 8 about here]

Columns (4) and (5) report results from within-firm analysis using the at-risk sample, where the dependent variable is either *#Violations* or *$Penalties*.[21] Column (4) shows that CEO ability is positively related to the frequency of corporate misconduct, where the coefficient of 0.1381 (p-value 0.010) in the log-level specification indicates that a one-standard-deviation increase in CEO ability (0.1528; Table 3) is associated with an increase of 2.1 percent in the number of violations for the average at-risk firm, which is twice as much as in the full sample. In column (5), CEO ability is also positively related to the severity of corporate

[21] I also find a significantly positive relation between CEO ability and the probability of corporate misconduct for at-risk firms (coefficient 0.1169; p-value 0.006), which, in economic terms, is twice as large as in the full sample and again corresponds to one-fifth of the documented firm size effect (untabulated). I therefore focus on the number of violations and dollar amount of penalties for the remainder of my study.

misconduct, and the coefficient of 0.5862 (p-value 0.028) implies that a one-standard-deviation increase in CEO ability results in an increase of 9.4 percent in the dollar amount of penalties. For the average firm, given a mean penalty of $4.86 million per firm-year in the at-risk sample, this corresponds to an increase of $456,840 penalties per firm-year ($8.68 million penalties over the sample period). Hence, in addition to the larger economic effect size, the results comport with those reported in columns (2) and (3), suggesting that my findings cannot be explained away by unobserved differences between violation and non-violation firms as they continue to hold in a smaller sample of at-risk firms, where firm-level and environmental factors are held constant (e.g., Heese and Pérez-Cavazos 2020; Heese et al. 2021).

Finally, to test the construct validity of my measure for CEO ability, columns (6) and (7) report results using the skill measure (*CEO_Skill*) from Daniel et al. (2020) in the full sample, where the sample spans the period 2000-2015. In column (6), using *#Violations* as the dependent variable, the coefficient of 0.0165 (p-value 0.044) indicates a significantly positive relation between CEO skill and the frequency of corporate misconduct. Similarly, using *$Penalties* as the dependent variable, column (7) shows a significantly positive relation with the severity of corporate misconduct, where, for example, the coefficient of 0.0877 (p-value 0.024) implies that a one-standard-deviation increase in CEO skill (0.3237; untabulated) translates into an increase of approximately 3 percent in the dollar amount of penalties for the average firm. Thus, albeit economically weaker, the results are consistent with those reported for the ability measure from Demerjian et al. (2012). More importantly, given that *CEO_Skill* captures investors' perceptions of CEOs reflected in stock market valuations rather than CEO efficiency in generating revenues from certain inputs, my findings are unlikely to be driven by measurement error in the variable of interest (e.g., Whited et al. 2021; Jennings et al. 2022).[22]

[22] In untabulated tests, I find significantly positive yet small Pearson (coefficient 0.0572; p-value <0.001) and Spearman (coefficient 0.0651; p-value <0.001) correlations between *CEO_Ability* and *CEO_Skill*, suggesting that the two measures capture different aspects of a CEO's talents or qualities.

Taken together, the results in Table 8 show a positive relation between CEO ability and corporate misconduct across the different specifications. This relation is both statistically significant and economically meaningful, and it is consistent with the opportunistic view of H2, suggesting that managers trade off the expected costs and benefits of engaging in corporate misconduct, where, on average, more able CEOs face higher net benefits. All findings remain unchanged, within numerical precision range, when I exclude singletons (e.g., Correia 2015; deHaan 2021), requiring at least two observations for each set of fixed effects (untabulated). That said, most of the coefficients on observed firm characteristics are in the same direction as those reported in prior studies. For example, looking at the at-risk sample, corporate misconduct is positively related to firm size and turnover (e.g., Caskey and Ozel 2017; Bradley et al. 2021), and I also find positive associations with a firm's financial constraints and capital intensity (e.g., Cohn and Wardlaw 2016; Xu and Kim 2022). Similarly, in terms of the number of violations, corporate misconduct seems to increase with CEOs' risk-taking incentives provided by their equity portfolio vega (e.g., Akey and Appel 2021; Raghunandan 2021). However, with the exception of firm size and capital intensity, the estimated coefficients are largely insignificant at conventional levels, suggesting that the variables included in the vector of time-varying firm characteristics exhibit little within-firm variation, while cross-sectional variation is cancelled out due to the inclusion of firm fixed effects, which is in line with the findings across different specifications in Cohn and Wardlaw (2016) and Caskey and Ozel (2017), among others.[23]

Next, I focus on differential effects of CEO ability for different offense types. Motivated by the first part of my analysis, Table 9 provides regression results from estimating equation (2) separately for firms' financial and non-financial misconduct in the full sample, where the dependent variable is either *#Violations* or *$Penalties*. In columns (1) and (2), using

[23] In untabulated tests, using the at-risk sample, I find that most of the coefficients on observed firm characteristics are in the expected direction and, except for a firm's market-to-book ratio and return on assets, also statistically significant at the 5% level or better when estimating equation (2) without firm fixed effects.

the measure from Demerjian et al. (2012), I fail to find a statistically significant effect of CEO ability on the frequency and severity of financial misconduct, where the coefficient on *CEO_Ability* in column (2) is also in the opposite direction. The same holds for the measure from Daniel et al. (2020) in columns (3) and (4), where the coefficients associated with *CEO_Skill* show the expected sign, however, the documented effects are economically weak and insignificant at conventional levels. Thus, the results comport with those from my set of non-directional tests reported in Tables 5 to 7, and they are also consistent with those in Demerjian et al. (2013) and Wells (2020), where the authors fail to find a significant relation between managerial ability and within-firm variation in accounting quality and earnings properties.

On the other hand, using the measure from Demerjian et al. (2012), columns (5) and (6) show significantly positive relations between CEO ability and the frequency and severity of non-financial misconduct, where the coefficients on *CEO_Ability* are similar or economically larger compared to those reported in columns (2) and (3) of Table 8. Again, the same holds for the measure from Daniel et al. (2020) in columns (7) and (8), where the coefficients associated with *CEO_Skill* are positive and statistically significant at the 10% level. Taken together, the results in Table 9 show a positive relation between CEO ability and non-financial misconduct that is both statistically and economically significant, corroborating the opportunistic view of H2. In addition, the results are consistent with the notion that CEOs differentially affect firms' financial and non-financial misconduct given that non-financial misconduct increases with CEO ability, while there is no association with financial misconduct. These findings are in line with the survey evidence in Graham et al. (2005), suggesting that managers prefer taking real actions, and prior studies that document a shift toward real activities manipulation in recent years (e.g., Cohen et al. 2008; Zang 2012).

[Table 9 about here]

One remaining concern is that my findings could be driven by endogenous matching of CEOs and firms, where CEOs self-select into certain corporate environments or firms replace CEOs to meet their time-varying needs (e.g., Fee et al. 2013; Pan 2017). In the last part of my analysis, to alleviate selection concerns, I exploit variation in the underlying reasons for CEO departures based on the classification from Gentry et al. (2021). Therefore, I assume CEO departures that involve sudden death, health problems, or voluntary retirements more likely to be exogenous than merely reflecting a firm's endogenous choice to hire a different CEO (i.e., forced CEO turnover). Table 10 provides regression results from estimating equation (2) for firms' non-financial misconduct in the full sample, where the sample is restricted to consecutive years of CEO tenure following turnover events that can be viewed as largely exogenous. The dependent variable is either *#Violations* or *$Penalties*. Despite reduced statistical power due to the smaller sample size, columns (1) and (2) again show significantly positive relations between CEO ability and the frequency and severity of non-financial misconduct, using the measure from Demerjian et al. (2012). The same holds for the skill measure from Daniel et al. (2020) in columns (3) and (4). Across the different specifications, the coefficients on *CEO_Ability* and *CEO_Skill* are similar or economically larger compared to those reported in columns (5) to (8) of Table 9. While it is important to note that my research design cannot rule out selection concerns, given that CEOs are not allocated randomly across firms, the results in Table 10 suggest that my findings are unlikely to be driven by endogenous matching of CEOs and firms as they continue to hold in a setting where the matching issue is arguably less problematic.[24]

[Table 10 about here]

[24] For example, even in the event of sudden death only CEO departure is clearly exogenous, while CEO succession is still endogenous. Thus, given that a firm's replacement choices are unobservable, I cannot rule out potential selection issues or alternative explanations.

5. Conclusion

This study examines CEO effects on corporate misconduct. First, in a set of non-directional tests, I investigate whether and to what extent corporate misconduct varies with individual CEOs. I find significant CEO fixed effects on the likelihood, frequency, and severity of corporate misconduct. These findings hold for financial, workplace safety, fair employment, and environmental offenses; however, the documented CEO effects matter more for some offense types than others and appear to be especially important in firms' non-financial misconduct. By correlating the fixed effect estimates across offense types, I find some evidence of overarching patterns in the observed violation behavior, suggesting that CEOs differentially affect financial and non-financial misconduct. Second, in a set of directional tests, I examine the relation between CEO ability and corporate misconduct. Consistent with the idea that more able CEOs face higher net benefits of engaging in corporate misconduct, I find that, on average, corporate misconduct increases with CEO ability. In terms of effect heterogeneity, considering that managers prefer taking real actions (e.g., Graham et al. 2005), I find a significantly positive relation between CEO ability and non-financial misconduct, while I fail to find a significant relation between CEO ability and financial misconduct. These findings hold in within-firm analysis using a sample of at-risk firms, holding firm-level and environmental factors constant, and they continue to hold in a setting where endogenous matching of CEOs and firms is arguably less prevalent. Besides the contributions to the literature, given that higher CEO ability can have negative externalities for employee welfare and society and public health, my findings can inform nomination and compensation committees, investors, and regulators, among others.

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Tables and Figures

TABLE 1

Sample Selection

This table presents the sample selection for the period 2000-2018. The full sample covers all firms from the ExecuComp/Compustat intersection with available CEO equity incentive (Coles, Daniel, and Naveen 2006) and ability data (Demerjian, Lev, and McVay 2012). The at-risk sample is restricted to include all firms with at least one violation in Violation Tracker over the sample period. Details on the sample composition are provided in section 3.

	Firm-years	Firms	CEOs	Firm-years with Violation (%)	Number of Violations	Penalties (in $m)
ExecuComp firms with available CEO data	37,092	3,226	6,642			
Less: Missing Compustat data	(3,493)	(173)	(428)			
Less: Financial and regulated firms (SIC codes 6000-6999 & 4900-4999)	(6,098)	(593)	(1,086)			
Less: Missing equity incentive data	(2,662)	(143)	(394)			
Less: Missing ability data	(262)	(12)	(34)			
Full sample	24,577	2,305	4,700	5,556 (22.61)	28,590	57,308.31
Less: Firms not covered in Violation Tracker	(12,781)	(1,533)	(2,764)	(0)	(0)	(0)
At-risk sample	11,796	772	1,936	5,556 (47.10)	28,590	57,308.31

FIGURE 1

Number of Violations and Penalties by Year

This figure shows the distribution of the total number of violations (28,590) and penalties ($57,308.31 million) in my sample over the period 2000-2018 by year.

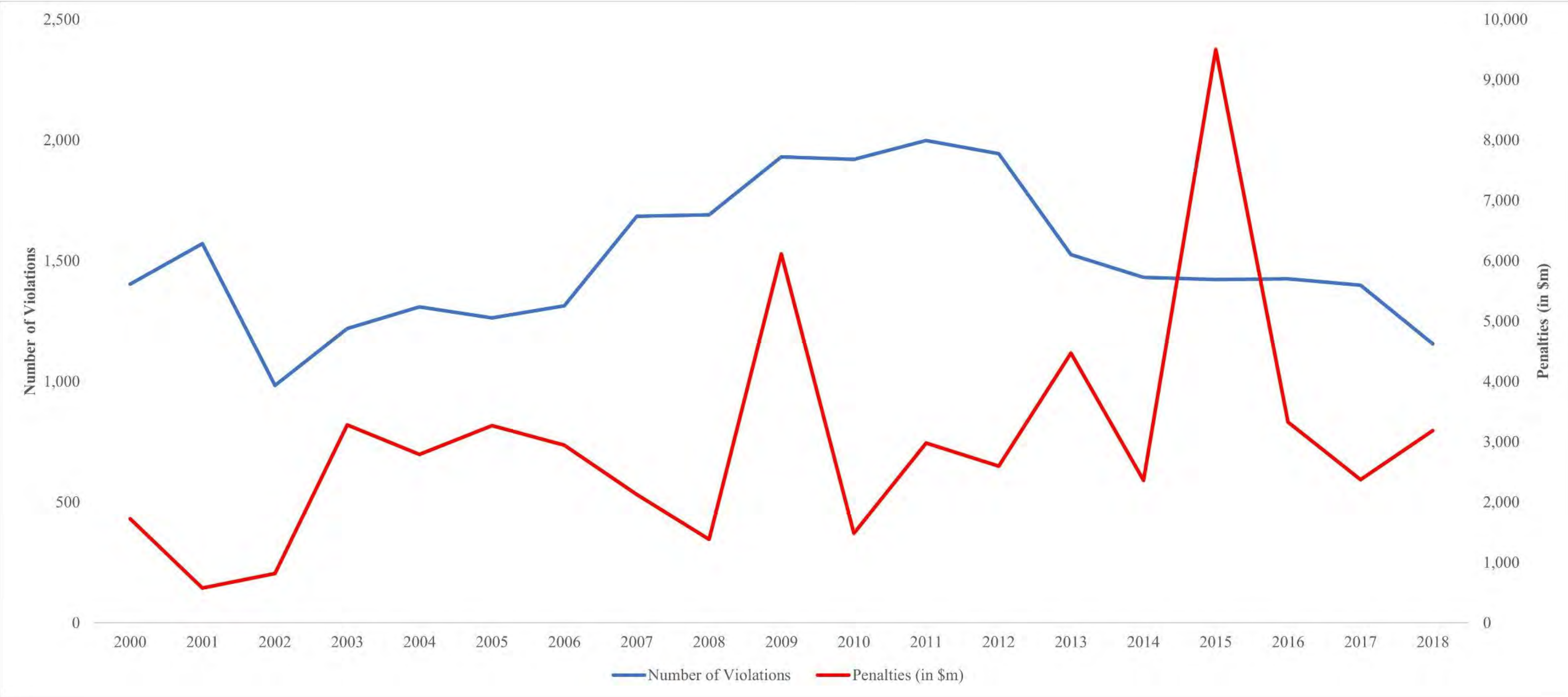

TABLE 2

Number of Violations and Penalties by Offense

This table presents a breakdown of the total number of violations and penalties in my sample by offense type (Panel A) and primary offense (Panel B) for the period 2000-2018.

Panel A: Violations by Offense Type

	Number of Violations	% of Total	Penalties (in $m)	% of Total
Workplace safety	19,665	68.78	2,992.15	5.22
Environmental	4,762	16.66	12,446.10	21.72
Fair employment	3,169	11.08	7,402.61	12.92
Financial	547	1.91	22,617.83	39.47
Other	447	1.57	11,849.62	20.67
Total	28,590	100.00	57,308.31	100.00

Panel B: Violations by Primary Offense

	Number of Violations	% of Total	Penalties (in $m)	% of Total
Workplace safety or health violation	9,165	32.06	141.50	0.25
Railroad safety violation	7,585	26.53	84.39	0.15
Environmental violation	4,629	16.19	12,439.20	21.71
Aviation safety violation	2,690	9.41	232.51	0.41
Wage and hour violation	1,339	4.68	3,087.11	5.39
Labor relations violation	1,033	3.61	182.84	0.32
Employment discrimination	474	1.66	1,272.37	2.22
False Claims Act and related	271	0.95	14,810.50	25.84
Benefit plan administrator violation	152	0.53	2,733.40	4.77
Export control violation	147	0.51	349.82	0.61
Other	1,105	3.86	21,974.67	38.33
Total	28,590	100.00	57,308.31	100.00

TABLE 3

Summary Statistics

This table provides summary statistics for the full sample and the at-risk sample over the period 2000-2018. Differences in mean (median) compare at-risk firms with all non-violation firms in the full sample. Bold values denote statistical significance at the 10% level or better based on two-tailed t-tests on the equality of means (Wilcoxon/Chi-squared tests on the equality of medians). Details on the definition of all variables are provided in the Appendix.

	Full sample				**At-risk sample**					
	N	Mean	Median	SD	N	Mean	Median	SD	Δ Mean	Δ Median
CEO_Ability	24,577	0.0091	-0.0284	0.1447	11,796	0.0092	-0.0347	0.1528	0.0001	**-0.0107**
CEO_Delta	24,577	5.2399	5.3000	1.6784	11,796	5.5340	5.5918	1.6358	**0.5655**	**0.5491**
CEO_Vega	24,577	3.5759	3.8765	1.9168	11,796	3.7834	4.1956	2.0536	**0.3989**	**0.5483**
Size	24,577	7.3316	7.2112	1.5672	11,796	7.9703	7.8364	1.5103	**1.2282**	**1.1994**
Leverage	24,577	0.2227	0.2044	0.1923	11,796	0.2458	0.2293	0.1815	**0.0444**	**0.0598**
Cash/Assets	24,577	0.1652	0.1009	0.1736	11,796	0.1216	0.0758	0.1313	**-0.0839**	**-0.0669**
PPE/Assets	24,577	0.2569	0.1882	0.2177	11,796	0.2845	0.2248	0.2132	**0.0530**	**0.0703**
Turnover	24,577	1.2208	1.0248	0.7968	11,796	1.2877	1.0880	0.8211	**0.1287**	**0.1230**
Capex/Assets	24,577	0.0575	0.0371	0.0618	11,796	0.0581	0.0393	0.0584	0.0011	**0.0042**
M/B	24,577	3.1839	2.3167	4.1732	11,796	3.2065	2.4112	3.9915	0.0433	**0.2009**
ROA	24,577	0.0491	0.0554	0.1081	11,796	0.0621	0.0615	0.0836	**0.0249**	**0.0130**

TABLE 4

CEO Fixed Effects on Corporate Misconduct

Panel A of this table provides regression results applying the connectedness method to the full sample, where the sample spans the period 2000-2018. The dependent variable in columns (1) to (3) is *Violation*, *#Violations*, and *$Penalties*, respectively. "Yes" indicates that a set of variables is included in the regression. Coefficient estimates are not reported in this table, but all specifications include corrections for robust standard errors that are clustered at the firm-level. The F-statistics test the exclusion restriction that CEO fixed effects are jointly different from zero. Panel B decomposes the model R^2 to separate out the relative importance of each set of variables in explaining corporate misconduct, using the estimation results from Panel A. Details on the definition of all variables are provided in the Appendix.

Panel A: Regression Results

	Dependent Variable =		
	Violation	***#Violations***	***$Penalties***
	(1)	**(2)**	**(3)**
Firm Characteristics	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
CEO FE	Yes	Yes	Yes
Testing CEO FE = 0			
F-statistics	3.28	7.73	3.51
(p-value)	(<0.001)	(<0.001)	(<0.001)
R^2	0.6459	0.8093	0.6382
Adj. R^2	0.5231	0.7432	0.5128
N	3,137	3,137	3,137

Panel B: Decomposition of Model R^2

	(1)	**(2)**	**(3)**
Firm Characteristics & Year FE	8.44%	4.50%	4.46%
Firm FE	34.13%	39.71%	30.52%
CEO FE	57.43%	55.79%	65.02%

TABLE 5

CEO Fixed Effects on Probability of Different Offense Types

Panel A of this table provides regression results by offense type applying the connectedness method to the full sample, where the sample spans the period 2000-2018. The dependent variable is *Violation*. Columns (1) to (4) report results for financial, workplace safety, fair employment, and environmental offenses, respectively. "Yes" indicates that a set of variables is included in the regression. Coefficient estimates are not reported in this table, but all specifications include corrections for robust standard errors that are clustered at the firm-level. The F-statistics test the exclusion restriction that CEO fixed effects are jointly different from zero. Panel B presents Spearman (bottom) and Pearson (top) correlations among the CEO fixed effect estimates, using the estimation results from Panel A. Bold values denote statistical significance at the 10% level or better (two-tailed). Details on the definition of all variables are provided in the Appendix.

Panel A: Regression Results

	Dependent Variable = *Violation*			
	Financial	**Workplace safety**	**Fair employment**	**Environmental**
	(1)	**(2)**	**(3)**	**(4)**
Firm Characteristics	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
CEO FE	Yes	Yes	Yes	Yes
Testing CEO FE = 0				
F-statistics	1.61	3.34	1.90	4.12
(p-value)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
R^2	0.3265	0.6137	0.4314	0.6357
Adj. R^2	0.0932	0.4836	0.2400	0.5094
N	3,137	3,137	3,137	3,137

Panel B: Correlations

	(1)	**(2)**	**(3)**	**(4)**
Financial		**0.2460**	**0.3544**	**-0.0703**
Workplace safety	**-0.2752**		**0.3475**	**0.5354**
Fair employment	-0.0043	**0.1820**		**0.2922**
Environmental	**-0.0816**	**0.2411**	**0.2392**	

TABLE 6

CEO Fixed Effects on Frequency of Different Offense Types

Panel A of this table provides regression results by offense type applying the connectedness method to the full sample, where the sample spans the period 2000-2018. The dependent variable is *#Violations*. Columns (1) to (4) report results for financial, work-place safety, fair employment, and environmental offenses, respectively. "Yes" indicates that a set of variables is included in the regression. Coefficient estimates are not reported in this table, but all specifications include corrections for robust standard errors that are clustered at the firm-level. The F-statistics test the exclusion restriction that CEO fixed effects are jointly different from zero. Panel B presents Spearman (bottom) and Pearson (top) correlations among the CEO fixed effect estimates, using the estimation results from Panel A. Bold values denote statistical significance at the 10% level or better (two-tailed). Details on the definition of all variables are provided in the Appendix.

Panel A: Regression Results

	Dependent Variable = #*Violations*			
	Financial	**Workplace safety**	**Fair employment**	**Environmental**
	(1)	**(2)**	**(3)**	**(4)**
Firm Characteristics	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
CEO FE	Yes	Yes	Yes	Yes
Testing CEO FE = 0				
F-statistics	1.50	7.04	2.27	5.49
(p-value)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
R^2	0.3217	0.7718	0.4760	0.7162
Adj. R^2	0.0867	0.6928	0.2944	0.6206
N	3,137	3,137	3,137	3,137

Panel B: Correlations

	(1)	**(2)**	**(3)**	**(4)**
Financial		**0.2030**	**0.3775**	-0.0328
Workplace safety	**-0.0696**		**0.3010**	**0.5151**
Fair employment	0.0226	**0.1522**		**0.2879**
Environmental	0.0045	**0.1000**	**0.2379**	

TABLE 7

CEO Fixed Effects on Severity of Different Offense Types

Panel A of this table provides regression results by offense type applying the connectedness method to the full sample, where the sample spans the period 2000-2018. The dependent variable is *$Penalties*. Columns (1) to (4) report results for financial, workplace safety, fair employment, and environmental offenses, respectively. "Yes" indicates that a set of variables is included in the regression. Coefficient estimates are not reported in this table, but all specifications include corrections for robust standard errors that are clustered at the firm-level. The F-statistics test the exclusion restriction that CEO fixed effects are jointly different from zero. Panel B presents Spearman (bottom) and Pearson (top) correlations among the CEO fixed effect estimates, using the estimation results from Panel A. Bold values denote statistical significance at the 10% level or better (two-tailed). Details on the definition of all variables are provided in the Appendix.

Panel A: Regression Results

	Dependent Variable = *$Penalties*			
	Financial	**Workplace safety**	**Fair employment**	**Environmental**
	(1)	**(2)**	**(3)**	**(4)**
Firm Characteristics	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
CEO FE	Yes	Yes	Yes	Yes
Testing CEO FE = 0				
F-statistics	1.78	4.35	1.70	4.70
(p-value)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
R^2	0.3523	0.6594	0.3947	0.6598
Adj. R^2	0.1279	0.5446	0.1850	0.5452
N	3,137	3,137	3,137	3,137

Panel B: Correlations

	(1)	**(2)**	**(3)**	**(4)**
Financial		**0.3478**	**0.3782**	-0.0164
Workplace safety	**-0.2643**		**0.4237**	**0.4829**
Fair employment	-0.0507	**0.2749**		**0.2899**
Environmental	**-0.0906**	**0.2826**	**0.1482**	

TABLE 8

CEO Ability and Corporate Misconduct

This table provides regression results from estimating the relation between CEO ability and corporate misconduct. Columns (1) to (3) report results for the full sample over the period 2000-2018 using the ability measure from Demerjian, Lev, and McVay (2012), where the dependent variable is *Violation*, *#Violations*, and *$Penalties*, respectively. Columns (4) and (5) report results for the at-risk sample over the same period, and columns (6) and (7) report results for the full sample over the period 2000-2015 using the skill measure from Daniel, Li, and Naveen (2020), where the dependent variable is either *#Violations* or *$Penalties*. P-values based on robust standard errors that are clustered at the firm-level are reported in parentheses below the coefficient estimates. All specifications include year and firm fixed effects. The results in columns (1) to (3), (4) and (5), and (6) and (7) are robust to dropping 165, 4, or 172 singleton observations (e.g., Correia 2015; deHaan 2021). Details on the definition of all variables are provided in the Appendix.

	Full sample			**At-risk sample**		**Full sample**	
	Dependent Variable =						
	Violation	***#Violations***	***$Penalties***	***#Violations***	***$Penalties***	***#Violations***	***$Penalties***
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
CEO_Ability	0.0677	0.0787	0.3382	0.1381	0.5862		
	(0.004)	(0.009)	(0.022)	(0.010)	(0.028)		
CEO_Skill						0.0165	0.0877
						(0.044)	(0.024)
CEO_Delta	0.0007	0.0005	0.0026	-0.0003	-0.0049	0.0007	0.0030
	(0.729)	(0.861)	(0.833)	(0.952)	(0.832)	(0.879)	(0.865)
CEO_Vega	-0.0018	0.0007	-0.0141	0.0023	-0.0204	-0.0015	-0.0078
	(0.367)	(0.787)	(0.252)	(0.614)	(0.341)	(0.719)	(0.697)
Size	0.0315	0.0619	0.2233	0.1186	0.3990	0.0683	0.2465
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
Leverage	-0.0285	-0.0111	-0.0324	0.0143	0.0755	-0.0794	-0.1439
	(0.134)	(0.652)	(0.757)	(0.804)	(0.753)	(0.010)	(0.280)
Cash/Assets	0.0059	0.0349	0.1474	0.0408	0.0908	0.0203	0.0375
	(0.771)	(0.183)	(0.265)	(0.577)	(0.806)	(0.563)	(0.837)
PPE/Assets	0.0698	0.1319	0.3569	0.2782	0.7755	0.1195	-0.0119
	(0.081)	(0.072)	(0.124)	(0.038)	(0.072)	(0.291)	(0.968)
Turnover	0.0014	0.0149	0.0333	0.0305	0.0624	0.0143	0.0319
	(0.829)	(0.101)	(0.366)	(0.135)	(0.457)	(0.267)	(0.537)
Capex/Assets	0.0302	0.0900	-0.1030	0.1884	-0.3239	0.0869	-0.2030
	(0.578)	(0.292)	(0.736)	(0.303)	(0.621)	(0.417)	(0.600)
M/B	0.0003	0.0005	-0.0010	0.0009	-0.0032	0.0001	-0.0027
	(0.577)	(0.449)	(0.766)	(0.519)	(0.655)	(0.883)	(0.571)
ROA	0.0013	-0.0214	-0.0847	0.0011	-0.0879	-0.0330	-0.0885
	(0.955)	(0.417)	(0.511)	(0.989)	(0.828)	(0.380)	(0.608)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.5904	0.7560	0.5875	0.6922	0.4702	0.7864	0.6190
Adj. R^2	0.5475	0.7304	0.5442	0.6697	0.4316	0.7521	0.5580
N	24,577	24,577	24,577	11,796	11,796	14,189	14,189

TABLE 9

Comparison of Financial and Non-Financial Misconduct

This table provides regression results from estimating the relation between CEO ability and financial versus non-financial misconduct in the full sample, where the dependent variable is either *#Violations* or *$Penalties*. Columns (1) to (4) report results for financial offenses using the ability measure from Demerjian, Lev, and McVay (2012) for the period 2000-2018 in columns (1) and (2) and the skill measure from Daniel, Li, and Naveen (2020) for the period 2000-2015 in columns (3) and (4), respectively. Columns (5) to (8) report results for all non-financial (i.e., workplace safety, fair employment, and environmental) offenses, other things being equal. P-values based on robust standard errors that are clustered at the firm-level are reported in parentheses below the coefficient estimates. All specifications include year and firm fixed effects. The results in columns (1), (2), (5), and (6) [columns (3), (4), (7), and (8)] are robust to dropping 165 [172] singleton observations (e.g., Correia 2015; deHaan 2021). Details on the definition of all variables are provided in the Appendix.

	Financial				**Non-Financial**			
	Dependent Variable =							
	#Violations	***$Penalties***	***#Violations***	***$Penalties***	***#Violations***	***$Penalties***	***#Violations***	***$Penalties***
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
CEO_Ability	0.0039	-0.0213			0.0821	0.4107		
	(0.685)	(0.833)			(0.006)	(0.002)		
CEO_Skill			0.0039	0.0425			0.0139	0.0615
			(0.138)	(0.126)			(0.075)	(0.070)
CEO_Delta	-0.0014	-0.0090	-0.0007	-0.0033	0.0013	0.0060	0.0005	-0.0032
	(0.070)	(0.264)	(0.559)	(0.796)	(0.653)	(0.548)	(0.907)	(0.826)
CEO_Vega	-0.0003	-0.0087	0.0006	0.0000	0.0016	0.0013	-0.0006	0.0079
	(0.700)	(0.314)	(0.627)	(1.000)	(0.545)	(0.904)	(0.881)	(0.650)
Size	0.0081	0.0889	0.0076	0.0814	0.0543	0.1436	0.0612	0.1589
	(<0.001)	(<0.001)	(0.007)	(0.009)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
Leverage	0.0078	0.0578	0.0047	0.0126	-0.0107	-0.0620	-0.0808	-0.1446
	(0.127)	(0.327)	(0.477)	(0.878)	(0.658)	(0.478)	(0.009)	(0.197)
Cash/Assets	0.0118	0.1403	-0.0016	-0.0466	0.0270	0.0365	0.0196	0.0331
	(0.064)	(0.075)	(0.846)	(0.635)	(0.296)	(0.726)	(0.570)	(0.812)
PPE/Assets	0.0042	0.0240	-0.0264	-0.3194	0.1210	0.2615	0.1254	0.0837
	(0.647)	(0.810)	(0.037)	(0.015)	(0.103)	(0.236)	(0.271)	(0.767)
Turnover	0.0028	0.0265	0.0015	0.0073	0.0098	-0.0211	0.0096	-0.0136
	(0.192)	(0.256)	(0.626)	(0.834)	(0.275)	(0.507)	(0.455)	(0.760)
Capex/Assets	-0.0119	-0.1397	-0.0074	-0.1145	0.1017	-0.0135	0.1267	0.0766
	(0.405)	(0.313)	(0.729)	(0.613)	(0.232)	(0.960)	(0.234)	(0.818)
M/B	-0.0002	-0.0027	-0.0002	-0.0017	0.0006	0.0010	0.0003	0.0004
	(0.272)	(0.266)	(0.567)	(0.628)	(0.323)	(0.726)	(0.719)	(0.926)
ROA	-0.0095	-0.1269	-0.0216	-0.2619	-0.0093	0.0283	-0.0093	0.1379
	(0.216)	(0.144)	(0.037)	(0.030)	(0.716)	(0.782)	(0.801)	(0.333)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.2428	0.2533	0.2637	0.2742	0.7562	0.6024	0.7873	0.6338
Adj. R^2	0.1633	0.1749	0.1457	0.1579	0.7307	0.5607	0.7532	0.5751
N	24,577	24,577	14,189	14,189	24,577	24,577	14,189	14,189

TABLE 10

Non-Financial Misconduct after Exogenous CEO Departures

This table provides regression results from estimating the relation between CEO ability and non-financial misconduct in the full sample, where the sample is restricted to include all firm-years following exogenous CEO departures identified by Gentry, Harrison, Quigley, and Boivie (2021). The dependent variable is either *#Violations* or *$Penalties*. Columns (1) and (2) report results for all non-financial (i.e., workplace safety, fair employment, and environmental) offenses over the period 2000-2018 using the ability measure from Demerjian, Lev, and McVay (2012), whereas columns (3) and (4) report results over the period 2000-2015 using the skill measure from Daniel, Li, and Naveen (2020). P-values based on robust standard errors that are clustered at the firm-level are reported in parentheses below the coefficient estimates. All specifications include year and firm fixed effects. The results in columns (1) and (2) [columns (3) and (4)] are robust to dropping 167 [125] singleton observations (e.g., Correia 2015; deHaan 2021). Details on the definition of all variables are provided in the Appendix.

