Regulatory Changes and Quality of Corporate Disclosure

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Abstract:

This paper examines how whistleblower-driven enforcement under the Dodd-Frank Act (DFA) influences both mandatory and voluntary corporate disclosure. While prior studies tend to examine these domains separately, we adopt an integrated framework that captures disclosure dynamics across reporting quality, forecasting behaviour, and information asymmetry. We measure changes in financial reporting integrity using a firm-year Financial Statement Divergence (FSD) Score, which is based on Benford's Law and avoids assumptions embedded in accrual-based models. Voluntary disclosure quality is assessed using management earnings forecasts (MEFs), focusing on their frequency, timeliness, and precision. Drawing on over 10,000 U.S. firm-year observations from 2007 to 2014, we find that the DFA led to significant reductions in FSD Scores and bid–ask spreads, along with timelier and more frequent MEFs. However, forecast precision declined slightly, suggesting strategic caution under heightened enforcement. The results are robust to entropy balancing, propensity score matching, and difference-in-differences designs. Overall, the findings highlight the effectiveness of whistleblower provisions in enhancing transparency across multiple disclosure channels and underscore the importance of enforcement incentives in shaping firms' communication strategies.

Keywords: Whistleblower Provisions, Dodd-Frank Act, Financial Statement Divergence (FSD) Score, Mandatory Financial Reporting, Voluntary Disclosure, Regulatory Enforcement, Corporate Transparency

JEL Classification: M41, G38, K22

1. Introduction

Whistleblowers play a crucial role in safeguarding the integrity and stability of capital markets by exposing corporate fraud and misconduct. Their contributions are particularly significant in upholding transparency, accountability, and ethical corporate behaviour. According to the Association of Certified Fraud Examiners, whistleblower tips uncover approximately 42% of corporate fraud cases (Figure 1), far surpassing other detection methods (ACFE, 2022). High-profile whistleblower actions, most notably those by Cynthia Cooper at WorldCom and Sherron Watkins at Enron in the early 2000s, prompted significant regulatory reform in the United States. These events led to the enactment of the Sarbanes-Oxley Act of 2002, which required publicly listed companies to implement anonymous reporting mechanisms for internal whistleblowing purposes.

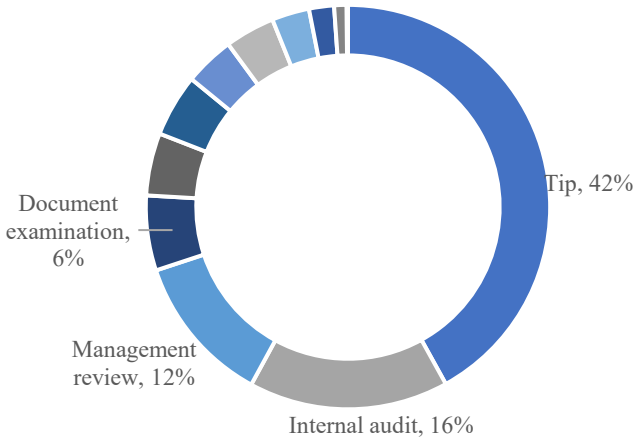


Figure 1. How is occupational fraud initially detected?

Source: ACFE 2022, Occupational Fraud 2022: A Report to the Nations, using 2,110 occupational *fraud cases from 133 countries investigated by ACFE between January 2020 and September 2021*

Despite ongoing regulatory efforts, the effectiveness of whistleblowing legislation remains contested, primarily due to ambiguous statutory language, onerous reporting procedures, and unclear evidentiary thresholds (Rapp, 2007; Yeoh, 2014). These institutional weaknesses were brought into sharp focus during the Global Financial Crisis (GFC) of 2008, which exposed serious deficiencies in the U.S. financial regulatory framework (GAO, 2013).

In response, the Dodd-Frank Act (DFA), enacted in 2010, implemented far-reaching reforms. These included generous whistleblower incentives, legal protections against retaliation, and strengthened confidentiality provisions designed to encourage the reporting of corporate misconduct (Blount & Markel, 2012; Du & Heo, 2022). Nevertheless, it remains empirically unclear whether these reforms have effectively altered corporate behaviour or improved the overall transparency of the information environment.

This paper investigates whether the DFA whistleblower provisions have improved corporate disclosure practices within the U.S. regulatory framework. While stronger enforcement is theoretically expected to deter misreporting by increasing detection risk and penalties (Ball et al., 2000; Christensen et al., 2013), empirical studies suggest that these effects vary depending on institutional factors, such as managerial incentives and regulatory capacity (Leuz & Wysocki, 2016). Although this study focuses solely on the U.S., enforcement outcomes can still vary within a single jurisdiction due to differences in firm-level governance structures, industry norms, and litigation risks. Accordingly, a nuanced understanding of the domestic enforcement environment is critical for evaluating how the DFA whistleblower provisions influence corporate disclosure behaviour.

This study investigates how the implementation and enforcement of the DFA whistleblower provisions influence corporate disclosure outcomes by taking an integrated view that encompasses financial reporting quality, voluntary disclosure practices, and information asymmetry. In doing so, the study addresses a notable gap in the literature, which tends to examine mandatory and voluntary disclosures in isolation, without considering their potential interdependence (Berger & Lee, 2022; Wiedman & Zhu, 2023). Prior research, including Huang et al. (2023), has primarily examined whistleblowing outcomes under earlier statutes such as the False Claims Act (FCA), focusing on both reporting behaviour and enforcement actions, but within pre-DFA settings. To the best of my knowledge, no existing study offers a

systematic evaluation of how the DFA's whistleblower provisions influence mandatory financial reporting and voluntary disclosure practices in the post-DFA era. This approach provides a novel empirical contribution by jointly assessing the effects of the DFA whistleblower provisions across mandatory and voluntary disclosure domains.

To empirically investigate these relationships, the study utilises a dataset comprising 10,192 firm-year observations spanning both the pre- and post-DFA periods (2007–2014). Earlier years, such as 2005 and 2006, are excluded to avoid confounding influences from the immediate post-SOX environment, while later years are omitted to minimise contamination from subsequent regulatory changes. This sampling strategy is consistent with prior studies examining the effects of major U.S. disclosure regulations (e.g., Berger and Lee (2022), Wiedman and Zhu (2023)). The analysis employs firm-year-level measures, most notably the Financial Statement Divergence (FSD) Score and several voluntary disclosure indicators.

The empirical analysis begins with a baseline pooled Ordinary Least Squares (OLS) regression that examines the association between the DFA provisions and corporate disclosure outcomes. To validate the robustness of the results, the study further employs alternative proxies for financial reporting quality, entropy balancing techniques, and industry-specific subsample analyses. Difference-in-differences (DiD) estimation is also applied to strengthen causal interpretation, leveraging institutional features unique to the DFA's implementation. These complementary methods help address concerns about sample selection bias and improve identification of regulatory effects amid concurrent changes.

The results indicate that the DFA whistleblower provisions significantly enhanced both mandatory financial reporting and voluntary disclosure practices. Firms exhibited improved financial statement integrity and became more likely to issue timely forward-looking forecasts. The information environment also improved, as evidenced by narrower bid-ask spreads and increased analyst coverage. These findings remain robust across a battery of robustness checks,

including alternative proxies, entropy balancing, matched samples, and DiD estimation. Overall, the analysis underscores the regulatory effectiveness of whistleblower provisions while revealing strategic disclosure adjustments shaped by firm characteristics and litigation exposure.

This research addresses several critical limitations in the existing literature. While prior studies often assume that regulatory enforcement unambiguously improves corporate transparency (Ball et al., 2000; Christensen et al., 2013), this study explicitly explores both the intended and unintended consequences of whistleblower-driven enforcement (Schantl & Wagenhofer, 2020). It further contributes by providing timely, U.S.-specific evidence on the institutional impact of the DFA, addressing an underexplored area in whistleblowing research (Berger & Lee, 2022; Huang et al., 2023; Wiedman & Zhu, 2023). Moreover, this study adopts a holistic approach by jointly evaluating mandatory and voluntary disclosures, highlighting their interdependence in shaping a firm's overall disclosure strategy and transparency outcomes (Ball et al., 2012; Frankel et al., 2021). This balanced analytical perspective allows for a critical assessment of both improvements in financial reporting and potential strategic adaptations in disclosure behaviour.

Beyond academic relevance, the findings also have important practical and policy implications. The evidence suggests that well-designed whistleblower provisions, such as those introduced by the DFA, can meaningfully improve the integrity of financial reporting and promote timely and detailed voluntary disclosures. These improvements contribute to a more transparent capital market and reduce information asymmetry. At the same time, the findings point to heterogeneity in firms' responses, shaped by litigation exposure and internal governance structures. Policymakers and regulators may benefit from these insights when designing or refining whistleblower frameworks, particularly in balancing enforcement strength with firm-level disclosure incentives.

In sum, this paper offers robust empirical support for the role of whistleblower provisions in enhancing corporate disclosure quality and market transparency. By systematically addressing key theoretical and empirical gaps, the study contributes to a more nuanced understanding of enforcement-driven disclosure behaviour. The remainder of this paper is structured as follows: Section 2 reviews relevant literature and develops the hypotheses. Section 3 outlines the research design, data sources, and key variables. Section 4 discusses empirical findings, and Section 5 concludes by summarising contributions and implications.

2. Literature Review

This section begins by outlining the institutional development of whistleblowing laws in the United States to provide a robust foundation for evaluating the effects of regulatory changes. It then reviews empirical literature on enforcement intensity, followed by an examination of how strengthened whistleblower protections influence financial reporting quality. Finally, it develops hypotheses based on theoretical insights and addresses key limitations in the existing literature.

2.1. Institutional Background

The United States is recognised as having one of the most developed legal infrastructures for incentivising whistleblowers, particularly in the context of corporate misconduct and securities fraud (Andon et al., 2018). The legislative foundation can be traced to the False Claims Act (FCA) of 1863, enacted during the Civil War to combat fraudulent claims against the government. Amendments in 1986 introduced enhanced protections and financial incentives (Rapp, 2012), establishing a precedent for future reforms in corporate accountability.

Subsequent legal developments expanded whistleblower protection, most notably the Sarbanes-Oxley Act (SOX) of 2002 and the Dodd-Frank Wall Street Reform and Consumer

Protection Act (DFA) of 2010. SOX introduced protections for employees reporting internal corporate fraud[1], while the DFA formalised a bounty programme and enabled whistleblowers to report violations directly to the Securities and Exchange Commission (SEC) with confidentiality safeguards and financial rewards (Du & Heo, 2022; Rapp, 2013; Yeoh, 2014). These reforms marked a fundamental shift toward incentivised, regulator-facing disclosures.

Despite these innovations, several structural limitations of the DFA have raised concerns. Whereas the FCA permits whistleblowers to file lawsuits under a *qui tam* provision, the DFA lacks such a mechanism and relies solely on administrative enforcement through the SEC, limiting whistleblowers' ability to initiate legal action (Bowen et al., 2010; Feldman & Lobel, 2009). Additional concerns involve restrictions on bounty recovery, inconsistent application of protections, and administrative delays. Although no major whistleblower laws were enacted between 2011 and 2014, proposed legislation such as the Whistleblower Improvement Act of 2011 and related SEC rulemakings helped refine the DFA framework[2].

Whistleblowing mechanisms are widely recognised as essential tools for promoting transparency and corporate compliance. Legal and empirical studies find that such provisions deter fraud, strengthen internal controls, and improve disclosure quality (Berger & Lee, 2022; Huang et al., 2023; Moberly, 2012; Wolfe, 2014). Nonetheless, debates persist over the clarity of legal protections, potential for frivolous claims, and variability in protections across jurisdictions (Schmidt, 2005; Wilde, 2017).

In sum, the DFA represents the most comprehensive U.S. reform to date in incentivising whistleblower reporting. Understanding its structural design and limitations provides essential context for evaluating its effect on corporate disclosure behaviour. These institutional

[1] The Sarbanes-Oxley Act was enacted in response to corporate scandals such as Enron and WorldCom. It offered whistleblower protections but lacked financial incentives or external reporting mechanisms. It has also been criticised for ambiguities in drafting and procedural burdens, which limited its deterrence effect Yeoh, P. (2014).
[2] In 2011, the SEC adopted Rule 21F to implement DFA provisions. Further refinements clarified the eligibility criteria for awards, anti-retaliation protections, and procedures for handling tips.

foundations also inform the identification strategy in this study and shape expectations about how enforcement incentives affect reporting outcomes.

2.2. Theoretical Background

Regulatory enforcement plays a pivotal role in shaping firms' financial disclosure behaviour. The economic crime model (Becker, 1968) posits that managers are less likely to engage in misreporting when the perceived probability and cost of detection are high. Strengthened enforcement increases the expected penalties associated with opportunistic behaviour and thereby aligns managerial incentives with compliance. While this logic may appear intuitive, it forms the theoretical foundation for numerous studies linking regulatory enforcement to improvements in financial reporting quality and market efficiency (Ewert & Wagenhofer, 2019; Florou et al., 2020).

Empirical studies provide broad support for the beneficial effects of regulatory enforcement on disclosure outcomes. For instance, accrual-based earnings management has been shown to decline following the introduction of the Sarbanes-Oxley Act (SOX) in the United States (Christensen et al., 2013) and after enforcement reforms in Germany (Ernstberger et al., 2012). Stronger enforcement environments are also associated with increased analyst forecast accuracy (Brown et al., 2014), higher audit quality (DeFond & Lennox, 2011; Hilary & Lennox, 2005), and improved investor confidence (Hope, 2003). These outcomes suggest that credible enforcement operates as an institutional mechanism that disciplines financial reporting and reduces agency problems (Ball et al., 2000; Christensen et al., 2020).

Nonetheless, the literature also identifies several limitations and unintended consequences of intensified enforcement. Excessive regulatory pressure may induce defensive reporting behaviours, such as over-compliance or conservative accounting choices, which can diminish accrual informativeness and elevate audit risk (Chan & Liu, 2022; Windisch, 2020). Smaller firms, in particular, may struggle to absorb compliance costs, resulting in

disproportionate burdens or reduced disclosure quality (Cohen et al., 2013; Defond et al., 2018). In cases where public and private enforcement mechanisms overlap, strategic interference may further erode effectiveness (Schantl & Wagenhofer, 2020).