	Dependent Variable =			
	#Violations	***$Penalties***	***#Violations***	***$Penalties***
	(1)	**(2)**	**(3)**	**(4)**
CEO_Ability	0.1043	0.4786		
	(0.060)	(0.046)		
CEO_Skill			0.0326	0.1509
			(0.053)	(0.043)
CEO_Delta	0.0033	-0.0109	0.0058	-0.0138
	(0.538)	(0.614)	(0.437)	(0.685)
CEO_Vega	0.0021	0.0232	-0.0042	0.0194
	(0.680)	(0.319)	(0.608)	(0.631)
Size	0.1070	0.3053	0.1226	0.3139
	(<0.001)	(<0.001)	(0.001)	(0.001)
Leverage	0.0493	-0.0441	0.0089	-0.0279
	(0.256)	(0.793)	(0.880)	(0.911)
Cash/Assets	0.0762	0.2806	0.0943	0.4305
	(0.193)	(0.223)	(0.293)	(0.147)
PPE/Assets	0.3948	1.1346	0.4693	1.0255
	(0.017)	(0.012)	(0.105)	(0.092)
Turnover	0.0397	0.0743	0.0351	0.1440
	(0.023)	(0.249)	(0.163)	(0.126)
Capex/Assets	-0.0328	-0.6804	0.0688	-0.6174
	(0.859)	(0.340)	(0.727)	(0.417)
M/B	0.0017	0.0008	0.0015	0.0004
	(0.125)	(0.855)	(0.401)	(0.963)
ROA	0.0313	0.2595	0.1022	0.5008
	(0.593)	(0.309)	(0.238)	(0.173)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
R^2	0.7799	0.6071	0.8002	0.6342
Adj. R^2	0.7478	0.5500	0.7607	0.5617
N	10,791	10,791	5,919	5,919

Appendix

Variable Definitions

Variable Name	Definition
Dependent Variables	
Violation	Indicator variable taking the value of one for firm-years with at least one violation in Violation Tracker, and zero otherwise
#Violations	Natural logarithm of one plus the number of violations per firm-year, winsorized at the 99th percentile
$Penalties	Natural logarithm of one plus total penalties for violations per firm-year (in $000s), winsorized at the 99th percentile
CEO Characteristics	
CEO_Ability	CEO ability measure developed by Demerjian, Lev, and McVay (2012)
CEO_Skill	CEO skill measure developed by Daniel, Li, and Naveen (2020)
CEO_Delta	Natural logarithm of one plus the sensitivity of the CEO's equity portfolio to a one percent change in the firm's stock price (in $000s)
CEO_Vega	Natural logarithm of one plus the sensitivity of the CEO's equity portfolio to a 0.01 change in the standard deviation of the firm's stock returns (in $000s)
Firm Characteristics	
Size	Natural logarithm of one plus total assets (in $m)
Leverage	Ratio of total long-term and short-term debt to total assets
Cash/Assets	Ratio of cash and short-term investments to total assets
PPE/Assets	Ratio of net property, plant, and equipment to total assets
Turnover	Current period sales divided by beginning total assets
Capex/Assets	Current period capital expenditures divided by beginning total assets
M/B	Ratio of market value of common equity to book value of common equity
ROA	Current period income before extraordinary items divided by beginning total assets

Who Matters More? The Roles of CEOs and CFOs in Financial Misreporting

Denny Kutter
University of Potsdam
kutter@uni-potsdam.de

Katharina Weiß
LMU Munich
weiss@lmu.de

Abstract

This study examines CEO and CFO effects on firms' financial misreporting. Leveraging the concept of "manager style", we compare the incremental effects of CEOs and CFOs on financial misreporting after controlling for known determinants and an extensive set of fixed effects. Specifically, we construct a manager-firm matched panel dataset that tracks individual CEOs and CFOs across firms over the period 1995-2016. We then combine three different methods to identify style effects—the manager mobility method, the connectedness method, and the spell method. While both top-level executives significantly affect their firms' financial misreporting, we find that the impact of CFOs is economically larger than that of CEOs. This greater economic impact of CFOs is particularly pronounced for fraudulent misreporting. These findings remain consistent across different samples, methods, misreporting measures, and specification choices for the underlying conceptual mechanism. Thus, our study highlights the important role of the CFO as a key player in the beyond-GAAP setting.

Data Availability: All data are publicly available from sources indicated in the text.

Keywords: CEO; CFO; Financial Misreporting; Fraud; Manager Style.

JEL Classification: G34; G38; M41; M48.

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1. Introduction

Do CEOs or CFOs play a more relevant role in firms' financial misreporting?[25] This question matters for corporate governance and is at the heart of recent legislative changes. However, empirical evidence on the relevance of the CEO versus the CFO is mixed. For example, Feng, Ge, Luo, and Shevlin (2011) show that CEOs' equity incentives provided by portfolio delta (i.e., the sensitivity of a manager's wealth to stock price) are associated with financial misreporting, whereas those of CFOs do not seem to matter. They interpret their findings as suggesting that CFOs become involved in financial misreporting because of pressure from CEOs. On the other hand, Armstrong, Larcker, Ormazabal, and Taylor (2013), identifying equity incentives from portfolio vega (i.e., the sensitivity of a manager's wealth to stock volatility) as one potential confounding factor, cannot confirm that CEOs' equity incentives matter. They find that incentive effects provided by vega subsume those of delta and that CEOs are no longer incrementally associated with financial misreporting when other members of the top management team, including the CFO, are considered.

This short discussion illustrates a myriad of potential confounding effects in the CEO-CFO relationship beyond those from equity incentives. For example, recent studies find systematic differences between CEOs and CFOs in terms of ability and risk attitudes (e.g., Graham, Harvey, and Puri 2013; Kaplan and Sorensen 2021), where those unobservables most likely drive both their exposure (and response) to equity incentives and (mis)reporting behavior (e.g., Milbourn 2003; Soltes 2016; Page 2018). We add to this debate by using a research design that allows us to address these confounding effects more broadly. Specifically, we lev-

[25] We define financial misreporting broadly to encompass material beyond-GAAP misstatements that unambiguously violate the concept of faithful representation, e.g., stated in FASB's Statement of Financial Accounting Concepts No. 8, when mapping a firm's underlying economics into financial statements. Conceptually, it is different from discretionary reporting decisions intended to signal private information or manage reported earnings by using the flexibility within the boundaries of GAAP (e.g., Dechow, Ge, and Schrand 2010; DeFond and Zhang 2014; Amiram, Bozanic, Cox, Dupont, Karpoff, and Sloan 2018). In the following, we use the term "financial misreporting" to refer to opportunistic reporting behavior in which managers overstep the limits of acceptable financial reporting, ranging from cases where they commit fraud to those where they intend to manage earnings within-GAAP but ultimately violate reporting standards.

erage the concept of manager style[26] to compare the incremental effects of CEOs and CFOs on financial misreporting, thereby making many of the unobserved confounding factors from prior studies the variable of interest. Overall, by examining to what extent CEO vis-à-vis CFO style explains financial misreporting after controlling for known determinants and an extensive set of fixed effects, we provide new evidence on the economic importance of CEOs and CFOs in the beyond-GAAP setting.

To address our research question, we construct a manager-firm matched panel dataset that tracks individual CEOs and CFOs across firms over the period from 1995 to 2016. We measure financial misreporting by using restatements covered in the Audit Analytics database, including a large number of Form 8-K Item 4.02 material event filings, which we complement with Accounting and Auditing Enforcement Releases (AAERs) issued by the Securities and Exchange Commission (SEC). Thus, our dataset combines information about managers (ExecuComp), firms (Compustat), restatements (Audit Analytics), and AAERs (Dechow, Ge, Larson, and Sloan 2011). To identify style effects, we combine three different methods: the manager mobility method, the connectedness method, and the spell method.

In a first set of tests, we regress firms' financial misreporting on known time-varying firm-specific determinants, year fixed effects, firm fixed effects, and manager fixed effects. First, we apply the mobility method introduced by Bertrand and Schoar (2003), which exploits the movement of managers across firms over time and thus allows us the isolation of manager-specific effects. For the CEO sample, we find that adding CEO fixed effects to the base model increases the (adjusted) R^2 by 3.77 (3.62) percentage points. This CEO-specific effect remains statistically significant after controlling for the incremental effect of CFOs,

[26] Consistent with the "manager style" literature (e.g., Bertrand and Schoar 2003; Ge, Matsumoto, and Zhang 2011; Wells 2020), we use the term "style" to refer to overarching patterns in a given manager's (mis)reporting behavior, governed by relevant (both observable and unobservable) and persistent differences across managers that arise from numerous factors, including their incentives and traits. Starting with Bertrand and Schoar (2003), style is inextricably linked with the presence of manager fixed effects, interpreted as managers' incremental effect on the firm behavior of interest. Thus, we use these terms interchangeably. However, to the extent that managers develop their style over time, our fixed effects approach may not fully capture their style.

using a combination of the spell method and the mobility method. For the CFO sample, adding CFO fixed effects increases the (adjusted) R^2 by 12.17 (9.37) percentage points. Similarly, this CFO-specific effect is statistically significant, regardless of whether we control for the incremental effect of CEOs.

In both samples, based on a simple comparison in providing additional explanatory power, the marginal effect of including CFOs is economically larger than that of CEOs. To analyze the economic meaning in more detail, we follow Graham, Li, and Qiu (2012) in decomposing the model R^2 and also examine the distribution of the estimated manager fixed effects. Using the estimation results from the most rigorous regression specifications, we find that CEOs versus CFOs account for a significant proportion of 26.93 and 42.94 percent of the explained variation in firms' financial misreporting and that the difference in the interquartile ranges of the CEO and CFO fixed effect estimates corresponds to an increase in a firm's misreporting probability of 14.32 and 22.97 percentage points, respectively. While CEOs and CFOs incrementally affect firms' financial misreporting, these results support the argument that CFOs have a greater economic impact.

While the mobility method provides the cleanest research design for investigating managers' incremental effect, it imposes strict conditions on the studied population of managers and firms. To illuminate the generalizability of our findings, our second and third methods are the connectedness and spell methods, both introduced by Abowd, Kramarz, and Margolis (1999). Using the connectedness method, we continue to find significant CEO- and CFO-specific effects on firms' financial misreporting. Results from a detailed analysis again indicate that CFOs play an economically more important role. Finally, using the spell method, we estimate a modified regression specification and still find that CEOs and CFOs show a significant effect on financial misreporting that is incremental to the impact of the firm. In addition, the marginal effect of including CFOs again is economically larger than that of CEOs. In sum, these results suggest that our inferences hold in larger samples of managers and firms.

In a second set of tests, we examine whether the documented difference in the incremental effects of CEOs and CFOs also carries over to fraudulent misreporting. First, we focus on irregularities, that is, restatements of more material and presumably intentional misstatements. For the CEO sample, we fail to find a statistically significant CEO-specific effect after controlling for the incremental effect of CFOs. For the CFO sample, on the other hand, we find that adding CFO fixed effects to the base model increases the (adjusted) R^2 by 11.03 (8.38) percentage points, which exceeds the marginal effect of including CEOs by 3.13 (3.14) percentage points. This CFO-specific effect is also statistically significant, regardless of whether we control for the incremental effect of CEOs. Taking a closer look at the economic magnitude of the estimated CFO fixed effects, again using the estimation results from the most rigorous regression specification, we find that CFOs account for 43.20 percent of the explained variation in irregularities across firms. Further, we find that the difference in the interquartile range of the CFO fixed effect estimates corresponds to an increase of 2.04 percentage points in the probability for irregularities, which is economically significant, given an observed mean probability of 1.52 percent. These results suggest that the greater economic impact of CFOs vis-à-vis CEOs increases in cases of fraudulent misreporting. Further analysis based on AAERs to address potential type-I errors in irregularities reinforces this notion.

In our additional analyses, we first show that our findings are not driven by equity incentives or the mediating effect of firm characteristics, that is, accrual-based and off-balance sheet activities, that are themselves potential outcomes of managers' reporting behavior (e.g., Gow, Larcker, and Reiss 2016; Whited, Swanquist, Shipman, and Moon 2021). Second, as endogenous pairing of managers and firms can bias our results (e.g., Fee, Hadlock, and Pierce 2013; Pan 2017), we replicate our analysis using a setting of plausibly exogenous management turnover and find consistent results. However, we cannot rule out confounding effects or alternative explanations as a random assignment of managers to firms does not exist. Thirdly, we test the construct validity of our dependent variables by addressing potential type-II errors in the restatement data, using the F-Score for a firm's predicted misreporting risk (Dechow et al. 2011). Our findings remain consistent. We interpret our collective evidence as supporting the notion that CFOs play a more relevant role in firms' financial misreporting than CEOs.

Our study makes several contributions to the literature. We provide new insights into the economic importance of CEOs and CFOs in the beyond-GAAP setting. Prior studies focusing on incentive effects provided by equity portfolios find inconsistent results (e.g., Feng et al. 2011; Armstrong et al. 2013). Addressing unobserved confounding factors and challenges to the separation of firm- and manager-specific effects, we investigate their total effects and find that CFOs have a greater economic impact than CEOs. While the findings of Feng et al. (2011) link CFO involvement in financial misreporting to CEO pressure, the documented CFO effect in our study, based on commonalities across different firms and CEO-firm regimes, cannot be considered a manifestation of the CEO's influence. Findings from an additional analysis where we incorporate CEO incentives (to pressure the CFO) reinforce this notion. Thus, we complement a concurrent study by Davidson (2021), who provides a more nuanced view of equity incentives within AAER firms. This evidence is consistent with a broader literature highlighting the important role of the CFO in the within-GAAP setting (e.g., Geiger and North 2006; Chava and Purnanandam 2010; Jiang, Petroni, and Wang 2010).

We also contribute to the literature on the determinants of financial misreporting (e.g., Guo, Huang, Zhang, and Zhou 2016; Demerjian, Lev, Lewis, and McVay 2013; Ham, Lang, Seybert, and Wang 2017), documenting that top-level executives in different positions influence their firms' financial misreporting. Our findings underscore the importance of considering both the CEO and CFO for a complete understanding of financial misreporting. However, the CFO's influence is more significant, which is even more pronounced for fraudulent misreporting. Finally, we add to the manager style literature (e.g., Ge et al. 2011; DeJong and Ling 2013; Wells 2020), showing that style effects on earnings management carry over to a reporting setting that is more reflective of managerial opportunism (e.g., Schipper 1989; Dechow and Skinner 2000; Amiram et al. 2018).

Our findings have important implications for regulators. While regulations have focused on strengthening management responsibilities (e.g., Sarbanes-Oxley Act), Feng et al. (2011) suggest that reform efforts should focus on improving CFOs' independence from CEOs. Our findings complement these suggestions by showing that CFO independence should not be viewed as panacea. Given that CFOs have significant influence over firms' financial misreporting that cannot be assigned to CEOs, our findings indicate that, in addition to reduced CEO pressure, regulators should also consider changing the legal basis for CFO liability and shifting corporate governance toward a stricter monitoring of CFOs.

The study proceeds as follows. Section 2 reviews prior literature and develops our hypotheses. Section 3 describes the empirical strategy for hypotheses testing. Section 4 presents and discusses our main findings. Section 5 provides additional analyses. Section 6 concludes.

2. Literature and Hypotheses Development

2.1. Determinants of Financial Misreporting

This study adds to three literature streams. The first stream studies the determinants of financial misreporting. These studies largely focus on firm characteristics. For example, prior studies show that a firm's operating characteristics like size, leverage, profitability, and its financing activities affect the likelihood of financial misreporting (e.g., Kinney and McDaniel 1989; Dechow et al. 2011). Further studies examine firms' capital market incentives as potential drivers of financial misreporting (e.g., DeFond and Jiambalvo 1991; Bens, Goodman, and Neamtiu 2012), and others document how governance mechanisms, like statutory audits (e.g., Francis, Michas, and Yu 2013; Eshleman and Guo 2014), internal controls (e.g., Plumlee and Yohn 2010; Donelson, Ege, and McInnis 2017), or employee treatment policies (e.g., Guo et al. 2016; Raghunandan 2021), can help explain financial misreporting.

Focusing on the firm's CEO, some studies find that managers respond to equity incentives when making (mis)reporting decisions (e.g., Burns and Kedia 2006; Efendi, Srivastava, and Swanson 2007), whereas others fail to find this association (e.g., Larcker, Richardson, and Tuna 2007; Armstrong, Jagolinzer, and Larcker 2010). In addition, there are a few studies that examine particular traits, showing that, for example, managerial ability, overconfidence, and narcissism can relate to financial misreporting (e.g., Schrand and Zechman 2012; Demerjian et al. 2013; Ham et al. 2017). Although the literature documents that top-level executives can influence their firms' financial misreporting, most studies focus on a small set of manager-specific factors for CEOs. We add to this literature stream by investigating the influence of a firm's top-level executives on financial misreporting more broadly. We shift the focus toward the combination of the CEO and CFO, given that managers work in teams (e.g., Zhang 2019), and our study differs from prior research in that we are interested in managers' total effects, rather than the isolated effect of some manager-specific factors. Thus, we seek to provide more insights into the roles played by top-level executives in different positions in determining financial misreporting.

2.2. CEOs versus CFOs

The second literature stream studies the importance of CEOs relative to CFOs for financial reporting outcomes. These studies draw on managers' equity incentives. For example, in the within-GAAP setting, prior studies find that both CEOs' and CFOs' equity incentives, captured by the sensitivities of their wealth to changes in stock price (delta) and volatility (vega), affect a wide range of firms' financial policies but that accrual quality is more sensitive to the CFO's incentives than to those of the CEO (e.g., Chava and Purnanandam 2010; Jiang et al. 2010). These findings comport with those of Geiger and North (2006), who document significant changes in the magnitude of discretionary accruals around the appointment of a new

CFO that cannot be explained away by the concurrent appointment of a new CEO. Taken together, these studies suggest that CFOs are more important for financial reporting outcomes.

On the other hand, in the beyond-GAAP setting, empirical evidence on the relevance of the CEO versus the CFO is mixed. For example, Feng et al. (2011) find that CEOs' equity incentives from portfolio delta are associated with material accounting manipulations, as measured by AAERs, while there is no link with the equity incentives of the CFO. Their findings suggest that CFOs, without being the primary perpetrator, become involved in financial misreporting because they succumb to CEO pressure. In contrast, Armstrong et al. (2013) cannot find such a link with CEOs' equity incentives, using both AAERs and restatements as measures of financial misreporting. Specifically, identifying equity incentives provided by managers' portfolio vega as one potential confounding factor, they show that the incentive effects from delta are subsumed by those of vega. Following, the authors fail to find an incremental effect of CEOs on financial misreporting after considering the CFO together with other members of the top management team, whereas they continue to find an incremental effect from equity incentives for these other C-suite managers. More recently, in response to a lack of consistent evidence on the relation between equity compensation and financial misreporting, Davidson (2021) shows that managers named as perpetrators in a sample of AAER firms have stronger equity incentives both from delta and vega than their within-firm peers as well as their peers at non-AAER firms, regardless of whether the manager is the CEO, CFO, or holds another position. However, given that the analysis compares implicated managers with non-implicated ones holding different (the same) positions in the same (different) firms, it remains unclear whether CEOs or CFOs play a more relevant role in this setting.

There are many potential confounding effects in the CEO-CFO relationship beyond those from equity incentives. Graham et al. (2013) and Kaplan and Sorensen (2021) find that managers in CEO positions differ significantly from those in CFO positions, for example, in terms of ability and their attitudes toward risk. On the other hand, managers have idiosyncratic compensation preferences, and compensation contracts are jointly determined by managers and firms (e.g., Morse, Nanda, and Seru 2011; Abernethy, Kuang, and Qin 2015). Hence, unobserved differences across CEOs and CFOs can drive their level of equity compensation (e.g., Milbourn 2003; Albuquerque, De Franco, and Verdi 2013; Page 2018). For example, consistent with the idea that equity compensation mitigates agency problems, differences in ability or risk aversion may affect CEO/CFO equity exposure (delta) and exposure to volatility (vega). Similarly, given that differences in traits prompt different responses to equity incentives (e.g., Wowak and Hambrick 2010), differences in ability (risk aversion) may affect a CEO's or CFO's response to pay-performance sensitivity from delta (risk-taking incentives from vega). If those unobservables do drive (mis)reporting behavior (e.g., Soltes 2016), an analysis based on equity incentives will incompletely depict their economic impact.[27] We add to this literature stream by addressing these confounding effects at large. While prior studies examine differences in the incremental effect from equity incentives across CEOs and CFOs, we study the difference in their total effects and thus can provide new insights into the economic importance of CEOs and CFOs in the beyond-GAAP setting.

2.3. Manager Style

The third literature stream is the manager style literature. These studies propose an alternative approach to investigating managers' incremental effect, picking up more of the relevant manager-specific variation in the firm behavior of interest. Starting with Bertrand and Schoar (2003), prior studies document manager fixed effects on a wide range of firm policies, such as investment and financing decisions, voluntary disclosure choices, tax avoidance, and CSR activities (e.g., Yang 2012; Davis, Ge, Matsumoto, and Zhang 2015; Davidson, Dey, and Smith 2019). Three studies investigating managers' style in the within-GAAP setting closely

[27] In line with this, the descriptive evidence of Davidson (2021) suggests that CEOs have much stronger equity incentives than CFOs, while being named as perpetrators 60 percent and 55 percent of the time, respectively. Our fixed effects approach picks up those unobserved differences if they are fairly sticky, i.e., time-invariant or slow-moving (e.g., Graham et al. 2012; Gormley and Matsa 2014).

relate to our study. In particular, Ge et al. (2011) show that CFO fixed effects help explain cross-sectional variation in a set of reporting practices and earnings properties (e.g., magnitude of discretionary accruals and earnings smoothing). DeJong and Ling (2013) find that both CEOs and CFOs play an important role in explaining the accrual component of earnings, where CEO fixed effects are driven by real activities and CFO fixed effects can be attributed to accounting choices. Finally, Wells (2020) finds that manager fixed effects explain a large degree of accrual variability across firms.

Taken together, the literature provides evidence for style effects on firms' earnings management, mostly measured by accrual quality. We add to this literature by jointly considering the roles of CEOs and CFOs in the beyond-GAAP setting. In contrast to prior studies, we are not interested in managers' style per se. Instead, we leverage the concept of manager style to capture a manager's total effect and investigate to what extent CEOs versus CFOs affect their firms' financial misreporting. This helps us address many of the potential confounding factors from prior studies as they essentially become part of the variable of interest. Further, we need to impose variation in managers while holding firm-specific differences constant, which helps us to disentangle CEOs' and CFOs' unique influences. Thus, drawing on their style effect differential, we seek to provide new evidence on the CEO-CFO relationship and how it relates to financial misreporting.

2.4. Hypotheses

Who matters more? The literature provides two opposing views on the interplay of CEOs and CFOs in financial reporting. On the one hand, given that the CFO is primarily responsible for many financial policies, CFOs are more "on the ground" and thus might exercise more discretion over financial reporting. They oversee the financial reporting system and are directly involved in reporting practices (e.g., Indjejikian and Matêjka 2009; Dichev, Graham, Harvey, and Rajgopal 2013). For example, CFOs are typically in charge of structuring transactions, choosing accounting treatments for those transactions, implementing internal controls, and preparing financial reports. Under this view, based on their direct impact, we would expect that CFOs play a more relevant role than CEOs.

On the other hand, given that the CEO is the most powerful top-level executive and ultimately in charge of major firm policies, CEOs may have the power to replace a CFO who does not meet the CEO's demands. Thus, the CEO may set the tone from the top or put pressure on the CFO that might override the CFO's direct impact (e.g., Mian 2001; Fee and Hadlock 2004). Friedman's (2014) analytical work highlights how in such cases a CFO could end up making accounting choices consistent with the CEO's preferences. In line with this, while CFOs become resistant to CEO pressure over time, Dikolli, Heater, Mayew, and Sethuraman (2021) show that, under certain conditions, CEOs can pressure CFOs into reporting practices that increase CEO compensation. Under this alternative view, without being involved in day-to-day financial reporting, we would expect that CEOs are relatively more important.

While the view of CFO discretion is generally supported in the within-GAAP setting (e.g., Geiger and North 2006; Chava and Purnanandam 2010; Jiang et al. 2010), there are several explanations for why this might not carry over to the beyond-GAAP setting. For example, managing earnings within-GAAP versus beyond-GAAP can capture substantially different reporting behavior, leading to different implications for financial reporting quality. In the within-GAAP setting, managed earnings can signal poor reporting quality when used opportunistically to distort the firm's underlying economics. However, managed earnings can also indicate high reporting quality when private information is used for "noise" reduction, for example, earnings smoothing to better reflect firm performance or give more weight on the persistent components of earnings to increase their predictability (e.g., Schipper 1989; Dechow and Skinner 2000; Graham, Harvey, and Rajgopal 2005).[28] In contrast, in the be-

[28] For example, Malmquist (1990) and Dechow (1994) provide early empirical evidence that, on average, managers make accounting choices to signal private information, rather than for opportunistic earnings manipulation.

yond-GAAP setting, the relation to financial reporting quality is less ambiguous, as it is difficult to argue that such reporting practices increase the informativeness of earnings; that is, this setting should be more reflective of managers' opportunistic reporting behavior (e.g., Dechow et al. 2010; DeFond and Zhang 2014; Amiram et al. 2018).

In terms of managerial opportunism, from an economic perspective, financial misreporting likely involves different cost-benefit tradeoffs than earnings management without pushing one's financial reporting over the GAAP line. For example, governance mechanisms, such as board oversight, statutory audits, and enforcement regulations, help ensure compliance with GAAP; that is, the nondiscretionary nature of GAAP compliance and scrutiny by the firm's institutional environment place greater or at least different limits on managers' (mis)reporting behavior. As a result, managers have competing incentives (not) to cross into financial misreporting as they might face high costs of noncompliance, including clawbacks or litigation from discovered misreporting, which may outweigh any additional benefits (e.g., Chung and Wynn 2008; Amiram, Huang, and Rajgopal 2020).

From a behavioral perspective, however, stopping one's misreporting beyond GAAP might no longer be a rational decision. For example, the interview evidence of Soltes (2016) suggests that white-collar criminals' behavior is often inconsistent with rational net benefit analyses. Similarly, following the slippery slope argument (e.g., Schrand and Zechman 2012; Chu, Dechow, Hui, and Wang 2019), managers may start managing earnings within GAAP, which then can put them on a path leading to a higher chance of resorting to reporting practices outside of GAAP.[29] Ultimately, given the competing theoretical arguments and differences in the underlying phenomena, whether CFOs act primarily on their own behalf ("*CFO discre-*

[29] For example, managers may engage in earnings management to show continued earnings growth, expecting that future performance will offset past accounting choices. However, if performance does not improve, some managers may resort to increasingly aggressive reporting practices to avoid reporting earnings declines.

tion view") or accede to the CEO ("*CEO pressure view*") in the beyond-GAAP setting is an empirical question. Thus, we posit the following hypothesis:[30]

H1: *The incremental effect on financial misreporting differs across CEOs and CFOs.*

Financial misreporting ranges from cases where managers commit fraud down to cases where they intend to manage earnings within the boundaries of GAAP but ultimately violate reporting standards, for example, due to unintentional misstatements or the slippery slope. In either case, financial statements are materially misstated, according to authoritative guidance such as SEC Staff Accounting Bulletin No. 99. However, there is considerable variation in terms of severity and intent across the different forms of financial misreporting. Hence, given its significant impact on capital markets, regulators like the SEC or the Public Company Accounting Oversight Board (PCAOB) are particularly concerned with corporate fraud (e.g., Richards 2008; Ceresney 2013), stating that the primary factor that defines fraud is whether the underlying action that results in a material misstatement is intentional (e.g., SEC Rule 10b-5; PCAOB Auditing Standard No. 2401).

Following the regulators' view, we would expect that managers' opportunistic reporting behavior is more reflected in those cases of financial misreporting where a manager deliberately misleads a firm's stakeholders, that is, commits fraud. However, the legal definition of fraud is challenging, as, for example, managerial intent is unobservable, and some cases of intentional misreporting may fall below legal thresholds. In line with this, recent studies suggest that managers attempt to hide their intentional misreporting; that is, they commit fraud that is hard to detect or appears unintentional on the surface (e.g., Hamilton and Smith 2021).

[30] These two explanations are not mutually exclusive. For example, compared to earnings management, financial misreporting may involve "more sensitive" reporting decisions, and thus CEOs might have a say in making these decisions without putting pressure on the CFO. However, identifying the primary explanation is important, as each has different policy implications. Under the CFO discretion view, we would expect to find CFO fixed effects after controlling for the influence of CEOs in the CFO sample, whereas, in the CEO sample, we should fail to find CEO fixed effects after controlling for the incremental effect of CFOs. On the other hand, under the CEO pressure view, we would expect to find CEO fixed effects (but no CFO fixed effects) when we control for the incremental effect of CFOs (CEOs) in the CEO (CFO) sample.

While we acknowledge the difficulties in establishing fraud per se (e.g., Amiram et al. 2018; Donelson, Kartapanis, McInnis, and Yust 2021), we can assume different levels of managerial influence underlying misstatements at different ends of the spectrum of financial misreporting. In that regard, the literature highlights the importance of distinguishing errors from irregularities, with the latter referring to more material and presumably intentional misstatements (e.g., Hennes, Leone, and Miller 2008; Karpoff, Koester, Lee, and Martin 2017). Thus, given that we predict the incremental effects of CEOs and CFOs to differ across the whole spectrum of financial misreporting, we expect this difference to be more pronounced for this group of more egregious cases of financial misreporting, which we label fraudulent misreporting. This leads to our second hypothesis:[31]

H2: *The difference in the incremental effects of CEOs and CFOs increases in fraudulent misreporting.*

3. Empirical Strategy

3.1. Methods for Identification of Manager Effects

We use the concept of manager style to capture CEOs' and CFO' total effects, combining three estimation methods with different fixed effects approaches to identify style effects. In particular, we employ the manager mobility method (Bertrand and Schoar 2003) as our primary method, but we also apply the connectedness method and the spell method (Abowd et al. 1999) as alternatives to address some limitations of the mobility method. Collectively, these methods help address several challenges to the separation of firm-specific and manager-specific effects, and they provide an intuitive framework for dealing with potential confounding effects due to unobserved differences across managers and firms.

[31] This hypothesis is not without tension, given that there is a market for executives (e.g., Karpoff, Lee, and Martin 2008; Hennes et al. 2008). However, recent studies document significant changes in the cost-benefit tradeoffs associated with (fraudulent) misreporting over the last 20 years, including a reduced likelihood of forced turnover and other labor market penalties (e.g., Chung and Wynn 2008; Amiram et al. 2020).

The mobility method exploits the movement of managers across firms over time, thereby restricting the analysis to managers employed by at least two different firms. For example, as depicted in Figure 1, managers A, D, and H in firms A to E would be included in the analysis when applying the mobility method. Intuitively, without managerial mobility, style effects of managers cannot be separated from the firm's fixed effect as the two effects are perfectly collinear. The mobility requirement helps generate variation in managers while holding constant firm-specific differences and thus allows us to isolate and quantify manager-specific effects. This method provides the cleanest research design to investigate the incremental role of managers, as it effectively removes firm-specific influences from the manager fixed effects. However, it imposes strict conditions on the population of managers and firms that can be studied, which may limit its generalizability. Specifically, moving managers may differ systematically from those who do not move, just as firms employing moving managers may differ from those that do not employ those managers. We apply the connectedness method and the spell method to alleviate selection bias concerns.

The connectedness method also exploits the movement of managers but levers managerial mobility to retrieve information about nonmoving managers employed by firms with at least one moving manager. While the population of firms is the same as for the mobility method, the population of managers that can be studied is extended to include both moving and nonmoving managers. For example, as shown in Figure 1, managers A to I in firms A to E would be included in the analysis when applying the connectedness method. Using insights from graph theory, the method is built upon forming groups of firms that are connected through managerial mobility. All managers and firms are separated into disjoint groups based on commonalities between them. As seen in Figure 1, there is managerial mobility within each of the two groups, but there is no mobility between the groups.[32] Abowd et al. (1999) formally prove that connectedness is necessary and sufficient for the separate identification of manager and firm effects, while mobility is sufficient but not a necessary condition. Graham et al. (2012) provide a more detailed discussion of the connectedness method and its econometric properties. The added value of the connectedness method is that the analysis is not confined to moving managers, who may differ from nonmoving ones. However, firms with moving managers may systematically differ from firms without those managers. We consider the spell method to address this latter issue.[33]

The spell method does not impose managerial mobility. Instead, manager- and firm-specific effects are combined in an interacted variable for each unique manager-firm combination ("spell"). For example, as depicted in Figure 1, 14 spells capturing managers A to K in firms A to F would be included in the analysis when applying the spell method. While the spell method controls for the combined influence of managers and firms and thus mitigates possible concerns about estimation bias, it does not separately identify manager- and firm-specific effects. As a result, only joint manager-firm effects can be estimated. The added value of the spell method, however, is that the analysis can be performed on the whole population of managers and firms using standard fixed effects approaches, which helps illuminate the generalizability of our findings.

[Figure 1 about here]

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[32] We use Stata's *felsdvreg* command developed by Cornelissen (2008) to implement the connectedness method. Note that, within each group, one firm fixed effect is not identified and serves as reference for the estimation of the other fixed effects. Thus, manager and firm fixed effects are estimated relative to a benchmark, and normalization becomes a practical estimation issue. To facilitate the comparability of our results across methods, we use the same normalization procedure when applying the mobility and the connectedness method. However, by estimation, manager fixed effects may still play a more important role in the connectedness method (e.g., Graham et al. 2012). We address this issue in section 4 and interpret our results accordingly.

[33] Andrews, Gill, Schank, and Upward (2008) note limited mobility bias as another potential shortcoming of the connectedness method. That is, when managerial mobility is limited, information on a few moving managers is used to back out nonmoving managers' fixed effects. As a result, both manager- and firm-specific effects may be estimated imprecisely. However, as seen in Table 1, managerial mobility appears to be sufficiently high in our study, as moving managers roughly account for one-fourth of the managers included in the samples used to implement the connectedness method.

Notwithstanding the different estimation methods implemented, we do not claim that our research design allows us to establish causality. While fixed effects capture the time-invariant or slow-moving dimension of unobserved confounding factors, we cannot rule out confounding effects caused by unobserved time-varying differences across managers and firms (e.g., Gormley and Matsa 2014; deHaan 2021). Similarly, matching of managers and firms can bias our results. That is, certain types of managers might self-select into certain corporate environments or firms choose managers based on their expected reporting behavior to meet a firm's time-varying needs (e.g., Fee et al. 2013; Pan 2017). In the former case, we argue that managers still have an incremental effect that extends beyond the firm's reporting environment. In the latter case, to systematically confound our analysis, firms would need to change reporting objectives and concurrently hire managers with different reporting styles, whereby the underlying reasons for these policy changes are not captured by firm fixed effects, year fixed effects, or observed firm characteristics. Note, however, that there is no such thing as a random assignment of managers to firms. Thus, endogenous pairing of managers and firms is present in some form in any study drawing on manager-firm matched data and is not a byproduct of the methods applied here (e.g., Graham et al. 2012). We further address matching concerns in an additional analysis using a setting of plausibly exogenous management turnover. However, our findings should be interpreted with this caveat in mind.

3.2. Data Sources and Sample Construction

The data used in our study come from the intersection of the Audit Analytics, Compustat, and ExecuComp databases for the period 1995 to 2016, supplemented with AAER data obtained from Dechow et al. (2011). We use restatements covered in the Audit Analytics database as the primary measure for financial misreporting for the following reasons. First, restatements unambiguously reflect an acknowledgement that firms' financial statements, as originally reported to the public and filed with the SEC, were not in accordance with GAAP. In line with this, a survey by Christensen, Glover, Omer, and Shelley (2016) identifies restatements as the number one publicly available signal of poor reporting quality from an investor's perspective. Second, the Audit Analytics database is largely based on Form 8-K Item 4.02 material event filings and does not include restatements due to technical issues, for example, GAAP-changes, stock splits, or dividend distributions (i.e., low type-I error rate). Third, the database covers all SEC registrants that have disclosed a financial statement restatement in electronic filings, which (to some extent) reduces the likelihood of type-II errors due to limited scope in the restatement data.[34] Finally, Audit Analytics indicates the fiscal year a subsequently restated financial statement was originally issued by providing information on the period over which the misreporting occurred. This feature allows us to identify misreporting firm-years that most likely differ from the restatement announcement dates.

Audit Analytics states that it tracks all financial statement restatements at least since January 1, 2001. As noted by Karpoff et al. (2017), however, it is possible to access restatement data in Audit Analytics dating back to March 29, 1995, which includes an additional of 878 restatements. We follow prior research and conduct our analysis using all the restatement data available. Thus, our sample period starts in 1995. We deliberately end the sample two years before the most recent period of data available to allow for enough time for a firm, its auditor, or the SEC to determine a financial statement should be restated. As documented, for example, by Czerney, Schmidt, and Thompson (2014), most restatements are disclosed within

[34] Thus, coverage in Audit Analytics is greater than in Compustat (ExecuComp), which covers larger Form 10-K and 10-Q fillers (S&P 1500 firms). Audit Analytics combines two restatement types, i.e., reissuance ("Big R") and revision ("little r") restatements. Big R restatements correct material misstatements in a given financial statement and require withdrawal and reissuance (e.g., filing a Form 10-K/A or 10-Q/A), where the firm must also file a Form 8-K Item 4.02 to notify investors that the previously issued financial statement can no longer be relied upon. Little r restatements correct misstatements from prior periods that are material in aggregate to a given financial statement and can be disclosed in periodic filings (e.g., Form 10-Ks or 10-Qs). Throughout our study, we use information on both restatement types to account for the possibility that firms' materiality judgments or disclosure choices to correct misstatements change over time (e.g., Tan and Young 2015; Choudhary, Merkley, and Schipper 2021; Thompson 2021). However, Big R restatements make up more than one third of the restatements included in our samples, and our findings are robust to excluding little r restatements.

two years following the end of the originally misstated fiscal year. Thus, we use a two-year cutoff, and our sample period ends in 2016.

We start our sample construction by collecting data from ExecuComp to construct a manager-firm matched panel dataset that tracks individual CEOs and CFOs across firms over time. To identify managers as CEO or CFO in a given firm-year, we use the ExecuComp variables *AnnualCEOFlag* and *AnnualTitle*.[35] To ensure that managers are given a reasonable amount of time to leave their mark on reporting outcomes, some studies impose the requirement that a manager must be in each firm for a certain number of fiscal years. However, inferences are generally not sensitive to this restriction, and we impose a one-year requirement only.[36] Next, we add annual financial data and firm fundamentals from Compustat, where firms must have available data for each variable used in the analysis for a given firm-year to be included in the sample. Finally, we merge the Compustat/ExecuComp data with restatement data obtained from Audit Analytics, using the variables *Resbegindate* and *Resenddate* to match misreporting firm-years.

Table 1 outlines the different samples used. The full CEO/CFO sample is used to implement the spell method and includes all observations from the Audit Analytics/Compustat/ExecuComp intersection with available CEO and CFO data. Thus, the sample covers more firms and managers than the samples used to implement the other two methods. Specifically, the full CEO/CFO sample contains 30,208 firm-years, 3,105 firms, 6,118 CEOs, and 6,659 CFOs. The CEO and CFO mobility samples are used to implement the mobility method, including all observations for moving CEOs or CFOs employed by at least two dif-

[35] *AnnualCEOFlag* is a flag variable indicating that a manager served as CEO for a given firm for all or most of the indicated fiscal year. *AnnualTitle* is a string variable that includes the title(s) of a manager as listed in the historical proxy statement for the indicated fiscal year. We use keywords closely related to “Chief Financial Officer” or “CFO” to classify a manager as CFO in a given firm-year. Whenever this approach identifies multiple managers as CFO (e.g., due to CFO turnover), we only classify the incoming manager as a firm’s CFO. Thus, our manager-firm matched panel dataset is arranged in firm-years with one CEO and one CFO per firm-year.

[36] Our findings are robust to imposing a minimum tenure of two or three years for a given manager.

ferent firms throughout the sample period.[37] The CEO mobility sample comprises 1,675 firm-years of 430 firms, 234 CEOs, and 641 CFOs, whereas the CFO mobility sample contains 5,011 firm-years for 1,072 firms with 1,560 CEOs and 680 CFOs. As seen in Table 1, managerial mobility is higher among CFOs, which comports with the evidence provided by Mian (2001). The CEO and CFO connectedness samples are used to implement the connectedness method, including all observations for CEOs or CFOs employed by firms with at least one moving manager. Specifically, the CEO connectedness sample comprises 4,723 firm-years of 430 firms, 963 CEOs, and 1,225 CFOs, whereas the CFO connectedness sample contains 12,505 firm-years, 1,072 firms, 2,586 CEOs, and 2,695 CFOs. While performed on the same sample of firms, the connectedness method levers managerial mobility to back out information about nonmoving managers and thus significantly increases the sample size. Note that the share of restated firm-years is fairly stable across samples, ranging from 11.99 percent (full CEO/CFO sample) to 13.74 percent (CEO connectedness sample). This also holds for the share of firm-years with irregularities, ranging from 1.49 percent (CEO mobility sample) to 2.33 percent (CEO connectedness sample).

[Table 1 about here]

[37] As we are particularly interested in the economic impact of CEOs relative to CFOs, managerial mobility is defined in terms of strict CEO and CFO movements. Recall that our dataset is arranged in firm-years, including one CEO and CFO per firm-year. Given the limited overlap in mobility (connectedness) for CEOs and CFOs, we split up the mobility (connectedness) samples. We choose this data structure to be able to control for the other top-level executive's influence, e.g., by including fixed effects for each CEO-firm regime instead of firm fixed effects in the CFO analysis, as Stata's *felsdvreg* command only allows for two high-dimensional fixed effects. Alternatively, prior studies draw on datasets arranged in manager-firm-years with one manager per observation. However, with this data structure one can hardly control for the incremental effect of other managers.

3.3. Empirical Model and Descriptive Statistics

We model a firm's likelihood of financial misreporting as a function of its economic fundamentals and individual managers. Using the three methods discussed above, we estimate the following three-way fixed effects model:[38]

Equation (1): $y_{it} = \varphi_i + \mu_t + \theta_j + \beta_k X_{kit} + \varepsilon_{it}$

The dependent variable (y_{it}) substitutes for the financial misreporting measures used in our study. To test H1, we start by exploiting all restatements available in Audit Analytics using *RESTATE* as the dependent variable, an indicator variable taking the value of one for firm-year observations with subsequent restatement. Note that restatements cover the whole spectrum of financial misreporting and are generally considered material by investors (e.g., Christensen et al. 2016; Amiram et al. 2018).[39] To test H2, however, we cull restatements to turn toward fraudulent misreporting using *IRREGULARITY* as dependent variable, an indicator variable taking the value of one for firm-year observations with subsequent restatement of more material and presumably intentional misstatements. Specifically, we follow the suggestions of Karpoff et al. (2017) and use the Audit Analytics variables *Resfraud*, *Resclericalerrors*, *Resaccounting*, and *Ressecinvestigation* to separate errors from irregularities.[40] As additional test, we use *AAER* to create an alternative sample of fraudulent misreporting firms.

In equation (1), there are three sets of fixed effects: firm fixed effects (φ_i), year fixed effects (μ_t), and manager fixed effects (θ_j). We include firm fixed effects to account for both

[38] Throughout our study, we use a linear probability model and estimate equation (1) by OLS (e.g., Wilde 2017; Raghunandan 2021). Intuitively, as our dependent variable is binary, one could use a nonlinear model, applying the maximum likelihood estimator. However, as detailed above, the estimation methods implemented in our study require us to include a large set of fixed effects. Including many fixed effects in nonlinear models is problematic, as the maximum likelihood estimator becomes biased and inconsistent due to the incidental parameters problem (e.g., Greene 2004). Note that our empirical model is reduced to a two-way-fixed effects model when we apply the spell method, including fixed effects for each manager-firm combination.

[39] Similarly, audit partners state that "it's obviously material; if it weren't material, it wouldn't be restated. For us, a restatement is referred to as the 'R' word—we just don't want them unless it's absolutely necessary" (Christensen et al. 2016, p. 1671).

[40] That is, *IRREGULARITY* equals one if a firm's financial misreporting is directly designated as fraud in the Audit Analytics database (i.e., *Resfraud* flagged), is nonclerical in nature and not a simple accounting rule application failure (i.e., neither *Resclericalerrors* nor *Resaccounting* flagged), or is associated with SEC, PCAOB, or other regulatory body investigations (i.e., *Ressecinvestigation* flagged).

observable and unobservable time-invariant or slow-moving firm characteristics (e.g., corporate culture or differences in economic environment). Year fixed effects are added to account for time-varying cross-sectional effects that are common to all firms for a given year (e.g., strength of enforcement or changes in macroeconomic conditions). To understand whether and to what extent CEOs vis-à-vis CFOs affect financial misreporting, manager fixed effects are added by including a dummy variable for each CEO and CFO. Note that, by construction, manager fixed effects capture commonalities in the likelihood of financial misreporting across the different firms for which a manager works that are distinct from (1) the average likelihood of misreporting over time for a given firm, (2) the average likelihood of misreporting across firms for a given year, and (3) misreporting driven by observed firm characteristics.

We follow the literature on the determinants of restatements (e.g., Armstrong et al. 2013; Guo et al. 2016; Ham et al. 2017) to identify the vector of time-varying firm-level controls (X_{kit}).[41] There is no generally accepted model of restatements and the set of consistent variables that act as controls is small (e.g., Cao, Myers, and Omer 2012). However, our empirical model covers most of the determinants from the literature and captures firms' operating characteristics and capital market incentives. Throughout our study, we winsorize continuous controls at the 1st/99th percentiles of their distribution in the full CEO/CFO sample to mitigate the effect of outliers and cluster-adjust standard errors at the firm-level to account for serial correlation. Details on the definition of all variables are provided in the Appendix.

To account for greater reporting resources and regulatory scrutiny of larger firms, we control for firm size (*SIZE*). We include a firm's market-to-book ratio (*MTB*) to capture optimism in growth expectations reflected in stock market valuations and control for leverage (*LEVERAGE*), as highly leveraged firms are expected to have incentives to misreport to avoid violating debt covenants. We include net external financing from shareholders and debthold-

[41] Our findings are robust to measuring these control variables one year prior to the measure of financial misreporting (e.g., Armstrong et al. 2013).

ers (*EXTERNAL FINANCE*) to account for reporting incentives from capital raising and control for free cash flow (*FCF*) to capture a firm's internal financing capacity. Acquisitions (*ACQUISITION*) are included to address the inherent misstatement risk in the aftermath of an acquisition and investment-related incentives when pursuing acquisitions. Similarly, we control for the effect of restructurings (*RESTRUCTURE*). We address reporting incentives driven by (past) accounting performance by controlling for return on assets (*ROA*), the one-year change in return on assets (*CHANGE ROA*), and the incidence of a prior loss (*PRIOR LOSS*). We further include firms' prior-year stock returns (*PRIOR RETURN*), as past good (bad) capital market performance may generate reporting incentives to maintain (improve) that performance. Consistent with the idea that larger auditors provide higher-quality statutory audits, we control for a firm's use of a Big N audit firm (*BIG N*).[42]

Drawing on the work of Dechow et al. (2011), many studies also control for a bunch of accounting tools to capture other means of aggressive reporting that may evolve into restatements. In our main analysis, we follow these studies and include the following variables as additional controls. We start with a broad measure for the magnitude of accruals (*ACCRUALS*), as prior research finds high-accrual firms to be more prone to financial misreporting.[43] We further include a firm's one-year change in accounts receivable (*CHANGE RECEIVABLES*) and inventory (*CHANGE INVENTORY*), as the two accrual components have direct links to revenue recognition and cost of goods sold and thus misstatements of these accounts may be used to inflate sales and gross margin. In addition, we directly control for growth in

[42] We cannot include other corporate governance characteristics (e.g., board composition, institutional ownership, or analyst following) due to data constraints. However, the link between many governance mechanisms and financial misreporting is theoretically unclear (e.g., Baber, Kang, Liang, and Zhu 2015; Samuels, Taylor, and Verrecchia 2021), and studies find that, without firm fixed effects, empirical proxies show little relation to restatements (e.g., Larcker et al. 2007; Daines, Gow, and Larcker 2010). In addition, prior research shows that the large publicly traded firms in our sample have very similar governance structures due to regulation and listing requirements (e.g., Shive and Forster 2020). For example, as seen in Table 2, more than 90 percent of firm-years are audited by a Big N audit firm. Thus, cross-sectional differences in corporate governance are unlikely to explain financial misreporting. To the extent that governance structures are sticky at the firm-level and evolve over time, however, their effects are captured by fixed effects (e.g., Gormley and Matsa 2014; deHaan 2021).

[43] Accruals are calculated using the cash flow approach. We use total accruals due to issues related to the decomposition of accruals (e.g., Chen, Hribar, and Melessa 2018). In addition, most discretionary accruals models tend to have low explanatory power for financial misreporting beyond total accruals (e.g., Dechow et al. 2011).

cash sales (*CSALES GROWTH*). We include a firm's capital intensity (*CAPITAL INTENSITY*) to broadly address the extra discretion in the accounting treatment of soft assets (e.g., due to changes in assumptions or forecasts) and augment this variable with a measure for a firm's unrecognized intangibles from R&D and advertising (*INTANGIBLES*) to account for substitution effects of expensing of intangible assets. Finally, to account for the effect of off-balance-sheet activities, we control for the one-year change in operating leases (*CHANGE OPLEASE*), which can be used for financial statement window dressing (e.g., reporting lower expenses, inflated earnings, and reduced debt).[44]

Table 2 provides summary statistics of the restatement variables and time-varying firm-level controls in our different samples. In general, the reported means and standard deviations are consistent with those reported elsewhere. Table 3 provides correlations for the restatement variables and time-varying firm-level controls in the full CEO/CFO sample, which closely resemble correlations in prior literature.[45]

[Tables 2 and 3 about here]

4. Main Findings

4.1. CEOs versus CFOs in Financial Misreporting Using the Mobility Method

Table 4 provides results from running various regression specifications of equation (1) on the samples used to implement the mobility method, using *RESTATE* as dependent variable. Panel A, columns (1) to (4), apply different fixed effects structures to the CEO mobility sample. Columns (5) to (8) of Panel A apply similar fixed effects structures to the CFO mobility sample. For the CEO mobility sample, we find that the (adjusted) R^2 for the firm fixed effects

[44] We recognize that these variables could rather act as mediators than confounders in our analysis, as they are most likely part of the conceptual mechanism of interest (e.g., Gow et al. 2016; Whited et al. 2021). For similar reasons, we do not condition on manager-specific factors like, for example, their equity incentives, as prior studies find that managers influence the design of their compensation contracts (e.g., Morse et al. 2011; Abernethy et al. 2015). We address these issues in our additional analyses.

[45] Most correlations in Table 3 are statistically significant at the 10% level or better (two-tailed). Untabulated results from running auxiliary regressions yield variance inflation factors of less than 4 for the variables included in the vector of time-varying firm-level controls, with a mean of 1.44 in the full CEO/CFO sample. Hence, multicollinearity is not a concern.

regression in column (2) is 50.74 (31.57) percent.[46] Adding CEO fixed effects in column (3) increases the (adjusted) R^2 by 3.77 (3.62) percentage points, where the F-statistic of 3.55 (p-value <0.001) indicates that the estimated CEO fixed effects are jointly different from zero.[47] When we replace firm fixed effects by spell fixed effects for each CFO-firm combination in column (4), using a combination of the spell and mobility methods, we find that additionally controlling for the influence of CFOs further increases the (adjusted) R^2 by 14.38 (9.94) percentage points. The F-statistic of 2.12 (p-value <0.001) indicates that the estimated CEO fixed effects are still jointly different from zero. That is, for the CEO mobility sample, we find a statistically significant CEO-specific effect, regardless of whether we control for the incremental effect of CFOs. However, based on simple changes in (adjusted) R^2, the marginal effect of including CFOs in column (4) exceeds that of CEOs in column (3) by 10.61 (6.32) percentage points.

Examining the CFO mobility sample, we find that the (adjusted) R^2 for the firm fixed effects regression in column (6) is 44.60 (28.81) percent, which is comparable to the CEO mobility sample results. When we add CFO fixed effects in column (7), the (adjusted) R^2 increases by 12.17 (9.37) percentage points and the F-statistic of 2.99 (p-value <0.001) indicates that the estimated CFO fixed effects are jointly different from zero. Replacing firm fixed effects by spell fixed effects for each CEO-firm combination in column (8) again increases the (adjusted) R^2 by 6.83 (2.87) percentage points, where the F-statistic of 3.02 (p-value <0.001) indicates that the estimated CFO fixed effects are still jointly different from zero. In other words, for the CFO mobility sample, we also find a statistically significant CFO-specific

[46] Throughout our study, compared to the firm fixed effects regressions, we find low (adjusted) R^2 values for the regression specifications only including the vector of time-varying firm-level controls and year fixed effects, ranging from 2.93 (2.44) to 5.45 (3.34) percent. While these differences point to unobserved firm characteristics, they are consistent with prior literature (e.g., Armstrong et al. 2013; Ham et al. 2017; Wilde 2017). More generally, the large cross-sectional variation in financial misreporting that is left unexplained by models that rely on firm-specific factors further highlights the relevance of our approach.

[47] Note that critical F-values vary across empirical specifications due to differences in degrees of freedom. For example, the critical value at the 95th percentile of the F-distribution in columns (3) and (4) of Panel A in Table 4 is around 1.17, whereas in columns (7) and (8) it is 1.10. Therefore, we provide empirical F-statistics and corresponding p-values in parentheses.

effect, regardless of whether we control for the incremental effect of CEOs. As in the CEO mobility sample, a simple comparison in providing additional explanatory power indicates that the marginal effect of including CFOs in column (7) exceeds that of CEOs in column (8) by 5.34 (6.50) percentage points in terms of (adjusted) R^2. Taken together, the results in Panel A of Table 4 suggest that the CFO effect is economically larger in both mobility samples, providing preliminary evidence for the CFO discretion view of H1.

Panels B and C provide further evidence on the economic magnitude of the estimated manager-specific effects. In Panel B, we decompose the model R^2 to separate out the relative importance of manager fixed effects in explaining firms' financial misreporting. In particular, we follow Graham et al. (2012) and use the coefficient estimates from columns (3), (4), (7), and (8) in Panel A to benchmark CEO/CFO fixed effects against firm and spell fixed effects for each CFO/CEO-firm regime. Note that the model R^2 can be calculated from the variance-covariance matrix as follows:

Equation (2): $$R^2 = \frac{cov\ (\varphi_i, y_{it})}{var\ (y_{it})} + \frac{cov\ (\mu_t, y_{it})}{var\ (y_{it})} + \frac{cov\ (\theta_j, y_{it})}{var\ (y_{it})} + \frac{cov\ (\beta_k X_{kit}, y_{it})}{var\ (y_{it})}$$

where y_{it} represents the dependent variable, and the normalized covariances for firm or spell fixed effects (φ_i), year fixed effects (μ_t), manager fixed effects (θ_j), and the vector of time-varying firm-level controls (X_{kit}) indicate how much each set of variables contributes to the variation in the dependent variable. Therefore, $\frac{cov\ (\theta_j, y_{it})}{var\ (y_{it})}$ can be interpreted as partial explanatory power of manager fixed effects for the variation in financial misreporting.

Panel B reports percentage shares of the model R^2 (i.e., the explained variation in financial misreporting) attributable to CEO/CFO fixed effects versus firm or CFO/CEO-spell fixed effects, respectively. Column (1) of Panel B shows that CEO fixed effects account for 54.70 percent of the explained variation in *RESTATE* in the CEO mobility sample, whereas firm fixed effects explain 41.49 percent. However, as shown in column (2), the economic importance of CEO fixed effects sharply drops to 26.93 percent when we replace firm fixed effects by spell fixed effects for each CFO-firm combination to control for the incremental effect of CFOs. For the CFO mobility sample, columns (3) and (4) of Panel B show that CFO fixed effects account for 39.59 percent of the explained variation in *RESTATE*, compared to 58.32 percent for firm fixed effects, and their economic importance increases to 42.94 percent when we replace firm fixed effects by spell fixed effects for each CEO-firm combination.[48]

In Panel C, we study the distribution of the estimated CEO/CFO fixed effects to assess how large the observed differences between managers are, using the coefficients from columns (3), (4), (7), and (8) in Panel A.[49] For the CEO mobility sample, column (1) of Panel C shows that replacing a CEO in the lower quartile by one in the upper quartile increases a firm's misreporting probability by 20.90 percentage points. This interquartile range drops to 14.32 percentage points when we replace firm fixed effects by CFO-spell fixed effects in column (2). Similarly, for the CFO mobility sample, columns (3) and (4) of Panel C show that replacing a CFO in the lower quartile by one in the upper quartile increases the probability of financial misreporting by 29.57 and 22.97 percentage points, respectively. Given an observed mean misreporting probability of 12.24 (13.01) percent in the CEO (CFO) mobility sample, we consider these effects economically significant. However, the findings in Panels B and C of Table 4 collectively support the notion that CFOs have a greater economic impact on firms' financial misreporting than CEOs, corroborating the CFO discretion view.

[Table 4 about here]

4.2. CEOs versus CFOs in Financial Misreporting Using the Connectedness Method

Table 5 replicates the analysis for the samples used to implement the connectedness method. Looking at the CEO connectedness sample in columns (3) and (4) of Panel A, we again find a

[48] Note that the incremental effect of CEOs/CFOs on financial misreporting is economically larger than that of managers on earnings management documented in prior studies. For example, using the mobility method, Wells (2020) finds that manager fixed effects account for 12.83 percent of the explained variability of accruals based on a modified version of the Dechow and Dichev (2002) model (Panel B, column (2) of Table 4 in her study).

[49] Due to the estimation procedure, means of the estimated manager fixed effects are normalized to zero. Note that manager fixed effects are estimated relative to a benchmark. While point estimates may change with different benchmarks, the shape of the distribution of the fixed effect estimates does not depend on benchmarks.

statistically significant CEO-specific effect, regardless of whether we control for the influence of CFOs. Similarly, for the CFO connectedness sample, we find a statistically significant CFO-specific effect, regardless of whether we control for the incremental effect of CEOs in columns (7) and (8).[50] Thus, the results in Panel A of Table 5 indicate that CEOs and CFOs significantly affect firms' financial misreporting. However, based on simple changes in (adjusted) R^2, their economic importance is less clear than in the mobility sample analysis. Specifically, for the CEO connectedness sample, the marginal effect of including CEOs in column (3) exceeds that of CFOs in column (4) by 10.21 (6.95) percentage points. On the other hand, for the CFO connectedness sample, the marginal effect of including CFOs in column (7) exceeds that of CEOs in column (8) by 21.07 (14.26) percentage points. One possible explanation is the estimation procedure of the connectedness method, where manager fixed effects play a more important role as compared to the mobility method, resulting in fixed effect estimates that are not fully purged of firm-specific influences (e.g., Graham et al. 2012). As a result, an analysis based on simple changes in (adjusted) R^2 can systematically overstate the marginal effect of including managers relative to firm or spell fixed effects when applying the connectedness method.[51]

Panels B and C further illuminate the economic meaning of the estimated CEO/CFO fixed effects. Using the estimation results from the most rigorous regression specifications, we find that CEOs and CFOs account for 41.36 or 52.28 percent of the explained variation in

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[50] Note that a small number of managers is connected through mobility in terms of firms but not in terms of spells. Thus, the analysis including firm, CEO, and CFO fixed effects by using a combination of the spell and connectedness methods in columns (4) and (8) of Panel A in Table 5 can only be performed on a subset of firm-years in the connectedness samples.

[51] Graham et al. (2012) point out that the higher relative importance of manager fixed effects is due to the fact that the connectedness samples include both moving and nonmoving managers. For moving managers, firm fixed effects contribute to their variation in financial misreporting because the managers switch between firms with different firm fixed effects. In contrast, for nonmoving managers in the same firm, firm fixed effects do not contribute to the variation in financial misreporting between these managers as they have the same firm fixed effect. Firm fixed effects only contribute to the variation of nonmoving managers across different firms. In other words, firm fixed effects contribute to the between- but not the within-firm variation, which is why they play a more important role for moving managers than for nonmoving ones. Hence, firm fixed effects are of higher relative importance in samples with higher managerial mobility, and applying the connectedness method, by estimation, shifts the relative importance toward manager-specific effects.

RESTATE in columns (2) and (4) of Panel B, and the difference in the interquartile ranges of the CEO and CFO fixed effect estimates in columns (2) and (4) of Panel C correspond to an increase in a firm's misreporting probability of 19.03 and 31.74 percentage points, respectively. Given an observed mean misreporting probability of 13.74 (13.47) percent in the CEO (CFO) connectedness sample, both effects are economically significant. However, the results in Panels B and C of Table 5 again indicate a greater economic impact of CFOs. We interpret the collective evidence as providing support for the CFO discretion view. Overall, these findings suggest that our mobility sample results generalize to a larger sample of managers.

We recognize that the effect size reported in Panels B and C of Tables 4 and 5 is quite large, which calls into question whether differences in financial misreporting can be attributed to CEOs/CFOs alone. These differences may, to some extent, be driven by unobserved firm characteristics (e.g., suggested by the low explanatory power for the regression specifications only including the vector of time-varying firm-level controls and year fixed effects) that can relate to CEO/CFO appointments. However, our empirical model resembles the specifications from prior studies (e.g., Armstrong et al. 2013; Guo et al. 2016; Ham et al. 2017).[52]

[Table 5 about here]

4.3. CEOs versus CFOs in Financial Misreporting Using the Spell Method

Table 6 replicates the analysis for the full CEO/CFO sample used to implement the spell method. When we apply the spell method with firm and CEO fixed effects in column (3), we find that adding CEO fixed effects increases the (adjusted) R^2 by 17.91 (11.65) percentage points compared to the firm fixed effects regression in column (2). However, replacing CEO-spell fixed effects by firm and CFO fixed effects in column (4) increases the (adjusted) R^2 by 23.54 (15.76) percentage points. Although both effects are statistically significant, based on simple changes in (adjusted) R^2, the marginal effect of including CFOs again exceeds that of

[52] In addition, our results on the economic importance of manager fixed effects relative to firm or spell fixed effects as well as CEOs versus CFOs should largely be unaffected by unobserved firm characteristics, if any.

CEOs by 5.63 (4.11) percentage points. While the spell method can only estimate the combined influence of managers and firms, these findings closely resemble the mobility sample results. Note that the analysis is performed on the same sample of firms for both CEOs and CFOs, and thus their style effect differential cannot be explained away by sample differences. Taken together, the results in Table 6 provide additional evidence for the CFO discretion view, showing that our main inferences hold in a larger sample of managers and firms.[53]

[Table 6 about here]

4.4. CEOs versus CFOs in Fraudulent Misreporting

In this section, we test H2 and focus on firms' fraudulent misreporting. Columns (1) to (4) of Table 7 replicate the mobility sample analysis using *IRREGULARITY* as dependent variable. For the CEO mobility sample, we fail to find a statistically significant CEO-specific effect after controlling for the influence of CFOs in column (2) of Panel A. In contrast, examining the CFO mobility sample, we find a statistically significant CFO-specific effect, regardless of whether we control for the incremental effect of CEOs in columns (3) and (4). In addition, a simple comparison in providing additional explanatory power in the CFO mobility sample indicates that the marginal effect of including CFOs exceeds that of CEOs by 3.13 (3.14) percentage points in terms of (adjusted) R^2 (untabulated). These results suggest a significant CFO effect, but they provide no evidence for an effect of CEOs.[54]

Panels B and C more closely examine the economic magnitude of the estimated CFO fixed effects. Using the estimation results from the most rigorous regression specification, we find that CFOs account for 43.20 percent of the explained variation in *IRREGULARITY*, as seen in column (4) of Panel B, and the difference in the interquartile range of the CFO fixed effect estimates in column (4) of Panel C corresponds to an increase of 2.04 percentage points

[53] A subset of firms in the full CEO/CFO sample only appears under one CEO or CFO throughout the entire sample period. For these firms, the estimated firm fixed effects (column (2) of Table 6) and spell fixed effects (columns (3) and (4) of Table 6) cannot be distinguished. However, untabulated results show that our inferences are unchanged after removing these firms from the samples used throughout our study.
[54] Untabulated results show that this CFO effect generalizes to the connectedness method and the spell method.

in a firm's probability for fraudulent misreporting. Given an observed mean probability of 1.52 percent for irregularities, we consider this CFO effect also economically significant.