Importantly, these outcomes are not uniform. The impact of enforcement is shaped by institutional and firm-specific characteristics such as legal origin, enforcement capacity, firm size, and growth opportunities (Allen et al., 2022; Dechow & Dichev, 2002). For example, younger or resource-constrained firms may exhibit limited responsiveness or even negative reactions to heightened enforcement. Moreover, regulations that increase reporting frequency have been shown to incentivise short-termism at the expense of long-term value creation (Ernstberger et al., 2017; Roychowdhury & Srinivasan, 2019).

While much of the existing literature focuses on enforcement regimes in Europe and cross-country contexts, relatively few studies have examined these dynamics within the US, particularly in relation to whistleblower-specific enforcement. As outlined in Section 2.1, the DFA introduces a distinctive mechanism by providing direct financial incentives for whistleblowers to report misconduct externally. Unlike conventional statutory enforcement, DFA provisions rely on incentivised, regulator-facing reporting rather than legal prosecution. This unique configuration of monetary incentives, regulator-facing reporting, and legal protections is expected to generate stronger deterrence effects and promote more timely and transparent disclosures than traditional statutory enforcement.

This institutional feature makes the U.S. setting particularly well-suited to evaluating the effects of strengthened whistleblower-based enforcement on financial reporting behaviour. Accordingly, this study builds on this theoretical foundation to investigate whether the DFA framework has improved reporting quality through credible deterrence and strategic transparency responses.

2.3. Whistleblower Enforcement: Evidence and Gaps

Whistleblower mechanisms, particularly those formalised under the DFA, have become a cornerstone of modern regulatory enforcement in the US. A growing body of empirical research demonstrates that whistleblower involvement enhances enforcement capacity by supplying regulators with credible, early-stage information on potential misconduct (Call et al., 2018; Ewert & Wagenhofer, 2019). The DFA strengthened these mechanisms by providing explicit legal protections, financial incentives, and confidentiality safeguards (Du & Heo, 2022). These reforms are especially effective in complex organisational settings where internal oversight may fail to detect wrongdoing[3] (Dyck et al., 2010; Zingales, 2004).

Firms affected by the DFA have shown measurable improvements in governance and disclosure practices. Documented benefits include reductions in misreporting and tax aggressiveness (Huang et al., 2023), enhanced internal monitoring structures (Leuz & Wysocki, 2016), and increased fraud detection via external channels (Feldman & Lobel, 2008; Miceli & Pollex Near, 2013). Moreover, analysts report greater transparency and forecast accuracy, contributing to improved investor trust (Berger & Lee, 2022; Wiedman & Zhu, 2023). However, critics raise valid concerns about the credibility and sustainability of these effects. Key risks include frivolous or self-interested reporting, strategic manipulation of bounty incentives, and limited media attention that may dilute deterrence power (Bowen et al., 2010; Miceli & Near, 1992; Wilde, 2017). Furthermore, the threat of retaliation and ambiguity in "public interest" definitions continue to challenge whistleblower protections (Call et al., 2016; Schmidt, 2005).

While prior studies affirm the role of the DFA in strengthening transparency, three important gaps in the literature remain. First, many studies rely on traditional accrual-based metrics, such as abnormal accruals or the F-score, which may not fully capture firm-level reporting quality. These proxies are often sensitive to managerial discretion and accounting

[3] In this study, whistleblower enforcement is treated as a form of regulatory oversight that complements formal monitoring mechanisms, particularly in detecting fraud and reinforcing disclosure quality.

policy choices, making it difficult to isolate the effect of regulatory enforcement. To overcome this limitation, this study adopts a firm-year based measure known as the FSD Score[4]. As introduced in Appendix C, the FSD Score evaluates statistical conformity in reported numbers based on Benford's Law, offering a model-free and less manipulable proxy for detecting misreporting and disclosure irregularity.

Second, the interdependence between mandatory reporting and voluntary disclosure has been largely overlooked in the whistleblowing literature. Although some studies examine firm-level governance outcomes or financial statement quality (Christensen et al., 2013; Dechow et al., 2010; Ernstberger et al., 2012), few assess whether whistleblower provisions influence voluntary forecasting behaviour. The Confirmation Hypothesis (Ball et al., 2012) suggests that mandatory and voluntary disclosures interact synergistically, with each reinforcing the credibility and informativeness of the other through mutual validation (Frankel et al., 2021; Gigler & Hemmer, 1998). Understanding how whistleblower enforcement affects both disclosure channels is therefore essential to fully evaluating its impact on the corporate information environment.

Third, the majority of empirical research focuses on earlier legal frameworks (e.g. the FCA and the SOX) or investigates enforcement regimes outside the US. While these studies offer valuable insights, they do not reflect the structural innovation introduced by the DFA, particularly its incentive-based, regulator-facing approach. Recent research by Huang et al. (2023) underscores this gap by examining the FCA in pre-DFA settings, leaving unanswered how the integrated whistleblower framework under the DFA affects disclosure in a post-crisis U.S. context.

[4] In this study, we use a firm-year based measure of reporting quality, the Financial Statement Divergence (FSD) Score. This indicator is derived from Benford's Law, which evaluates the statistical distribution of digits in financial statement items. It provides a model-free proxy for detecting irregularities in reported numbers. Further details are provided in Section 3.2.1 and Appendix C.

In response to these limitations, this study aims to contribute in three key areas. First, it examines whether the implementation of the DFA improves firm-year level mandatory disclosure quality, as measured by the FSD Score. Second, it evaluates whether strengthened enforcement influences voluntary disclosure behaviour, drawing on the logic of the Confirmation Hypothesis. Third, it assesses whether improved transparency and disclosure reduce information asymmetry, thereby supporting more efficient capital market outcomes.

2.4. Hypothesis Development

Building on the institutional context and conceptual gaps identified in the preceding sections, this study develops three hypotheses to evaluate how the strengthened whistleblowing enforcement under the DFA shapes corporate disclosure practices. Unlike prior regulations, the DFA's structure features financial incentives, external reporting channels, and formal protections that offer a distinct mechanism for deterring misreporting and improving disclosure integrity.

Prior research has documented that the DFA is associated with reductions in accounting fraud and improved governance outcomes (Berger & Lee, 2022; Wiedman & Zhu, 2023). However, these studies primarily rely on accrual-based proxies such as the F-score, which are sensitive to managerial discretion and may not fully capture the extent of reporting irregularities. To address this limitation, this study adopts the FSD Score, a parsimonious statistical measure based on Benford's Law, to provide a broader assessment of reporting anomalies beyond traditional accrual-based indicators (Amiram et al., 2015).

H1: The implementation of the Dodd-Frank Act (2011) positively impacts firm-year based mandatory disclosure quality.

Second, the Confirmation Hypothesis suggests that mandatory and voluntary disclosures function as mutual complements. As firms improve the credibility of audited

financials through strengthened enforcement (as posited in H1), they may face stronger incentives to issue voluntary disclosures that are timely, specific, and accurate, thereby enhancing the overall transparency of the information environment (Ball et al., 2012; Frankel et al., 2021; Gigler & Hemmer, 1998).

H2: The implementation of the Dodd-Frank Act (2011) positively impacts voluntary disclosure quality.

Third, transparency across both mandatory and voluntary channels can reduce investors' uncertainty, thereby lowering information asymmetry. If DFA enforcement increases credible disclosure, this should enhance market efficiency by improving the availability and reliability of firm-specific information. Prior studies suggest that greater disclosure reduces information asymmetry by lowering the cost of acquiring and interpreting firm-level data (Easley & O'Hara, 2004; Verrecchia, 1983). Furthermore, improved disclosure has been associated with narrower bid-ask spreads, greater analyst coverage, and enhanced capital allocation efficiency (Brown & Hillegeist, 2007; La Porta et al., 2006). Building on this premise, this study evaluates whether strengthened whistleblower enforcement under the DFA leads to measurable reductions in information asymmetry.

H3: Firms operating under the Dodd-Frank Act (2011) experience improvements in information asymmetry.

The following section describes the empirical approach used to test these hypotheses and evaluate the effect of the DFA on disclosure outcomes across mandatory, voluntary, and market-facing dimensions.

3. Methodology

This section outlines the empirical approach adopted to evaluate the impact of the DFA on corporate disclosure practices and the broader information environment. Section 3.1 describes the data sources and sample construction. Section 3.2 defines the key dependent variables used to capture three dimensions of disclosure: mandatory financial reporting quality, voluntary disclosure, and information asymmetry. Section 3.3 presents the empirical model specification, and Section 3.4 discusses the control variables included to account for firm-level characteristics.

3.1. Data and Sample Construction

This study examines how the DFA affects firms' disclosure practices and the broader information environment in the United States. The analysis draws on a comprehensive dataset of publicly listed US firms from 2007 to 2014, compiled from Compustat, CRSP, Refinitiv Eikon, and Datastream (IBES). Compustat provides detailed financial statement data used to construct mandatory disclosure quality measures. CRSP supplies historical stock return data. Forecast-related variables, including analyst and management earnings forecasts, are obtained from Refinitiv Eikon and IBES. The sample focuses on publicly listed firms because the DFA's whistleblower provision primarily targets violations of US federal securities laws enforced by the SEC.

Reflecting the legislative timeline outlined in Wiedman and Zhu (2023), the sample is divided into a four-year pre-DFA period (2007–2010) and a four-year post-DFA period (2011–2014) to facilitate balanced longitudinal comparison. The Whistleblower Program was formally established in July 2010, with rule details finalised in November 2010 and implementation effective from August 12, 2011. However, whistleblowers became eligible for rewards for information submitted on or after July 22, 2010. This periodisation captures firm behaviour both before and after the introduction of whistleblower incentives.

To mitigate the potential confounding effects arising from the Global Financial Crisis (GFC), which overlaps with the early sample period, firms in finance and utility sectors are excluded from the analysis. The adoption of a four-year window on either side of the DFA's implementation enables a balanced longitudinal comparison while capturing disclosure behaviour under distinct regulatory regimes. Earlier years (e.g., 2005–2006) are excluded to minimise residual effects from the post-SOX period, while later years are omitted to avoid contamination from subsequent regulatory changes. This sampling strategy is consistent with prior studies examining the effects of major U.S. disclosure reforms (Berger & Lee, 2022; Wiedman & Zhu, 2023). The design allows for a meaningful post-treatment assessment of the DFA's impact without excessively lengthening the observation horizon (see Figure 2).

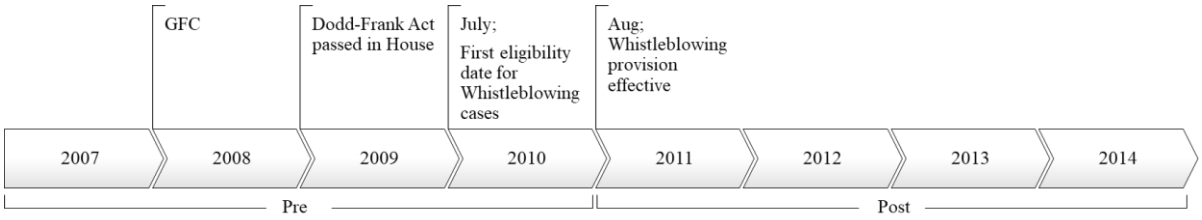


Figure 2. Legislative Timeline of the Dodd-Frank Whistleblower Program Implementation

Table 1 summarises the sample construction process and distribution of firm-year observations. Panel A outlines the stepwise exclusions from the initial sample of 30,524 firm-year observations from 6,608 firms with Compustat coverage. Firms operating in finance and utility sectors, those with missing values for the FSD Scores, and firms without valid observations in both the pre- and post- are excluded. After applying these criteria, the final sample comprises 10,115 firm-year observations across 1,958 unique firms. Panel B presents the annual distribution of observations, with 45.93 percent originating from the pre-DFA period and 54.07 percent from the post-DFA period, indicating a relatively balanced panel suitable for longitudinal comparison.

[Insert Table 1 here]

3.2. Dependent Variables

This study employs three categories of dependent variables to evaluate corporate disclosure behaviour from complementary perspectives: financial reporting quality (FRQ), voluntary disclosure practices, and the overall transparency of the information environment. Together, these dimensions offer a comprehensive framework for assessing how the DFA, as a regulatory intervention, has influenced firms' disclosure strategies across both mandatory and voluntary channels.

3.2.1. Financial Reporting Quality: FSD Score

Prior studies assessing the impact of the DFA on financial reporting quality (FRQ) have predominantly relied on accrual-based indicators, such as the F-score and M-score (Berger & Lee, 2022; Wiedman & Zhu, 2023). While widely used, these proxies are sensitive to managerial discretion and rely on estimation assumptions, which pose endogeneity concerns in regulatory settings. To overcome these limitations, this study adopts the FSD Score[5], a firm-year-based, model-free measure developed by (Amiram et al., 2015) to evaluate whether the DFA improved mandatory reporting quality (H1).

The FSD Score quantifies the extent to which a firm's financial statement numbers conform to Benford's Law, which describes the expected frequency distribution of first digits in naturally occurring datasets. Higher FSD Scores reflect greater divergence from the expected Benford's digit patterns and are interpreted as signals of potential reporting anomalies, regardless of managerial intent (Boyle et al., 2021).

The FSD Score does not differentiate between intentional manipulation and unintentional anomalies; it is designed to flag statistical irregularities that merit further investigation, rather than to establish managerial fraud per se. Recent studies highlight this limitation and caution against interpreting FSD divergence as direct evidence of managerial

[5] The conceptual and technical details of the FSD Score, including its derivation from Benford's Law and its statistical properties, are discussed in Appendix C.

misconduct (Beneish & Vorst, 2022; Kauko, 2024). Nonetheless, such deviations in numerical patterns may still reflect compromised reporting processes or weakened internal controls, which ultimately undermine financial statement integrity.

Importantly, unlike traditional financial reporting quality (FRQ) proxies such as the F-score or M-score, the FSD Score is not derived from accrual models or expectations linked to firm fundamentals. This model-free construction mitigates the risk of mechanical correlation when analysing its relationship with forward-looking voluntary disclosure variables, such as management earnings forecasts (MEFs) (Christensen et al., 2019; Dechow et al., 2010). By employing the FSD Score, this study offers a more granular and assumption-light evaluation of how strengthened whistleblower enforcement under the DFA affects firm-year reporting behaviour.

3.2.2. Voluntary Disclosure

To assess how the DFA influences firms' voluntary disclosure practices (H2), this study employs a set of variables derived from management earnings forecasts (MEFs), a central form of forward-looking corporate communication. MEFs have been shown to shape investor expectations, influence stock prices (Waymire, 1985), and improve analyst forecast accuracy (Baginski & Hassell, 1990). Compared to other voluntary disclosure channels such as conference calls, press releases, SEC filings, and MD&A sections, MEFs are relatively standardised and consistently observable, making them well-suited for large-sample empirical analysis (Ball et al., 2012).

This study examines the impact of the DFA on MEF behaviour by analysing five forecast-related variables. The first is an indicator variable, ***Forecaster***, which equals 1 if the firm issues at least one MEF during a given fiscal year *t*, and 0 otherwise. To capture disclosure characteristics more comprehensively, four additional continuous variables are constructed:

Number of Forecasts, ***Horizon***, ***Precision***, and ***Specificity***. These variables allow for a detailed assessment of the frequency, timeliness, and informativeness of voluntary disclosure.

(1) ***Number of forecasts*** measures the total count of annual and quarterly earnings per share (EPS) forecasts issued during a given fiscal year (Ball et al., 2012). It serves as a proxy for disclosure frequency, with higher values indicating more active managerial communication. This measure has been widely used to capture firms' forecast activity intensity (Francis et al., 1994; Johnson et al., 2001; Kasznik & Lev, 1995; Skinner, 1994, 1997).

(2) ***HORIZON*** is computed as the natural logarithm of one plus the average number of days between the forecast issuance date and the fiscal year-end (Ball et al., 2012). A larger horizon value suggests that managers are making forecasts further in advance of earnings announcements, indicating more timely and forward-looking disclosure (Rogers & Van Buskirk, 2009).

(3) ***PRECISION*** is coded on a four-point scale based on the format of the earnings forecast. Point estimates are assigned a value of 4, range forecasts a value of 3, open-ended estimates (e.g., "at least" or "no more than") a value of 2, and qualitative statements (e.g., "approximately breakeven") a value of 1. Higher values indicate greater informativeness and specificity (Armstrong et al., 2014; Rogers & Van Buskirk, 2009).

(4) ***SPECIFICITY*** is calculated as minus one times the average relative width of EPS forecast ranges, scaled by the firm's stock price in the month preceding the forecast date (Ball et al., 2012; Frankel et al., 2021). Point forecasts are assigned a value of 0. A narrower range (i.e., higher specificity) suggests greater forecast clarity and is associated with stronger investor responses (Baginski et al., 1993; Baginski & Hassell, 1997).

Together, these five MEF-based variables provide a structured and multidimensional lens for analysing firms' voluntary disclosure strategies. By capturing variation in disclosure frequency, timing, and content quality, the analysis enables a detailed evaluation of how firms

respond to strengthened regulatory enforcement under the DFA. All MEF-related variables are winsorised at the 1st and 99th percentiles to mitigate the influence of outliers. Detailed variable definitions and construction procedures are provided in Appendix A.

3.2.3. Information Asymmetry

To examine how the DFA influences firms' external information environment (H3), this study employs two widely used proxies for information asymmetry: the bid–ask spread and analyst coverage. These measures capture the perceived transparency or opacity of firm-specific information from the perspective of market participants and information intermediaries.

The first variable, ***Bid-Ask Spread***, is calculated as the average daily difference between the closing ask and bid prices divided by the midpoint over the fiscal year. A wider spread indicates greater uncertainty and lower market liquidity, reflecting higher levels of information asymmetry among investors (Diamond & Verrecchia, 1991).

The second variable, ***Analyst Coverage***, is measured as the natural logarithm of one plus the number of individual analysts issuing earnings forecasts for a given firm during the fiscal year (Barry & Brown, 1985; Botosan et al., 2004). Prior studies have shown that firms exhibiting greater transparency and voluntary disclosure tend to attract broader analyst coverage (Healy & Palepu, 2001; Lang & Lundholm, 1996). In this context, analyst coverage serves as an indirect measure of disclosure quality, capturing how internal improvements, particularly those driven by regulatory enforcement like the DFA, may spill over to the broader external information environment[6].

Together, these two measures jointly capture both internal (liquidity-based) and external (intermediary-based) dimensions of information asymmetry. Each variable is

[6] Although analyst coverage is provided by third-party financial intermediaries, it is widely regarded as a robust proxy for firm-level information availability. It reflects both market demand for, and accessibility of, public disclosure Healy, P. M., & Palepu, K. G. (2001).

winsorised at the 1st and 99th percentiles to mitigate the influence of outliers. Detailed variable definitions and construction procedures are provided in Appendix A.

3.3. Empirical Model

This study employs a pooled ordinary least squares (OLS) regression using panel data to estimate the effect of the DFA as a strengthened enforcement mechanism, on firms' information disclosure practices. Following the approach of Wiedman and Zhu (2023), the empirical model evaluates the overall influence of the DFA across three dimensions of corporate disclosure: (1) mandatory FRQ, (2) voluntary disclosure through MEFs, and (3) information asymmetry. By integrating these complementary dimensions, the analysis provides a comprehensive framework to assess how whistleblowing enforcement affects firms' disclosure behaviour under the strengthened regulatory landscape.

The primary specification is as follows:

$$Disclosure\ measures_{i,t} = \beta_1 * Post_t + Controls_{i,t} + IndustryFE + \varepsilon_{it} \quad \textbf{(1)}$$

where $Disclosure\ Measures_{i,t}$ denotes the dependent variables described in Section 3.2, which include: (H1) Mandatory FRQ: FSD Score, (H2) Voluntary Disclosure: Forecaster, Number of Forecast, Precision, Horizon, Specificity, and (H3) Information asymmetry: Bid-Ask Spread and Analyst Coverage. $\boldsymbol{Post_t}$ a binary treatment indicator equal to 1 if the fiscal year-end of firm *i* falls in or after 2011, and 0 otherwise. This binary specification is designed to capture the exogenous regulatory shift triggered by the implementation of the DFA.

To strengthen the credibility of the causal interpretation, several identification strategies are employed. First, industry fixed effects are included in the baseline specification to control for unobserved sector-level heterogeneity while firm fixed effects are introduced in robustness tests to account for time-invariant firm-specific characteristics. Second, standard errors are clustered at the firm level to address autocorrelation and unobserved firm-level shocks (Petersen, 2009).

To further address concerns that the estimated post-DFA effect may capture unrelated contemporaneous events in 2011, this study adopts a unified sub-sample analysis based on firms' voluntary disclosure behaviour. Specifically, the baseline regressions are re-estimated separately for firms that issued at least one MEF (VD = 1) and those that did not (VD = 0). If the estimated effect merely reflects general time trends or macroeconomic shocks, the results should be consistent across both sub-samples. However, systematically stronger effects among voluntary disclosers would suggest that regulatory enforcement interacts with firms' endogenous disclosure strategies, consistent with the Confirmation Hypothesis (Ball et al., 2012; Frankel et al., 2021).

The coefficient β_1 captures the average treatment effect of the DFA on firms' disclosure practices. The interpretation of β_1 depends on the nature of each outcome variable. For H1 (FSD Score), a negative coefficient indicates improved reporting quality. For H2 (voluntary disclosure measures), positive coefficients reflect enhanced frequency, clarity, and timeliness. For H3 (information asymmetry), lower bid–ask spreads and higher analyst coverage both indicate reduced asymmetry. These directional expectations guide the interpretation of results across the three dimensions.

Lastly, the model controls for firm-level characteristics that may confound the relationship between the DFA and disclosure outcomes. Control variables are drawn from established literature and include measures of firm size, profitability, liquidity, governance structure, and audit-related characteristics (Ahn, 2022; Amiram et al., 2015; Berger & Lee, 2022; Wiedman & Zhu, 2023; Windisch, 2020). Their inclusion aims to mitigate omitted variable bias and improve identification validity.

3.4. Control Variables

Building on this framework, the baseline regression includes a comprehensive set of control variables capturing firm-specific characteristics that may influence disclosure

behaviour under strengthened regulatory enforcement. Variable selection is guided by prior research (Amiram et al., 2015; Ball et al., 2012; Berger & Lee, 2022; Wiedman & Zhu, 2023) ensuring analytical robustness and comparability.

Firm size, measured as the natural logarithm of market value (Size), is included to account for the effect of scale on disclosure practices, as larger firms are subject to more intensive regulatory and market scrutiny (Correia, 2014). The Market-to-Book ratio proxies for growth opportunities, which may influence firms' disclosure incentives. Free Cash Flow reflects internal financing capacity and potential for earnings management, as suggested by Dechow et al. (2003). Cash holdings (Cash) are controlled for given their link to liquidity and disclosure behaviour (Du & Heo, 2022).

Ownership Stake and Dividends serve as indicators of profitability and capital distribution policies, which may influence transparency incentives and stakeholder monitoring. Audit-related controls include the natural logarithm of audit fees (Ln(Aud_Fee)), capturing risk-based audit pricing (Berger & Lee, 2022). A Big 4 auditor indicator (Big4) is included as a proxy for audit quality and verification credibility (Ball et al., 2012).

Institutional Ownership is included to reflect external governance pressure, particularly from investors with stronger preferences for transparency. The expansion of shareholder rights under the DFA (e.g., proxy access, say-on-pay votes) may further amplify this monitoring effect (Wiedman & Zhu, 2023). All control variables are winsorised at the 1st and 99th percentiles to reduce the influence of extreme outliers. Detailed variable definitions and data sources are provided in Appendix A.

4. Results

4.1. Descriptive Statistics

Table 2 presents the summary statistics for all variables used to assess the impact of the DFA on corporate disclosure practices. To minimise the influence of extreme values, all continuous variables are winsorised at the 1st and 99th percentiles.

The average value of FSD Score, the study's primary proxy for mandatory financial reporting quality, is 2.718 (standard deviation = 0.869). These values are consistent with the pre-percentage-scaled figures reported by Amiram et al. (2015), who documented a mean of 0.0296 and standard deviation of 0.0087.

The binary variable *Forecaster*, which captures the incidence of voluntary MEFs, shows that approximately 47.3% of firm-year observations involve at least one forecast. Among these firms, the average *Number of Forecasts* issued per year is 1.572, with a standard deviation of 0.555. The mean value of *Precision* is 2.047 (SD = 0.867), suggesting that firms more frequently issue open-ended or range-based forecasts rather than precise point estimates.

The average *Horizon*, measured as the natural logarithm of the number of days between forecast issuance and the fiscal year-end, is 5.43, reflecting relatively forward-looking disclosure behaviour. The average *Specificity* is -0.605 (SD = 1.079), consistent with prior studies. Since *Specificity* is defined only for forecasts containing numeric ranges or point estimates, the number of valid observations is lower (N = 2,974).

MEF-related variables (*Number of Forecasts, Precision, Horizon,* and *Specificity*) are calculated only for firm-year observations with at least one MEF and non-missing forecast characteristics. Overall, the descriptive statistics for these voluntary disclosure measures are broadly consistent with those reported in earlier studies (Ball et al., 2012; Frankel et al., 2021).

The mean value of the *Post* indicator is 0.541, indicating a relatively balanced distribution of observations across pre- and post-DFA periods. Control variables such as *Size*, *Audit Fees*, *Institutional Ownership*, and *Free Cash Flow* also exhibit values consistent with prior literature, supporting the representativeness of the sample.

[Insert Table 2 here]

4.2. Correlation Matrix

Table 3 presents the pairwise correlation coefficients among the variables used in the empirical analysis. As expected, the *Post* indicator (Row 10) is significantly correlated with several key dependent variables. Notably, *Post* is negatively correlated with *FSD Score* and *Bid-Ask Spread*, and positively correlated with *Forecaster* and *Analyst Coverage*. These correlations are consistent with the study's hypotheses and provide preliminary support for anticipated improvements in mandatory reporting quality, voluntary disclosure, and information transparency following the implementation of the DFA.

The correlation matrix also reveals that most control variables exhibit statistically significant relationships with the dependent variables. For instance, *Size, Audit Fees*, and *Institutional Ownership* are significantly correlated with both *FSD Score* and voluntary disclosure measures, consistent with prior literature.

Although moderate correlations are observed among some control variables (e.g., Size and Cash), none of the coefficients exceed conventional thresholds that would indicate serious multicollinearity concerns. This suggests that the empirical models are unlikely to suffer from estimation instability due to collinearity.

Overall, the observed correlations support the appropriateness of the variable construction and reinforce the internal validity of the empirical framework.

[Insert Table 3 here]

4.3. H1: Firm-Year Based Mandatory Reporting Quality and the DFA

Table 4 presents the baseline regression results evaluating the effect of the DFA on firm-year based mandatory reporting quality. The dependent variable is the FSD Score, which serves as a proxy for reporting quality. It measures the extent to which the distribution of first digits in financial statement items deviates from the expected Benford distribution.

[Insert Table 4 here]

Columns (1) and (2) report the estimated coefficients on the *Post* indicator under firm and industry fixed effects, respectively. In both models, the coefficient on *Post* is significantly negative (-0.065 in Column 1 and -0.056 in Column 2, $p < 0.01$), indicating a 6.5% and 5.6% reduction in FSD Scores following the implementation of the DFA. These findings support Hypothesis 1, which anticipates that strengthened whistleblowing enforcement enhances the quality of mandatory financial reporting and improves disclosure integrity (Allen et al., 2022; Beneish, 1999; Dechow et al., 2010).

To further test the robustness of these results, Columns (3) and (4) replicate the regression within sub-samples based on firms' voluntary disclosure behaviour (VD). Among firms that issued at least one MEF during the year (VD = 1), the reduction in FSD Scores is larger (-0.063 in Column 4, $p < 0.01$) than among non-disclosers (-0.058 in Column 3, $p < 0.05$). This asymmetric effect suggests that the enforcement impact is more pronounced among firms that are already inclined toward transparency, consistent with the Confirmation Hypothesis and the literature on disclosure complementarity (Ball et al., 2012; Frankel et al., 2021).