Note that, while covering the whole spectrum of financial misreporting, restatements may be an incomplete measure for fraudulent misreporting (e.g., Amiram et al. 2018). In particular, recent studies suggest potential type-I errors in fraudulent restatements, with many of these restatements lacking credible fraud allegations (e.g., Karpoff et al. 2017; Donelson et al. 2021). Therefore, columns (5) to (8) of Table 7 replicate the former analysis using *AAER* as dependent variable, an indicator variable taking the value of one for firm-year observations where the SEC identified a misstatement in an AAER. While we find a marginally significant CEO effect after controlling for the influence of CFOs in column (6) of Panel A, the findings comport with those reported in columns (1) to (4); that is, the CFO effect is more pronounced and economically more important.[55]

Taken together, the results in Table 7 suggest a significant CFO effect on firms' fraudulent misreporting, while we fail to find a meaningful effect of CEOs. We view the collective evidence in this section as providing support for H2, particularly the "CFO discretion view". Having said that, this finding appears to contradict the findings of Feng et al. (2011), who suggest that CFOs in a sample of AAER firms contribute to material accounting manipulations because of CEO pressure resulting from the CEO's incentives and power. However, for CFO fixed effects to matter under the mobility method, financial misreporting must be correlated across (at least) two different firms or CEO-firm regimes when the same CFO is present. Specifically, the CFO fixed effects in columns (3), (4), (7), and (8) of Table 7 are constructed in a way that captures commonalities in firms' misreporting associated with a given CFO that are distinct from the average misreporting over time for a given firm (including firm fixed

[55] The observed mean probability of receiving an AAER is around one percent (untabulated). Note the much higher F-statistic of 5.55 for CFO fixed effects in column (8) as compared to 1.55 for CEO fixed effects in column (6) of Panel A, where the critical value at the 95th percentile of the F-distribution is 1.18 and 1.10, respectively. Further, as seen in column (8) of Panel C, CFOs seem to increase the probability for fraudulent misreporting across the distribution of fixed effect estimates.

effects) and CEO-firm combination (including CEO-spell fixed effects), respectively. Hence, CFOs need to face the same setup of CEO incentives and power across different firms and, especially, CEO-firm regimes to explain away their incremental effect, making a pressure-based interpretation of our findings unlikely. To further rule out this alternative explanation, we examine incentives provided by managers' equity portfolios in the next section.

[Table 7 about here]

5. Additional Analyses

5.1. Mediation Analyses

Throughout our study, we do not condition on managers' equity incentives as they are part of the conceptual mechanism of interest. While managers may respond to equity incentives, equity incentives are not allocated randomly across managers and firms. For example, prior studies show that managers influence the design of their compensation contracts (e.g., Morse et al. 2011; Abernethy et al. 2015), where others suggest that differences in ability (e.g., Milbourn 2003; Albuquerque et al. 2013) and risk aversion (e.g., Graham et al. 2013; Page 2018) primarily drive compensation, particularly equity compensation. In line with this, Graham et al. (2012) and Coles and Li (2020) find that manager fixed effects explain most of the variation in executive compensation, including equity incentives from portfolio delta and vega. As a result, given that equity incentives are determined by managers themselves, conditioning on managers' equity incentives can introduce bias, as equity incentives represent an indirect path for their total effect; that is, they mediate the relation between managers and firms' financial misreporting (e.g., Gow et al. 2016; Whited et al. 2021).

In the CEO–CFO relationship, however, equity incentives of one manager (e.g., the CEO) might also affect the behavior of another (e.g., the CFO), which may confound the analysis under certain conditions (e.g., Friedman 2014; Dikolli et al. 2021). To examine this possibility, we augment equation (1) with variables capturing CEOs' and CFOs' pay-performance sensitivity from their portfolio delta and risk-taking incentives provided by vega (Core and Guay 2002), obtained from Coles, Daniel, and Naveen (2006). Columns (1) to (4) of Table 8 replicate the mobility sample analysis using *RESTATE* as dependent variable. The results closely resemble those in Table 4; that is, we continue to find an incremental effect of CFOs that is more significant than that of CEOs, conditional on their equity incentives.[56] Untabulated results show negative and insignificant coefficients on *CEO DELTA* and *CFO DELTA* across the different regression specifications. However, the coefficients on *CEO VEGA* and *CFO VEGA* are strictly positive, where *CEO VEGA* is also statistically significant in columns (3) and (4) of Panel A (p-value of 0.031 and 0.019, respectively). While we include an extensive set of fixed effects, these results reconcile the findings from prior literature, suggesting that risk-taking incentives provided by vega subsume those of delta (e.g., Armstrong et al. 2013), top-level executives in different positions respond differently to equity incentives (e.g., Davidson 2021), and CEOs' equity incentives matter for financial misreporting (e.g., Feng et al. 2011). More importantly, these results show that CEO incentives (to pressure the CFO) are not driving our findings, and they are consistent with the idea that our fixed effects approach picks up unobserved confounding factors in the CEO-CFO relationship beyond those from equity incentives, for example, due to differences in ability or risk aversion (e.g., Graham et al. 2012; Gormley and Matsa 2014).

Alternatively, manager fixed effects work indirectly through managers' impact on different firm characteristics. This would imply that some of our control variables might pick up variation in financial misreporting that is indeed attributable to managers' total effect when these variables are part of the conceptual mechanism, which may affect our findings on the economic importance of CEOs and CFOs. For example, managers can use accounting tools to achieve reporting goals, for example, by means of overstated inventory, inflated sales, or dis-

[56] Note that time-varying differences in equity incentives are captured by *CEO DELTA*, *CEO VEGA*, *CFO DELTA*, and *CFO VEGA*, whereas time-invariant or slow-moving differences are subsumed by fixed effects.

cretion in impairments of purchased intangibles like, for example, goodwill. In line with this, a large body of literature argues that managers use accruals to adjust financial statements, that is, to signal private information or manage reported earnings opportunistically to mask certain aspects of a firm's underlying economics, and prior studies find that, prior to financial misreporting, managers make within-GAAP accounting choices to report higher accruals (e.g., Schrand and Zechman 2012; Chu et al. 2019). Similarly, managers can use off-balance-sheet activities like operating leases to front-load earnings and reduce reported debt.

As it is unclear whether those earnings management variables act as confounders or mediators in our analysis, we also perform an unconditional analysis where we estimate equation (1) without additional controls as described in section 3. The results in columns (5) to (8) of Table 8 resemble those in Table 4. Thus, our findings are not driven by firm characteristics that are (to varying degrees) under the influence of the CEO versus the CFO, i.e., potential outcomes of managers' behavior. Taken together, the results in Table 8 suggest that our findings are not sensitive to specification choices for the underlying conceptual mechanism.

[Table 8 about here]

5.2. Exogenous Turnover

A remaining concern is that our findings could be driven by matching of managers and firms; that is, managers might self-select into certain firms or firms choose managers based on their reporting style to meet their time-varying needs (e.g., Fee et al. 2013; Pan 2017). To alleviate matching concerns, we replicate our analysis using a setting of plausibly exogenous management turnover. Following Fee et al. (2013), we assume CEO/CFO departures that involve death, health problems, or natural retirements based on age thresholds more likely to be exogenous than merely reflecting a firm's endogenous choice to hire managers with different reporting styles. Therefore, we exploit a manager's age and use the ExecuComp variable *Rea-*

sonLeftCompany to identify a subset of firms with CEO/CFO turnovers that can be viewed as largely exogenous for our purposes.[57]

Table 9 provides results from running various regression specifications of equation (1) on the restricted mobility samples, using *RESTATE* as dependent variable. While this exogeneity restriction only yields 85 CEO fixed effects and 69 CFO fixed effects, the results resemble those in Table 4. Specifically, we find statistically significant CEO- and CFO-specific effects across the different regression specifications in Panel A. In Panels B and C, using the estimation results from the most rigorous regression specifications, we find that CEOs versus CFOs account for 40.86 and 47.42 percent of the explained variation in *RESTATE*, and the difference in the interquartile ranges of the CEO and CFO fixed effect estimates correspond to an increase in a firm's misreporting probability of 10.01 and 54.26 percentage points, respectively.[58] Hence, our findings remain consistent in a setting where the matching issue is arguably less problematic. However, given that a firm's management replacement choices are unobservable, we cannot rule out confounding effects or alternative explanations.[59] Note that, unlike in the mobility samples used in our main analysis, exogenous turnover appears to be less common for CFOs than CEOs, which again comports with the findings of Mian (2001). Thus, our findings are not driven by the higher mobility of CFOs.[60]

[Table 9 about here]

5.3. Predicted Misreporting Risk

In our main analysis, we use restatements covered in Audit Analytics as dependent variable. However, restatements can be an incomplete proxy for measuring the construct of financial

[57] Specifically, this subset of firms includes nonmoving managers' turnovers at the age of 63 or older and turnovers classified as "retired" or "deceased" in the ExecuComp database.
[58] The observed mean misreporting probability in the restricted CEO (CFO) mobility sample is 9.77 (12.12) percent (untabulated).
[59] For example, even in the event of sudden death, only the departure of the incumbent manager is clearly exogenous, while the appointment of a replacement manager is still endogenous.
[60] More generally, as seen in Table 1, the share of CEOs and CFOs included in the analysis varies across the different samples used throughout our study. However, we find consistent results for these samples, and thus our findings cannot be viewed as spurious correlations due to differences in mobility among CEOs and CFOs.

misreporting. In particular, while unambiguously reflecting material beyond-GAAP reporting behavior as determined by a third party (i.e., low type-I error rate), the type-II error rate of samples based on restatements might be high as misreporting firms erroneously are classified as non-misreporting firms, either due to data omissions in Audit Analytics or because their misreporting goes undetected (e.g., Dechow et al. 2010; Karpoff et al. 2017). For example, using a stock option backdating setting, Curtis, Donelson, and Hopkins (2019) find that many firms suspected of financial misreporting fail to restate their financial statements. Therefore, we examine whether the documented CEO/CFO effects hold for firms' predicted misreporting risk, using the F-Score developed by Dechow et al. (2011) as dependent variable.[61]

Table 10 replicates the mobility sample analysis, where each sample is restricted to include all observations with non-missing F-Score values. The dependent variable in columns (1) to (4) is *FSCORE>1*, an indicator variable taking the value of one for firm-year observations with an estimated F-Score larger than one, which is used to capture firms' above normal misreporting risk. For the CEO mobility sample in columns (1) and (2) of Panel A, we find a statistically significant CEO-specific effect in both regression specifications controlling for firm fixed effects and CFO-spell fixed effects. Similarly, for the CFO mobility sample in columns (3) and (4), we find a statistically significant CFO-specific effect, regardless of whether we control for the incremental effect of CEOs. Panels B and C suggest that manager-specific effects are also economically meaningful. Specifically, using the estimation results from the most rigorous regression specifications, we find that CEOs and CFOs account for 56.58 or 40.71 percent of the explained variation in *FSCORE>1* in columns (2) and (4) of Panel B, and the difference in the interquartile ranges of the CEO and CFO fixed effect estimates in columns (2) and (4) of Panel C corresponds to an increase of 79.41 and 85.28 percentage points in a firm's probability of having an above normal misreporting risk. Given an observed mean

[61] For the purpose of the following analysis, *EXTERNAL FINANCE*, *CHANGE ROA*, *ACCRUALS*, *CHANGE RECEIVABLES*, *CHANGE INVENTORY*, *CSALES GROWTH*, and *CAPITAL INTENSITY* are not included in the vector of time-varying firm-level controls as similar variables are used to calculate a firm's F-Score.

probability of 44.78 (43.61) percent of having an estimated F-Score larger than one in the CEO (CFO) mobility sample (untabulated), we consider both effects economically significant. In sum, except for the somewhat higher partial explanatory power of CEO fixed effects, these results largely serve to confirm our findings in Table 4.[62]

Next, we examine the effect of CEOs and CFOs on a firm's likelihood of having a high misreporting risk. Columns (5) to (8) of Table 10 replicate the former analysis, but here we use *FSCORE>Q3* as dependent variable, an indicator variable taking the value of one for firm-year observations with an estimated upper quartile F-Score. For both mobility samples, we again find statistically significant CEO-specific and CFO-specific effects across the different regression specifications in Panel A. Panels B and C indicate that these effects are also economically significant. However, the economic importance of CFOs vis-à-vis CEOs increases in firms with a high predicted misreporting risk, which is consistent with our findings in Table 7. Taken together, the results in Table 10 suggest that our findings are not driven by type-II errors in the restatement data.

[Table 10 about here]

6. Conclusion

We study the economic importance of CEOs and CFOs in the beyond-GAAP setting. Prior studies focusing on equity incentives find inconsistent results (e.g., Feng et al. 2011; Armstrong et al. 2013). Addressing unobserved confounding factors and challenges to the separation of firm-specific and manager-specific effects, we use the concept of manager style to investigate CEOs' versus CFOs' total effects and combine three different methods to identify style effects. Our findings can be summarized as follows. First, both top-level executives significantly affect their firms' financial misreporting. Second, in terms of economic magnitude, the impact of CFOs is greater than that of CEOs. Third, the greater economic impact of CFOs

[62] Untabulated results using raw values of F-Score as dependent variable are similar. Note that the larger economic importance of CEOs may be due to the fact that F-Score is constructed from different firm characteristics, e.g., firm performance and financing activities, which may rather reflect changes in general firm policies.

vis-à-vis CEOs increases in cases of fraudulent misreporting. These findings remain consistent across different samples, methods, misreporting measures, and specification choices for the underlying conceptual mechanism. Thus, we interpret the collective evidence in our study as providing support for the notion that CFOs play a more relevant role in firms' financial misreporting than CEOs.

Besides the contributions to the literature, these findings can inform regulators when designing and implementing corporate governance reforms intended to increase financial reporting quality. While the CFO is generally viewed as watchdog over the financial reporting process, our findings indicate that not all CFOs meet these requirements. The documented CFO effect in our study cannot be attributed to CEO incentives (to pressure the CFO). Therefore, in addition to redesigning compensation structures and improving the CFO's independence from the CEO, reform efforts should consider changing the legal basis for CFO liability and shifting corporate governance toward a stricter monitoring of the CFO's behavior.

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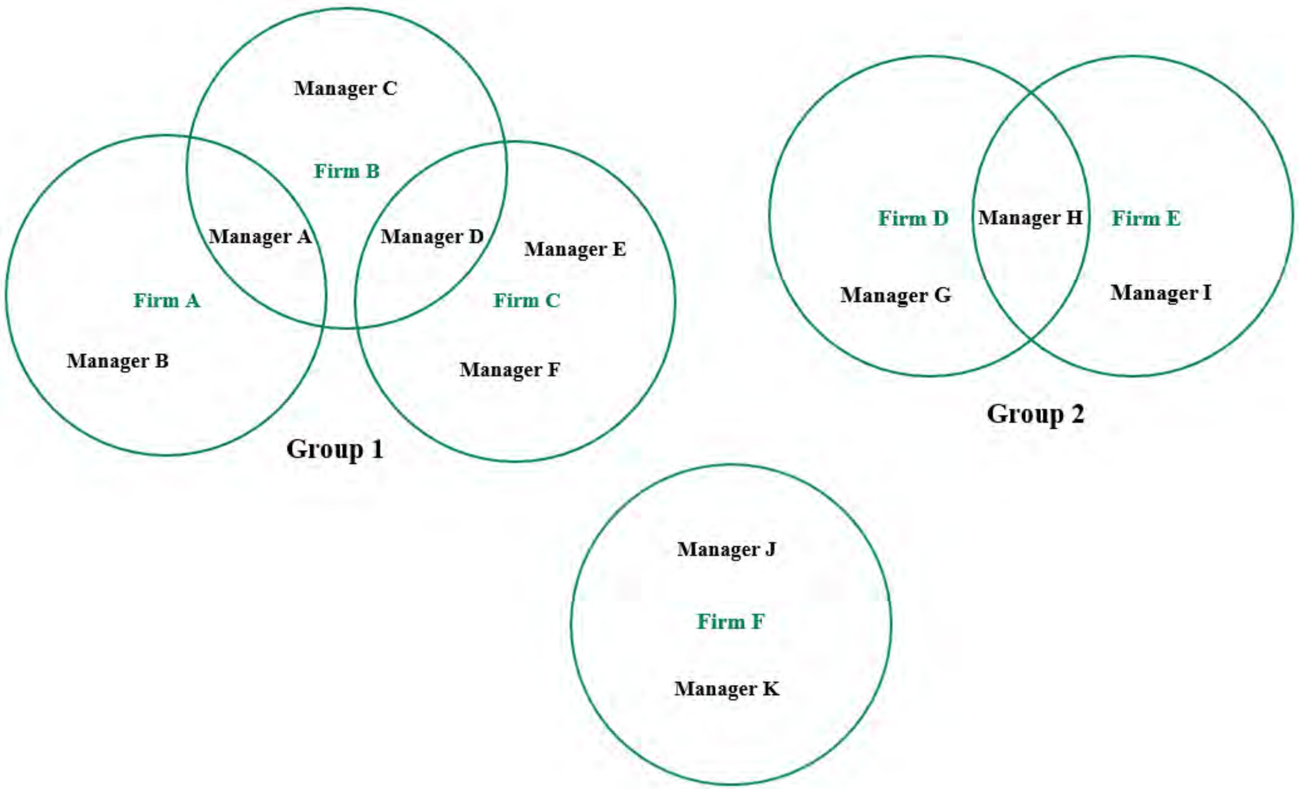
Tables and Figures

FIGURE 1

Identification of Manager Effects: Mobility Method, Connectedness Method, and Spell Method

This figure illustrates the universe of firms and managers taken into account by the three methods implemented in this study. Firms are represented by circles, managers are located within the circles. The mobility method (Bertrand and Schoar 2003) restricts the analysis to managers employed by at least two different firms. The connectedness method (Abowd et al. 1999) extends the analysis to include nonmoving managers employed by firms with at least one moving manager. When applying the spell method (Abowd et al. 1999), all unique manager-firm combinations are included in the analysis.



Mobility Method

- 3 Managers
- 5 Firms

Connectedness Method

- 9 Managers
- 5 Firms

Spell Method

- 14 Manager-Firm Combinations

TABLE 1

Sample Composition

This table provides a summary of data availability for the samples used throughout our study. "Full CEO/CFO" represents the sample used to implement the spell method and includes all observations from the Audit Analytics/Compustat/ExecuComp intersection with available CEO and CFO data. "CEO Mobility" and "CFO Mobility" represents the samples used to implement the mobility method, including all observations for moving CEOs/CFOs employed by at least two firms. "CEO Connectedness" and "CFO Connectedness" represents the samples used to implement the connectedness method, including all observations for CEOs/CFOs employed by firms with at least one moving CEO/CFO. Details on the sample construction are provided in section 3.

	Full CEO/CFO	**CEO Mobility**	**CFO Mobility**	**CEO Connectedness**	**CFO Connectedness**
Firms	3,105	430	1,072	430	1,072
Firm-years	30,208	1,675	5,011	4,723	12,505
Restatements	1,881	128	392	326	841
Firm-years with restatement (%)	3,621 (11.99)	205 (12.24)	652 (13.01)	649 (13.74)	1,684 (13.47)
Irregularities	209	14	42	46	101
Firm-years with irregularities (%)	516 (1.71)	25 (1.49)	76 (1.52)	110 (2.33)	261 (2.09)
CEOs	6,118	234	1,560	963	2,586
CFOs	6,659	641	680	1,225	2,695

TABLE 2

Summary Statistics

This table provides summary statistics (means and standard deviations) of the restatement variables and time-varying firm-level controls in the samples used to implement the spell method ("Full CEO/CFO"), the mobility method ("CEO Mobility" and "CFO Mobility"), and the connectedness method ("CEO Connectedness" and "CFO Connectedness"). Sample size refers to the largest number of observations available for each method where the unit of analysis is at the firm-year level. Details on the definition of all variables are provided in the Appendix.

	Full CEO/CFO		**CEO Mobility**		**CFO Mobility**		**CEO Connectedness**		**CFO Connectedness**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
RESTATE	0.120	0.325	0.122	0.328	0.130	0.336	0.137	0.344	0.135	0.341
IRREGULARITY	0.017	0.130	0.015	0.121	0.015	0.122	0.023	0.151	0.021	0.143
SIZE	7.409	1.644	7.827	1.603	7.625	1.560	7.795	1.624	7.630	1.604
MTB	2.920	3.604	2.768	3.894	2.734	3.414	2.824	4.059	2.955	3.637
LEVERAGE	0.228	0.193	0.264	0.203	0.235	0.189	0.258	0.195	0.230	0.187
EXTERNAL FINANCE	0.077	0.224	0.075	0.231	0.063	0.222	0.070	0.226	0.075	0.228
FCF	0.052	0.096	0.047	0.086	0.050	0.090	0.051	0.090	0.054	0.090
ACQUISITION	0.646	0.478	0.666	0.472	0.699	0.459	0.650	0.477	0.680	0.466
RESTRUCTURE	0.385	0.487	0.447	0.497	0.514	0.500	0.447	0.497	0.473	0.499
ROA	0.044	0.104	0.037	0.096	0.034	0.103	0.042	0.101	0.043	0.102
CHANGE ROA	-0.003	0.093	-0.001	0.092	-0.004	0.099	-0.003	0.095	-0.003	0.095
PRIOR LOSS	0.248	0.432	0.299	0.458	0.303	0.460	0.285	0.452	0.279	0.448
PRIOR RETURN	0.197	0.636	0.216	0.681	0.176	0.647	0.203	0.665	0.202	0.659
BIG N	0.915	0.279	0.960	0.196	0.951	0.217	0.963	0.188	0.946	0.226
ACCRUALS	-0.544	2.073	-0.609	2.237	-0.600	2.436	-0.533	2.133	-0.563	2.236
CHANGE RECEIVABLES	0.016	0.052	0.013	0.051	0.011	0.050	0.013	0.051	0.014	0.051
CHANGE INVENTORY	0.008	0.036	0.007	0.035	0.006	0.033	0.007	0.034	0.008	0.035
CSALES GROWTH	0.091	0.397	0.092	0.373	0.076	0.340	0.089	0.356	0.090	0.342
CAPITAL INTENSITY	0.259	0.233	0.282	0.238	0.252	0.220	0.282	0.234	0.255	0.215
INTANGIBLES	0.054	0.101	0.051	0.082	0.065	0.104	0.050	0.082	0.060	0.096
CHANGE OPLEASE	0.006	0.025	0.005	0.025	0.005	0.025	0.005	0.026	0.006	0.026
N	30,208		1,675		5,011		4,723		12,505	

TABLE 3

Full CEO/CFO Sample Correlations

This table provides Pearson correlations (bottom) and Spearman rank correlations (top) for the restatement variables and time-varying firm-level controls in the full CEO/CFO sample. Bold values indicate statistical significance at the 10% level or better (two-tailed). All correlations are similar for the other samples used throughout our study. Details on the definition of all variables are provided in the Appendix.

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
(1) *RESTATE*		**0.357**	**-0.036**	**-0.043**	**0.018**	-0.008	**-0.033**	**0.023**	**0.069**	**-0.063**	**-0.018**
(2) *IRREGULARITY*	**0.357**		-0.001	-0.003	**0.021**	0.005	**-0.019**	**0.023**	**0.010**	**-0.028**	**-0.017**
(3) *SIZE*	**-0.037**	-0.001		**0.369**	**0.130**	**0.126**	**0.246**	**0.148**	**0.081**	**0.286**	**0.058**
(4) *MTB*	**-0.019**	-0.005	**0.235**		**-0.107**	**0.268**	**0.419**	**0.065**	**-0.060**	**0.499**	**0.128**
(5) *LEVERAGE*	**0.020**	**0.018**	**0.064**	**-0.063**		**-0.064**	**-0.230**	**0.067**	**0.068**	**-0.222**	**-0.041**
(6) *EXTERNAL FINANCE*	0.007	**0.017**	**0.110**	**0.134**	0.001		**0.221**	**0.101**	**-0.181**	**0.482**	**0.157**
(7) *FCF*	**-0.021**	**-0.012**	**0.263**	**0.260**	**-0.211**	**0.168**		**0.127**	0.000	**0.634**	**0.145**
(8) *ACQUISITION*	**0.023**	**0.023**	**0.153**	**0.027**	**0.035**	**0.106**	**0.111**		**0.142**	**0.032**	**-0.054**
(9) *RESTRUCTURE*	**0.069**	**0.010**	**0.076**	**-0.050**	**0.060**	**-0.130**	**-0.018**	**0.142**		**-0.157**	**0.010**
(10) *ROA*	**-0.045**	**-0.020**	**0.319**	**0.246**	**-0.194**	**0.343**	**0.639**	**0.023**	**-0.153**		**0.303**
(11) *CHANGE ROA*	-0.007	**-0.011**	**0.054**	**0.066**	**-0.025**	**0.151**	**0.128**	**-0.043**	0.006	**0.401**	
(12) *PRIOR LOSS*	**0.054**	**0.016**	**-0.308**	**-0.074**	**0.087**	**-0.099**	**-0.272**	**-0.103**	**0.174**	**-0.369**	**0.224**
(13) *PRIOR RETURN*	**0.014**	**0.014**	**0.071**	**0.134**	**-0.060**	**0.254**	**0.109**	0.008	**-0.087**	**0.184**	**0.027**
(14) *BIG N*	**0.033**	**0.020**	**0.234**	**0.022**	**0.108**	**-0.010**	**0.038**	**0.045**	**0.067**	**0.036**	-0.005
(15) *ACCRUALS*	-0.006	-0.008	**0.035**	**0.042**	**-0.071**	**0.043**	**0.012**	-0.007	**-0.025**	**0.126**	**0.085**
(16) *CHANGE RECEIVABLES*	**-0.010**	**0.016**	**0.049**	**0.099**	**-0.041**	**0.453**	**0.020**	**0.103**	**-0.140**	**0.178**	**0.085**
(17) *CHANGE INVENTORY*	0.002	**0.013**	**0.035**	**0.061**	**-0.021**	**0.362**	**-0.040**	**0.062**	**-0.105**	**0.198**	**0.047**
(18) *CSALES GROWTH*	**0.019**	**0.026**	**0.044**	**0.110**	**-0.024**	**0.298**	**0.120**	**0.056**	**-0.093**	**0.149**	**0.101**
(19) *CAPITAL INTENSITY*	**-0.028**	**-0.033**	**0.040**	**-0.063**	**0.276**	**-0.039**	**-0.226**	**-0.199**	**-0.141**	**-0.015**	**-0.017**
(20) *INTANGIBLES*	**0.016**	0.007	**-0.079**	**0.118**	**-0.149**	**0.019**	**-0.112**	-0.008	**0.101**	**-0.277**	**-0.050**
(21) *CHANGE OPLEASE*	**0.018**	**0.012**	0.002	**0.088**	**-0.067**	**0.280**	**0.086**	**0.029**	**-0.124**	**0.137**	**-0.043**

TABLE 3 *(continued)*

	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
(1) *RESTATE*	**0.054**	-0.003	**0.033**	**-0.044**	**-0.018**	-0.005	0.006	**-0.026**	**0.033**	0.008
(2) *IRREGULARITY*	**0.016**	0.006	**0.020**	**-0.011**	**0.013**	0.009	**0.024**	**-0.030**	**0.010**	**0.016**
(3) *SIZE*	**-0.295**	**0.149**	**0.236**	**0.103**	**0.070**	**0.062**	**0.082**	**0.033**	**-0.035**	**0.060**
(4) *MTB*	**-0.177**	**0.235**	**0.043**	**0.241**	**0.190**	**0.145**	**0.255**	**-0.016**	**0.223**	**0.147**
(5) *LEVERAGE*	**0.049**	**-0.059**	**0.128**	**-0.188**	**-0.071**	**-0.030**	**-0.067**	**0.301**	**-0.252**	**-0.026**
(6) *EXTERNAL FINANCE*	**-0.190**	**0.270**	-0.008	**0.205**	**0.424**	**0.355**	**0.419**	**-0.015**	0.004	**0.269**
(7) *FCF*	**-0.264**	**0.153**	**0.024**	**0.108**	**0.055**	**0.014**	**0.224**	**-0.193**	**0.158**	**0.119**
(8) *ACQUISITION*	**-0.103**	**0.044**	**0.045**	0.000	**0.117**	**0.068**	**0.103**	**-0.152**	**0.057**	**0.091**
(9) *RESTRUCTURE*	**0.174**	**-0.108**	**0.067**	**-0.066**	**-0.149**	**-0.108**	**-0.176**	**-0.088**	**0.184**	**-0.128**
(10) *ROA*	**-0.378**	**0.283**	**0.021**	**0.492**	**0.241**	**0.265**	**0.335**	**0.038**	**0.052**	**0.191**
(11) *CHANGE ROA*	**0.251**	**0.079**	0.001	**0.183**	**0.135**	**0.075**	**0.155**	**-0.014**	-0.006	**-0.030**
(12) *PRIOR LOSS*		**-0.103**	**-0.052**	**-0.146**	**-0.121**	**-0.131**	**-0.099**	**-0.027**	**0.136**	**-0.141**
(13) *PRIOR RETURN*	0.009		0.009	**0.173**	**0.228**	**0.225**	**0.341**	**-0.032**	-0.007	**0.151**
(14) *BIG N*	**-0.052**	**-0.011**		**-0.032**	**-0.014**	-0.009	0.001	**0.075**	**-0.039**	**0.030**
(15) *ACCRUALS*	**-0.049**	**0.060**	**-0.011**		**0.212**	**0.208**	**0.134**	**-0.156**	**0.106**	**0.050**
(16) *CHANGE RECEIVABLES*	**-0.096**	**0.185**	**-0.032**	**0.039**		**0.317**	**0.290**	**-0.136**	-0.003	**0.221**
(17) *CHANGE INVENTORY*	**-0.098**	**0.182**	-0.009	**0.040**	**0.340**		**0.352**	**0.021**	**0.023**	**0.235**
(18) *CSALES GROWTH*	**-0.012**	**0.223**	0.000	**0.025**	**0.168**	**0.222**		**-0.012**	**0.015**	**0.264**
(19) *CAPITAL INTENSITY*	**-0.029**	**-0.043**	**0.070**	**-0.051**	**-0.135**	**-0.053**	0.006		**-0.260**	**0.026**
(20) *INTANGIBLES*	**0.230**	**0.026**	**-0.034**	**0.037**	**-0.027**	**-0.029**	**0.042**	**-0.256**		-0.008
(21) *CHANGE OPLEASE*	**-0.101**	**0.147**	**0.024**	**0.017**	**0.202**	**0.252**	**0.177**	**0.025**	**-0.027**	

TABLE 4

Testing CEO versus CFO Effects on Financial Misreporting: Mobility Method

This table provides regression results applying different fixed effects structures to the samples used to implement the mobility method. The dependent variable is *RESTATE*. Columns (1) to (4) of Panel A report results for the CEO mobility sample, where (1) is a pooled OLS regression only including the vector of time-varying firm-level controls as described in section 3 and year fixed effects, (2) is the firm fixed effects regression, (3) is a regression including both firm and CEO fixed effects using the mobility method, (4) is a regression that includes firm, CFO, and CEO fixed effects by using a combination of the spell method and the mobility method. Columns (5) to (8) of Panel A apply similar fixed effects structures to the CFO mobility sample. "YES" indicates that a set of variables is included in the regression. Coefficient estimates are not reported in this table, but all specifications include corrections for robust standard errors that are clustered at the firm-level. The F-statistics test the exclusion restriction that CEO/CFO fixed effects are jointly different from zero. Panel B, columns (1) to (4) decompose the model R^2 to separate out the relative importance of particular fixed effects in explaining variation in firms' misreporting, using the estimation results from Panel A, columns (3), (4), (7), and (8), respectively. Panel C summarizes the distributional properties of the estimated CEO/CFO fixed effects coefficients from the specifications in Panel A, columns (3), (4), (7), and (8), where the means are normalized to zero. Details on the definition of all variables are provided in the Appendix.