The coefficients on control variables also offer supporting evidence. For instance, lower audit fees (*LnAudit_Fee*) are associated with lower FSD Scores, consistent with reduced audit risk, while higher cash holdings are positively associated with reporting divergence, potentially reflecting investor scepticism toward excessive liquidity buffers. These patterns collectively

reinforce the interpretation that the DFA contributed to meaningful improvements in mandatory financial reporting quality across firms.

4.4. H2: Management Earnings Forecast and DFA

Table 5 explores how the DFA influences voluntary disclosure behaviour, particularly focusing on MEFs. The analysis considers five key dimensions: frequency, precision, timeliness, specificity, and decision to disclose.

[Insert Table 5 here]

Columns (1) and (2) report the impact of the DFA on the *Number of Forecasts*. The coefficient on *Post* is 0.051 ($p < 0.01$) under firm fixed effects and 0.041 ($p < 0.01$) under industry fixed effects, indicating that the frequency of MEFs increased by approximately 5.1% and 4.1%, respectively, following the implementation of the DFA. These results suggest that firms became more likely to communicate forward-looking information in response to enhanced regulatory scrutiny.

In contrast, Columns (3) and (4) reveal a decline in *Precision* (-0.058 and -0.063; $p < 0.05$). This suggests that although firms issued forecasts more frequently, they may have deliberately avoided overly precise numerical targets. Instead, firms appear to favour ambiguous language such as qualitative ranges or open-ended estimates, to maintain flexibility under heightened enforcement. This is consistent with prior literature suggesting that precision may be strategically reduced to manage litigation risk or manage investor expectations (Baginski et al., 2002; Ball et al., 2012; Skinner, 1994).

Despite this decline in precision, Columns (5) and (6) show a significant increase in *Horizon* (0.049 and 0.035; $p < 0.01$), suggesting more timely forward-looking disclosure. *Specificity* also improves modestly, with coefficients of 0.044 ($p < 0.10$) and 0.076 ($p < 0.05$) in Columns (7) and (8), suggesting improvements in forecast clarity, as firms provided more informative ranges (Baginski et al., 1993; Baginski & Hassell).

However, Columns (9) and (10) reveal mixed effects on *Forecaster*, which captures the decision to issue any MEF. The coefficient is statistically insignificant under firm fixed effects but turns negative and significant (-0.015; $p < 0.05$) under industry fixed effects[7]. This modest decline may reflect firms' increased caution in initiating voluntary disclosures due to perceived compliance burdens or reputational risks that vary by industries (Healy & Palepu, 2001; Verrecchia, 1983).

Taken together, these results provide partial support for Hypothesis 2. The DFA appears to have encouraged firms to issue more timely and informative forecasts. However, the observed decline in disclosure incidence in some specifications highlights a strategic trade-off: while disclosure quality may improve, some firms may limit their voluntary disclosure activity altogether. These dynamics complement the findings in Section 4.3, suggesting that although strong enforcement can enhance both mandatory and voluntary disclosure, the net effects are shaped by firms' strategic cost–benefit considerations[8].

4.5. H3: Information Asymmetry and DFA

This section evaluates whether strengthened regulatory enforcement via the DFA reduced information asymmetry, using two well-established market-based proxies: Bid-Ask Spread and Analyst Coverage. Table 6 presents the regression results across the full sample and subsamples based on voluntary disclosure activity.

[Insert Table 6 here]

Columns (1) and (2) show that the coefficient on Post is negative and statistically significant (−0.031 and −0.023; $p < 0.01$), indicating that bid–ask spreads declined following

[7] A possible explanation for the negative effect in Column 10 is that firms facing increased enforcement may adopt a more cautious stance toward initiating any form of voluntary disclosure. Prior studies note that regulatory reforms can raise compliance costs or increase perceived litigation exposure, leading firms to delay or withhold initial disclosure decisions despite improving the quality of forecasts once issued Healy, P. M., & Palepu, K. G. (2001).

[8] These mixed findings are common in disclosure literature, where firms' strategic considerations under uncertainty or enforcement vary depending on firm size, industry sensitivity, or prior disclosure norms. The observed trade-off between increased quality and reduced incidence may reflect the tension between transparency incentives and risk aversion under regulatory scrutiny Ball, R., Jayaraman, S., & Shivakumar, L. (2012), Frankel, R. M., Kalay, A., Sadka, G., & Zou, Y. (2021).

the implementation of the DFA. This suggests an improvement in market liquidity and a reduction in asymmetric information among investors (Brown & Hillegeist, 2007; Diamond & Verrecchia, 1991).

Similarly, Columns (5) and (6) report a significant increase in analyst coverage post-DFA (0.095 and 0.075; $p < 0.01$). As analyst following is influenced by the availability and reliability of firm-level public information, this result supports the idea that regulatory interventions can enhance the broader information environment (Healy & Palepu, 2001; Lang & Lundholm, 1996; Yu, 2008).

To test for heterogeneity in the treatment effect, Columns (3) and (4) re-estimate the model for Bid-Ask Spread, while Columns (7) and (8) focus on Analyst Coverage, using subsamples split by firms' voluntary disclosure behaviour (VD). Firms that issued at least one management earnings forecast (VD = 1) experienced a more pronounced decline in bid–ask spreads (−0.024 in Column 4; $p < 0.01$) and a larger increase in analyst coverage (0.077 in Column 8; $p < 0.01$). These findings align with prior research indicating that voluntary disclosure enhances the effectiveness of regulatory mechanisms in reducing information asymmetry (Balakrishnan et al., 2014; Frankel et al., 1995).

Overall, these results offer strong support for Hypothesis 3. The DFA not only improved internal reporting quality and voluntary disclosure but also strengthened external information channels such as market liquidity and analyst following. The amplified effects among firms with existing voluntary disclosure practices further suggest a complementarity between internal transparency and market-based monitoring. These findings reinforce the view that regulatory enforcement can meaningfully reduce information asymmetry by improving both the quality and accessibility of firm-level information (Ball et al., 2012; Lang & Lundholm, 1996).

4.6. Robustness Tests

To reinforce the internal validity and generalisability of the baseline findings, this section presents a series of robustness tests addressing concerns related to proxy sensitivity, sample selection, and firm heterogeneity. Specifically, four supplementary analyses are conducted: the use of alternative proxies for reporting quality and uncertainty; application of reweighting and matching methods to address selection bias; examination of variation across high-litigation industries; and implementation of a difference-in-differences approach based on regulatory exposure. These tests verify whether the observed effects of the DFA persist under alternative assumptions and empirical strategies.

4.6.1. Alternative measures and DFA

This subsection further validates the baseline results using alternative proxies for financial reporting quality and firm-level uncertainty. Panel A of Table 7 employs established accrual-based metrics, F-Score and M-Score, to assess whether the observed improvements in reporting quality post-DFA hold across conventional fraud detection measures. Panel B evaluates whether these improvements might instead reflect reduced uncertainty, using analyst forecast accuracy and return volatility as proxies.

[Insert Table 7 here]

Accrual-Based Financial Reporting Quality Measures

Panel A presents regression results using two widely used accrual-based indicators of earnings manipulation. The F-Score, developed by Dechow et al. (2010), estimates the likelihood of financial statement fraud using financial ratios, while the M-Score, introduced by Beneish (1999), detects potential misstatements from abnormal financial patterns. Both have been extensively employed as robust proxies for reporting anomalies and fraud risk (Berger & Lee, 2022; Wiedman & Zhu, 2023).

Columns 1-4 report the effect of the DFA on F-Score, while Columns 5-8 present analogous regression results for M-Score. Across both measures, the *Post* coefficients are significantly negative in all models with either firm fixed effects (Columns 1 and 5) or industry

fixed effects (Columns 2 and 6), indicating a consistent reduction in manipulation risk following the implementation of the DFA. These findings reinforce the baseline results in Section 4.3 and confirm that improvements in reporting quality are not driven solely by the FSD Score.

Subsample regressions based on voluntary disclosure status (VD = 0 vs. VD = 1) show a modest asymmetry. The reductions in F-Score (−0.060 vs. −0.053) and M-Score (−0.031 vs. −0.019) are slightly larger for firms that did not issue earnings forecasts. This suggests that even firms not engaging in MEFs responded to regulatory pressure by enhancing their mandatory reporting quality.

Interestingly, this pattern contrasts with the baseline results in Section 4.3, where voluntary disclosers exhibited greater improvements in FSD Scores. One possible interpretation is that firms may reallocate their transparency efforts between channels by substituting voluntary disclosures with higher-quality financial reporting when faced with regulatory pressure and heightened litigation risk. This shift is also consistent with the observed decline in forecast precision in Section 4.4, suggesting a deliberate restraint in voluntary disclosures despite broader improvements in overall reporting integrity.

Earnings Uncertainties During the DFA

Panel B investigates whether the improvements in disclosure quality could alternatively be attributed to reductions in firm-level uncertainty. Two commonly used measures are examined: Analyst Forecast Accuracy and Return Volatility, which respectively capture the clarity of information and market-based noise (Chen et al., 2015; Frankel et al., 2021).

Columns 1-4 examine changes in forecast accuracy, while Columns 5 to 8 examine return volatility. Across both measures, the results show that analyst forecast accuracy improves significantly post-DFA, while return volatility declines. These outcomes suggest that

stronger enforcement mechanisms reduced uncertainty and enhanced the forecasting environment (Dhaliwal et al., 2012; Hope, 2003).

Interestingly, both effects are more pronounced among firms that did not issue voluntary disclosures (VD = 0), echoing the asymmetric pattern observed in Panel A. This finding reinforces the interpretation that even firms refraining from MEFs nonetheless improved the quality of mandatory disclosure in response to enforcement pressure. However, this also implies a potential trade-off in that firms withholding forward-looking guidance may sacrifice voluntary-channel informativeness, despite improvements in overall transparency through mandatory reports.

Taken together, the results from both panels underscore a complex interplay between enforcement, disclosure strategy, and uncertainty. The consistency of findings across alternative proxies for fraud and uncertainty bolsters the robustness of the baseline results and affirms the study's core conclusion that the DFA significantly improved corporate disclosure quality.

4.6.2. Entropy Balancing and Propensity Score Matching

To reinforce the causal interpretation of the baseline findings, this study implements two widely recognised reweighting and matching approaches: entropy balancing and propensity score matching (PSM). Both methods aim to mitigate potential bias arising from systematic differences in firm characteristics between the pre- and post-DFA periods, which may otherwise confound treatment effect estimation (Ball et al., 2000; Christensen et al., 2013).

Entropy Balancing

Following McMullin and Schonberger (2020), entropy balancing weights are constructed to align the covariates of treatment (post-DFA) and control (pre-DFA) groups in terms of their mean, variance, and skewness. Panels A and B of Table 8 confirm successful balancing across key firm-level variables. Panel C reports regression results using the balanced

sample, which remain directionally and statistically consistent with those of the baseline models.

[Insert Table 8 here]

Specifically, the Post coefficient remains significantly negative for FSD Score (−0.063, $p < 0.01$), and significantly positive for Number of Forecasts (0.051, $p < 0.01$), Horizon (0.047, $p < 0.01$), and Analyst Forecast Accuracy (0.097, $p < 0.01$). These results reaffirm that the DFA improved mandatory financial reporting quality, encouraged voluntary disclosure, and enhanced the external information environment. Marginal effects such as the negative coefficient on Precision (−0.062, $p < 0.01$) also persist. While some effects (e.g., Forecast Specificity) become statistically insignificant, their directionality remains. For market-based measures, Bid-Ask Spread continues to decline significantly (−0.029, $p < 0.01$), while Analyst Forecast Accuracy becomes marginally stronger after balancing, likely reflecting increased noise from a greater number of firms issuing less precise guidance post-DFA (see Section 4.4). Overall, these results indicate that the main empirical patterns are robust to concerns about imbalanced sample characteristics.

Propensity Score Matching (PSM)

As a complementary robustness test, PSM is applied to further address potential selection bias. The method generates a matched sample of pre- and post-DFA firms with similar observable characteristics, thereby providing a more credible counterfactual for causal inference (Bonsall et al., 2020; Nagar et al., 2019; Shang et al., 2021). Propensity scores are estimated using key firm-level covariates, and 1:1 nearest-neighbour matching is performed with a calliper of 0.01.

Appendix B presents the matching diagnostics and regression results. After matching, the balanced sample consists of 6,678 firm-year observations. The Post coefficient remains significant and directionally consistent with both the unweighted and entropy-balanced

estimates, with a significant decline in FSD Score (−0.066, $p < 0.01$) and significant increases in Number of Forecasts (0.050, $p < 0.01$), Horizon (0.050, $p < 0.01$), and Analyst Forecast Accuracy (0.097, $p < 0.01$). The Bid-Ask Spread remains negative and significant (−0.032, $p < 0.01$), further suggesting improved transparency.

The close convergence of results across entropy balancing and PSM supports the robustness of the baseline findings. Improvements in reporting quality, voluntary disclosure, and the information environment are not attributable to sample selection bias or firm heterogeneity. Collectively, these findings confirm that the core effects are attributable to the strengthened enforcement provisions of the DFA.

4.6.3. High litigation industry

Firms operating in high-litigation industries often face greater pressure to comply with disclosure regulations due to the heightened legal and reputational costs associated with misstatements (Ball et al., 2012). Prior research suggests that these firms are more inclined to issue voluntary disclosures (particularly forward-looking statements) as a strategy to mitigate litigation risk and pre-empt regulatory scrutiny (Field et al., 2005; Skinner, 1994). Voluntary disclosure behaviour is frequently shaped by perceived legal vulnerability and the firm's incentives to manage transparency under heightened scrutiny (Frankel et al., 2021; Healy & Palepu, 2001).

Building on this premise, this robustness test examines whether voluntary disclosure responses to the DFA differ across litigation environments. We focus exclusively on voluntary disclosure outcomes, rather than mandatory reporting quality, as the strategic use of forecasts is more likely to vary with legal exposure. High-litigation industries are identified following established classifications, including biotechnology, computing, electronics, retail, and other sectors with elevated litigation risk, defined by SIC codes (2833–2836, 3570–3577, 3600–3674, 5200–5961, 7370–7374, 8731–8734) (Ball et al., 2012; Francis et al., 1994; Rogers & Van Buskirk, 2009).

[Insert Table 9 here]

To assess differential effects, we estimate interaction models between the Post indicator and a High Litigious Industry dummy. Among the five MEF-based disclosure measures, only Number of Forecasts and Horizon show statistically significant interaction terms. Thus, Table 9 presents results only for these outcomes, with findings for Precision, Specificity, and Forecaster omitted for brevity due to statistical insignificance.

Column (1) shows that firms in high-litigation industries issued fewer forecasts post-DFA relative to others (interaction coefficient = –0.060, $p < 0.05$), suggesting a more cautious disclosure posture aimed at avoiding litigation associated with forecast inaccuracy or misinterpretation. In contrast, Column (2) indicates that these firms increased the timeliness of their forecasts (interaction coefficient = 0.033, $p < 0.10$), releasing guidance approximately 3.3 days earlier than before. This pattern suggests that firms accelerated the timing of their disclosures to signal compliance and reduce perceived information opacity, while simultaneously limiting the frequency of forecasts.

Overall, the findings suggest that firms in high-litigation industries respond to regulatory enforcement not by uniformly expanding disclosure, but by strategically adjusting both its volume and timing. These behavioural shifts are consistent with the notion that voluntary disclosure decisions are shaped by complex cost-benefit trade-offs, particularly under overlapping legal and regulatory pressures.

4.6.4. Difference-in-Differences (DiD) Test

A central challenge in regulatory impact studies is isolating the effect of a given policy intervention from other contemporaneous regulatory or economic influences. This issue is widely acknowledged in the empirical programme evaluation literature (Angrist & Jörn-Steffen, 2008; Bertrand et al., 2004; Wooldridge & Imbens, 2009). While it is not possible to eliminate all sources of confounding in observational settings, well-designed Difference-in-Differences

(DiD) models offer a robust quasi-experimental framework to mitigate these identification concerns.

This study implements DiD analyses using the FSD Score as the dependent variable and leverages two distinct sources of variation in firms' exposure to whistleblower-related enforcement, as identified in prior literature (Berger & Lee, 2022; Wiedman & Zhu, 2023). The first source relates to firms' prior exposure to whistleblower frameworks through the FCA, while the second involves firms' regulatory exemption status under the DFA. By comparing changes in financial reporting quality across treatment and control groups before and after the DFA, these models construct credible counterfactuals.

[Insert Table 10 here]

In Columns (1) and (2) of Table 10, FCA[9] firms are defined as the treatment group, while non-FCA firms serve as the control. This distinction is conceptually appropriate, as the FCA historically provided strong incentives and legal risk exposure for whistleblowers, creating a natural comparison for evaluating the incremental effects of the DFA. The interaction term **False Claims Act × Post** is used to identify the treatment effect, and only this interaction is included when firm fixed effects are present, as the group and time dummies are absorbed by these controls (Bertrand et al., 2004).

The estimated coefficients for False Claims Act × Post are –0.065 before matching (Column 1) and –0.064 after entropy balancing (Column 2), both significant at the 1% level. These negative coefficients indicate a significant reduction in the FSD Score for FCA firms

[9] The False Claims Act (FCA) is a United States federal law that imposes liability on individuals and organisations defrauding governmental programmes. Notably, the FCA includes a qui tam provision, which allows private individuals (whistleblowers) to file actions on behalf of the government and receive a share (typically 15%–25%) of any recovered damages as a reward—often referred to as a "bounty" Bowen, R. M., Call, A. C., & Rajgopal, S. (2010). Bucy, P. H., Diesenhaus, J., Raspanti, M. S., Chestnut, H., Merrell, K., & Vacarella, C. (2010). Both federal and state FCA statutes focus primarily on healthcare cases. Prior research shows that whistleblower provisions under the FCA are particularly effective in fraud detection within the healthcare industry, where employees identified fraud in 41% of cases involving bounty incentives, compared to 17% in the broader sample Dyck, A., Morse, A., & Zingales, L. (2010). However, evidence also suggests that employee whistleblowers frequently experience retaliation (reported in 82% of such cases), highlighting the importance of expanded whistleblower protections implemented under the DFA Whistleblower Program.

after the DFA, implying an improvement in financial reporting quality attributable to strengthened whistleblower incentives and prior experience with Whistleblowing law. The robustness of these results across matching approaches further reinforces this finding. These findings suggest that firms with prior exposure to whistleblower incentives and legal frameworks are better positioned to adapt to regulatory changes, emphasising the value of organisational learning and compliance infrastructure in maximising policy effectiveness.

Columns 3 and 4 extend the DiD analysis to Smaller Reporting Companies (SRC[10]) firms, following Wiedman and Zhu (2023). The treatment effect is captured by **Small Reporting Companies × Post**. The estimated coefficients are 0.117 ($p < 0.10$) before matching (Column 3) and 0.200 ($p < 0.05$) after entropy balancing (Column 4), indicating a statistically significant increase in the FSD Score for SRC firms following the DFA. Unlike other DFA provisions from which SRC firms typically enjoyed exemptions or delayed implementations, the Whistleblower Program was immediately effective for these companies. Thus, SRC firms experienced a sudden and pronounced regulatory shock, providing a unique opportunity to assess the isolated impact of enhanced whistleblower protections on smaller firms.

Year fixed effects further ensure that the estimated treatment effect specifically captures the Whistleblower Program's impact, net of broader temporal trends (Bertrand et al., 2004). The positive and significant coefficients for SRC firms suggest a relative deterioration in mandatory reporting quality, likely reflecting SRC firms' limited managerial and financial resources to rapidly adapt to heightened compliance requirements, consistent with prior evidence that smaller firms disproportionately bear regulatory costs (Dechow, 2010; Wiedman,

[10] Smaller Reporting Companies (SRC) are firms defined by the U.S. Securities and Exchange Commission (SEC) as those with a public float of less than USD 75 million, determined annually and disclosed in the firm's annual report. During the period studied, SRCs were generally exempt from, or subject to a two-year deferral for, most provisions of the Dodd-Frank Act, including rules on executive compensation and disclosure of leadership structure. However, the Whistleblower Program did not include any exemption or deferral for SRCs, ensuring these firms were fully impacted by this component of the Act. This institutional setting allows for a cleaner identification of the Whistleblower Program's effect on reporting quality for SRCs, as other components of the Dodd-Frank Act were less likely to confound the results for this group (Wiedman, C., & Zhu, C. (2023).

2023). Additionally, smaller firms' reduced capacity to manage increased litigation risks or compliance complexities arising from whistleblower incentives may have unintentionally encouraged more cautious or defensive reporting behaviours.

Overall, the DiD results in Table 10 confirm that the impact of the DFA whistleblower provisions varies across firm types. Specifically, regulatory enforcement leads to improvements in reporting quality for firms with prior exposure to whistleblower incentives (FCA firms), whereas smaller firms (SRCs) experience a relative decline, likely due to greater resource constraints and compliance challenges.

By exploiting exogenous policy variation and constructing credible counterfactuals, the DiD models provide robust causal evidence that not only supports but also extends the baseline results. Collectively, these findings strengthen the internal validity and generalisability of the study's conclusions, and highlight the need for policymakers to consider firm heterogeneity and capacity when designing and implementing regulatory enforcement mechanisms (Berger & Lee, 2022; Wiedman & Zhu, 2023).

5. Conclusion

This study investigates the effects of the DFA on corporate disclosure practices, focusing on both mandatory and voluntary dimensions. Recognising their complementary relationship, we posit and empirically demonstrate that strengthened enforcement via the DFA whistleblowing provision significantly improves mandatory financial reporting quality and voluntary disclosure, while reducing information asymmetry. Firms that actively engage in voluntary disclosure also exhibit improved financial reporting quality and reduced financial misconduct, underscoring the interdependence between disclosure channels. Moreover, our findings show that the DFA incentivises timelier and more detailed voluntary disclosures, contributing to a more transparent information environment.

The robustness and credibility of these findings are supported by a comprehensive set of sensitivity and causal identification analyses. In addition to robustness tests using alternative financial reporting quality measures, we apply entropy balancing, PSM, and industry-specific analyses focused on high-litigation-risk sectors. We also implement a DiD framework to strengthen causal inference. The convergence of results across these empirical strategies provides strong evidence for the reliability and validity of our conclusions.

This study makes several key contributions to the literature and practice. First, it situates the DFA's whistleblower provisions within their historical and institutional context, clarifying their regulatory significance. Second, it addresses a gap in the literature by examining the impact of the DFA on firm-year-based mandatory disclosure quality measures, an area previously underexplored (Berger & Lee, 2022; Huang et al., 2023; Wiedman & Zhu, 2023). Third, by jointly analysing mandatory and voluntary disclosures, this study highlights their interconnectedness in shaping firm-level disclosure strategy and information transparency (Ball et al., 2012; Frankel et al., 2021).

Through rigorous empirical analysis and hypothesis testing, this chapter provides nuanced insights into the effectiveness of regulatory enforcement, while also revealing its potential unintended consequences. In particular, the evidence of strategic adjustments in disclosure, such as reduced forecast precision or selective disclosure timing, suggests that firms may respond to heightened enforcement not only by enhancing transparency, but also by managing perceived risks through more cautious communication. These findings offer important implications for policymakers, regulators, and corporate stakeholders, underscoring the role of strengthened enforcement in promoting transparency, investor confidence, and ethical governance, while highlighting the complex behavioural trade-offs that such policies may induce.

This study also acknowledges several limitations. First, the inherent difficulties in accurately measuring disclosure quality highlights the need for continued exploration of alternative proxies to validate the findings. Second, the study's specific focus on the DFA may not fully capture the broader institutional dynamics of regulatory enforcement or the variation in corporate responses across different legal and economic environments. Lastly, while disclosure quality appears to improve during the DFA period, it remains difficult to disentangle whether such changes reflect proactive transparency, defensive compliance, or a combination of both. As a result, the positive and negative consequences of enforcement cannot be fully separated. In particular, it is unclear whether these changes ultimately benefit firms by fostering greater stakeholder trust, or whether they simply reflect cost-driven compliance without generating substantive organisational improvements.

Table 1 Sample Selection Process and Sample Distribution

This table presents the sample selection process and the distribution of firm-year observations across time. Panel A outlines the stepwise criteria used to construct the final sample, which includes 10,115 firm-year observations from 1,958 unique firms spanning the pre-DFA period (2007–2010) and post-DFA period (2011–2014). Panel B reports the annual distribution of these observations, highlighting the dispersion across the sample years.

Panel A: Sample selection process

	Firm-years	Firms
All firm-years on annual Compustat data between 2007 and 2014	30,524	6,608
Less: Stringent Industries (Finance and Utilities)	6,911	1,431
Less: Missing values of FSD Score between 2007 and 2014	2,570	1,680
Less: firms without at least one firm-year observation in the pre or post-periods	10,928	1,539
Final sample:	**10,115**	**1,958**

Panel B: Sample distribution by year

Year	Frequency	Percent
Pre:		**45.93%**
2007	969	9.58%
2008	1,088	10.76%
2009	1,236	12.22%
2010	1,353	13.38%
Post:		**54.07%**
2011	1,492	14.75%
2012	1,493	14.76%
2013	1,269	12.55%
2014	1,215	12.01%
Total	**10,115**	**100.00%**

Table 2 Descriptive Statistics

This table presents the summary statistics for all variables used in the analysis. Column (1) reports the number of observations, followed by the mean, standard deviation, first quartile, median, and third quartile in Columns (2) to (6), respectively. All continuous firm-level variables are winsorised at the 1st and 99th percentiles. Variable definitions are provided in Appendix A.

	N	Mean	SD	p25	Median	p75
FSD Score (%)	10,115	2.718	0.869	2.092	2.605	3.212
Forecaster	10,115	0.473	0.499	0.000	0.000	1.000
Number of Forecasts	4,786	1.572	0.555	1.099	1.609	1.946
Precision	4,786	2.047	0.867	1.000	2.200	3.000
Specificity	2,974	-0.605	1.079	-0.607	-0.291	-0.131
Horizon	4,786	5.430	0.327	5.296	5.467	5.605
Bid-Ask Spread	10,032	0.122	0.347	0.022	0.040	0.087
Analyst Coverage	9,714	2.114	0.772	1.610	2.080	2.708
Post	10,115	0.541	0.498	0.000	1.000	1.000
Ln(Audit Fee)	10,115	13.979	1.112	13.244	13.916	14.646
Ownership Stake	10,115	0.027	0.062	0.000	0.001	0.027
Dividends	10,115	0.001	0.016	0.000	0.000	0.000
Size	10,115	1.562	1.676	0.659	1.122	1.852
Big4 Auditor	10,115	0.792	0.406	1.000	1.000	1.000
Institutional Ownership (%)	10,115	74.437	26.537	59.472	81.165	93.939
Free Cash Flow	10,115	0.016	0.181	-0.007	0.049	0.097
Cash	10,115	0.158	0.161	0.045	0.109	0.213
Market to Book Ratio	10,115	4.833	14.043	0.361	1.005	3.130

Table 3 Correlation Matrix

This table reports the correlation matrix showing the pairwise correlation coefficients among the variables.***, **, and * denote levels of statistical significance at 1%, 5%, and 10% respectively. All variables are defined in Appendix A.