Panel A: Regression Results Using Mobility Samples

	Dependent Variable = ***RESTATE***							
	CEOs				**CFOs**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Time-Varying Firm-Level Controls	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects		YES	YES			YES	YES	
Firm and CFO Fixed Effects (Spell)				YES				
CEO Fixed Effects			YES	YES				
Firm and CEO Fixed Effects (Spell)								YES
CFO Fixed Effects							YES	YES
Testing CEO/CFO Fixed Effects = 0								
F-statistics			3.55	2.12			2.99	3.02
(p-value)			(<0.001)	(<0.001)			(<0.001)	(<0.001)
R^2	5.45%	50.74%	54.51%	68.89%	3.22%	44.60%	56.77%	63.60%
Adj. R^2	3.14%	31.57%	35.19%	45.13%	2.44%	28.81%	38.18%	41.05%
N	1,675	1,675	1,675	1,675	5,011	5,011	5,011	5,011

TABLE 4 *(continued)*

Panel B: Decomposition of Model R^2

	CEOs		CFOs	
	(1)	**(2)**	**(3)**	**(4)**
Firm Fixed Effects	41.49%		58.32%	
Firm and CFO Fixed Effects (Spell)		69.28%		
CEO Fixed Effects	54.70%	26.93%		
Firm and CEO Fixed Effects (Spell)				55.53%
CFO Fixed Effects			39.59%	42.94%

Panel C: Distribution of Estimated CEO/CFO Fixed Effects

	CEOs		CFOs	
	(1)	**(2)**	**(3)**	**(4)**
Mean Effect	0.0000	0.0000	0.0000	0.0000
Standard Deviation	0.3082	0.2611	0.6201	0.4076
25th Percentile	-0.1662	-0.1196	-0.0381	-0.1423
Median Effect	-0.0886	-0.0493	0.0447	-0.0952
75th Percentile	0.0428	0.0236	0.2576	0.0874
Interquartile Range	0.2090	0.1432	0.2957	0.2297

TABLE 5

Testing CEO versus CFO Effects on Financial Misreporting: Connectedness Method

This table provides regression results applying different fixed effects structures to the samples used to implement the connectedness method. The dependent variable is *RESTATE*. Columns (1) to (4) of Panel A report results for the CEO connectedness sample, where (1) is a pooled OLS regression only including the vector of time-varying firm-level controls as described in section 3 and year fixed effects, (2) is the firm fixed effects regression, (3) is a regression including both firm and CEO fixed effects using the connectedness method, (4) is a regression that includes firm, CFO, and CEO fixed effects by using a combination of the spell method and the connectedness method. Columns (5) to (8) of Panel A apply similar fixed effects structures to the CFO connectedness sample. "YES" indicates that a set of variables is included in the regression. Coefficient estimates are not reported in this table, but all specifications include corrections for robust standard errors that are clustered at the firm-level. The F-statistics test the exclusion restriction that CEO/CFO fixed effects are jointly different from zero. Panel B, columns (1) to (4) decompose the model R^2 to separate out the relative importance of particular fixed effects in explaining variation in firms' misreporting, using the estimation results from Panel A, columns (3), (4), (7), and (8), respectively. Panel C summarizes the distributional properties of the estimated CEO/CFO fixed effects coefficients from the specifications in Panel A, columns (3), (4), (7), and (8), where the means are normalized to zero. Details on the definition of all variables are provided in the Appendix.

Panel A: Regression Results Using Connectedness Samples

	Dependent Variable = ***RESTATE***							
	CEOs				**CFOs**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Time-Varying Firm-Level Controls	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects		YES	YES			YES	YES	
Firm and CFO Fixed Effects (Spell)				YES				
CEO Fixed Effects			YES	YES				
Firm and CEO Fixed Effects (Spell)								YES
CFO Fixed Effects							YES	YES
Testing CEO/CFO Fixed Effects = 0								
F-statistics			3.60	2.68			3.53	3.19
(p-value)			(<0.001)	(<0.001)			(<0.001)	(<0.001)
R^2	4.16%	28.31%	52.23%	65.94%	3.31%	26.87%	55.94%	63.94%
Adj. R^2	3.34%	20.41%	35.45%	43.54%	3.00%	19.73%	38.67%	43.35%
N	4,723	4,723	4,723	4,289	12,505	12,505	12,505	11,069

TABLE 5 *(continued)*

Panel B: Decomposition of Model R^2

	CEOs		CFOs	
	(1)	**(2)**	**(3)**	**(4)**
Firm Fixed Effects	19.65%		33.21%	
Firm and CFO Fixed Effects (Spell)		55.45%		
CEO Fixed Effects	76.47%	41.36%		
Firm and CEO Fixed Effects (Spell)				46.38%
CFO Fixed Effects			64.73%	52.28%

Panel C: Distribution of Estimated CEO/CFO Fixed Effects

	CEOs		CFOs	
	(1)	**(2)**	**(3)**	**(4)**
Mean Effect	0.0000	0.0000	0.0000	0.0000
Standard Deviation	0.3425	0.3656	0.6422	0.4559
25th Percentile	-0.1823	-0.1356	-0.0661	-0.1536
Median Effect	-0.0908	-0.0622	0.0447	-0.1080
75th Percentile	0.1284	0.0547	0.3166	0.1637
Interquartile Range	0.3106	0.1903	0.3827	0.3174

TABLE 6

Testing CEO versus CFO Effects on Financial Misreporting: Spell Method

This table provides regression results applying different fixed effects structures to the full CEO/CFO sample. The dependent variable is *RESTATE*. Column (1) is a pooled OLS regression only including the vector of time-varying firm-level controls as described in section 3 and year fixed effects, (2) is the firm fixed effects regression, (3) and (4) are regressions including both firm and CEO or CFO fixed effects using the spell method. "YES" indicates that a set of variables is included in the regression. Coefficient estimates are not reported in this table, but all specifications include corrections for robust standard errors that are clustered at the firm-level. The F-statistics test the exclusion restriction that firm and CEO/CFO fixed effects are jointly different from zero. Details on the definition of all variables are provided in the Appendix.

	Dependent Variable = *RESTATE*			
	(1)	**(2)**	**(3)**	**(4)**
Time-Varying Firm-Level Controls	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES
Firm Fixed Effects		YES		
Firm and CEO Fixed Effects (Spell)			YES	
Firm and CFO Fixed Effects (Spell)				YES
Testing Firm and CEO/CFO Fixed Effects = 0				
F-statistics			2.91	2.99
(p-value)			(<0.001)	(<0.001)
R^2	2.93%	27.52%	45.43%	51.06%
Adj. R^2	2.80%	19.10%	30.75%	34.86%
N	30,208	30,208	30,208	30,208

TABLE 7

Testing CEO versus CFO Effects on Fraudulent Misreporting

This table provides regression results applying different fixed effects structures to the samples used to implement the mobility method. The dependent variable in columns (1) to (4) of Panel A is *IRREGULARITY*. Columns (1) and (2) of Panel A report results for the CEO mobility sample, where (1) is a regression including both firm and CEO fixed effects using the mobility method and (2) is a regression that includes firm, CFO, and CEO fixed effects by using a combination of the spell method and the mobility method. Columns (3) and (4) of Panel A apply similar fixed effects structures to the CFO mobility sample. Columns (5) to (8) of Panel A report results when using *AAER* as dependent variable. "YES" indicates that a set of variables is included in the regression. Coefficient estimates are not reported in this table, but all specifications include corrections for robust standard errors that are clustered at the firm-level. The F-statistics test the exclusion restriction that CEO/CFO fixed effects are jointly different from zero. Panel B decomposes the model R^2 to separate out the relative importance of particular fixed effects in explaining variation in firms' misreporting, using the estimation results from Panel A, columns (1) to (8). Panel C summarizes the distributional properties of the estimated CEO/CFO fixed effects coefficients from the specifications in Panel A, columns (1) to (8), where the means are normalized to zero. Details on the definition of all variables are provided in the Appendix.

Panel A: Regression Results Using Mobility Samples

	Dependent Variable = *IRREGULARITY*				**Dependent Variable = *AAER***			
	CEOs		**CFOs**		**CEOs**		**CFOs**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Time-Varying Firm-Level Controls	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES		YES		YES		YES	
Firm and CFO Fixed Effects (Spell)		YES				YES		
CEO Fixed Effects	YES	YES			YES	YES		
Firm and CEO Fixed Effects (Spell)				YES				YES
CFO Fixed Effects			YES	YES			YES	YES
Testing CEO/CFO Fixed Effects = 0								
F-statistics	5.16	0.53	2.38	5.04	2.98	1.55	7.71	5.55
(p-value)	(<0.001)	(≈1.000)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
R^2	57.84%	67.86%	60.14%	68.04%	44.98%	58.93%	75.18%	78.48%
Adj. R^2	39.93%	43.31%	42.99%	48.23%	21.61%	27.56%	64.50%	65.15%
N	1,675	1,675	5,011	5,011	1,675	1,675	5,011	5,011

TABLE 7 *(continued)*

Panel B: Decomposition of Model R^2

	CEOs		CFOs		CEOs		CFOs	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Firm Fixed Effects	36.01%		70.72%		41.89%		68.07%	
Firm and CFO Fixed Effects (Spell)		/				78.76%		
CEO Fixed Effects	62.44%	/			52.38%	17.15%		
Firm and CEO Fixed Effects (Spell)				56.48%				59.59%
CFO Fixed Effects			28.54%	43.20%			32.02%	40.60%

Panel C: Distribution of Estimated CEO/CFO Fixed Effects

	CEOs		CFOs		CEOs		CFOs	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Mean Effect	0.0000	/	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Standard Deviation	0.1106	/	0.1835	0.2382	0.0605	0.0399	0.2399	0.1545
25th Percentile	-0.0269	/	-0.0321	-0.0703	-0.0161	-0.0181	0.0440	0.0011
Median Effect	-0.0168	/	0.0025	-0.0587	-0.0003	-0.0017	0.0552	0.0098
75th Percentile	-0.0021	/	0.0205	-0.0499	0.0129	0.0165	0.0643	0.0165
Interquartile Range	0.0248	/	0.0526	0.0204	0.0290	0.0347	0.0204	0.0155

TABLE 8

Testing CEO versus CFO Effects on Financial Misreporting Using Mediation Analyses

This table provides regression results applying different fixed effects structures to the samples used to implement the mobility method. The dependent variable is *RESTATE*. Columns (1) to (4) of Panel A include the following variables as additional controls: *CEO DELTA*, *CEO VEGA*, *CFO DELTA*, and *CFO VEGA*. Columns (1) and (2) of Panel A report results for the CEO mobility sample, where (1) is a regression including both firm and CEO fixed effects using the mobility method and (2) is a regression that includes firm, CFO, and CEO fixed effects by using a combination of the spell method and the mobility method. Columns (3) and (4) of Panel A apply similar fixed effects structures to the CFO mobility sample. Columns (5) to (8) of Panel A report results when the following variables are excluded from the vector of time-varying firm-level controls: *ACCRUALS*, *CHANGE RECEIVABLES*, *CHANGE INVENTORY*, *CSALES GROWTH*, *CAPITAL INTENSITY*, *INTANGIBLES*, and *CHANGE OPLEASE*. "YES" indicates that a set of variables is included in the regression. Coefficient estimates are not reported in this table, but all specifications include corrections for robust standard errors that are clustered at the firm-level. The F-statistics test the exclusion restriction that CEO/CFO fixed effects are jointly different from zero. Panel B decomposes the model R^2 to separate out the relative importance of particular fixed effects in explaining variation in firms' misreporting, using the estimation results from Panel A, columns (1) to (8). Panel C summarizes the distributional properties of the estimated CEO/CFO fixed effects coefficients from the specifications in Panel A, columns (1) to (8), where the means are normalized to zero. Details on the definition of all variables are provided in the Appendix.

Panel A: Regression Results Using Mobility Samples

	Dependent Variable = ***RESTATE***							
	Equity Incentives				**Unconditional**			
	CEOs		**CFOs**		**CEOs**		**CFOs**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Time-Varying Firm-Level Controls	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES		YES		YES		YES	
Firm and CFO Fixed Effects (Spell)		YES				YES		
CEO Fixed Effects	YES	YES			YES	YES		
Firm and CEO Fixed Effects (Spell)				YES				YES
CFO Fixed Effects			YES	YES			YES	YES
Testing CEO/CFO Fixed Effects = 0								
F-statistics	3.52	2.13	2.97	3.00	3.56	2.12	2.97	3.00
(p-value)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
R^2	54.58%	69.12%	56.92%	63.78%	53.94%	68.53%	56.59%	63.34%
Adj. R^2	35.06%	45.29%	38.31%	41.25%	34.76%	44.89%	38.03%	40.76%
N	1,675	1,675	5,011	5,011	1,675	1,675	5,011	5,011

TABLE 8 *(continued)*

Panel B: Decomposition of Model R^2

	CEOs		CFOs		CEOs		CFOs	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Firm Fixed Effects	41.47%		58.30%		42.18%		58.41%	
Firm and CFO Fixed Effects (Spell)		69.05%				70.78%		
CEO Fixed Effects	55.00%	28.09%			57.86%	29.07%		
Firm and CEO Fixed Effects (Spell)				54.90%				55.52%
CFO Fixed Effects			39.13%	43.07%			39.86%	43.28%

Panel C: Distribution of Estimated CEO/CFO Fixed Effects

	CEOs		CFOs		CEOs		CFOs	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Mean Effect	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Standard Deviation	0.3111	0.2656	0.6323	0.4085	0.3017	0.2700	0.6197	0.4057
25th Percentile	-0.1671	-0.1158	-0.0448	-0.1462	-0.1601	-0.1059	-0.0267	-0.1349
Median Effect	-0.0918	-0.0461	0.0561	-0.0862	-0.0950	-0.0693	0.0473	-0.0975
75th Percentile	0.0532	0.0288	0.2405	0.0858	0.0576	-0.0009	0.2784	0.0969
Interquartile Range	0.2203	0.1446	0.2853	0.2320	0.2176	0.1050	0.3051	0.2318

TABLE 9

Testing CEO versus CFO Effects on Financial Misreporting Using Exogenous Turnover

This table provides regression results applying different fixed effects structures to the samples used to implement the mobility method, where each sample is restricted to include all firms with largely exogenous turnover events. The dependent variable is *RESTATE*. Columns (1) to (4) of Panel A report results for the CEO mobility sample, where (1) is a pooled OLS regression only including the vector of time-varying firm-level controls as described in section 3 and year fixed effects, (2) is the firm fixed effects regression, (3) is a regression including both firm and CEO fixed effects using the mobility method, (4) is a regression that includes firm, CFO, and CEO fixed effects by using a combination of the spell method and the mobility method. Columns (5) to (8) of Panel A apply similar fixed effects structures to the CFO mobility sample. "YES" indicates that a set of variables is included in the regression. Coefficient estimates are not reported in this table, but all specifications include corrections for robust standard errors that are clustered at the firm-level. The F-statistics test the exclusion restriction that CEO/CFO fixed effects are jointly different from zero. Panel B, columns (1) to (4) decompose the model R^2 to separate out the relative importance of particular fixed effects in explaining variation in firms' misreporting, using the estimation results from Panel A, columns (3), (4), (7), and (8), respectively. Panel C summarizes the distributional properties of the estimated CEO/CFO fixed effects coefficients from the specifications in Panel A, columns (3), (4), (7), and (8), where the means are normalized to zero. Details on the definition of all variables are provided in the Appendix.

Panel A: Regression Results Using Mobility Samples

	Dependent Variable = ***RESTATE***							
	CEOs				**CFOs**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Time-Varying Firm-Level Controls	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects		YES	YES			YES	YES	
Firm and CFO Fixed Effects (Spell)				YES				
CEO Fixed Effects			YES	YES				
Firm and CEO Fixed Effects (Spell)								YES
CFO Fixed Effects							YES	YES
Testing CEO/CFO Fixed Effects = 0								
F-statistics			4.03	3.57			2.30	3.62
(p-value)			(<0.001)	(<0.001)			(<0.001)	(<0.001)
R^2	11.00%	55.55%	57.61%	68.84%	9.76%	50.50%	53.10%	58.53%
Adj. R^2	4.79%	34.96%	38.57%	46.64%	2.22%	28.04%	31.24%	31.24%
N	614	614	614	614	520	520	520	520

TABLE 9 *(continued)*

Panel B: Decomposition of Model R^2

	CEOs		**CFOs**	
	(1)	**(2)**	**(3)**	**(4)**
Firm Fixed Effects	23.53%		57.60%	
Firm and CFO Fixed Effects (Spell)		56.24%		
CEO Fixed Effects	68.36%	40.86%		
Firm and CEO Fixed Effects (Spell)				37.60%
CFO Fixed Effects			24.79%	47.42%

Panel C: Distribution of Estimated CEO/CFO Fixed Effects

	CEOs		**CFOs**	
	(1)	**(2)**	**(3)**	**(4)**
Mean Effect	0.0000	0.0000	0.0000	0.0000
Standard Deviation	0.2758	0.2660	0.3593	0.3967
25th Percentile	-0.1705	-0.0681	-0.1654	-0.2942
Median Effect	-0.1024	-0.0267	-0.0725	-0.0503
75th Percentile	0.0197	0.0320	0.1501	0.2485
Interquartile Range	0.1902	0.1001	0.3155	0.5426

TABLE 10

Testing CEO versus CFO Effects on Predicted Misreporting Risk

This table provides regression results applying different fixed effects structures to the samples used to implement the mobility method, where each sample is restricted to include all observations with non-missing F-Score values. The dependent variable in columns (1) to (4) of Panel A is *FSCORE>1*. Columns (1) and (2) of Panel A report results for the CEO mobility sample, where (1) is a regression including both firm and CEO fixed effects using the mobility method and (2) is a regression that includes firm, CFO, and CEO fixed effects by using a combination of the spell method and the mobility method. Columns (3) and (4) of Panel A apply similar fixed effects structures to the CFO mobility sample. Columns (5) to (8) of Panel A report results when using *FSCORE>Q3* as dependent variable. "YES" indicates that a set of variables is included in the regression. Coefficient estimates are not reported in this table, but all specifications include corrections for robust standard errors that are clustered at the firm-level. The F-statistics test the exclusion restriction that CEO/CFO fixed effects are jointly different from zero. Panel B decomposes the model R^2 to separate out the relative importance of particular fixed effects in explaining variation in firms' misreporting, using the estimation results from Panel A, columns (1) to (8). Panel C summarizes the distributional properties of the estimated CEO/CFO fixed effects coefficients from the specifications in Panel A, columns (1) to (8), where the means are normalized to zero. Details on the definition of all variables are provided in the Appendix.

Panel A: Regression Results Using Mobility Samples

	Dependent Variable = *FSCORE>1*				Dependent Variable = *FSCORE>Q3*			
	CEOs		CFOs		CEOs		CFOs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Time-Varying Firm-Level Controls	YES	YES	YES	YES	YES	YES	YES	YES
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES		YES		YES		YES	
Firm and CFO Fixed Effects (Spell)		YES				YES		
CEO Fixed Effects	YES	YES			YES	YES		
Firm and CEO Fixed Effects (Spell)				YES				YES
CFO Fixed Effects			YES	YES			YES	YES
Testing CEO/CFO Fixed Effects = 0								
F-statistics	6.17	4.73	4.44	4.68	5.21	4.06	3.51	3.83
(p-value)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(<0.001)
R^2	73.81%	79.72%	73.11%	77.16%	68.75%	76.53%	69.20%	72.72%
Adj. R^2	61.85%	62.85%	61.11%	62.55%	54.49%	57.01%	55.45%	55.28%
N	1,188	1,188	3,928	3,928	1,188	1,188	3,928	3,928

TABLE 10 *(continued)*

Panel B: Decomposition of Model R^2

	CEOs		**CFOs**		**CEOs**		**CFOs**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Firm Fixed Effects	25.81%		52.79%		39.63%		59.00%	
Firm and CFO Fixed Effects (Spell)		35.10%				45.41%		
CEO Fixed Effects	68.71%	56.58%			52.22%	44.81%		
Firm and CEO Fixed Effects (Spell)				51.34%				49.61%
CFO Fixed Effects			38.93%	40.71%			34.84%	44.37%

Panel C: Distribution of Estimated CEO/CFO Fixed Effects

	CEOs		**CFOs**		**CEOs**		**CFOs**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Mean Effect	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Standard Deviation	0.5047	0.4409	0.5596	0.5260	0.4458	0.3588	0.4692	0.4680
25th Percentile	-0.4192	-0.3872	-0.3688	-0.3909	-0.2731	-0.2255	-0.2377	-0.2930
Median Effect	-0.1936	-0.0861	-0.0642	-0.1062	-0.1223	-0.1173	-0.0649	-0.1314
75th Percentile	0.5135	0.4069	0.4665	0.4618	0.2571	0.1342	0.2466	0.2054
Interquartile Range	0.9327	0.7941	0.8354	0.8528	0.5302	0.3597	0.4843	0.4984

Appendix

Variable Definitions

Variable Name	Definition
Dependent Variables	
RESTATE	Indicator variable taking the value of one for firm-year observations with subsequent restatement, and zero otherwise
IRREGULARITY	Indicator variable taking the value of one for firm-year observations with subsequent restatement of more material and presumably intentional misstatements, and zero otherwise
AAER	Indicator variable taking the value of one for firm-year observations where the SEC identified a misstatement in an Accounting and Auditing Enforcement Release, and zero otherwise
FSCORE>1	Indicator variable taking the value of one for firm-year observations with an estimated F-Score larger than one, using model 1 from Dechow et al. (2011), and zero otherwise
FSCORE>Q3	Indicator variable taking the value of one for firm-year observations with an estimated upper quartile F-Score, using model 1 from Dechow et al. (2011), and zero otherwise
Firm-Level Controls	
SIZE	Natural logarithm of the market value of common equity plus one
MTB	Market value of common equity divided by the book value of common equity
LEVERAGE	Ratio of the book value of total debt to total assets
EXTERNAL FINANCE	One-year change in the book value of common equity and the book value of long-term debt scaled by lagged total assets
FCF	Difference between operating cash flow and average capital expenditures over the last three years scaled by lagged total assets
ACQUISITION	Indicator variable taking the value of one if a firm engages in any acquisitions in the given or the prior year, and zero otherwise
RESTRUCTURE	Indicator variable taking the value of one if a firm reports any restructuring activities in the given or the prior year, and zero otherwise
ROA	Ratio of income before extraordinary items to lagged total assets
CHANGE ROA	One-year change in return on assets
PRIOR LOSS	Indicator variable taking the value of one if a firm's income before extraordinary items is negative in either of the prior two years, and zero otherwise
PRIOR RETURN	One-year lagged percentage change in the market value of common equity
BIG N	Indicator variable taking the value of one if a firm is audited by one of the 4(5) biggest audit firms after (before) 2001, and zero otherwise
ACCRUALS	Difference between income before extraordinary items and operating cash flow scaled by operating cash flow
CHANGE RECEIVABLES	One-year change in accounts receivable scaled by lagged total assets

CHANGE INVENTORY	One-year change in inventory scaled by lagged total assets
CSALES GROWTH	One-year percentage change in cash sales calculated as the difference between total sales and the one-year change in accounts receivable
CAPITAL INTENSITY	Ratio of net property, plant, and equipment to total assets
INTANGIBLES	Research and development and advertising expense (set to zero if missing) scaled by total sales
CHANGE OPLEASE	One-year change in the present value of future non-cancelable operating lease obligations (set to zero if missing) scaled by lagged total assets
Equity Incentives	
CEO DELTA	Natural logarithm of the sensitivity of the CEO's equity portfolio to a one percent change in the firm's stock price plus one
CEO VEGA	Natural logarithm of the sensitivity of the CEO's equity portfolio to a 0.01 change in the standard deviation of the firm's stock returns plus one
CFO DELTA	Natural logarithm of the sensitivity of the CFO's equity portfolio to a one percent change in the firm's stock price plus one
CFO VEGA	Natural logarithm of the sensitivity of the CFO's equity portfolio to a 0.01 change in the standard deviation of the firm's stock returns plus one

Equity Incentives and Financial Reporting Outcomes: The Role of Research Design Choices

Denny Kutter
University of Potsdam
kutter@uni-potsdam.de

Abstract

This study reexamines the relation between equity incentives and financial reporting outcomes. There is little consistency in the empirical evidence in the literature. Thus, I first explore variation in research design choices and findings in prior studies. I document large variation in the empirical measures for firm size as standard control variable, equity incentives as key explanatory variables, and the reporting outcome of interest. Next, I assess the extent to which variation in these design choices affects empirical results. For a given set of design choices, I am able to replicate most results from prior studies. However, I find that choosing among reasonable alternatives for the measurement of firm size, equity incentives, and a given reporting outcome can have a direct bearing on the statistical significance of empirical results, with changes in t-statistics that often exceed typical thresholds for statistical significance. The findings hold for the magnitude of accruals, earnings management through discretionary accruals, and material misstatements, where accrual-based outcomes are more sensitive to design-specific factors. Overall, my study highlights how common design choices can have a large impact on whether equity incentives effects are considered significant or not.

Data Availability: All data are publicly available from sources indicated in the text.

Keywords: Earnings Management; Equity Incentives; Executive Compensation; Financial Misreporting; Fraud.

JEL Classification: C52; G34; J33; M41; M52.

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1. Introduction

Economists like incentives, financial economists like equity incentives. Of particular interest in the financial economics literature is the relation between equity incentives and financial reporting outcomes—e.g., earnings management and financial reporting fraud. So far, despite the considerable academic interest, no consistent pattern of results has emerged from the literature (e.g., Armstrong, Larcker, Ormazabal, and Taylor 2013; Davidson 2021; Mayberry, Park, and Xu 2021). In fact, over the last two decades, prior studies have found positive, negative, or no associations. While those findings have important implications for corporate governance, the sources of the lack of consistent evidence in the literature are not well understood. In this study, I reexamine the relation between equity incentives and financial reporting outcomes from a research design perspective.

Some prior studies conjecture that differences in research designs such as analyzing unmatched versus matched samples contribute to the mixed empirical results (e.g., Armstrong, Jagolinzer, and Larcker 2010). To consider this possibility, I explore variation in research design choices and findings in the literature. The literature review shows that, for studies using both unmatched and matched samples, regression and matched-pair designs yield similar results within each study. At the same time, equally designed studies, either based on unmatched-sample or matched-sample tests, still find conflicting results. Beyond that, I find little differences for other salient research design choices like, for example, controlling for unobserved heterogeneity across firms (e.g., Gormley and Matsa 2014).[63]

Recent work by Mitton (2022) highlights that more subtle research design choices, such as dependent variable selection, variable transformation, and outlier treatment, significantly affect inferences in corporate finance research, and that even minor changes in the measurement of routine control variables can have a direct bearing on the statistical significance of

[63] Appendix A presents details on research design choices and findings in prior literature.

empirical results. My survey of the literature shows that, across the different studies that examine the relation between equity incentives and financial reporting outcomes, there is little variation in the treatment of outliers, with the vast majority of studies using some form of winsorizing. However, similar to Mitton (2022), I document considerable variation in the measurement of firm size as most commonly used control variable (e.g., based on total assets or market value of equity) and the definition of equity incentives as key explanatory variables (e.g., using dollar values or log transformations), as well as variable selection for a given dependent construct. While this variation could be driven by theoretical considerations, the literature review suggests that, in general, prior studies use an ad hoc approach when choosing among reasonable alternatives, given that they provide no explanation for the design choices at hand.

Next, I assess the extent to which variation in these research design choices affects inferences about the relation between equity incentives and financial reporting outcomes. Therefore, I estimate a series of regressions and examine the absolute change in the t-statistics for the coefficients on equity incentives associated with changes in the empirical measures for firm size, equity incentives, and the reporting outcome of interest, while keeping all other design-specific factors constant. I start with the relation between equity incentives and aggressive accrual management, where, for example, Bergstresser and Philippon (2006) find a positive relation with CEO delta (i.e., the sensitivity of the CEO's equity portfolio to stock price), while Chava and Purnanandam (2010) find a negative relation with CFO vega (i.e., the sensitivity of the CFO's equity portfolio to stock price volatility) but no relation with CEO equity incentives.

My findings can be summarized as follows. First, for a given set of research design choices, I am able to replicate the results documented in prior studies. Second, these results can change significantly with common design choices. For example, using total accruals calculated from the cash flow statement as empirical measure for accrual management, I find that the t-statistics for the coefficients on equity incentives change by 0.32, on average, when I change the measure of firm size from total assets to market value of equity, with individual changes in the amount of up to 1.20 (e.g., for CEO equity incentives from delta).[64] On the other hand, changing the measurement of equity incentives from levels (in $000s) to log transformations results in an average change in the t-statistics for the equity incentives coefficients of 1.33, with individual changes amounting to 2.51 (e.g., for CFO equity incentives provided by vega).

Using total accruals calculated from the balance sheet as alternative measure for accrual management, I find that the t-statistics change by 1.36, on average, when changing the firm size measure, while changing the equity incentives measure is associated with an average change of 3.70, where individual changes reach up to 4.97 and 7.81, respectively. More importantly, compared to total accruals using the cash flow approach, I find that changing the dependent variable to total accruals using the balance sheet approach results in an average change in the t-statistics of 4.26, with individual changes of up to 11.25. Given typical thresholds of approximately 2.58, 1.96, and 1.64 for statistical significance at the 1%, 5%, and 10% levels, these findings suggest that common research design choices can have a great impact on inferences about the relation between equity incentives and accrual management.

The results are similar for earnings management through discretionary accruals. For brevity, I focus on discretionary accruals from the model in Dechow, Sloan, and Sweeney (1995) and accrual estimation errors in the spirit of Dechow and Dichev (2002), which are extensively used in the literature (e.g., Larcker, Richardson, and Tuna 2007; Jiang, Petroni, and Wang 2010; Armstrong et al. 2013). In view of recent work by Mayberry et al. (2021), I also examine performance-adjusted discretionary accruals (Kothari, Leone, and Wasley

[64] Appendix C provides details on the changes in t-statistics for equity incentives coefficients.

2005). Again, I am able to replicate the results from prior studies for a given set of design choices. However, using discretionary accruals based on Dechow et al. (1995), I find that changing the firm size measure changes the t-statistics for the equity incentives coefficients by 0.58, on average, with a maximum of 1.89. On the other hand, changing the equity incentives measure is associated with an average change of 1.54, where individual changes reach up to 3.08. By comparison, for accrual estimation errors, I find that the t-statistics change by only 0.33, on average, when changing the firm size measure, and changing the equity incentives measure results in a similar average change of 0.35, with individual changes amounting to 0.73 and 1.15, respectively. Compared to discretionary accruals from Dechow et al. (1995), changing the dependent variable is associated with an average change in the t-statistics of 1.72, with individual changes of up to 4.23. Finally, using performance-adjusted discretionary accruals, changing the firm size (equity incentive) measure results in an average change in the t-statistics of 0.94 (1.35), with a maximum of 3.82 (4.12). Compared to Dechow et al. (1995), changing the dependent variable changes the t-statistics by 2.56, on average, with individual changes of up to 8.63. Collectively, these findings suggest that common research design choices can have a large impact on inferences about the relation between equity incentives and earnings management.

The last set of tests examines the relation between equity incentives and material misstatements as the most extreme form of earnings management. Here, I fail to replicate the positive association with equity incentives provided by vega first documented in Armstrong et al. (2013). Beyond that, using restatements from Form 8-K Item 4.02 material event filings as dependent variable (e.g., Larcker et al. 2007; Efendi, Srivastava, and Swanson 2007), I find that changing the firm size measure changes the t-statistics for the equity incentives coefficients by only 0.18, on average, with individual changes not larger than 0.49. Changing the equity incentives measure results in a modest average change of 1.07, with individual changes of up to 2.00. By comparisons, using misstatements identified in an Accounting and Auditing Enforcement Release (AAER) by the Securities and Exchange Commission (SEC) as alternative dependent variable (e.g., Feng, Ge, Luo, and Shevlin 2011; Davidson 2021), changing the firm size (equity incentive) measure is associated with an average change in the t-statistics of only 0.16 (0.90), with a maximum of 0.70 (1.94). Similarly, compared to restatements, changing the dependent variables results in an average change in the t-statistics of 1.12, where individual changes reach up to 2.89. Thus, while individual results can change with the measure of equity incentives or dependent variable selection, these findings suggest that inferences about the relation between equity incentives and material misstatements determined by a third party are more robust to common research design choices.

My study makes two contributions to the literature on the relation between equity incentives and financial reporting outcomes. First, despite the lack of consistent evidence in the literature, I am able to replicate most results from prior studies, where the positive effect of risk-taking incentives provided by vega on material misstatements is the exception. This is in line with recent archival evidence presented in Mayberry et al. (2021) and the survey evidence in Graham, Harvey, and Rajgopal (2005) and Dichev, Graham, Harvey, and Rajgopal (2013), where executives respond that they manage earnings to increase stock prices and reduce stock price volatility, suggesting that delta is more important in providing reporting incentives.

Second, the findings in my study suggest that inferences about the relation between equity incentives and financial reporting outcomes can change significantly with the measurement of firm size, definition of equity incentives, and dependent variable selection, both in terms of statistical significance and economic magnitude of empirical results. The findings hold for CEO and CFO equity incentives from both delta and vega; with inferences about the relation between equity incentives and accrual-based outcomes being more sensitive to design-specific factors. Overall, my study demonstrates how subtle research design choices can have a large impact on whether equity incentives effects are considered significant or not. This can be of interest to future studies when assessing the robustness of findings to first-order design choices. Given the mixed empirical results from prior studies and their possible implications for corporate governance, this can also inform policymakers and regulators when making evidence-based decisions.

The remainder of my study is organized as follows. Section 2 provides the conceptual basis for equity incentives effects and reviews the related literature. Section 3 describes the research design. I present and discuss my findings in section 4. Finally, section 5 concludes.