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
(1) FSD Score (%)	1												
(2) F-Score	-0.025**	1											
(3) M-Score	0.008	-0.018	1										
(4) Forecaster	-0.189***	0.020*	0.009	1									
(5) Number of Forecasts	-0.098***	-0.024	0.049***	-	1								
(6) Precision	0.042***	-0.031*	0.032**	-	0.419***	1							
(7) Specificity	-0.032*	-0.035	0.065***	-	0.140***	0.011	1						
(8) Horizon	-0.001	-0.039**	-0.005	-	0.074***	0.02	-0.014	1					
(9) Bid-Ask Spread	0.109***	0.014	-0.018*	-0.158***	-0.148***	-0.058***	-0.123***	-0.028*	1				
(10) Post	-0.030***	-0.020*	0.040***	-0.018*	0.048***	-0.027*	0.062***	0.056***	-0.034***	1			
(11) Ln(Audit Fee)	-0.336***	0.024**	-0.013	0.315***	0.264***	-0.028*	0.144***	0.083***	-0.273***	-0.002	1		
(12) Ownership Stake	-0.060***	-0.021*	-0.016	0.142***	0.067***	0.078***	0.138***	0.044***	-0.064***	0.004	0.082***	1	
(13) Dividends	0.026***	-0.003	0.017*	-0.048***	-0.030**	-0.034**	-0.103***	0.003	0.057***	0.007	-0.081***	-0.011	1
(14) Size	0.173***	-0.037***	0.033***	-0.042***	0.037***	0.117***	0.187***	0.023	-0.067***	0.027***	-0.200***	0.159***	0.144***
(15) Big4 Auditor	-0.130***	-0.006	-0.018*	0.223***	0.131***	0.018	0.080***	0.02	-0.241***	-0.036***	0.473***	0.088***	-0.078***
(16) Institutional Ownership (%)	-0.214***	-0.001	0.033***	0.313***	0.164***	0.120***	0.174***	0.053***	-0.333***	0.01	0.409***	0.171***	-0.097***
(17) Free Cash Flow	-0.295***	0.007	-0.044***	0.241***	0.123***	0.069***	0.186***	0.036**	-0.095***	-0.032***	0.257***	0.220***	-0.160***
(18) Cash	0.252***	-0.040***	-0.030***	-0.154***	-0.123***	0.021	-0.02	0.003	0.037***	-0.033***	-0.242***	0.038***	0.081***
(19) Market to Book Ratio	-0.078***	-0.022*	0.009	0.099***	0.165***	-0.009	0.103***	0.053***	-0.078***	0.025**	0.402***	0.062***	-0.016*

Variables	(14)	(15)	(16)	(17)	(18)	(19)
(14) Size	1					
(15) Big4 Auditor	-0.070***	1				
(16) Institutional Ownership (%)	0.002	0.407***	1			
(17) Free Cash Flow	-0.168***	0.115***	0.268***	1		
(18) Cash	0.377***	-0.071***	-0.113***	-0.269***	1	
(19) Market to Book Ratio	0.106***	0.128***	0.002	0.083***	-0.051***	1

Table 4 H1: Firm-year mandatory reporting quality and DFA

This table reports the results of regressions examining the effect of the DFA on mandatory reporting quality, proxied by the FSD Score (%). The analysis uses firm-year observations from 2007 to 2014 with complete data for all variables. The main independent variable, Post, is a binary indicator equal to 1 if a firm's fiscal year-end is in 2011 or later, and 0 otherwise, capturing the enforcement shock introduced by the DFA. Pooled ordinary least squares (OLS) regressions are estimated with standard errors clustered at the firm level. Column (1) includes firm fixed effects, while Columns (2) to (4) incorporate industry fixed effects based on the Fama-French 12 classification. Columns (3) and (4) report sub-sample regressions based on firms' voluntary disclosure behaviour. Column (3) restricts the sample to firms that did not issue management earnings forecasts in a given year (VD = 0), whereas Column (4) includes only firms that issued at least one forecast (VD = 1). This sub-sample approach enables a comparison of the DFA's effect on mandatory reporting quality conditional on voluntary disclosure behaviour. Fixed effect coefficients are omitted for brevity. All regressions include a constant term. Statistical significance is denoted by ***, *, and * for the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

VARIABLES		FSD Score %			
		(1)	(2)	(3)	(4)
		Firm FE	Industry FE	Industry FE	
	Pred			VD =0	VD =1
Post	-	-0.065***	-0.056***	-0.058**	-0.063***
		(0.017)	(0.017)	(0.024)	(0.022)
Ln(Audit_Fee)	+	-0.012	-0.191***	-0.205***	-0.152***
		(0.032)	(0.012)	(0.017)	(0.016)
Ownership Stake	-	-0.087	-0.089	-0.376	0.107
		(0.159)	(0.142)	(0.228)	(0.190)
Dividends	+	-1.596**	-2.421***	-2.278***	-3.465*
		(0.761)	(0.426)	(0.466)	(2.048)
Size	+	0.014	0.026***	0.017*	0.052***
		(0.010)	(0.008)	(0.009)	(0.017)
Big4 Auditor	+	0.060	0.085***	0.134***	-0.001
		(0.062)	(0.030)	(0.038)	(0.046)
Institutional Ownership	+	-0.001	-0.002***	-0.002***	-0.002**
		(0.001)	(0.000)	(0.001)	(0.001)
Free Cash Flow	-	-0.285***	-0.780***	-0.705***	-0.850***
		(0.103)	(0.074)	(0.079)	(0.309)
Cash	-	0.403***	0.655***	0.655***	0.496***
		(0.121)	(0.076)	(0.097)	(0.122)
Market to Book Ratio	-	0.001	0.002**	0.003**	0.000
		(0.001)	(0.001)	(0.001)	(0.001)
Constant		2.842***	5.397***	5.601***	4.813***
		(0.461)	(0.162)	(0.224)	(0.228)
Observations		10,099	10,115	5,329	4,786
Adjusted R-squared		0.310	0.193	0.211	0.094
Firm FE		Yes	No	No	No
Industry FE		No	Yes	Yes	Yes

Table 5 H2: Voluntary Disclosure and DFA

This table presents the results from pooled OLS regressions estimating the effect of the DFA on various voluntary disclosure characteristics. The analysis is based on firm-year observations from 2007 to 2014 with complete data on all relevant variables. The main independent variable, Post, is a treatment indicator equal to 1 for firm-years ending in or after 2011, and 0 otherwise. The five voluntary disclosure measures used as dependent variables are: Number of Forecasts (Columns 1–2), Precision (Columns 3–4), Horizon (Columns 5–6), Specificity (Columns 7–8), and Forecaster (Columns 9–10), the last of which is a binary indicator equal to 1 if a firm issued at least one management earnings forecast in a given year. Odd-numbered columns include firm fixed effects, while even-numbered columns include industry fixed effects based on the Fama-French 12 industry classification. All regressions include a constant term. Robust standard errors are reported in parentheses and are clustered at the establishment level. Statistical significance is denoted by ***, *, and * at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

VARIABLES		Number of Forecasts		Precision		Horizon		Specificity		Forecaster	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Pred	Firm FE	Industry FE	Firm FE	Industry FE	Firm FE	Industry FE	Firm FE	Industry FE	Firm FE	Industry FE
Post	+	0.051***	0.041***	-0.058**	-0.063**	0.049***	0.035***	0.044*	0.074**	-0.003	-0.015**
		(0.014)	(0.015)	(0.023)	(0.025)	(0.010)	(0.009)	(0.023)	(0.036)	(0.006)	(0.008)
Ln(Audit_Fee)	-	-0.088***	0.115***	0.003	-0.029	-0.005	0.027***	-0.080	0.127***	-0.015	0.083***
		(0.029)	(0.017)	(0.050)	(0.026)	(0.022)	(0.007)	(0.054)	(0.037)	(0.012)	(0.011)
Ownership Stake	+	0.118	0.126	0.510**	0.403*	0.082	0.143**	0.588***	0.641**	0.186***	0.456***
		(0.120)	(0.138)	(0.210)	(0.243)	(0.086)	(0.067)	(0.117)	(0.292)	(0.067)	(0.108)
Dividends	-	-0.621	-1.352	-5.914*	-8.200**	1.503	0.884	-47.313	-72.769	0.046	0.386***
		(1.790)	(2.894)	(3.088)	(3.872)	(2.663)	(1.172)	(37.124)	(51.129)	(0.084)	(0.111)
Size	-	0.001	0.023**	0.007	0.059***	-0.003	0.001	0.093***	0.158***	0.000	0.005
		(0.008)	(0.010)	(0.016)	(0.017)	(0.007)	(0.005)	(0.016)	(0.028)	(0.003)	(0.005)
Big4 Auditor	-	0.001	0.041	0.164*	-0.005	-0.133**	-0.024	0.066	0.058	-0.002	0.049**
		(0.059)	(0.042)	(0.098)	(0.077)	(0.052)	(0.021)	(0.093)	(0.138)	(0.018)	(0.025)
Institutional Ownership	-	0.004***	0.003***	0.004***	0.005***	0.001	0.001**	0.009***	0.008***	0.001***	0.003***
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.002)	(0.002)	(0.000)	(0.000)
Free Cash Flow	+	0.189	0.470***	0.367*	0.232	0.060	0.040	-0.337	1.410***	0.052**	0.283***
		(0.118)	(0.127)	(0.207)	(0.257)	(0.099)	(0.075)	(0.317)	(0.512)	(0.025)	(0.036)
Cash	+	-0.425***	-0.464***	-0.233	-0.191	-0.058	-0.032	-0.213	-0.798***	-0.096***	-0.281***
		(0.102)	(0.096)	(0.170)	(0.175)	(0.077)	(0.049)	(0.258)	(0.293)	(0.035)	(0.050)
Market to Book Ratio	+	-0.000	0.001	-0.000	-0.000	-0.000	0.000	-0.002	0.001	-0.000	0.000
		(0.001)	(0.001)	(0.001)	(0.002)	(0.000)	(0.000)	(0.002)	(0.002)	(0.000)	(0.001)
Constant		2.549***	-0.407*	1.508**	2.011***	5.542***	4.975***	-0.364	-3.470***	0.591***	-0.933***
		(0.419)	(0.216)	(0.726)	(0.357)	(0.317)	(0.102)	(0.765)	(0.638)	(0.173)	(0.145)
Observations		4,703	4,786	4,703	4,786	4,703	4,786	2,795	2,974	10,099	10,115
Adjusted R-squared		0.530	0.145	0.534	0.039	0.199	0.020	0.753	0.138	0.733	0.206
Firm FE		Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Industry FE		No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Table 6 H3: Information Asymmetry and DFA

This table presents the baseline OLS regression results examining the effect of the DFA on information asymmetry. The dependent variables are Bid–Ask Spread (Columns 1–4) and Analyst Coverage (Columns 5–8). The main independent variable, Post, is a binary indicator equal to 1 if the firm's fiscal year ends in or after 2011, and 0 for years 2007–2010. Voluntary Disclosure (VD) is a dummy variable equal to 1 if a firm issued at least one management earnings forecast in year t; this is used to split the sample in Columns 3–4 (Bid–Ask Spread) and Columns 7–8 (Analyst Coverage). Columns 1 and 5 include firm fixed effects, while Columns 2–4 and 6–8 include industry fixed effects based on the Fama-French 12 industry classification. A constant term is included in all models. Robust standard errors are reported in parentheses and are clustered at the establishment level. Statistical significance is denoted by ***, *, and * at the 1%, 5%, and 10% levels, respectively. All variables are defined in Appendix A.

VARIABLES		Bid-ask spread					Analyst Coverage			
		(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)
	Pred	Firm FE	Industry FE	Industry FE VD =0	Industry FE VD =1	Pred	Firm FE	Industry FE	Industry FE VD =0	Industry FE VD =1
Post	-	-0.031***	-0.023***	-0.026**	-0.024***	+	0.095***	0.075***	0.070***	0.077***
		(0.006)	(0.007)	(0.012)	(0.005)		(0.009)	(0.011)	(0.017)	(0.014)
Ln(Audit_Fee)	+	-0.007	-0.054***	-0.068***	-0.031***	+	0.056***	0.266***	0.218***	0.301***
		(0.011)	(0.006)	(0.011)	(0.003)		(0.015)	(0.016)	(0.022)	(0.018)
Ownership Stake	-	0.072**	0.058	0.026	0.044*	+	0.225***	0.649***	0.845***	0.313*
		(0.034)	(0.039)	(0.084)	(0.023)		(0.063)	(0.150)	(0.210)	(0.185)
Dividends	+	0.951	0.565	0.464	-0.444	-	-0.043	-1.533*	-1.409*	-4.355**
		(0.778)	(0.840)	(0.856)	(0.316)		(0.356)	(0.842)	(0.847)	(2.184)
Size	+	-0.010***	-0.023***	-0.029***	-0.013***	+	0.013***	0.082***	0.066***	0.118***
		(0.002)	(0.003)	(0.004)	(0.002)		(0.004)	(0.009)	(0.009)	(0.013)
Big4 Auditor	+	-0.014	-0.068***	-0.085***	-0.011	+	0.001	0.144***	0.173***	0.082*
		(0.030)	(0.015)	(0.022)	(0.012)		(0.030)	(0.033)	(0.039)	(0.045)
Institutional Ownership	+	-0.002***	-0.003***	-0.003***	-0.002***	+	0.006***	0.009***	0.009***	0.007***
		(0.000)	(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.001)	(0.001)
Free Cash Flow	-	-0.092**	-0.019	-0.017	-0.037	+	0.014	0.032	-0.065	0.074
		(0.037)	(0.031)	(0.039)	(0.028)		(0.038)	(0.055)	(0.059)	(0.144)
Cash	-	0.025	-0.022	-0.055	0.029	-	-0.069	-0.225***	-0.181**	-0.223*
		(0.034)	(0.030)	(0.038)	(0.040)		(0.054)	(0.069)	(0.080)	(0.114)
Market to Book Ratio	-	0.000	0.000	0.000	0.000	+	0.001**	0.009***	0.011***	0.006***
		(0.000)	(0.000)	(0.000)	(0.000)		(0.000)	(0.001)	(0.002)	(0.001)
Constant		0.367**	1.202***	1.447***	0.712***		0.826***	-2.604***	-2.021***	-2.884***
		(0.154)	(0.097)	(0.157)	(0.064)		(0.206)	(0.197)	(0.275)	(0.244)
Observations		10,001	10,032	5,251	4,781		9,555	9,638	4,906	4,732
Adjusted R-squared		0.413	0.154	0.144	0.146		0.885	0.491	0.473	0.433
Firm FE		Yes	No	No	No		Yes	No	No	No
Industry FE		No	Yes	Yes	Yes		No	Yes	Yes	Yes

Table 7 Alternative measures

This table presents regression results using alternative measures to assess the robustness of the baseline findings. The analysis is based on firm-year observations from 2007 to 2014 with complete data. The key independent variable, Post, is a binary indicator equal to 1 if a firm's fiscal year ends in or after 2011, capturing the impact of the DFA over a four-year event window.

Panel A reports results using two widely used accrual-based proxies for financial reporting quality: the F-Score developed by Dechow et al. (2011) (Columns 1–4), which predicts the likelihood of earnings manipulation, and the M-Score by Beneish (1999) (Columns 5–8), which detects potential financial statement fraud. Panel B examines the effect of the DFA on earnings-related uncertainty, using Analyst Forecast Accuracy (Columns 1–4) and Return Volatility (Columns 5–8) as dependent variables.