2. Conceptual Underpinnings and Related Literature

Executive pay in publicly traded firms typically includes fixed and variable components, where each component involves a mix of cash and equity. While cash payments are, in principle, not sensitive to stock market valuations, equity compensation from stock and option grants ties executive wealth to the firm's stock price to align executives' interests with those of shareholders, with payoffs from stock (options) being linear (convex) in stock price movements (e.g., Guay 1999; Coles, Daniel, and Naveen 2006; Gormley, Matsa, and Milbourn 2013). Prior studies find that, in relation to cash payments, equity compensation is higher and increases throughout executives' tenure. As a result, beyond vesting status and holding restrictions, many executives build undiversified equity portfolios much larger than minimum requirements (e.g., Core and Guay 1999; Armstrong, Core, and Guay 2015; Guay, Kepler, and Tsui 2019). Given their payoff structure, these equity portfolios play a key role in providing executives with incentives.

While there are many sources of incentives, the literature largely focuses on the sensitivities of executives' stock and option portfolios to changes in stock price (delta) and volatility (vega). In terms of financial reporting, to the extent that reported earnings translate into stock market valuations, pay-performance sensitivity from delta provides incentives for income-increasing reporting behavior to inflate the firm's stock price, given that executives participate in shareholders' gains from higher stock market valuations. However, while this reward effect in higher delta encourages executives to increase equity value, increasing equity value artificially is accompanied by an increase in equity risk, e.g., the risk of strong negative stock returns upon detection (e.g., Dechow, Sloan, and Sweeney 1996; Palmrose, Richardson, and Scholz 2004; Karpoff, Lee, and Martin 2008), where, for undiversified executives, delta magnifies this risk effect due to higher exposure of their equity portfolios to the firm's idiosyncratic risk. Given that risk-averse executives are expected to trade off the countervailing incentive effects, prior research argues that the link between delta and financial reporting outcomes is ambiguous (e.g., Lambert, Larcker, and Verrecchia 1991; Chava and Purnanandam 2010; Armstrong et al. 2013). On the other hand, vega provides incentives to increase stock price volatility, where executives with higher vega will be less averse to equity risk that accompanies an artificial increase in equity value from reporting inflated earnings. Therefore, ceteris paribus, vega is expected to unambiguously encourage executives to engage in risky reporting behavior.

In sum, stock and option portfolios provide executives with incentives, where incentives from both delta and vega can theoretically relate to financial reporting outcomes, either because they tie executives' wealth to the firm's stock price or its volatility. That said, the survey evidence presented in Graham et al. (2005) and Dichev et al. (2013) suggest that delta is more important in providing reporting incentives, given that executives respond that they manage earnings primarily to keep stock prices high and rising, while, at the same time, they seek to maintain or reduce stock price volatility. Looking at prior archival studies, however, there is little consistency in the empirical evidence on the relation between equity incentives and financial reporting outcomes. For example, investigating the role of equity incentives in aggressive accrual management, Bergstresser and Philippon (2006) show that CEO delta is associated with higher total accruals. In contrast, Chava and Purnanandam (2010) and Jiang et al. (2010) fail to find such an association, while they document that total accruals increase (decrease) with CFO equity incentives from delta (vega).

In terms of earnings management through discretionary accruals, Cheng and Warfield (2005) and Bergstresser and Philippon (2006) find positive associations with CEO stock and option ownership and CEO delta, respectively. However, the findings of Jiang et al. (2010) again suggest that, compared CEO equity incentives, discretionary accruals are more sensitive to CFO delta, which comports with the findings of Kim, Li, and Zhang (2011), who document that CFO delta, rather than CEO delta, increases firms' future stock price crash risk. Motivated by prior analytical and empirical work, Armstrong et al. (2013) argue that incentive effects from delta are subsumed by those of vega. In line with this, they find a positive relation between executive vega and discretionary accruals, but no relation with delta, whereas other studies fail to find any relation between discretionary accruals and executives' equity incentives, neither in terms of stock ownership nor delta or vega (e.g., Larcker et al. 2007; Mayberry et al. 2021).

Focusing on more egregious misstatements, Larcker et al. (2007) fail to find a relation between executive stock ownership and restatements. However, analyzing specific components of CEO equity incentives, Burns and Kedia (2006) and Efendi et al. (2007) find that the likelihood of restatements increases both in option portfolio delta and its intrinsic value. The findings comport with those of Cheng and Farber (2008), documenting a decline in CEO option-based compensation following restatements. In contrast, the findings of Johnson, Ryan, and Tian (2009) suggest that CEO delta from unrestricted stockholdings, rather than options, increases the likelihood of AAERs. Feng et al. (2011) find a positive association between CEO delta, but not CFO delta, and AAERs, while others fail to find any effect of delta (e.g., Erickson, Hanlon, and Maydew 2006; Armstrong et al. 2010). Armstrong et al. (2013) show that executive vega is positively related to restatements and AAERs, while there is no relation with delta, which is in line with the findings of Chen, Gul, Veeraraghavan, and Zolotoy (2015) and Kim, Li, and Li (2015), who document that auditors incorporate risk-taking incentives provided by vega in their pricing decisions. Recently, Davidson (2021) finds that executive equity incentives from both delta and vega increase reporting risk in the form of AAERs. However, a concurrent study by Mayberry et al. (2021) suggests the opposite, where the authors fail to find any relation between executive equity incentives from delta or vega and restatements and AAERs, respectively.

Overall, no consistent pattern of results emerges from the literature. Prior studies recognize that differences in research designs, such as unmatched-sample versus matched-sample tests or analyzing certain forms of reporting outcomes, may drive the mixed empirical results (e.g., Armstrong et al. 2010; Armstrong et al. 2013; Davidson 2021). Thus, I survey research design choices and findings in studies that focus on the relation between equity incentives and financial reporting outcomes. The results are presented in Appendix A. I find that, for studies using both unmatched and matched samples, regression and matched-pair designs yield similar results within each study (e.g., Erickson et al. 2006; Armstrong et al. 2013). At the same time, for example in matched-sample analysis, similarly designed studies still find conflicting results (e.g., Erickson et al. 2006; Feng et al. 2011). In a similar vein, as detailed above, empirical results are not only inconsistent across different forms of reporting outcomes (e.g., Jiang et al. 2010; Davidson 2021), but also within each group (e.g., Jiang et al. 2010; Mayberry et al. 2021). Across the different studies, I further find little differences for other influential research design choices such as standard error adjustments (e.g., Petersen 2009; Conley, Goncalves, and Hansen 2018) or including firm fixed effects to control for unobserved heterogeneity across firms (e.g., Gormley and Matsa 2014; deHaan 2021).

In a recent study, Mitton (2022) highlights that more subtle research design choices, such as dependent variable selection, variable transformation, and outlier treatment, significantly affect inferences in corporate finance research. Similarly, even minor differences in, for example, the measurement of routine control variables such as firm size can have a direct bearing on the statistical significance of empirical results. As seen in Appendix A, there appears to be little variation in the treatment of outliers in the literature, given that most studies winsorize variables at the 1st/99th percentiles to mitigate the impact of extreme observations. However, similar to Mitton (2022), I document considerable variation in the measurement of firm size as most commonly used control variable, the definition of equity incentives as key explanatory variables, and variable selection for a given dependent construct—that is, within each group of reporting outcomes. That said, except for the typical phrase "consistent with prior studies", the literature provides no discussion of the research design choices at hand. Therefore, I examine the extent to which variation in these research design choices affects inferences about the relation between equity incentives and financial reporting outcomes.[65]

3. Research Design

3.1. Data and Sample

Table 1 outlines the sample selection for the period 1995-2016. Following prior studies that separately investigate the roles of CEOs and CFOs (e.g., Jiang et al. 2010; Feng et al. 2011), I begin my sample selection by collecting all firm-years with available CEO/CFO data from ExecuComp to construct an executive-firm matched panel dataset. I remove firm-years with missing data on equity incentives obtained from Coles et al. (2006), including the sensitivities of executives' stock and option portfolios to changes in stock price (delta) and volatility (ve-

[65] In line with this, Armstrong et al. (2013) conjecture that "the lack of standardized measures of equity incentives may potentially explain the conflicting results reported in the literature" (p. 332 in their study). Kurshev and Strebulaev (2015) note that "firm size has become such a routine control variable in empirical corporate finance studies that it receives little or no discussion in most research papers, even though it is not uncommon among the most significant variables" (p. 2 in their study).

ga). In line with the literature that uses accrual-based reporting outcomes (e.g., Cheng and Warfield 2005; Chava and Purnanandam 2010), due to systematic differences in balance sheet ratios, I exclude financial firms (SIC codes 6000-6999). For the remaining sample firms, I add annual financial data and firm fundamentals from Compustat, which are used as control variables in the empirical analysis. To be included in the sample, firms must have available data for each of these controls for a given firm-year. As seen in Table 1, my final sample comprises 23,608 firm-years of 2,632 firms, including 5,055 CEOs and 5,498 CFOs. In the last step, I add restatement data from Audit Analytics and SEC AAERs obtained from Dechow, Ge, Larson, and Sloan (2011).

[Table 1 about here]

3.2. Empirical Model

I examine the relation between equity incentives and financial reporting outcomes in an OLS regression framework. Therefore, I estimate a series of regressions of the following form (firm, year, and CEO/CFO subscripts suppressed):

Equation (1):

$$y = \beta_1 CEO\_Delta + \beta_2 CEO\_Vega + \beta_3 CFO\_Delta + \beta_4 CFO\_Vega + \beta_k X_k + \varphi + \mu + \varepsilon$$

where the dependent variable (y) substitutes for the different measures of reporting outcomes used in my study. Based on prior literature, as shown in Appendix A, financial reporting outcomes can be loosely assigned to three groups. I start with two measures capturing the magnitude of accruals, i.e., components of earnings that are not reflected in current cash flows, which have been used to proxy for income-increasing earnings management (e.g., Bergstresser and Philippon 2006; Jiang et al. 2010) or volatility-decreasing earnings smoothing (e.g., Chava and Purnanandam 2010). The first measure (*Accruals*) estimates total accruals using the cash flow approach, while the second measure (*RSST05_Accruals*) uses total accruals calculated from the balance sheet, following the procedure in Richardson, Sloan, Soliman, and Tuna (2005). Given the findings of Bergstresser and Philippon (2006) and Chava and Purnanandam (2010), I use both accrual measures in my empirical tests. However, I recognize that, as opposed to measuring accruals from the cash flow statement, measuring accruals as the change in consecutive balance sheet accounts can be subject to measurement error (e.g., Hribar and Collins 2002).

Accruals address timing and matching problems in realized cash flows as a measure of firm performance, including both nondiscretionary and discretionary components (e.g., Dechow 1994; Dechow and Skinner 2000). Thus, a broad literature attempts to model the accrual process to decompose accruals into components that are determined by the firm's underlying economics and discretionary accruals, which are extensively used as a proxy for earnings management (e.g., Cheng and Warfield 2005; Armstrong et al. 2013; Mayberry et al. 2021). For brevity, I focus on the two most common discretionary accrual measures. The first measure (*DSS95_DAC*) estimates discretionary accruals as the residual from the model in Dechow et al. (1995).[66] The second measure (*DD02_SD(DAC)*) is directed at estimation error in discretionary accruals, calculated as the standard deviation of the residual from the model in Dechow and Dichev (2002) as modified by McNichols (2002).[67] In view of recent work by Mayberry et al. (2021), I also report results using performance-adjusted discretionary accruals (*KLW05_DAC*) in the spirit of Kothari et al. (2005), extending the model by Dechow et al. (1995) to account for performance effects in the accrual process. Throughout my study, I use signed (discretionary) accrual measures as dependent variable, so that positive coefficients on equity incentives capture income-increasing reporting behavior (e.g., Hribar and Nichols 2007).

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[66] Using the model in Dechow et al. (1995), total accruals are regressed on a firm-specific constant, the change in revenues less the change in receivables, and property, plant, and equipment (all scaled by lagged total assets).
[67] Following McNichols (2002), working capital accruals are regressed on past, current, and future operating cash flows, as well as the change in revenues and property, plant, and equipment (all scaled by average total assets).

It is well understood that discretionary accrual models are prone to misspecification issues, leading to both Type I and II errors in identifying earnings management (e.g., Dechow, Ge, and Schrand 2010; Gerakos 2012; Chen, Hribar, and Melessa 2018). Therefore, the third group of reporting outcomes covers two measures of material misstatements as determined by a third party, which can be considered the most extreme form of earnings management and are used frequently to proxy for fraudulent misreporting (e.g., Burns and Kedia 2006; Armstrong et al. 2013; Davidson 2021). Following these studies, the first measure (*BigR*) is an indicator variable based on restatements from Form 8-K Item 4.02 material event filings, whereas the second measure (*AAER*) indicates misstatements identified in an AAER issued by the SEC.

To test equity incentives effects on the different reporting outcomes, following Core and Guay (2002), I include measures for pay-performance sensitivity from delta (*CEO_Delta*; *CFO_Delta*) and risk-taking incentives provided by vega (*CEO_Vega*; *CFO_Vega*) as key explanatory variables. As seen in Appendix A, there is considerable variation in the definition of equity incentives in the literature, where variables defined in dollar values (e.g., Erickson et al. 2006; Johnson et al. 2009; Davidson 2021) and log transformations (e.g., Burns and Kedia 2006; Armstrong et al. 2013; Mayberry et al. 2021) are among the most common measures.[68] Note that, while textbook econometrics suggests taking logs of large dollar amounts to account for skewed distributions (e.g., Greene 2018; Wooldridge 2019), measuring equity incentives in levels is consistent with the broader risk-taking literature (e.g., Coles et al. 2006; Low 2009; Gormley et al. 2013). Hence, throughout my study, I measure equity incentives either in levels ($000s) or logs (natural logarithm of one plus the dollar amount).

The vector of controls (X_k) includes a number of firm characteristics from prior studies. While the set of consistent variables that act as controls is small, firm size is the standard con-

[68] Other studies use the ratio of equity incentives to total compensation (e.g., Bergstresser and Philippon 2006; Jiang et al. 2010; Feng et al. 2011).

trol variable and used consistently in the literature. The documented firm size effect is statistically significant and economically large in most studies, which is consistent with a broader set of studies in the financial economics and corporate finance literature (e.g., Kurshev and Strebulaev 2015; Mitton 2021). However, similar to the definition of equity incentives, Appendix A shows that there is large variation in the measurement of firm size across the different studies, with variables using log-transformed values of total assets (e.g., Bergstresser and Philippon 2006; Chava and Purnanandam 2010; Jiang et al. 2010) and market value of equity (e.g., Cheng and Warfield 2005; Armstrong et al. 2013; Davidson 2021) being by far the most common. Following, I control for firm size measured either in total assets (*Size(Assets)*) or market value of equity (*Size(Market)*) throughout my study.

Other controls include a firm's market-to-book ratio (*MTB*), leverage (*Leverage*), net external financing from shareholders and debtholders (*Finance*), free cash flow (*FCF*), indicators for acquisitions (*Acquisition*) and restructuring activities (*Restructure*), profitability (*ROA*) and its one-year change (*ROA_Change*), and an indicator for the use of a Big N auditor (*BigN*). Different from prior studies, I do not control for the volatility of operating cash flows and sales given that I use signed (discretionary) accrual measures (e.g., Hribar and Nichols 2007). Following the suggestions of Chen et al. (2018), however, I append all the first-stage regressors from discretionary accrual models to equation (1) when estimating the relation between equity incentives and discretionary accruals.[69]

I further include year fixed effects (μ) to account for general time trends, capturing period-specific factors that might drive incentive effects from delta and vega (e.g., Brick, Palmon, and Wald 2012; Mayberry et al. 2021). Throughout my study, similar to Armstrong et al. (2013), I report results from estimating equation (1) both with and without firm fixed effects

[69] While Chen et al. (2018) propose a one-step approach as straightforward way to estimating discretionary accruals, the authors show that including all the first-stage regressors as additional controls in the second-stage estimation generates the same coefficients and t-statistics obtained from a one-step approach (pp. 782-783 in their study).

(φ). While excluding firm fixed effects may cause concerns about estimation bias from unobserved differences across firms, the intuition behind the estimation of pooled (firm fixed effects) regressions is to test equity incentives effects on cross-sectional (within-firm) variation in financial reporting outcomes.[70] Note that including firm fixed effects reduces the sample size as it requires at least two observations for each firm (e.g., Correia 2015; deHaan 2021). Consistent with the majority of studies in Appendix A, I winsorize continuous variables at the 1st/99th percentiles to reduce the possible influence of outliers. Standard errors are clustered by firm. In addition, except for the first-stage regressors for discretionary accruals, all right-hand side variables are measured one year prior to financial reporting outcomes. Details on the definition of all variables used in the analysis are provided in Appendix B.

3.3. Changes in t-Statistics

While replicating prior research can be useful in its own right (e.g., Harvey 2017; Hail, Lang, and Leuz 2020), the primary objective of my study is to assess how differences in research designs contribute to the mixed empirical results on the relation between equity incentives and financial reporting outcomes documented in the literature. Therefore, in the spirit of Mitton (2022), I examine the sensitivity of findings to common research design choices related to the measurement of firm size, definition of equity incentives, and dependent variable selection as shown in Appendix A, based on the absolute change in the t-statistics for the coefficients on equity incentives. I note that, in either case, the expected (or tolerable) change in the t-statistics likely exceeds zero. That said, values of 2.58, 1.96, and 1.64 as typical thresholds for statistical significance at the 1%, 5%, and 10% levels, respectively, can be used as upper benchmarks. By comparison, in simulations with randomly generated explanatory variables, Mitton (2022) shows that a t-statistic would be expected to change by 1.13, on average, when

[70] As shown in Appendix A, except for Armstrong et al. (2013), Davidson (2021), and Mayberry et al. (2021), most prior studies do not control for firm fixed effects.

replacing the original explanatory variable with a completely new randomly generated variable.

To better illustrate my empirical strategy, consider an example from estimating the relation between equity incentives and a given reporting outcome, say the magnitude of accruals. First, I estimate equation (1) using total accruals calculated from the cash flow statement as dependent variable, with equity incentives measured in levels and firm size measured in total assets. Second, all else equal, I estimate equation (1) with firm size measured in market value of equity and calculate the absolute change in the t-statistics for the coefficients on equity incentives associated with the change in the firm size measure. I repeat the first two steps with equity incentives measured in logs to calculate the absolute change in the t-statistics associated with a change in the equity incentives measure. Finally, I estimate the same series of regressions from steps 1 to 3 using total accruals calculated from the balance sheet as dependent variable and compute the absolute change in the t-statistics to examine the sensitivity of findings to dependent variable selection. To assess the overall impact of these research design choices on inferences about the relation between equity incentives and financial reporting outcomes, I perform the same sensitivity analysis within each group of reporting outcomes, and I calculate the average absolute changes in the t-statistics for the coefficients on equity incentives across the different reporting outcomes. I provide details on the changes in t-statistics in Appendix C.

3.4. Descriptive Statistics

Table 2 provides descriptive statistics for my sample, with Panel A reporting summary statistics for the distribution of financial reporting outcomes. For example, expressed as a percentage of total assets, the mean signed value of total accruals calculated from the cash flow statement (balance sheet) is -0.07 (0.02). Using unsigned accrual measures (untabulated), this translates into a mean of 0.08 (0.09). The averages are comparable to those reported in the literature, where, for example, the means for unsigned total accruals in Chava and Purnanandam (2010) are 0.12 and 0.09, respectively. The means for signed discretionary accruals range from -0.03 (*KLW05_DAC*) to 0.05 (*DSS95_DAC*), which translates into means of 0.07 and 0.10 for unsigned discretionary accruals (untabulated). Again, the averages comport with those reported in prior studies. For example, Mayberry et al. (2021) find a mean of 0.06 for the unsigned value of performance-adjusted discretionary accruals, while Armstrong et al. (2013) report mean unsigned discretionary accruals of 0.12 from the model by Dechow et al. (1995). Similarly, I find observed mean probabilities of 0.04 and 0.01 for restatements and AAERs in my sample, where, for example, Armstrong et al. (2013) report averages of 0.03 and 0.02, respectively.

Panel B presents Pearson correlations among financial reporting outcomes. Across the different reporting outcomes, I find that correlations are not particularly high, and often insignificant at conventional levels, with a maximum correlation of 0.58. More importantly, I also find relatively low correlations within each group of reporting outcomes. For example, the correlation between the two measures for the magnitude of accruals is 0.30, while Form 8-K Item 4.02 restatements and SEC AAERs exhibit a correlation of only 0.13. The correlations indicate that dependent variable selection might have a large impact on empirical results.

Panel C reports summary statistics for the distribution of equity incentives. For example, using equity incentives measured in levels ($000s), I find means of 481.74, 107.19, 81.72, and 31.11 for *CEO_Delta*, *CEO_Vega*, *CFO_Delta*, and *CFO_Vega*, respectively, while the means are 5.21, 3.56, 3.52, and 2.51 for equity incentives measured in logs. In sum, the averages are similar to those in prior studies. For example, using log-transformed measures of equity incentives, Mayberry et al. (2021) report means for portfolio sensitivities from delta and vega of 4.49 and 2.96, respectively. Similarly, focusing on CEO equity incentives, Armstrong et al. (2013) find a mean delta (vega) of 5.24 (3.46). On the other hand, using equity incentives measured in levels, Chava and Purnanandam (2010) report means for CEO delta and vega of 607.05 and 97.10, while the means for CFO equity incentives are 71.49 and 25.48, respectively. Finally, Panel D presents summary statistics for the distribution of firm characteristics, which are in line with the literature (e.g., Erickson et al. 2006; Armstrong et al. 2013; Mayberry et al. 2021).

[Table 2 about here]

4. Findings

4.1. Equity Incentives and Accruals

First, I examine the relation between equity incentives and accrual management. Table 3 provides regression results from estimating equation (1) using the signed value of total accruals calculated from the cash flow statement as dependent variable. Columns (1) to (4) report results when equity incentives are measured in levels ($000s), whereas columns (5) to (8) report results using log transformations. Following Armstrong et al. (2013), I report results from estimating both pooled (odd-numbered columns) and firm fixed effects regressions (even-numbered columns), where cross-sectional variation is cancelled out and the coefficients on equity incentives pick up effects from within-firm variation. Throughout my study, standardized coefficients are reported to better illustrate the relative economic magnitude of equity incentives effects (e.g., Chava and Purnanandam 2010; Mitton 2021).

Using total assets as measure of firm size, column (1) shows that, on average, accruals increase with CFO risk-taking incentives, where the coefficient on *CFO_Vega* is positive and statistically significant at the 10% level. On the other hand, the estimated coefficients on *CFO_Delta* and CEO equity incentives are negative and insignificant at conventional levels. Column (2) shows that the positive relation between *CFO_Vega* and accruals is robust to including firm fixed effects, suggesting that the incentive effects of vega explain not only variation in accrual management across firms, but also variation within firms. Consistent with Bergstresser and Philippon (2006), I also find a significantly positive (negative) effect of *CEO_Delta* (*CFO_Delta*) after controlling for firm fixed effects, which indicates that equity incentives provided by delta are more important in explaining within-firm variation in accrual management.

Columns (3) and (4) use the market value of equity to control for firm size. Allowing for cross-sectional variation, the results in column (3) closely resemble those in column (1). Column (4) again shows a significantly positive (negative) effect of *CFO_Vega* (*CFO_Delta*) after controlling for firm fixed effects. However, in contrast to column (2), I fail to find a significant relation between *CEO_Delta* and accruals. While the two firm size measures are highly correlated (coefficient 0.85 with p-value <0.001; untabulated), the t-statistic for *CEO_Delta* changes by 1.20 when changing the measure of firm size, which is substantial given critical values of approximately 2.58, 1.96, and 1.64 for statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively. By comparison, changing the firm size measure in columns (1) to (4) changes the t-statistics for the coefficients on equity incentives by 0.32 on average. Details on the changes in t-statistics are presented in Appendix C.

Using log-transformed measures of equity incentives, the results in columns (5) to (8) are quite different. For example, measuring firm size in total assets, the estimated coefficients on CEO/CFO equity incentives in column (5) are insignificant at conventional levels, whereas the significantly positive coefficient on *CEO_Delta* in column (6) suggests that accrual management within firms increases with CEO pay-performance sensitivity from delta. Similarly, measuring firm size in market value of equity, I find a positive and highly significant relation between *CEO_Delta* and accruals in column (8), which is at odds with the results in column (4). Note that the t-statistic for *CEO_Delta* again changes by 1.12 when changing the measure of firm size, where the average absolute change in the t-statistics for the coefficients on equity incentives in columns (5) to (8) is 0.31. More importantly, across the specifications in columns (1) to (8), changing the equity incentives measure changes the t-statistics by 1.33 on average, with a minimum (maximum) average change of 0.33 (2.02) for *CEO_Vega* (*CEO_Delta*).

[Table 3 about here]

Table 4 replicates the analysis using the signed value of total accruals calculated from the balance sheet as dependent variable. As seen in Panel B of Table 2, the correlation between the two accrual measures is 0.30, indicating that dependent variable selection might have a large impact on regression results. In line with this, the results in Table 4 differ widely from those reported in Table 3. For example, using equity incentives measured in levels, I find a significantly positive effect for CEO/CFO pay-performance sensitivity from delta in column (1), where accruals also seem to decrease with CEO risk-taking incentives provided by vega. However, column (2) shows that the negative (positive) relation between *CEO_Vega* (*CFO_Delta*) and accruals is not robust to including firm fixed effects, while the coefficient on *CEO_Delta* remains positive and highly significant, which again comports with the findings of Bergstresser and Philippon (2006). Columns (3) and (4) show a significantly positive (negative) relation between *CEO_Delta* (*CEO_Vega*) and accruals when measuring firm size in market value of equity. The same holds for log-transformed measures of equity incentives in columns (5) to (8). However, consistent with Chava and Purnanandam (2010) and Jiang et al. (2010), I also find a positive (negative) and highly significant effect of *CFO_Delta* (*CFO_Vega*).

Across the specifications in columns (1) to (8), I find that the t-statistics for the coefficients on equity incentives change by 1.36, on average, when changing the measure of firm size, ranging from 0.51 for *CFO_Delta* to 2.87 for *CEO_Vega*. On the other hand, changing the equity incentives measure changes the t-statistics by 3.70 on average, with a minimum (maximum) average change of 1.68 (7.38) for *CEO_Vega* (*CFO_Delta*). Compared to Table 3, when changing the dependent variable, the average absolute change in the t-statistics is 4.26, where *CEO_Vega* (*CFO_Delta*) again is least (most) affected with an average change of 2.82 (6.05). Taken together, the findings in this section suggest that common research design choices can have a large impact on inferences about the relation between equity incentives and accrual management, both in terms of statistical significance and economic magnitude of empirical results, where the sensitivity of findings to changes in firm size and equity incentives measures appears to be exacerbated by measurement error in accruals calculated from the balance sheet (e.g., Hribar and Collins 2002).

[Table 4 about here]

4.2. Equity Incentives and Discretionary Accruals

Next, to circumvent the concern that unobserved firm characteristics driving nondiscretionary accruals affect my results, I examine the relation between equity incentives and earnings management through discretionary accruals. Table 5 provides regression results from estimating equation (1) using the signed value of discretionary accruals estimated from the model in Dechow et al. (1995) as dependent variable. Using equity incentives measured in levels, consistent with Armstrong et al. (2013), column (1) shows that discretionary accruals increase with CEO/CFO risk-taking incentives provided by vega when measuring firm size in total assets, while the coefficient on *CFO_Delta* is negative and marginally significant. As seen in column (2), however, the positive relation between *CEO_Vega* and discretionary accruals is not robust to including firm fixed effects, where I find instead a significantly positive effect of *CEO_Delta*, which is in line with the findings of Cheng and Warfield (2005) and Bergstresser and Philippon (2006), among others. Columns (3) and (4) show a significantly positive (negative) effect of *CFO_Vega* (*CFO_Delta*) when measuring firm size in market value of equity, while I fail to find a significant relation between CEO equity incentives and discretionary accruals (e.g., Larcker et al. 2007; Mayberry et al. 2021).

On the other hand, using log-transformed measures of equity incentives, I consistently find a significantly positive coefficient on *CEO_Delta* in columns (5) to (8), where columns (5) and (7) further indicate a significantly positive effect of *CFO_Vega* on earnings management in the cross-section, and column (8) provides weak evidence for a negative effect of *CFO_Delta* on earnings management within firms. Across the different specifications in Table 5, changing the measure of firm size changes the t-statistics for the coefficients on equity incentives by 0.58 on average, with a modest minimum (maximum) average change of 0.17 (0.88) for *CFO_Vega* (*CEO_Delta*). However, when changing the equity incentives measure, the average absolute change in the t-statistics is 1.54, ranging from 0.39 for *CFO_Vega* to 2.08 for *CEO_Vega*.

[Table 5 about here]

Table 6 replicates the analysis using the standard deviation of discretionary accruals estimated from the model in Dechow and Dichev (2002) as modified by McNichols (2002) as dependent variable. In sum, the results are in sharp contrast to those reported in Table 5. For example, while I find a significantly positive effect of *CEO_Delta* in six specifications (e.g., Cheng and Warfield 2005; Bergstresser and Philippon 2006), the relation between *CEO_Vega* and estimation error in discretionary accruals is negative and marginally significant within firms in columns (4), (6), and (8). In addition, consistent with Larcker et al. 2007 and Mayberry et al. 2021, the coefficients on CFO equity incentives are constantly insignificant at conventional levels. Across the different specifications, I find that the t-statistics for the coefficients on equity incentives change by 0.33, on average, when changing the firm size measure, ranging from 0.26 for *CEO_Vega* to 0.42 for *CFO_Delta*. Similarly, changing the equity incentives measure changes the t-statistics by 0.35 on average, with a minimum (maximum) average change of 0.10 (0.52) for *CEO_Vega* (*CFO_Delta*). On the other hand, compared to Table 5, changing the dependent variable results in an average absolute change in the t-statistics of 1.72, where *CEO_Delta* (*CEO_Vega*) is least (most) affected with an average change of 1.37 (2.03).

[Table 6 about here]

Recent work by Mayberry et al. (2021) suggests that the positive effect of vega on earnings management documented in Armstrong et al. (2013) is sensitive to misspecification in the discretionary accrual models, where the authors, among other things, fail to find a significant relation between vega and performance-adjusted or performance-matched discretionary accruals. Therefore, Table 7 replicates the analysis using the signed value of performance-adjusted discretionary accruals estimated from the model in Kothari et al. (2005) as dependent variable. In general, across the different specifications, I find that earnings management decreases with CEO/CFO pay-performance sensitivity from delta. However, while I also find a significantly negative effect of *CEO_Vega* on earnings management in the cross-section of firms in columns (1) and (3) when equity incentives are measured in levels, the results regarding risk-taking incentives provided by vega are largely consistent with Mayberry et al. (2021).[71]

More importantly, I find that changing the measure of firm size changes the t-statistics for the coefficients on equity incentives by 0.94 on average, ranging from 0.29 for *CFO_Vega* to 1.54 for *CEO_Vega*. By comparison, changing the measure of equity incentives changes the t-statistics by 1.35 on average, with a minimum (maximum) average change of 0.91 (2.24) for *CFO_Delta* (*CEO_Vega*). Compared to Table 5, changing the dependent variable results

[71] Note that performance-adjusted discretionary accruals are estimated conditional on net income scaled by lagged total assets (Kothari et al. 2005). To avoid potential over-specification issues in the two-step approach, I repeat the analysis excluding (1) *FCF* and (2) *ROA* and *ROA_Change* as controls in the second-stage estimation (e.g., Leone, Minutti-Meza, and Wasley 2019; Chen, Hribar, and Melessa 2022). Untabulated results are very similar to those reported in Table 7, indicating that omitting these variables has a much smaller impact on the sensitivity of findings than, for example, changing the firm size measure.

in an average absolute change in the t-statistics of 2.56, where *CFO_Delta* (*CEO_Delta*) shows the smallest (largest) average change of 1.60 (3.99), indicating that the results in Mayberry et al. (2021) are, to some extent, also sensitive to common research design choices. Taken together, the findings in this section suggest that inferences about the relation between equity incentives and earnings management through discretionary accruals can change significantly with the measurement of firm size, definition of equity incentives, and dependent variable selection, where the influence of changes in firm size and equity incentives measures seems less pronounced for models that use the standard deviation of discretionary accruals as proposed by Dechow and Dichev (2002).