All regressions are estimated using pooled OLS. Firm fixed effects are included in Columns 1 and 5 of each panel, while industry fixed effects (based on the Fama-French 12 classification) are applied in Columns 2–4 and 6–8. The sample is further split by Voluntary Disclosure (VD) status to examine heterogeneity in firms' responses to regulatory enforcement. Columns 3–4 (F-Score / Analyst Forecast Accuracy) and Columns 7–8 (M-Score / Return Volatility) compare firms that issued at least one management earnings forecast (VD = 1) with those that did not (VD = 0) in a given fiscal year. A constant term is included in all models. Robust standard errors are reported in parentheses and clustered at the firm level. Statistical significance is denoted by ***, *, and * at the 1%, 5%, and 10% levels, respectively. All variables are defined in Appendix A.

Panel A: Accrual-based mandatory reporting quality measures

VARIABLES		F-Score				M-Score			
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Firm FE	Industry FE	Industry FE		Firm FE	Industry FE	Industry FE	
	Pred			VD =0	VD =1			VD =0	VD =1
Post	-	-0.065***	-0.057***	-0.060***	-0.053***	-0.024***	-0.024***	-0.031***	-0.019**
		(0.011)	(0.010)	(0.014)	(0.015)	(0.006)	(0.006)	(0.009)	(0.007)
Ln(Aud_Fee)	-/+	0.081***	0.020***	0.026***	0.012	-0.040***	-0.016***	-0.015**	-0.015***
		(0.022)	(0.008)	(0.010)	(0.013)	(0.013)	(0.004)	(0.006)	(0.004)
Ownership Stake	-	-0.068	-0.285***	-0.339***	-0.168	-0.039	-0.017	0.094	-0.032
		(0.100)	(0.082)	(0.121)	(0.108)	(0.057)	(0.051)	(0.096)	(0.051)
Dividends	-	0.241	0.141	0.061	1.825	1.756***	0.670**	0.728**	0.074
		(0.251)	(0.236)	(0.240)	(2.515)	(0.471)	(0.287)	(0.288)	(1.486)
Size	-	-0.022***	-0.036***	-0.030***	-0.047***	0.008	0.009***	0.008*	0.014***
		(0.005)	(0.004)	(0.005)	(0.008)	(0.005)	(0.003)	(0.004)	(0.005)
Big4 Auditor	-	0.004	-0.030*	-0.031	-0.028	-0.017	-0.016	-0.016	-0.013
		(0.045)	(0.016)	(0.020)	(0.027)	(0.023)	(0.010)	(0.013)	(0.014)
Institutional Ownership	-	-0.001*	-0.001***	-0.001***	-0.001	-0.000	-0.000	-0.000	0.000
		(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Free Cash Flow	+	0.110**	-0.031	0.011	-0.159	-0.261***	-0.298***	-0.249***	-0.518***
		(0.052)	(0.032)	(0.034)	(0.101)	(0.059)	(0.037)	(0.043)	(0.070)
Cash	+	-0.152**	-0.102***	-0.064	-0.175***	-0.211***	0.033	0.027	0.036
		(0.063)	(0.038)	(0.045)	(0.065)	(0.049)	(0.027)	(0.037)	(0.037)
Market to Book Ratio	-	0.000	-0.002***	-0.002***	-0.001	-0.000	0.000	-0.000	0.001**
		(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Constant		-0.704**	0.169	0.099	0.282*	0.721***	0.332***	0.331***	0.284***
		(0.301)	(0.103)	(0.127)	(0.170)	(0.186)	(0.048)	(0.074)	(0.061)
Observations		7,379	7,482	4,090	3,392	9,059	9,098	4,616	4,482
Adjusted R-squared		0.149	0.041	0.037	0.050	0.099	0.038	0.040	0.028
Firm FE		Yes	No	No	No	Yes	No	No	No
Industry FE		No	Yes	Yes	Yes	No	Yes	Yes	Yes

Table 7 (continued)

Panel B: Earnings related uncertainties

VARIABLES		Analyst Forecast Accuracy					Return Volatility			
		(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)
		Firm FE	Industry FE	Industry FE			Firm FE	Industry FE	Industry FE	
	Pred			VD =0	VD =1	Pred			VD =0	VD =1
Post	+	0.330***	0.351***	0.504***	0.209***	-	-0.036***	-0.038***	-0.042***	-0.034***
		(0.040)	(0.041)	(0.071)	(0.036)		(0.001)	(0.001)	(0.002)	(0.001)
Ln(Aud_Fee)	+	-0.414***	0.028	-0.071	0.070**	-	0.006**	-0.008***	-0.003*	-0.011***
		(0.097)	(0.029)	(0.046)	(0.028)		(0.002)	(0.001)	(0.002)	(0.001)
Ownership Stake	+	0.902***	1.196***	2.196***	0.542***	-	-0.056***	-0.093***	-0.133***	-0.054***
		(0.237)	(0.264)	(0.580)	(0.206)		(0.010)	(0.014)	(0.029)	(0.014)
Dividends	+	-3.053	-8.114***	-8.331***	-9.739	-	0.070	0.170	0.138	1.795**
		(5.201)	(1.602)	(1.634)	(10.263)		(0.133)	(0.141)	(0.132)	(0.701)
Size	+	0.060***	0.166***	0.175***	0.136***	-	-0.002**	-0.003***	-0.002**	-0.005***
		(0.020)	(0.018)	(0.027)	(0.019)		(0.001)	(0.001)	(0.001)	(0.001)
Big4 Auditor	+	-0.155	0.018	0.066	-0.062	-	0.006	0.002	0.001	0.001
		(0.171)	(0.077)	(0.107)	(0.090)		(0.005)	(0.002)	(0.003)	(0.003)
Institutional Ownership	+	0.021***	0.014***	0.016***	0.010***	-	-0.001***	-0.000***	-0.000***	-0.000***
		(0.003)	(0.001)	(0.002)	(0.002)		(0.000)	(0.000)	(0.000)	(0.000)
Free Cash Flow	-	-0.374	1.327***	1.177***	1.840***	+	-0.004	-0.081***	-0.074***	-0.089***
		(0.340)	(0.206)	(0.254)	(0.384)		(0.011)	(0.006)	(0.007)	(0.016)
Cash	-	-0.415	-0.778***	-0.623**	-0.854***	+	0.032***	0.050***	0.038***	0.061***
		(0.346)	(0.186)	(0.250)	(0.212)		(0.009)	(0.006)	(0.008)	(0.009)
Market to Book Ratio	+	0.005	0.007***	0.013***	0.001	-	0.000	-0.000***	-0.000***	-0.000***
		(0.003)	(0.001)	(0.003)	(0.001)		(0.000)	(0.000)	(0.000)	(0.000)
Constant		3.303**	-2.739***	-1.815***	-2.578***		0.108***	0.280***	0.226***	0.321***
		(1.368)	(0.381)	(0.578)	(0.410)		(0.033)	(0.014)	(0.020)	(0.019)
Observations		8,524	8,589	4,328	4,261		10,060	10,080	5,295	4,785
Adjusted R-squared		0.366	0.138	0.119	0.114		0.454	0.264	0.230	0.260
Firm FE		Yes	No	No	No		Yes	No	No	No
Industry FE		No	Yes	Yes	Yes		No	Yes	Yes	Yes

Table 8 Entropy Balancing

This table presents the results of the entropy balancing analysis. Panel A and Panel B report the mean, variance, and skewness of the covariates for the treatment and control groups before and after entropy balancing, respectively. These panels demonstrate the effectiveness of the balancing procedure in aligning the distributional characteristics of the covariates across groups. Panel C presents the results of the baseline regressions re-estimated using entropy balancing weights. The specifications include all control variables consistent with the baseline models, as well as a constant term. Firm fixed effects are included but not reported for brevity. Robust standard errors are shown in parentheses and are clustered at the firm level. Symbols ***, *, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. All variables are defined in Appendix A.

Panel A. Before weighting

	Treat (Post=1)			Control (Post=0)		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Mean	Variance	Skewness	Mean	Variance	Skewness
Ln(Aud_Fee)	13.980	1.267	0.335	13.980	1.199	0.329
Ownership Stake	0.028	0.004	5.155	0.027	0.004	5.274
Dividends	0.001	0.000	23.090	0.001	0.000	54.630
Size	1.604	2.980	5.444	1.513	2.603	6.230
Big4 Auditor	0.779	0.172	-1.342	0.808	0.155	-1.564
Institutional Ownership	74.680	716.700	-0.895	74.150	689.600	-0.726
Free Cash Flow	0.011	0.032	-3.761	0.022	0.034	-3.852
Cash	0.154	0.024	2.022	0.164	0.028	1.777
Market to Book Ratio	5.155	210.300	4.620	4.453	181.500	5.016

Panel B. After weighting

	Treat (Post=1)			Control (Post=0)		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Mean	Variance	Skewness	Mean	Variance	Skewness
Ln(Aud_Fee)	13.980	1.267	0.335	13.980	1.267	0.335
Ownership Stake	0.028	0.004	5.155	0.028	0.004	5.155
Dividends	0.001	0.000	23.090	0.001	0.000	23.150
Size	1.604	2.980	5.444	1.604	2.980	5.445
Big4 Auditor	0.779	0.172	-1.342	0.779	0.172	-1.342
Institutional Ownership	74.680	716.700	-0.895	74.680	716.700	-0.895
Free Cash Flow	0.011	0.032	-3.761	0.011	0.032	-3.761
Cash	0.154	0.024	2.022	0.154	0.024	2.022
Market to Book Ratio	5.155	210.300	4.620	5.155	210.300	4.620

Table 8 (continued)

Panel C. Entropy Balanced regression

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	FSD Score		Number of Forecast		Precision		Horizon	
VARIABLES	Before matching	Entropy Balanced	Before matching	Entropy Balanced	Before matching	Entropy Balanced	Before matching	Entropy Balanced
Post	-0.065***	-0.063***	0.051***	0.051***	-0.058**	-0.062***	0.049***	0.047***
	(0.017)	(0.017)	(0.014)	(0.014)	(0.023)	(0.023)	(0.010)	(0.010)
Ln(Aud_Fee)	-0.012	-0.004	-0.088***	-0.091***	0.003	-0.001	-0.005	-0.008
	(0.032)	(0.033)	(0.029)	(0.029)	(0.050)	(0.050)	(0.022)	(0.023)
Ownership Stake	-0.087	-0.228	0.118	0.080	0.510**	0.574***	0.082	0.062
	(0.159)	(0.153)	(0.120)	(0.127)	(0.210)	(0.196)	(0.086)	(0.094)
Dividends	-1.596**	-2.000*	-0.621	-0.666	-5.914*	-6.237*	1.503	1.093
	(0.761)	(1.149)	(1.790)	(1.898)	(3.088)	(3.208)	(2.663)	(3.329)
Size	0.014	0.019*	0.001	0.003	0.007	0.005	-0.003	-0.003
	(0.010)	(0.012)	(0.008)	(0.008)	(0.016)	(0.017)	(0.007)	(0.007)
Big4 Auditor	0.060	0.077	0.001	0.016	0.164*	0.160*	-0.133**	-0.137**
	(0.062)	(0.063)	(0.059)	(0.059)	(0.098)	(0.094)	(0.052)	(0.053)
Institutional Ownership	-0.001	-0.001	0.004***	0.003***	0.004***	0.004***	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Free Cash Flow	-0.285***	-0.283***	0.189	0.213*	0.367*	0.364*	0.060	0.081
	(0.103)	(0.102)	(0.118)	(0.123)	(0.207)	(0.201)	(0.099)	(0.098)
Cash	0.403***	0.412***	-0.425***	-0.462***	-0.233	-0.220	-0.058	-0.080
	(0.121)	(0.127)	(0.102)	(0.103)	(0.170)	(0.181)	(0.077)	(0.079)
Market to Book Ratio	0.001	0.001	-0.000	-0.001	-0.000	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Constant	2.842***	2.718***	2.549***	2.611***	1.508**	1.575**	5.542***	5.619***
	(0.461)	(0.468)	(0.419)	(0.425)	(0.726)	(0.728)	(0.317)	(0.331)
Observations	10,099	10,099	4,703	4,703	4,703	4,703	4,703	4,703
Adjusted R-squared	0.310	0.324	0.530	0.534	0.534	0.533	0.199	0.199
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 8 (continued)

Panel C. Entropy Balanced regression (continued)

	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Specificity		Forecaster		Bid-ask spread		Analyst Forecast Accuracy	
VARIABLES	Before matching	Entropy Balanced	Before matching	Entropy Balanced	Before matching	Entropy Balanced	Before matching	Entropy Balanced
Post	0.044*	0.036	-0.014*	-0.017**	-0.031***	-0.029***	0.095***	0.097***
	(0.023)	(0.022)	(0.008)	(0.008)	(0.006)	(0.006)	(0.009)	(0.009)
Ln(Aud_Fee)	-0.080	-0.076	0.082***	0.080***	-0.007	-0.007	0.056***	0.059***
	(0.054)	(0.054)	(0.011)	(0.011)	(0.011)	(0.011)	(0.015)	(0.015)
Ownership Stake	0.588***	0.680***	0.385***	0.389***	0.072**	0.073**	0.225***	0.227***
	(0.117)	(0.128)	(0.106)	(0.116)	(0.034)	(0.034)	(0.063)	(0.064)
Dividends	-47.313	-51.516	0.316***	0.476**	0.951	0.410	-0.043	-0.172
	(37.124)	(37.608)	(0.120)	(0.225)	(0.778)	(0.510)	(0.356)	(0.286)
Size	0.093***	0.094***	0.003	0.003	-0.010***	-0.010***	0.013***	0.010***
	(0.016)	(0.016)	(0.004)	(0.004)	(0.002)	(0.003)	(0.004)	(0.004)
Big4 Auditor	0.066	0.054	0.062***	0.068***	-0.014	-0.012	0.001	0.011
	(0.093)	(0.090)	(0.024)	(0.024)	(0.030)	(0.030)	(0.030)	(0.032)
Institutional Ownership	0.009***	0.009***	0.003***	0.003***	-0.002***	-0.002***	0.006***	0.006***
	(0.002)	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Free Cash Flow	-0.337	-0.277	0.242***	0.258***	-0.092**	-0.092***	0.014	0.026
	(0.317)	(0.313)	(0.036)	(0.037)	(0.037)	(0.035)	(0.038)	(0.039)
Cash	-0.213	-0.218	-0.312***	-0.323***	0.025	0.032	-0.069	-0.084
	(0.258)	(0.258)	(0.049)	(0.049)	(0.034)	(0.032)	(0.054)	(0.058)
Market to Book Ratio	-0.002	-0.002	0.000	0.000	0.000	0.000	0.001**	0.001*
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-0.364	-0.420	-0.900***	-0.870***	0.367**	0.375**	0.826***	0.786***
	(0.765)	(0.776)	(0.143)	(0.145)	(0.154)	(0.155)	(0.206)	(0.213)
Observations	2,795	2,795	10,115	10,115	10,001	10,001	9,555	9,555
Adjusted R-squared	0.753	0.748	0.238	0.246	0.413	0.417	0.885	0.886
Firm FE	Yes	Yes			Yes	Yes	Yes	Yes
Industry FE			Yes	Yes				

Table 9 High Litigation Industry

This table presents the regression results examining whether the effect of the DFA on voluntary disclosure varies across high-litigation industries. The dependent variables are Number of Forecasts (Column 1) and Horizon (Column 2), both derived from firm-year observations spanning 2007 to 2014 with complete data. The key independent variable is the interaction term High Litigious Industry × Post, where Post is a binary indicator equal to 1 for fiscal year-ends in or after 2011, and 0 for 2007–2010. High Litigious Industry is defined based on SIC codes associated with elevated litigation risk (2833–2836, 3570–3577, 3600–3674, 5200–5961, 7370–7374, and 8731–8734). All models are estimated using pooled ordinary least squares (OLS) with industry fixed effects based on Fama-French 12 classifications. Coefficients on industry fixed effects are omitted for brevity. A constant term is included in all models. Robust standard errors, clustered at the firm level, are reported in parentheses. Symbols ***, *, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. All variables are defined in Appendix A.