[Table 7 about here]

4.3. Equity Incentives and Material Misstatements

In the last set of tests, to address concerns about construct validity of accrual-based outcomes, I examine the relation between equity incentives and material misstatements as determined by a third party. Table 8 provides regression results from estimating equation (1) using restatements identified in Form 8-K Item 4.02 material event filings as dependent variable. Using equity incentives measured in levels, except for a significantly negative coefficient on *CEO_Vega*, I largely fail to find a significant relation between equity incentives and the likelihood of restatements across firms in column (1), controlling for firm size in total assets. Focusing on within-firm variation, column (2) shows that the negative relation between *CEO_Vega* and restatements is not robust to including firm fixed effects; however, I find a significantly positive effect of *CEO_Delta*, which is consistent with Burns and Kedia (2006) and Efendi et al. (2007), among others. Similarly, columns (3) and (4) show a positive and marginally significant effect of *CEO_Delta* when measuring firm size in market value of equity, where column (3) again provides weak evidence of a negative relation between *CEO_Vega* and restatements in the cross-section. Across the specifications in columns (1) to

(4), consistent with Feng et al. (2011), the coefficients on CFO equity incentives are insignificant at conventional levels.

Using log-transformed measures of equity incentives, I fail to find a significant relation between equity incentives and restatements in the cross-section of firms in columns (5) and (7), which comports with the findings of Armstrong et al. (2010) and Mayberry et al. (2021), among others. However, measuring firm size in total assets, column (6) shows that the likelihood of restatements within firms increases with CFO pay-performance sensitivity from delta, where I also find a positive (negative) and marginally significant coefficient on *CEO_Delta* (*CEO_Vega*). Note that, while the positive (negative) effect of *CFO_Delta* (*CEO_Vega*) is largely robust to measuring firm size in market value of equity, I fail to find a significant relation between *CEO_Delta* and restatements in column (8). Across the different specifications in Table 8, the t-statistics for the coefficients on equity incentives change by only 0.18, on average, when changing the firm size measure, ranging from 0.05 for *CFO_Vega* to 0.27 for *CEO_Delta*. By contrast, changing the equity incentives measure changes the t-statistics by 1.07 on average, with a minimum (maximum) average change of 0.55 (1.61) for *CEO_Delta* (*CFO_Vega*).

[Table 8 about here]

Finally, Table 9 replicates the analysis using AAERs from SEC investigations as dependent variable. Using equity incentives measured in levels, consistent with Erickson et al. (2006) and Armstrong et al. (2010), I generally fail to find a significant effect of equity incentives on the likelihood of receiving an AAER in columns (1) to (4). When equity incentives are measured in logs, using total assets as measure of firm size, column (6) shows a positive and highly significant effect for CEO pay-performance sensitivity from delta on within-firm variation in fraudulent misreporting, which is in line with the findings of Johnson et al. (2009) and Feng et al. (2011), among others. Similarly, measuring firm size in market value of equity, I find a positive and (marginally) significant effect of *CEO_Delta* on within-firm (cross-sectional) variation in fraudulent misreporting, where columns (7) and (8) also provide weak evidence for a positive effect of *CFO_Delta* and *CFO_Vega*, respectively. In sum, two insights are particularly notable: First, except for columns (7) and (8), the estimated coefficients on CFO equity incentives again are insignificant at conventional levels (e.g., Feng et al. 2011). Second, similar to Mayberry et al. (2021), I largely fail to replicate the positive effect of risk-taking incentives provided by vega documented in Armstrong et al. (2013) and Davidson (2021).

Across the specifications in columns (1) to (8), I find that changing the firm size measure changes the t-statistics for the coefficients on equity incentives by 0.16 on average, ranging from 0.05 for *CEO_Vega* and *CFO_Vega* to 0.34 for *CEO_Delta*. By comparison, changing the equity incentives measure changes the t-statistics by 0.90 on average, with a minimum (maximum) average change of 0.22 (1.42) for *CFO_Delta* (*CEO_Vega*). Compared to Table 8, while the correlation between the two misstatement measures is only 0.13 (Panel B of Table 2), changing the dependent variable results in a modest absolute change in the t-statistics of 1.12, on average, where *CEO_Delta* (*CFO_Vega*) shows the smallest (largest) average change of 0.60 (1.49). Taken together, while individual results might change with the measure of equity incentives or dependent variable selection, the findings in this section suggest that inferences about the relation between equity incentives and misstatements identified by an external party are more robust to common research design choices than those based on accrual-based measures.

[Table 9 about here]

5. Conclusion

This study reexamines the relation between equity incentives and financial reporting outcomes, which is characterized by a lack of consistent evidence in the literature. Prior studies recognize that differences in research designs like, for example, analyzing unmatched versus matched samples most likely contribute to the inconclusive findings (e.g., Armstrong et al. 2010; Armstrong et al. 2013; Davidson 2021). Therefore, I first explore variation in research design choices and findings in the literature. The literature review reveals that, for studies using both unmatched and matched samples, regression and matched-pair designs yield similar results within each study. On the other hand, similarly designed studies still produce inconsistent results. Across the different studies, I find little differences for influential research design choices such as fixed effects estimations or outlier treatment. However, I document large variation in the measurement of firm size as most commonly used control variable, the definition of equity incentives as key explanatory variables, and variable selection for the dependent construct.

Following Mitton (2022), I next examine the extent to which variation in these research design choices affects inferences about the relation between equity incentives and financial reporting outcomes, including the magnitude of accruals, earnings management through discretionary accruals, and material misstatements (e.g., Bergstresser and Philippon 2006; Armstrong et al. 2013; Davidson 2021). I am able to replicate most results from prior literature, where the positive association between risk-taking incentives provided by vega and material misstatements is the exception (e.g., Mayberry et al. 2021). More importantly, I find that reasonable alternatives for firm size and equity incentives measures result in an average change in the t-statistics for the coefficients on equity incentives of 0.55 and 1.46, with individual changes in the amount of up to 4.97 and 7.81, respectively. On the other hand, selecting an alternative dependent variable for the same conceptual outcome changes the t-statistics by 2.42 on average, ranging from 0.07 to 11.25. Given typical thresholds of 2.58, 1.96, and 1.64 for statistical significance at the 1%, 5%, and 10% levels, these findings suggest that common research design choices can have a large impact on whether equity incentives effects are considered significant or not. The findings hold for CEO and CFO equity incentives from both delta and vega; however, inferences about the relation between equity incentives and accrual-based outcomes are generally more sensitive to design-specific factors. Overall, my study highlights how subtle research design choices related to the measurement of firm size, definition of equity incentives, and dependent variable selection contribute to the mixed empirical results documented in prior studies, which can be problematic given their possible implications for corporate governance.

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Tables

TABLE 1
Sample Selection

This table presents the sample selection for the period 1995-2016. Details on the sample composition are provided in section 3.

	Firm-years	Firms	CEOs	CFOs
ExecuComp firms with available CEO/CFO data	34,521	3,471	6,788	7,376
Less: Missing equity incentives data	(6,742)	(330)	(916)	(934)
Less: Financial firms (SIC codes 6000-6999)	(3,876)	(491)	(784)	(900)
Less: Missing Compustat data	(295)	(18)	(33)	(44)
Final sample	**23,608**	**2,632**	**5,055**	**5,498**

TABLE 2
Descriptive Statistics

This table presents descriptive statistics for the sample used in my study. Panel A reports summary statistics for the distribution of financial reporting outcomes. Panel B provides Pearson correlations among the financial reporting outcomes, where bold values denote statistical significance at the 10% level or better (two-tailed). Panels C and D report summary statistics for the distribution of equity incentives and firm characteristics, respectively. Details on the definition of all variables are provided in Appendix B.

Panel A: Financial Reporting Outcomes

	N	Mean	SD	p1	p25	Median	p75	p99
Accruals	22,697	-0.0655	0.1067	-0.3917	-0.0950	-0.0546	-0.0245	0.1674
RSST05_Accruals	19,646	0.0235	0.1540	-0.4603	-0.0260	0.0232	0.0776	0.4838
DSS95_DAC	21,695	0.0499	0.1527	-0.3564	-0.0169	0.0379	0.1128	0.4967
DD02_SD(DAC)	20,013	0.0453	0.0364	0.0070	0.0230	0.0355	0.0555	0.1874
KLW05_DAC	21,695	-0.0275	0.0947	-0.2949	-0.0702	-0.0205	0.0190	0.2143
BigR	23,608	0.0371	0.1891	0	0	0	0	1
AAER	23,608	0.0116	0.1073	0	0	0	0	1

Panel B: Pearson Correlations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Accruals	**1**						
RSST05_Accruals	**0.3008**	**1**					
DSS95_DAC	**0.5834**	**0.2465**	**1**				
DD02_SD(DAC)	**-0.0257**	-0.0061	**-0.0140**	**1**			
KLW05_DAC	**0.5302**	**0.0615**	**0.4216**	0.0008	**1**		
BigR	-0.0094	**0.0161**	-0.0009	**0.0373**	**0.0209**	**1**	
AAER	-0.0009	**0.0140**	0.0061	0.0078	0.0049	**0.1331**	**1**

Panel C: Equity Incentives

	N	Mean	SD	p1	p25	Median	p75	p99
Levels ($000s)								
CEO_Delta	23,608	481.74	761.05	2.10	71.22	184.97	507.00	3,565.89
CEO_Vega	23,608	107.19	152.02	0.00	12.19	44.57	129.05	627.17
CFO_Delta	23,608	81.72	186.50	0.31	13.17	34.08	82.79	699.04
CFO_Vega	23,608	31.11	58.03	0.00	3.63	12.48	33.22	282.92
Logs								
CEO_Delta	23,608	5.21	1.50	1.13	4.28	5.23	6.23	8.18
CEO_Vega	23,608	3.56	1.80	0.00	2.58	3.82	4.87	6.44
CFO_Delta	23,608	3.52	1.34	0.27	2.65	3.56	4.43	6.55
CFO_Vega	23,608	2.51	1.45	0.00	1.53	2.60	3.53	5.65

Panel D: Firm Characteristics

	N	Mean	SD	p1	p25	Median	p75	p99
Size(Assets)	23,608	7.3258	1.5735	4.0700	6.1831	7.2297	8.3699	11.0302
Size(Market)	23,608	7.3430	1.5800	3.7193	6.2821	7.2468	8.3442	11.0672
MTB	23,608	3.0367	5.2349	-9.9458	1.4365	2.2105	3.6212	22.9293
Leverage	23,608	0.2331	0.2052	0.0000	0.0609	0.2192	0.3474	0.8095
Finance	23,608	0.0974	0.3346	-0.3657	-0.0173	0.0424	0.1276	1.3529
FCF	23,608	0.0438	0.1210	-0.3610	0.0003	0.0486	0.0990	0.3324
Acquisition	23,608	0.6454	0.4784	0	0	1	1	1
Restructure	23,608	0.3842	0.4864	0	0	0	1	1
ROA	23,608	0.0445	0.1671	-0.4126	0.0180	0.0528	0.0965	0.3399
ROA_Change	23,608	-0.0020	0.1851	-0.3987	-0.0258	0.0004	0.0226	0.3800
BigN	23,608	0.9228	0.2669	0	1	1	1	1

TABLE 3
Equity Incentives and Total Accruals (Cash Flow Approach)

This table provides regression results from estimating the relation between equity incentives and total accruals using the cash flow approach. Columns (1) to (4) report results when equity incentives are measured in levels ($000s) and columns (5) to (8) report results when equity incentives are measured in logs. Columns (1), (2), (5), and (6) control for firm size in total assets, whereas columns (3), (4), (7), and (8) use the market value of equity. Each column reports standardized coefficients associated with a one-standard-deviation increase in the explanatory variable as a percentage of the standard deviation of the dependent variable. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively. Standard errors are clustered at the firm-level and t-statistics are reported in parentheses. Details on the definition of all variables are provided in Appendix B. Changes in t-statistics for the coefficients on equity incentives are presented in Appendix C.

	Dependent Variable = *Accruals*							
	Levels Equity Incentives				**Logs Equity Incentives**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
CEO_Delta	-0.0058	0.0305***	-0.0084	0.0168	0.0175	0.0661***	0.0151	0.0522***
	(-0.55)	(2.58)	(-0.79)	(1.38)	(1.44)	(4.57)	(1.22)	(3.45)
CEO_Vega	-0.0149	-0.0048	-0.0114	-0.0178	-0.0104	-0.0004	-0.0079	-0.0043
	(-0.95)	(-0.23)	(-0.76)	(-0.88)	(-0.70)	(-0.02)	(-0.53)	(-0.25)
CFO_Delta	-0.0437	-0.0708**	-0.0441	-0.0740**	-0.0259	0.0009	-0.0270*	-0.0076
	(-1.57)	(-2.14)	(-1.58)	(-2.20)	(-1.62)	(0.05)	(-1.70)	(-0.45)
CFO_Vega	0.0295*	0.0461**	0.0299*	0.0429**	0.0069	-0.0059	0.0073	-0.0084
	(1.80)	(2.18)	(1.83)	(2.01)	(0.40)	(-0.33)	(0.42)	(-0.47)
Size(Assets)	0.0476***	-0.1112**			0.0435***	-0.1307***		
	(3.61)	(-2.44)			(3.50)	(-2.74)		
Size(Market)			0.0377***	0.0787**			0.0337**	0.0302
			(2.84)	(2.54)			(2.55)	(0.92)
MTB	-0.0158	-0.0003	-0.0210*	-0.0010	-0.0201*	-0.0079	-0.0245**	-0.0058
	(-1.38)	(-0.02)	(-1.82)	(-0.07)	(-1.77)	(-0.59)	(-2.13)	(-0.43)
Leverage	0.0073	0.0357	0.0170	0.0410*	0.0093	0.0423*	0.0184	0.0401*
	(0.57)	(1.51)	(1.40)	(1.74)	(0.72)	(1.80)	(1.50)	(1.71)
Finance	-0.0334*	-0.0163	-0.0364**	-0.0262	-0.0378**	-0.0264	-0.0402**	-0.0321
	(-1.88)	(-0.86)	(-2.06)	(-1.39)	(-1.96)	(-1.29)	(-2.10)	(-1.56)
FCF	0.0243	0.0897**	0.0220	0.0850**	0.0220	0.0857**	0.0200	0.0823**
	(0.97)	(2.26)	(0.87)	(2.12)	(0.87)	(2.15)	(0.79)	(2.04)
Acquisition	0.0050	-0.0031	0.0057	-0.0108	0.0062	-0.0034	0.0071	-0.0102
	(0.62)	(-0.33)	(0.71)	(-1.10)	(0.75)	(-0.35)	(0.86)	(-1.03)
Restructure	0.0104	0.0079	0.0124	0.0139	0.0126	0.0142	0.0145	0.0163*
	(1.14)	(0.87)	(1.35)	(1.55)	(1.41)	(1.59)	(1.61)	(1.86)
ROA	0.1518***	0.0641*	0.1512***	0.0589*	0.1522***	0.0603*	0.1520***	0.0602*
	(6.25)	(1.91)	(6.26)	(1.80)	(6.16)	(1.82)	(6.16)	(1.81)
ROA_Change	-0.0475	-0.0279	-0.0478	-0.0246	-0.0469	-0.0267	-0.0473	-0.0246
	(-1.58)	(-1.08)	(-1.60)	(-0.96)	(-1.56)	(-1.03)	(-1.58)	(-0.96)
BigN	-0.0048	0.0035	-0.0027	-0.0042	-0.0038	0.0019	-0.0015	-0.0037
	(-0.47)	(0.15)	(-0.26)	(-0.18)	(-0.37)	(0.08)	(-0.15)	(-0.16)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.0453	0.2632	0.0447	0.2631	0.0442	0.2625	0.0436	0.2616
Adj. R^2	0.0438	0.1768	0.0432	0.1767	0.0427	0.1760	0.0421	0.1750
N	22,697	22,479	22,697	22,479	22,697	22,479	22,697	22,479

TABLE 4

Equity Incentives and Total Accruals (Balance Sheet Approach)

This table provides regression results from estimating the relation between equity incentives and total accruals using the balance sheet approach. Columns (1) to (4) report results when equity incentives are measured in levels ($000s) and columns (5) to (8) report results when equity incentives are measured in logs. Columns (1), (2), (5), and (6) control for firm size in total assets, whereas columns (3), (4), (7), and (8) use the market value of equity. Each column reports standardized coefficients associated with a one-standard-deviation increase in the explanatory variable as a percentage of the standard deviation of the dependent variable. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively. Standard errors are clustered at the firm-level and t-statistics are reported in parentheses. Details on the definition of all variables are provided in Appendix B. Changes in t-statistics for the coefficients on equity incentives are presented in Appendix C.

	Dependent Variable = *RSST05_Accruals*							
	Levels Equity Incentives				**Logs Equity Incentives**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
CEO_Delta	0.0598***	0.0887***	0.0513***	0.0612***	0.0837***	0.1380***	0.0788***	0.1127***
	(6.08)	(5.82)	(5.25)	(4.10)	(7.79)	(7.82)	(7.36)	(6.10)
CEO_Vega	-0.0257**	-0.0103	-0.0749***	-0.0648***	-0.0303**	-0.0343**	-0.0448***	-0.0584***
	(-2.50)	(-0.76)	(-7.47)	(-4.81)	(-2.42)	(-2.01)	(-3.57)	(-3.33)
CFO_Delta	0.0206*	0.0072	0.0138	0.0046	0.1295***	0.1393***	0.1140***	0.1403***
	(1.82)	(0.47)	(1.23)	(0.33)	(9.63)	(7.58)	(8.46)	(7.70)
CFO_Vega	-0.0002	-0.0036	-0.0087	-0.0212	-0.0696***	-0.0805***	-0.0699***	-0.1106***
	(-0.01)	(-0.28)	(-0.82)	(-1.61)	(-4.40)	(-3.84)	(-4.38)	(-5.19)
Size(Assets)	-0.1061***	-0.7655***			-0.1469***	-0.8138***		
	(-8.23)	(-17.63)			(-11.10)	(-18.31)		
Size(Market)			0.0194*	0.0498			-0.0622***	-0.0886***
			(1.90)	(1.58)			(-6.17)	(-2.64)
MTB	0.0540***	0.0497***	0.0583***	0.0634***	0.0398***	0.0387***	0.0524***	0.0596***
	(4.58)	(3.57)	(4.89)	(4.42)	(3.51)	(2.87)	(4.46)	(4.24)
Leverage	0.0659***	0.2464***	0.0365**	0.2202***	0.0744***	0.2660***	0.0391**	0.2216***
	(3.54)	(8.76)	(2.30)	(7.12)	(3.85)	(9.65)	(2.42)	(7.22)
Finance	0.0848***	0.0481**	0.0852***	0.0150	0.0653***	0.0301	0.0736***	0.0070
	(4.82)	(2.52)	(4.79)	(0.74)	(3.68)	(1.57)	(4.11)	(0.35)
FCF	0.0373**	0.1079***	0.0375**	0.0895***	0.0335**	0.1060***	0.0368**	0.0917***
	(2.18)	(5.11)	(2.19)	(4.23)	(2.01)	(5.05)	(2.18)	(4.35)
Acquisition	0.0079	-0.0011	0.0017	-0.0413***	0.0025	0.0008	-0.0014	-0.0365***
	(1.10)	(-0.11)	(0.23)	(-4.19)	(0.35)	(0.08)	(-0.19)	(-3.69)
Restructure	-0.0481***	-0.0396***	-0.0529***	-0.0263**	-0.0354***	-0.0220**	-0.0443***	-0.0173
	(-6.11)	(-3.75)	(-6.85)	(-2.47)	(-4.58)	(-2.12)	(-5.75)	(-1.63)
ROA	0.1895***	0.1674***	0.1775***	0.1628***	0.1748***	0.1491***	0.1708***	0.1569***
	(6.62)	(4.60)	(6.29)	(4.56)	(6.44)	(4.39)	(6.18)	(4.45)
ROA_Change	-0.0086	-0.0410**	-0.0039	-0.0268	-0.0069	-0.0384*	-0.0034	-0.0278
	(-0.42)	(-2.06)	(-0.19)	(-1.32)	(-0.34)	(-1.91)	(-0.17)	(-1.34)
BigN	-0.0077	-0.0299*	-0.0255***	-0.0633***	-0.0092	-0.0360**	-0.0223***	-0.0636***
	(-0.94)	(-1.67)	(-3.07)	(-3.33)	(-1.12)	(-2.04)	(-2.67)	(-3.37)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.0951	0.2473	0.0888	0.2160	0.1067	0.2569	0.0953	0.2223
Adj. R^2	0.0935	0.1500	0.0872	0.1146	0.1052	0.1608	0.0937	0.1218
N	19,646	19,418	19,646	19,418	19,646	19,418	19,646	19,418

TABLE 5
Equity Incentives and Discretionary Accruals (Dechow, Sloan, and Sweeney 1995)

This table provides regression results from estimating the relation between equity incentives and discretionary accruals using the model in Dechow, Sloan, and Sweeney (1995). Columns (1) to (4) report results when equity incentives are measured in levels ($000s) and columns (5) to (8) report results when equity incentives are measured in logs. Columns (1), (2), (5), and (6) control for firm size in total assets, whereas columns (3), (4), (7), and (8) use the market value of equity. Each column reports standardized coefficients associated with a one-standard-deviation increase in the explanatory variable as a percentage of the standard deviation of the dependent variable. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively. Standard errors are clustered at the firm-level and t-statistics are reported in parentheses. Details on the definition of all variables are provided in Appendix B. Changes in t-statistics for the coefficients on equity incentives are presented in Appendix C.

	Dependent Variable = *DSS95_DAC*							
	Levels Equity Incentives				**Logs Equity Incentives**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
CEO_Delta	0.0042	0.0325**	-0.0025	0.0204	0.0373***	0.0629***	0.0279**	0.0468***
	(0.33)	(2.48)	(-0.19)	(1.53)	(2.87)	(4.36)	(2.10)	(3.07)
CEO_Vega	0.0521***	0.0211	0.0206	0.0122	0.0014	-0.0138	-0.0075	-0.0152
	(3.16)	(1.17)	(1.27)	(0.68)	(0.08)	(-0.78)	(-0.46)	(-0.86)
CFO_Delta	-0.0367*	-0.0542**	-0.0414**	-0.0573**	0.0154	-0.0160	0.0008	-0.0286*
	(-1.86)	(-2.09)	(-2.08)	(-2.18)	(0.95)	(-0.98)	(0.05)	(-1.77)
CFO_Vega	0.0446***	0.0390**	0.0380**	0.0371**	0.0511***	0.0266	0.0502***	0.0286
	(2.74)	(2.18)	(2.33)	(2.08)	(2.78)	(1.51)	(2.73)	(1.63)
Size(Assets)	-0.0572***	-0.0616			-0.0591***	-0.0678		
	(-3.09)	(-1.31)			(-3.21)	(-1.40)		
Size(Market)			0.0322*	0.0771**			0.0082	0.0578
			(1.75)	(2.34)			(0.42)	(1.62)
MTB	0.0096	-0.0024	0.0073	-0.0048	0.0026	-0.0069	0.0030	-0.0077
	(0.96)	(-0.23)	(0.73)	(-0.46)	(0.26)	(-0.66)	(0.30)	(-0.73)
Leverage	0.0055	0.0301	-0.0030	0.0368*	0.0097	0.0339	-0.0002	0.0366*
	(0.41)	(1.37)	(-0.25)	(1.73)	(0.72)	(1.56)	(-0.02)	(1.73)
Finance	0.0068	-0.0171	0.0066	-0.0241*	-0.0032	-0.0237	-0.0003	-0.0280*
	(0.45)	(-1.25)	(0.44)	(-1.73)	(-0.20)	(-1.58)	(-0.02)	(-1.85)
FCF	-0.0260	0.0802***	-0.0257	0.0790***	-0.0279	0.0774***	-0.0273	0.0767***
	(-1.35)	(3.02)	(-1.30)	(2.91)	(-1.44)	(2.88)	(-1.37)	(2.80)
Acquisition	-0.0005	-0.0236**	-0.0052	-0.0276***	-0.0035	-0.0239**	-0.0061	-0.0276***
	(-0.05)	(-2.50)	(-0.53)	(-2.95)	(-0.36)	(-2.55)	(-0.63)	(-2.95)
Restructure	0.0306***	0.0206**	0.0273**	0.0258**	0.0366***	0.0246**	0.0314***	0.0272***
	(2.88)	(1.99)	(2.54)	(2.51)	(3.45)	(2.39)	(2.93)	(2.65)
ROA	0.0993***	0.0364*	0.0996***	0.0319	0.0953***	0.0354*	0.0979***	0.0339
	(5.68)	(1.74)	(5.70)	(1.53)	(5.50)	(1.67)	(5.56)	(1.58)
ROA_Change	-0.0148	-0.0211	-0.0149	-0.0190	-0.0139	-0.0206	-0.0140	-0.0190
	(-0.67)	(-1.25)	(-0.67)	(-1.14)	(-0.63)	(-1.23)	(-0.63)	(-1.14)
BigN	-0.0266**	-0.0216	-0.0330***	-0.0262	-0.0299**	-0.0231	-0.0341***	-0.0264
	(-2.20)	(-1.31)	(-2.74)	(-1.61)	(-2.47)	(-1.41)	(-2.82)	(-1.62)
First-Stage	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.0418	0.3171	0.0409	0.3175	0.0429	0.3165	0.0413	0.3165
Adj. R^2	0.0401	0.2352	0.0393	0.2356	0.0412	0.2345	0.0397	0.2345
N	21,695	21,475	21,695	21,475	21,695	21,475	21,695	21,475

TABLE 6
Equity Incentives and Discretionary Accruals (Dechow and Dichev 2002)

This table provides regression results from estimating the relation between equity incentives and the standard deviation of discretionary accruals using the model in Dechow and Dichev (2002) as modified by McNichols (2002). Columns (1) to (4) report results when equity incentives are measured in levels ($000s) and columns (5) to (8) report results when equity incentives are measured in logs. Columns (1), (2), (5), and (6) control for firm size in total assets, whereas columns (3), (4), (7), and (8) use the market value of equity. Each column reports standardized coefficients associated with a one-standard-deviation increase in the explanatory variable as a percentage of the standard deviation of the dependent variable. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively. Standard errors are clustered at the firm-level and t-statistics are reported in parentheses. Details on the definition of all variables are provided in Appendix B. Changes in t-statistics for the coefficients on equity incentives are presented in Appendix C.

	Dependent Variable = *DD02_SD(DAC)*							
	Levels Equity Incentives				**Logs Equity Incentives**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
CEO_Delta	0.0466*	0.0836**	0.0603**	0.0868**	0.0293	0.0615	0.0467*	0.0656*
	(1.75)	(2.08)	(2.30)	(2.12)	(1.13)	(1.63)	(1.86)	(1.70)
CEO_Vega	-0.0262	-0.0421	-0.0325	-0.0551*	-0.0415	-0.0794*	-0.0483	-0.0857*
	(-1.07)	(-1.49)	(-1.33)	(-1.95)	(-1.12)	(-1.77)	(-1.30)	(-1.91)
CFO_Delta	0.0070	-0.0067	0.0102	-0.0023	-0.0213	-0.0193	-0.0035	-0.0045
	(0.38)	(-0.47)	(0.55)	(-0.17)	(-0.77)	(-0.74)	(-0.12)	(-0.17)
CFO_Vega	0.0001	0.0092	-0.0018	0.0033	0.0068	0.0313	0.0013	0.0147
	(0.01)	(0.53)	(-0.11)	(0.20)	(0.21)	(1.01)	(0.04)	(0.48)
Size(Assets)	-0.2260***	-0.3682***			-0.2090***	-0.3571***		
	(-11.35)	(-6.69)			(-10.99)	(-6.45)		
Size(Market)			-0.2019***	-0.1331***			-0.1934***	-0.1319***
			(-10.16)	(-3.64)			(-9.56)	(-3.33)
MTB	0.0433***	0.0197*	0.0661***	0.0305***	0.0484***	0.0222**	0.0682***	0.0322***
	(2.84)	(1.86)	(4.15)	(2.73)	(3.07)	(2.00)	(4.19)	(2.79)
Leverage	0.0158	0.0216	-0.0183	-0.0031	0.0130	0.0226	-0.0192	-0.0005
	(0.97)	(0.93)	(-1.09)	(-0.11)	(0.80)	(0.95)	(-1.14)	(-0.02)
Finance	0.0466***	0.0439***	0.0574***	0.0388***	0.0498***	0.0441***	0.0578***	0.0382***
	(4.12)	(4.99)	(5.05)	(4.38)	(4.47)	(4.95)	(5.13)	(4.26)
FCF	-0.0498**	-0.0037	-0.0622***	-0.0054	-0.0510**	-0.0023	-0.0600***	-0.0034
	(-2.50)	(-0.20)	(-3.11)	(-0.28)	(-2.57)	(-0.12)	(-3.01)	(-0.18)
Acquisition	-0.0594***	-0.0199*	-0.0684***	-0.0330***	-0.0593***	-0.0192*	-0.0689***	-0.0317***
	(-5.01)	(-1.87)	(-5.72)	(-3.08)	(-5.01)	(-1.81)	(-5.77)	(-2.98)
Restructure	-0.0144	-0.0192	-0.0279**	-0.0201	-0.0157	-0.0184	-0.0270**	-0.0183
	(-1.21)	(-1.51)	(-2.29)	(-1.58)	(-1.32)	(-1.42)	(-2.22)	(-1.42)
ROA	-0.0193	-0.0199	-0.0153	-0.0137	-0.0186	-0.0185	-0.0167	-0.0134
	(-0.95)	(-1.36)	(-0.74)	(-0.92)	(-0.90)	(-1.23)	(-0.78)	(-0.87)
ROA_Change	-0.0108	-0.0023	-0.0087	0.0001	-0.0104	-0.0029	-0.0083	-0.0006
	(-0.81)	(-0.23)	(-0.64)	(0.01)	(-0.79)	(-0.29)	(-0.61)	(-0.06)
BigN	-0.0196	-0.0225	-0.0250*	-0.0308	-0.0185	-0.0234	-0.0245*	-0.0316*
	(-1.36)	(-1.20)	(-1.69)	(-1.62)	(-1.27)	(-1.25)	(-1.65)	(-1.67)
First-Stage	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.1586	0.5753	0.1523	0.5699	0.1581	0.5746	0.1513	0.5695
Adj. R^2	0.1569	0.5242	0.1506	0.5182	0.1564	0.5235	0.1496	0.5177
N	20,013	19,806	20,013	19,806	20,013	19,806	20,013	19,806

TABLE 7
Equity Incentives and Discretionary Accruals (Kothari, Leone, and Wasley 2005)

This table provides regression results from estimating the relation between equity incentives and performance-adjusted discretionary accruals using the model in Kothari, Leone, and Wasley (2005). Columns (1) to (4) report results when equity incentives are measured in levels ($000s) and columns (5) to (8) report results when equity incentives are measured in logs. Columns (1), (2), (5), and (6) control for firm size in total assets, whereas columns (3), (4), (7), and (8) use the market value of equity. Each column reports standardized coefficients associated with a one-standard-deviation increase in the explanatory variable as a percentage of the standard deviation of the dependent variable. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively. Standard errors are clustered at the firm-level and t-statistics are reported in parentheses. Details on the definition of all variables are provided in Appendix B. Changes in t-statistics for the coefficients on equity incentives are presented in Appendix C.

	Dependent Variable = *KLW05_DAC*							
	Levels Equity Incentives				**Logs Equity Incentives**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
CEO_Delta	-0.0225**	-0.0336***	-0.0135	-0.0205	-0.0409***	-0.0295**	-0.0248**	-0.0078
	(-2.01)	(-2.67)	(-1.18)	(-1.61)	(-3.40)	(-1.98)	(-2.02)	(-0.52)
CEO_Vega	-0.0766***	-0.0206	-0.0231*	-0.0070	-0.0194	0.0094	-0.0028	0.0121
	(-5.47)	(-1.53)	(-1.65)	(-0.52)	(-1.35)	(0.58)	(-0.19)	(0.75)
CFO_Delta	-0.0488***	-0.0404**	-0.0416**	-0.0383*	-0.0785***	-0.0406**	-0.0531***	-0.0267
	(-2.89)	(-1.96)	(-2.50)	(-1.89)	(-5.01)	(-2.35)	(-3.28)	(-1.54)
CFO_Vega	-0.0046	0.0194	0.0067	0.0232	-0.0181	-0.0001	-0.0162	0.0008
	(-0.29)	(1.28)	(0.43)	(1.54)	(-1.06)	(-0.01)	(-0.94)	(0.04)
Size(Assets)	0.1298***	0.1748***			0.1249***	0.1800***		
	(8.13)	(3.90)			(7.93)	(3.89)		
Size(Market)			-0.0243	-0.0502			-0.0006	-0.0492
			(-1.63)	(-1.63)			(-0.04)	(-1.47)
MTB	-0.0471***	-0.0143	-0.0463***	-0.0143	-0.0448***	-0.0157*	-0.0469***	-0.0167*
	(-4.97)	(-1.56)	(-4.84)	(-1.53)	(-4.74)	(-1.70)	(-4.89)	(-1.76)
Leverage	0.0304**	0.0287	0.0498***	0.0276	0.0281*	0.0252	0.0485***	0.0265
	(1.98)	(1.19)	(3.05)	(1.10)	(1.88)	(1.04)	(3.04)	(1.06)
Finance	-0.0337**	-0.0133	-0.0356***	-0.0047	-0.0274**	-0.0133	-0.0336**	-0.0066
	(-2.57)	(-1.03)	(-2.72)	(-0.37)	(-2.07)	(-1.01)	(-2.54)	(-0.50)
FCF	-0.2157***	-0.0476	-0.2167***	-0.0468	-0.2196***	-0.0499	-0.2199***	-0.0491
	(-7.28)	(-1.39)	(-7.32)	(-1.37)	(-7.51)	(-1.45)	(-7.48)	(-1.43)
Acquisition	-0.0024	0.0120	0.0056	0.0193**	0.0044	0.0116	0.0094	0.0188**
	(-0.28)	(1.34)	(0.63)	(2.13)	(0.50)	(1.30)	(1.05)	(2.08)
Restructure	-0.0152	0.0202**	-0.0085	0.0145	-0.0212**	0.0173*	-0.0112	0.0137
	(-1.59)	(2.12)	(-0.86)	(1.56)	(-2.24)	(1.84)	(-1.16)	(1.47)
ROA	-0.0656*	-0.0785**	-0.0668*	-0.0755**	-0.0585*	-0.0746**	-0.0624*	-0.0738**
	(-1.76)	(-2.14)	(-1.80)	(-2.09)	(-1.64)	(-2.07)	(-1.73)	(-2.06)
ROA_Change	0.0316	0.0149	0.0317	0.0121	0.0321*	0.0152	0.0318	0.0126
	(1.63)	(1.02)	(1.63)	(0.84)	(1.65)	(1.05)	(1.63)	(0.87)
BigN	-0.0054	-0.0075	0.0054	-0.0007	-0.0004	-0.0058	0.0074	-0.0001
	(-0.50)	(-0.47)	(0.50)	(-0.05)	(-0.03)	(-0.36)	(0.68)	(-0.00)
First-Stage	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.0874	0.3247	0.0808	0.3236	0.0891	0.3242	0.0821	0.3230
Adj. R^2	0.0857	0.2437	0.0792	0.2424	0.0874	0.2431	0.0805	0.2418
N	21,695	21,475	21,695	21,475	21,695	21,475	21,695	21,475

TABLE 8
Equity Incentives and Accounting Restatements

This table provides regression results from estimating the relation between equity incentives and Form 8-K Item 4.02 restatements. Columns (1) to (4) report results when equity incentives are measured in levels ($000s) and columns (5) to (8) report results when equity incentives are measured in logs. Columns (1), (2), (5), and (6) control for firm size in total assets, whereas columns (3), (4), (7), and (8) use the market value of equity. Each column reports standardized coefficients associated with a one-standard-deviation increase in the explanatory variable as a percentage of the standard deviation of the dependent variable. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively. Standard errors are clustered at the firm-level and t-statistics are reported in parentheses. Details on the definition of all variables are provided in Appendix B. Changes in t-statistics for the coefficients on equity incentives are presented in Appendix C.

	Dependent Variable = *BigR*							
	Levels Equity Incentives				**Logs Equity Incentives**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
CEO_Delta	0.0219	0.0319**	0.0252*	0.0285*	0.0100	0.0309*	0.0166	0.0291
	(1.63)	(1.98)	(1.88)	(1.75)	(0.65)	(1.76)	(1.06)	(1.58)
CEO_Vega	-0.0329**	-0.0234	-0.0278*	-0.0247	-0.0037	-0.0325*	-0.0025	-0.0323*
	(-2.19)	(-1.42)	(-1.82)	(-1.49)	(-0.19)	(-1.74)	(-0.13)	(-1.73)
CFO_Delta	-0.0057	0.0106	-0.0044	0.0094	0.0037	0.0365**	0.0119	0.0343*
	(-0.73)	(1.00)	(-0.54)	(0.89)	(0.23)	(2.07)	(0.72)	(1.83)
CFO_Vega	-0.0004	0.0052	0.0007	0.0051	-0.0304	-0.0265	-0.0313	-0.0253
	(-0.03)	(0.39)	(0.05)	(0.39)	(-1.52)	(-1.31)	(-1.57)	(-1.25)
Size(Assets)	-0.0184	0.0132			-0.0228*	0.0135		
	(-1.45)	(0.30)			(-1.84)	(0.30)		
Size(Market)			-0.0311**	0.0306			-0.0377***	0.0127
			(-2.38)	(0.95)			(-2.85)	(0.34)
MTB	-0.0103	-0.0091	-0.0071	-0.0105	-0.0106	-0.0103	-0.0073	-0.0110
	(-1.09)	(-0.88)	(-0.75)	(-1.01)	(-1.12)	(-0.99)	(-0.77)	(-1.05)
Leverage	0.0062	0.0153	0.0037	0.0191	0.0057	0.0188	0.0027	0.0204
	(0.54)	(0.89)	(0.33)	(1.12)	(0.50)	(1.08)	(0.24)	(1.19)
Finance	0.0092	0.0057	0.0114*	0.0039	0.0096	0.0025	0.0108	0.0024
	(1.39)	(0.86)	(1.68)	(0.61)	(1.40)	(0.39)	(1.56)	(0.36)
FCF	0.0017	0.0078	0.0035	0.0070	0.0017	0.0080	0.0036	0.0079
	(0.17)	(0.63)	(0.34)	(0.58)	(0.17)	(0.66)	(0.35)	(0.65)
Acquisition	-0.0011	-0.0052	-0.0006	-0.0056	-0.0002	-0.0045	-0.0004	-0.0042
	(-0.12)	(-0.49)	(-0.06)	(-0.52)	(-0.02)	(-0.42)	(-0.04)	(-0.39)
Restructure	0.0190*	0.0149	0.0182*	0.0164	0.0187*	0.0184	0.0187*	0.0187
	(1.73)	(1.23)	(1.67)	(1.34)	(1.67)	(1.51)	(1.67)	(1.52)
ROA	0.0150	0.0110	0.0174	0.0088	0.0153	0.0083	0.0168	0.0077
	(1.36)	(0.83)	(1.55)	(0.66)	(1.36)	(0.64)	(1.48)	(0.59)
ROA_Change	-0.0010	0.0045	-0.0014	0.0050	-0.0014	0.0041	-0.0017	0.0042
	(-0.12)	(0.53)	(-0.17)	(0.60)	(-0.16)	(0.49)	(-0.20)	(0.50)
BigN	-0.0046	-0.0170	-0.0033	-0.0178	-0.0035	-0.0180	-0.0027	-0.0179
	(-0.50)	(-1.10)	(-0.36)	(-1.16)	(-0.38)	(-1.17)	(-0.29)	(-1.16)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.0392	0.2971	0.0395	0.2971	0.0391	0.2975	0.0395	0.2975
Adj. R^2	0.0378	0.2145	0.0381	0.2146	0.0377	0.2150	0.0380	0.2150
N	23,608	23,401	23,608	23,401	23,608	23,401	23,608	23,401

TABLE 9

Equity Incentives and Accounting and Auditing Enforcement Releases

This table provides regression results from estimating the relation between equity incentives and SEC Accounting and Auditing Enforcement Releases. Columns (1) to (4) report results when equity incentives are measured in levels ($000s) and columns (5) to (8) report results when equity incentives are measured in logs. Columns (1), (2), (5), and (6) control for firm size in total assets, whereas columns (3), (4), (7), and (8) use the market value of equity. Each column reports standardized coefficients associated with a one-standard-deviation increase in the explanatory variable as a percentage of the standard deviation of the dependent variable. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels (two-tailed), respectively. Standard errors are clustered at the firm-level and t-statistics are reported in parentheses. Details on the definition of all variables are provided in Appendix B. Changes in t-statistics for the coefficients on equity incentives are presented in Appendix C.

	Dependent Variable = *AAER*							
	Levels Equity Incentives				**Logs Equity Incentives**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
CEO_Delta	0.0220	0.0367*	0.0215	0.0272	0.0231	0.0527***	0.0254*	0.0418**
	(1.38)	(1.68)	(1.33)	(1.20)	(1.63)	(2.76)	(1.74)	(2.06)
CEO_Vega	0.0072	0.0162	0.0071	0.0131	-0.0148	-0.0301	-0.0139	-0.0301
	(0.34)	(0.64)	(0.33)	(0.50)	(-0.65)	(-1.30)	(-0.61)	(-1.29)
CFO_Delta	0.0229	0.0265	0.0228	0.0230	0.0283	0.0288	0.0314*	0.0174
	(1.41)	(1.25)	(1.41)	(1.10)	(1.57)	(1.32)	(1.75)	(0.78)
CFO_Vega	-0.0158	0.0059	-0.0158	0.0059	0.0052	0.0386	0.0049	0.0432*
	(-1.25)	(0.46)	(-1.26)	(0.46)	(0.25)	(1.47)	(0.24)	(1.64)
Size(Assets)	0.0054	0.0484			-0.0020	0.0344		
	(0.37)	(0.94)			(-0.16)	(0.65)		
Size(Market)			0.0058	0.0902***			-0.0097	0.0629*
			(0.39)	(2.84)			(-0.71)	(1.77)
MTB	0.0100	0.0144*	0.0093	0.0099	0.0090	0.0137*	0.0096	0.0113
	(1.25)	(1.81)	(1.17)	(1.22)	(1.12)	(1.69)	(1.18)	(1.33)
Leverage	-0.0158	-0.0366*	-0.0148	-0.0250	-0.0139	-0.0304	-0.0137	-0.0239
	(-1.62)	(-1.92)	(-1.60)	(-1.38)	(-1.44)	(-1.63)	(-1.45)	(-1.32)
Finance	0.0243**	0.0118	0.0239**	0.0070	0.0217**	0.0092	0.0218*	0.0073
	(2.17)	(1.27)	(2.14)	(0.78)	(1.96)	(0.96)	(1.95)	(0.79)
FCF	-0.0101	-0.0053	-0.0105	-0.0073	-0.0102	-0.0040	-0.0097	-0.0052
	(-0.95)	(-0.41)	(-0.99)	(-0.57)	(-0.95)	(-0.31)	(-0.91)	(-0.41)
Acquisition	0.0249***	0.0041	0.0249***	0.0034	0.0223**	0.0040	0.0224**	0.0037
	(2.79)	(0.41)	(2.83)	(0.35)	(2.54)	(0.40)	(2.56)	(0.38)
Restructure	0.0114	-0.0088	0.0116	-0.0046	0.0133	-0.0046	0.0136	-0.0031
	(1.21)	(-0.87)	(1.24)	(-0.45)	(1.39)	(-0.45)	(1.44)	(-0.30)
ROA	0.0020	0.0018	0.0017	-0.0048	-0.0018	-0.0040	-0.0012	-0.0070
	(0.15)	(0.14)	(0.13)	(-0.37)	(-0.13)	(-0.33)	(-0.09)	(-0.54)
ROA_Change	-0.0011	0.0012	-0.0011	0.0028	-0.0008	0.0018	-0.0010	0.0027
	(-0.19)	(0.24)	(-0.19)	(0.54)	(-0.14)	(0.35)	(-0.17)	(0.51)
BigN	0.0004	0.0275	0.0004	0.0255	-0.0009	0.0251	-0.0002	0.0244
	(0.05)	(1.23)	(0.06)	(1.14)	(-0.12)	(1.14)	(-0.02)	(1.09)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.0135	0.3798	0.0135	0.3804	0.0138	0.3805	0.0138	0.3807
Adj. R^2	0.0121	0.3069	0.0121	0.3076	0.0123	0.3077	0.0124	0.3080
N	23,608	23,401	23,608	23,401	23,608	23,401	23,608	23,401

Appendix A

Research Design Choices and Findings in Prior Literature

Study	**Un/matched Sample**	**Dependent Variable**	**Equity Incentives Measure**	**Firm Size Measure**	**Firm Fixed Effects**	**Outlier Treatment**	**Findings**
Cheng and Warfield (2005)	Unmatched	Discretionary accruals: Jones (1991) model	CEO stock and option ownership and compensation	Natural logarithm of market value of equity	No	Trimming	Positive relation
Bergstresser and Philippon (2006)	Unmatched	Total accruals: cash flow approach; balance sheet approach Discretionary accruals: Jones (1991) model; modified Jones model from Dechow, Sloan, and Sweeney (1995) *untabulated*	CEO delta from stock and option portfolio in levels (scaled by total compensation)	Natural logarithm of total assets	No	Winsorizing (1st/99th percentiles)	Positive relation
Burns and Kedia (2006)	Unmatched	Restatements	CEO delta from option portfolio in logs	Natural logarithm of market value of equity	No	Trimming	Positive relation
Erickson, Hanlon, and Maydew (2006)	Both	AAERs	Executive delta from stock and option portfolio in levels	Natural logarithm of market value of equity	No	Winsorizing (1st/99th percentiles)	No relation

Efendi, Srivastava, and Swanson (2007)	Matched	Restatements	CEO in-the-money option holdings CEO delta from option portfolio in logs (measure from Burns and Kedia 2006)	Natural logarithm of total assets	No	Winsorizing (1st/99th percentiles)	Positive relation
Larcker, Richardson, and Tuna (2007)	Unmatched	Discretionary accruals: modified Jones model from Dechow, Sloan, and Sweeney (1995), extended by book-to-market ratio and operating cash flow; standard deviation of residual from Dechow and Dichev (2002) model *untabulated* Restatements	Executive stock ownership and compensation	Natural logarithm of market value of equity	No	Winsorizing (2nd/98th percentiles)	No relation

Johnson, Ryan, and Tian (2009)	Matched	AAERs	CEO delta from (un/restricted) stock and (un/vested) option portfolio in levels Executive delta from (un/restricted) stock and (un/vested) option portfolio in levels *untabulated*	Natural logarithm of total sales Natural logarithm of total assets *untabulated* Natural logarithm of market value of equity *untabulated*	No	None	Positive relation unrestricted stock No relation restricted stock and options (both vested and unvested)
Armstrong, Jagolinzer, and Larcker (2010)	Matched	Restatements AAERs Class action lawsuits	CEO delta from stock and option portfolio in levels (quintile ranks)	Natural logarithm of market value of equity	No	None	No relation
Chava and Purnanandam (2010)	Unmatched	Total accruals: cash flow approach; balance sheet approach *untabulated*	CEO/CFO delta and vega from stock and option portfolio in logs	Natural logarithm of total assets	No	Winsorizing (1st/99th percentiles)	No relation CEO delta and vega Positive (negative) relation CFO delta (vega)

Jiang, Petroni, and Wang (2010)	Unmatched	Total accruals: cash flow approach Discretionary accruals: modified Jones model from Dechow, Sloan, and Sweeney (1995), extended by lagged total accruals and future sales growth	CEO/CFO delta from stock and option portfolio in levels (measure from Bergstresser and Philippon 2006)	Natural logarithm of total assets	No	Winsorizing (1st/99th percentiles)	Total accruals: no relation CEO; positive relation CFO Discretionary accruals: positive relation CEO and CFO (economically larger) [post-SOX period: no relation, except negative relation CFO discretionary accruals]
Feng, Ge, Luo, and Shevlin (2011)	Matched	AAERs	CEO/CFO delta from stock and option portfolio in levels (measure from Bergstresser and Philippon 2006)	Natural logarithm of total assets	No	Winsorizing (1st/99th percentiles)	Positive relation CEO No relation CFO

Armstrong, Larcker, Ormazabal, and Taylor (2013)	Both	Discretionary accruals: modified Jones model from Dechow, Sloan, and Sweeney (1995); residual from Dechow and Dichev (2002) model; residual from Dechow and Dichev (2002) model as modified by McNichols (2002) Restatements AAERs	Executive delta and vega from stock and option portfolio in logs CEO versus other executive delta and vega from stock and option portfolio in logs *additionally*	Natural logarithm of market value of equity	Yes	Winsorizing (1st/99th percentiles)	Positive (no) relation vega (delta) No relation CEO delta and vega
Davidson (2021)	Both	AAERs	Executive delta and vega from stock and option portfolio in levels CEO/CFO versus other executive delta and vega from stock and option portfolio in levels *additionally*	Natural logarithm of market value of equity	Yes	Winsorizing (1st/99th percentiles)	Positive relation implicated executive delta and vega [both within AAER firms and compared to non-AAER firms] [both pre- and post-SOX periods]

Mayberry, Park, and Xu (2021)	Unmatched	Discretionary accruals: modified Jones model from Dechow, Sloan, and Sweeney (1995); performance-adjusted and performance-matched models from Kothari, Leone, and Wasley (2005); residual from Dechow and Dichev (2002) model *untabulated*; residual from Dechow and Dichev (2002) model as modified by McNichols (2002) *untabulated* Restatements AAERs	Executive delta and vega from stock and option portfolio in logs	Natural logarithm of market value of equity	Yes	Winsorizing (1st/99th percentiles)	No relation delta and vega

Appendix B

Variable Definitions

Variable Name	Definition
Dependent Variables	
Accruals	Difference between income before extraordinary items and operating cash flow scaled by lagged total assets
RSST05_Accruals	Sum of one-year changes in working capital accruals, long-term operating assets, and long-term operating liabilities scaled by average total assets (Richardson, Sloan, Soliman, and Tuna 2005)
DSS95_DAC	Discretionary accruals as the residual from the model in Dechow, Sloan, and Sweeney (1995), estimated by 2-digit SIC industry-year with at least 20 observations
DD02_SD(DAC)	Standard deviation of discretionary accruals as the residual from the model in Dechow and Dichev (2002) as modified by McNichols (2002), where discretionary accruals are estimated by 2-digit SIC industry-year with at least 20 observations and the standard deviation is calculated over a minimum (maximum) period of 3 (5) years
KLW05_DAC	Performance-adjusted discretionary accruals as the residual from the model in Kothari, Leone, and Wasley (2005), estimated by 2-digit SIC industry-year with at least 20 observations
BigR	Indicator variable taking the value of one for firm-years with subsequent restatement via Form 8-K Item 4.02 filing, and zero otherwise
AAER	Indicator variable taking the value of one for firm-years where the SEC identified a misstatement in an Accounting and Auditing Enforcement Release, and zero otherwise
Equity Incentives	
CEO_Delta	Sensitivity of the CEO's stock and option portfolio to a one percent change in the firm's stock price, measured either in levels ($000s) or logs (natural logarithm of one plus the dollar amount)
CEO_Vega	Sensitivity of the CEO's stock and option portfolio to a 0.01 change in the standard deviation of the firm's stock returns, measured either in levels ($000s) or logs (natural logarithm of one plus the dollar amount)
CFO_Delta	Sensitivity of the CFO's stock and option portfolio to a one percent change in the firm's stock price, measured either in levels ($000s) or logs (natural logarithm of one plus the dollar amount)
CFO_Vega	Sensitivity of the CFO's stock and option portfolio to a 0.01 change in the standard deviation of the firm's stock returns, measured either in levels ($000s) or logs (natural logarithm of one plus the dollar amount)
Firm Characteristics	
Size(Assets)	Natural logarithm of one plus total assets (in $m)
Size(Market)	Natural logarithm of one plus the market value of common equity (in $m)
MTB	Market value of common equity divided by the book value of common equity

Leverage	Book value of total debt divided by total assets
Finance	One-year change in the book value of common equity and the book value of long-term debt scaled by lagged total assets
FCF	Difference between operating cash flow and capital expenditures scaled by lagged total assets
Acquisition	Indicator variable taking the value of one if a firm engages in any acquisitions in the given or the prior year, and zero otherwise
Restructure	Indicator variable taking the value of one if a firm reports any restructuring activities in the given or the prior year, and zero otherwise
ROA	Income before extraordinary items scaled by lagged total assets
ROA_Change	One-year change in return on assets
BigN	Indicator variable taking the value of one if a firm is audited by one of the 4(5) biggest audit firms after (before) 2001, and zero otherwise

Appendix C

Changes in t-Statistics for Equity Incentives Coefficients

TABLE 3

Absolute Changes t-Statistics when Changing Firm Size Measure

	Without Firm Fixed Effects			**With Firm Fixed Effects**			**Average**
	Levels (3-1)	Logs (7-5)	Average	Levels (4-2)	Logs (8-6)	Average	
CEO_Delta	0.24	0.22	0.23	1.20	1.12	1.16	0.70
CEO_Vega	0.19	0.17	0.18	0.65	0.23	0.44	0.31
CFO_Delta	0.01	0.08	0.05	0.06	0.50	0.28	0.16
CFO_Vega	0.03	0.02	0.03	0.17	0.14	0.16	0.09
Average	0.12	0.12	0.12	0.52	0.50	0.51	**0.32**

Absolute Changes t-Statistics when Changing Equity Incentives Measure

	Without Firm Fixed Effects			**With Firm Fixed Effects**			**Average**
	Size(Assets) (5-1)	*Size(Market)* (7-3)	Average	*Size(Assets)* (6-2)	*Size(Market)* (8-4)	Average	
CEO_Delta	1.99	2.01	2.00	1.99	2.07	2.03	2.02
CEO_Vega	0.25	0.23	0.24	0.21	0.63	0.42	0.33
CFO_Delta	0.05	0.12	0.09	2.19	1.75	1.97	1.03
CFO_Vega	1.40	1.41	1.40	2.51	2.48	2.50	1.95
Average	0.92	0.94	0.93	1.73	1.73	1.73	**1.33**

TABLE 4

Absolute Changes t-Statistics when Changing Firm Size Measure

	Without Firm Fixed Effects			**With Firm Fixed Effects**			**Average**
	Levels (3-1)	Logs (7-5)	Average	Levels (4-2)	Logs (8-6)	Average	
CEO_Delta	0.83	0.43	0.63	1.72	1.72	1.72	1.18
CEO_Vega	4.97	1.15	3.06	4.05	1.32	2.69	2.87
CFO_Delta	0.59	1.17	0.88	0.14	0.12	0.13	0.51
CFO_Vega	0.81	0.02	0.42	1.33	1.35	1.34	0.88
Average	1.80	0.69	1.25	1.81	1.13	1.47	**1.36**

Absolute Changes t-Statistics when Changing Equity Incentives Measure

	Without Firm Fixed Effects			**With Firm Fixed Effects**			**Average**
	Size(Assets) (5-1)	*Size(Market)* (7-3)	Average	*Size(Assets)* (6-2)	*Size(Market)* (8-4)	Average	
CEO_Delta	1.71	2.11	1.91	2.00	2.00	2.00	1.96
CEO_Vega	0.08	3.90	1.99	1.25	1.48	1.37	1.68
CFO_Delta	7.81	7.23	7.52	7.11	7.37	7.24	7.38
CFO_Vega	4.39	3.56	3.98	3.56	3.58	3.57	3.77
Average	3.50	4.20	3.85	3.48	3.61	3.54	**3.70**

Absolute Changes t-Statistics when Changing Dependent Variable (with TABLE 3)

	Levels Equity Incentives				**Logs Equity Incentives**				**Average**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
CEO_Delta	6.63	3.24	6.04	2.72	6.35	3.25	6.14	2.65	4.63
CEO_Vega	1.55	0.53	6.71	3.93	1.72	1.99	3.04	3.08	2.82
CFO_Delta	3.39	2.61	2.81	2.53	11.25	7.53	10.16	8.15	6.05
CFO_Vega	1.81	2.46	2.65	3.62	4.80	3.51	4.80	4.72	3.55
Average	3.35	2.21	4.55	3.20	6.03	4.07	6.04	4.65	**4.26**

TABLE 5

Absolute Changes t-Statistics when Changing Firm Size Measure

	Without Firm Fixed Effects			**With Firm Fixed Effects**			**Average**
	Levels (3-1)	Logs (7-5)	Average	Levels (4-2)	Logs (8-6)	Average	
CEO_Delta	0.52	0.77	0.65	0.95	1.29	1.12	0.88
CEO_Vega	1.89	0.54	1.22	0.49	0.08	0.29	0.75
CFO_Delta	0.22	0.90	0.56	0.09	0.79	0.44	0.50
CFO_Vega	0.41	0.05	0.23	0.10	0.12	0.11	0.17
Average	0.76	0.57	0.66	0.41	0.57	0.49	**0.58**

Absolute Changes t-Statistics when Changing Equity Incentives Measure

	Without Firm Fixed Effects			**With Firm Fixed Effects**			**Average**
	Size(Assets) (5-1)	*Size(Market)* (7-3)	Average	*Size(Assets)* (6-2)	*Size(Market)* (8-4)	Average	
CEO_Delta	2.54	2.29	2.42	1.88	1.54	1.71	2.06
CEO_Vega	3.08	1.73	2.41	1.95	1.54	1.75	2.08
CFO_Delta	2.81	2.13	2.47	1.11	0.41	0.76	1.62
CFO_Vega	0.04	0.40	0.22	0.67	0.45	0.56	0.39
Average	2.12	1.64	1.88	1.40	0.99	1.19	**1.54**

TABLE 6

Absolute Changes t-Statistics when Changing Firm Size Measure

	Without Firm Fixed Effects			**With Firm Fixed Effects**			**Average**
	Levels (3-1)	Logs (7-5)	Average	Levels (4-2)	Logs (8-6)	Average	
CEO_Delta	0.55	0.73	0.64	0.04	0.07	0.06	0.35
CEO_Vega	0.26	0.18	0.22	0.46	0.14	0.30	0.26
CFO_Delta	0.17	0.65	0.41	0.30	0.57	0.44	0.42
CFO_Vega	0.12	0.17	0.15	0.33	0.53	0.43	0.29
Average	0.28	0.43	0.35	0.28	0.33	0.31	**0.33**

Absolute Changes t-Statistics when Changing Equity Incentives Measure

	Without Firm Fixed Effects			**With Firm Fixed Effects**			**Average**
	Size(Assets) (5-1)	*Size(Market)* (7-3)	Average	*Size(Assets)* (6-2)	*Size(Market)* (8-4)	Average	
CEO_Delta	0.62	0.44	0.53	0.45	0.42	0.44	0.48
CEO_Vega	0.05	0.03	0.04	0.28	0.04	0.16	0.10
CFO_Delta	1.15	0.67	0.91	0.27	0.00	0.14	0.52
CFO_Vega	0.20	0.15	0.18	0.48	0.28	0.38	0.28
Average	0.51	0.32	0.41	0.37	0.19	0.28	**0.35**

Absolute Changes t-Statistics when Changing Dependent Variable (with TABLE 5)

	Levels Equity Incentives				**Logs Equity Incentives**				**Average**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
CEO_Delta	1.42	0.40	2.49	0.59	1.74	2.73	0.24	1.37	1.37
CEO_Vega	4.23	2.66	2.60	2.63	1.20	0.99	0.84	1.05	2.03
CFO_Delta	2.24	1.62	2.63	2.01	1.72	0.24	0.17	1.60	1.53
CFO_Vega	2.73	1.65	2.44	1.88	2.57	0.50	2.69	1.15	1.95
Average	2.66	1.58	2.54	1.78	1.81	1.12	0.99	1.29	**1.72**

TABLE 7

Absolute Changes t-Statistics when Changing Firm Size Measure

	Without Firm Fixed Effects			**With Firm Fixed Effects**			**Average**
	Levels (3-1)	Logs (7-5)	Average	Levels (4-2)	Logs (8-6)	Average	
CEO_Delta	0.83	1.38	1.11	1.06	1.46	1.26	1.18
CEO_Vega	3.82	1.16	2.49	1.01	0.17	0.59	1.54
CFO_Delta	0.39	1.73	1.06	0.07	0.81	0.44	0.75
CFO_Vega	0.72	0.12	0.42	0.26	0.05	0.16	0.29
Average	1.44	1.10	1.27	0.60	0.62	0.61	**0.94**

Absolute Changes t-Statistics when Changing Equity Incentives Measure

	Without Firm Fixed Effects			**With Firm Fixed Effects**			**Average**
	Size(Assets) (5-1)	*Size(Market)* (7-3)	Average	*Size(Assets)* (6-2)	*Size(Market)* (8-4)	Average	
CEO_Delta	1.39	0.84	1.12	0.69	1.09	0.89	1.00
CEO_Vega	4.12	1.46	2.79	2.11	1.27	1.69	2.24
CFO_Delta	2.12	0.78	1.45	0.39	0.35	0.37	0.91
CFO_Vega	0.77	1.37	1.07	1.29	1.50	1.40	1.23
Average	2.10	1.11	1.61	1.12	1.05	1.09	**1.35**

Absolute Changes t-Statistics when Changing Dependent Variable (with TABLE 5)

	Levels Equity Incentives				**Logs Equity Incentives**				**Average**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
CEO_Delta	2.34	5.15	0.99	3.14	6.27	6.34	4.12	3.59	3.99
CEO_Vega	8.63	2.70	2.92	1.20	1.43	1.36	0.27	1.61	2.52
CFO_Delta	1.03	0.13	0.42	0.29	5.96	1.37	3.33	0.23	1.60
CFO_Vega	3.03	0.90	1.90	0.54	3.84	1.52	3.67	1.59	2.12
Average	3.76	2.22	1.56	1.29	4.38	2.65	2.85	1.76	**2.56**

TABLE 8

Absolute Changes t-Statistics when Changing Firm Size Measure

	Without Firm Fixed Effects			**With Firm Fixed Effects**			**Average**
	Levels (3-1)	Logs (7-5)	Average	Levels (4-2)	Logs (8-6)	Average	
CEO_Delta	0.25	0.41	0.33	0.23	0.18	0.21	0.27
CEO_Vega	0.37	0.06	0.22	0.07	0.01	0.04	0.13
CFO_Delta	0.19	0.49	0.34	0.11	0.24	0.18	0.26
CFO_Vega	0.08	0.05	0.07	0.00	0.06	0.03	0.05
Average	0.22	0.25	0.24	0.10	0.12	0.11	**0.18**

Absolute Changes t-Statistics when Changing Equity Incentives Measure

	Without Firm Fixed Effects			**With Firm Fixed Effects**			**Average**
	Size(Assets) (5-1)	*Size(Market)* (7-3)	Average	*Size(Assets)* (6-2)	*Size(Market)* (8-4)	Average	
CEO_Delta	0.98	0.82	0.90	0.22	0.17	0.20	0.55
CEO_Vega	2.00	1.69	1.85	0.32	0.24	0.28	1.06
CFO_Delta	0.96	1.26	1.11	1.07	0.94	1.01	1.06
CFO_Vega	1.49	1.62	1.56	1.70	1.64	1.67	1.61
Average	1.36	1.35	1.35	0.83	0.75	0.79	**1.07**

TABLE 9

Absolute Changes t-Statistics when Changing Firm Size Measure

	Without Firm Fixed Effects			**With Firm Fixed Effects**			**Average**
	Levels (3-1)	Logs (7-5)	Average	Levels (4-2)	Logs (8-6)	Average	
CEO_Delta	0.05	0.11	0.08	0.48	0.70	0.59	0.34
CEO_Vega	0.01	0.04	0.03	0.14	0.01	0.08	0.05
CFO_Delta	0.00	0.18	0.09	0.15	0.54	0.35	0.22
CFO_Vega	0.01	0.01	0.01	0.00	0.17	0.09	0.05
Average	0.02	0.09	0.05	0.19	0.36	0.27	**0.16**

Absolute Changes t-Statistics when Changing Equity Incentives Measure

	Without Firm Fixed Effects			**With Firm Fixed Effects**			**Average**
	Size(Assets) (5-1)	*Size(Market)* (7-3)	Average	*Size(Assets)* (6-2)	*Size(Market)* (8-4)	Average	
CEO_Delta	0.25	0.41	0.33	1.08	0.86	0.97	0.65
CEO_Vega	0.99	0.94	0.97	1.94	1.79	1.87	1.42
CFO_Delta	0.16	0.34	0.25	0.07	0.32	0.20	0.22
CFO_Vega	1.50	1.50	1.50	1.01	1.18	1.10	1.30
Average	0.73	0.80	0.76	1.03	1.04	1.03	**0.90**

Absolute Changes t-Statistics when Changing Dependent Variable (with TABLE 8)

	Levels Equity Incentives				**Logs Equity Incentives**				**Average**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
CEO_Delta	0.25	0.30	0.55	0.55	0.98	1.00	0.68	0.48	0.60
CEO_Vega	2.53	2.06	2.15	1.99	0.46	0.44	0.48	0.44	1.32
CFO_Delta	2.14	0.25	1.95	0.21	1.34	0.75	1.03	1.05	1.09
CFO_Vega	1.22	0.07	1.31	0.07	1.77	2.78	1.81	2.89	1.49
Average	1.54	0.67	1.49	0.71	1.14	1.24	1.00	1.22	**1.12**