		(1)		(2)
VARIABLES	Pred	Number of Forecast	Pred	Horizon
High Litigious Industry×Post	-	-0.060**	+	0.033*
		(0.030)		(0.020)
High Litigious Industry	+	0.063	+	0.016
		(0.040)		(0.020)
Post	+	0.064***	+	0.022*
		(0.019)		(0.012)
Ln(Aud_Fee)	-	0.116***	-	0.028***
		(0.016)		(0.007)
Ownership Stake	+	0.122	+	0.138**
		(0.138)		(0.067)
Dividends	-	-1.430	-	0.951
		(2.898)		(1.201)
Size	-	0.023**	-	0.001
		(0.010)		(0.005)
Big4 Auditor	-	0.042	-	-0.023
		(0.042)		(0.021)
Institutional Ownership	-	0.003***	-	0.001**
		(0.001)		(0.000)
Free Cash Flow	+	0.477***	+	0.046
		(0.127)		(0.075)
Cash	+	-0.479***	+	-0.043
		(0.097)		(0.049)
Market to Book Ratio	+	0.001	+	0.000
		(0.001)		(0.000)
Constant		-0.445**		4.958***
		(0.216)		(0.103)
Observations		4,786		4,786
Adjusted R-squared		0.146		0.021
Industry FE		Yes		Yes

Table 10 Difference-in-Differences (DiD) Test

This table reports the results of Difference-in-Differences (DiD) regressions examining the impact of the DFA whistleblower provisions on financial reporting quality, measured by the FSD Score. The main independent variables are interaction terms: False Claims Act × Post and Small Reporting Companies × Post, where Post is a binary indicator equal to 1 for fiscal years ending in or after 2011 and 0 otherwise. Columns (1) and (2) present results for the treatment group defined by the False Claims Act (FCA). Column (1) reports estimates using the unmatched sample, while Column (2) uses entropy balancing to improve covariate balance. Columns (3) and (4) extend the DiD analysis to Smaller Reporting Companies (SRC), with Column (3) based on the unmatched sample and Column (4) incorporating entropy-balanced weights. Entropy balancing was performed independently for the FCA and SRC subsamples to ensure covariate balance prior to DiD estimation. All regressions include firm fixed effects, while Columns (3) and (4) additionally include year fixed effects to control for time-varying shocks. A constant term is included in all models. Robust standard errors are reported in parentheses and are clustered at the establishment level. Symbols such as ∗∗∗, ∗∗, and ∗ denote statistical significance at the 1%, 5%, and 10% levels, respectively. All variables are defined in Appendix A.

VARIABLES	Pre	(1) Before matching	(2) Entropy Balanced	Pre	(3) Before matching	(4) Entropy Balanced
False Claims Act × Post	-	-0.065***	-0.064***			
		(0.019)	(0.019)			
Small Reporting Companies × Post				+	0.117*	0.200**
					(0.065)	(0.092)
Ln(Aud_Fee)	-	-0.013	-0.025	-	-0.004	0.064
		(0.032)	(0.039)		(0.033)	(0.072)
Ownership Stake	-	-0.092	-0.341*	-	-0.055	-1.104
		(0.159)	(0.191)		(0.164)	(0.857)
Dividends	-	-1.636**	-0.665	-	-1.606**	-1.210
		(0.762)	(1.533)		(0.782)	(0.856)
Size	-	0.015	0.023*	-	0.017	0.001
		(0.010)	(0.013)		(0.010)	(0.022)
Big4 Auditor	-	0.065	0.100	-	0.065	-0.121
		(0.062)	(0.071)		(0.062)	(0.137)
Institutional Ownership	-	-0.001	-0.000	-	-0.000	-0.001
		(0.001)	(0.001)		(0.001)	(0.002)
Free Cash Flow	+	-0.278***	-0.198	+	-0.288***	-0.289**
		(0.103)	(0.129)		(0.103)	(0.125)
Cash	+	0.404***	0.418***	+	0.384***	0.747**
		(0.121)	(0.119)		(0.122)	(0.293)
Market to Book Ratio	+	0.001	0.000	+	0.001	0.002
		(0.001)	(0.001)		(0.001)	(0.003)
Constant		2.846***	2.890***		2.653***	2.196**
		(0.460)	(0.561)		(0.470)	(0.916)
Observations		10,099	10,099		10,099	10,099
Adjusted R-squared		0.309	0.274		0.310	0.360
Firm FE		Yes	Yes		Yes	Yes
Year FE		No	No		Yes	Yes

Appendix A Variable Definitions

Variable	Definition
Dependent Variables	
FSD Score	The sum of absolute differences between the leading digit frequencies in actual annual financial statements data and the expected theoretical distribution prescribed by Benford's Law across the 9 leading digits (1 to 9) (Amiram et al., 2015) Calculated using annual financial statement data from Compustat.
Forecaster	An indicator variable equals to 1 if the firm issues at least one forecast in three out of four quarters in a given fiscal year, and 0 otherwise (Rogers & Van Buskirk, 2009). Management forecast activity is obtained from Refinitiv Eikon.
Number of Forecasts	Natural logarithm of (1 + the number of forecasts issued during a given fiscal year) Management forecast activity is obtained from Refinitiv Eikon.
Precision	Average forecast precision in a given fiscal year. Coded as 4 for point estimates, 3 for range estimates, 2 for open-ended estimates, and 1 for qualitative estimates (Armstrong et al., 2014). Management forecast activity is obtained from Refinitiv Eikon.
Horizon	Natural log of (1 + average forecast horizon), where horizon is the number of days between the forecast date and the fiscal period-end (Ball et al., 2012). Higher values indicate more timely forecasts. Computed only for firm-years with non-missing forecast dates. Management forecast activity is obtained from Refinitiv Eikon.
Specificity	Negative average specificity of forecasts. For range forecasts, specificity equals the width of the forecast range divided by the stock price one month prior to the forecast date. For point forecasts, specificity = 0. This variable is multiplied by -1 so that higher values indicate more specific forecasts. Management forecast activity is obtained from Refinitiv Eikon.
Bid-Ask Spread	Average daily bid-ask spread over the fiscal year, calculated as (ask - bid) / midpoint. Data obtained from CRSP.
Analyst Coverage	Natural logarithm of (1+ number of analyst forecasts issued for the firm in a given year) Analyst coverage data is obtained from Refinitiv Datastream (I/B/E/S).
F-Score	Indicator variable equals to 1 if the firm's F-Score > 1.85 in year t, and 0 otherwise. F-Score $= [(e^{predicted\ value})/(1 - e^{predicted\ value})]/Unconditional\ probability$, Where predicted value = -7.893 + 0.790*RSST_ACC+ 2.518*CH_REC + 1.191*CH_INV+1.979*SOFT_ASSETS+0.171*CH_CS-0.932*CH_ROA + 1.029*ISSUE and unconditional probability equals 0.0037 as calculated by Dechow et al. (2011). Missing values in component variables are set to null.
M-Score	Indicator variable equal to 1 if the firm's M-Score > -1.78 in year t, and 0 otherwise (Wiedman & Zhu, 2023) M-Score = -4.84 + 0.920*DSRI+ 0.528*GMI+ 0.404* AQI + 0.892*SGI+ 0.115*DEPI -0.172*SGAI+ 4.679*TATA-0.327* LVGI Formula by Beneish (1999) Missing values in component variables are set to null.
Analyst Forecast Accuracy	Average of absolute analyst forecast error $=\|Analyst\ Forecast\ EPS - actual\ EPS\|/price$ Formula by Dhaliwal et al. (2012) Analyst earnings forecast data is obtained from Refinitiv Datastream (I/B/E/S).

Variable	Definition
Return Volatility	Standard deviation of monthly returns over the fiscal year. Requires at least 10 months of return data. Daily return-based standard deviation used as per Ball et al. (2012) and Frankel et al. (2021). Data obtained from CRSP.
Time Dummy Variable	
Post	Indicator variable equal to 1 if the fiscal year-end falls between 2011 and 2014, and 0 if between 2007 and 2010.
Control Variables	
Ln(Aud_Fee)	Natural logarithm of total annual audit fees. Source: Refinitiv Eikon.
Ownership Stake	Purchase of common and preferred stocks (PRSTKC) divided by Total Assets (AT) Source: Compustat.
Dividends	Indicator variable equals 1 if a firm issued dividends, and 0 otherwise. Source: Compustat.
Size	Natural logarithm of market capitalisation (CSHO*PRCC_F) at year-end. Source: Compustat.
Big4 Auditor	Indicator variable equals 1 if the firm is audited by a Big 4 accounting firm, and 0 otherwise. Source: Refinitiv Eikon.
Institutional Ownership%	Percentage of shares held by institutional investors. Source: Refinitiv Eikon.
Free Cash Flow	Operating Activities Net Cash Flow(OANCF)-Capital Expenditures(CAPX) / Total Assets (AT) Source: Compustat.
Cash	Cash (CH) divided by Total Assets (AT). Source: Compustat.
Market-To-Book	market capitalisation (CSHO*PRCC_F) divided by Book value of total stockholders' equity Source: Compustat.

Appendix B Propensity Score Matching (PSM)

This table presents the results of the propensity score matching (PSM) analysis conducted to validate the robustness of the baseline findings. Columns (1) and (2) display the first-stage logit regressions used to estimate propensity scores before and after matching, respectively. The remaining columns report second-stage regression results based on the matched sample, evaluating the effect of the Dodd-Frank Act (Post) on key disclosure and information environment variables. These include the FSD Score, Number of Forecasts, Precision, Horizon, Specificity, Forecaster, Bid-Ask Spread, and Analyst Forecast Accuracy. All models include the same control variables used in the baseline regressions, as well as a constant term. Robust standard errors clustered at the establishment level are reported in parentheses. Statistical significance is denoted by ***, **, and *, corresponding to the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1st stage		2nd stage							
VARIABLES	Pre match	Post match	FSD Score	Number of Forecasts	Precision	Horizon	Specificity	Forecaster	Bid-ask spread	Analyst Forecast Accuracy
Post			-0.066***	0.050***	-0.062**	0.050***	0.073***	-0.004	-0.032***	0.097***
			(0.019)	(0.017)	(0.025)	(0.011)	(0.025)	(0.007)	(0.007)	(0.010)
Ln(Aud_Fee)	-1.377***	-1.509***	-0.038	-0.104**	0.044	-0.043	-0.117	-0.018	-0.006	0.057***
	(0.069)	(0.092)	(0.042)	(0.041)	(0.061)	(0.032)	(0.074)	(0.017)	(0.010)	(0.020)
Ownership Stake	-3.740***	-2.601	0.117	0.087	0.484*	0.055	0.751***	0.175**	0.082***	0.176***
	(1.359)	(1.733)	(0.189)	(0.138)	(0.270)	(0.091)	(0.140)	(0.075)	(0.031)	(0.073)
Dividends	9.686***	9.849***	-2.051*	3.040	-1.157	-2.144	-23.868	0.228	7.802**	1.482
	(2.058)	(2.395)	(1.145)	(4.310)	(7.661)	(6.203)	(21.829)	(0.256)	(3.611)	(1.096)
Size	-0.540***	-0.571***	0.002	0.000	-0.008	-0.003	0.082***	0.000	-0.009***	0.015***
	(0.040)	(0.053)	(0.013)	(0.010)	(0.019)	(0.008)	(0.017)	(0.004)	(0.003)	(0.005)
Big4 Auditor	-0.144	-0.047	0.203**	0.020	0.215	-0.063	0.136	0.025	0.007	-0.002
	(0.100)	(0.130)	(0.098)	(0.089)	(0.157)	(0.063)	(0.128)	(0.024)	(0.042)	(0.044)
Institutional Ownership	-0.027***	-0.031***	0.001	0.004***	0.005***	0.000	0.010***	0.002***	-0.001***	0.006***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.003)	(0.000)	(0.000)	(0.001)
Free Cash Flow	-1.342***	-1.499***	-0.255*	-0.093	0.301	0.027	0.162	0.088**	-0.140***	0.026
	(0.192)	(0.262)	(0.134)	(0.151)	(0.266)	(0.125)	(0.331)	(0.037)	(0.050)	(0.050)
Cash	1.194***	1.346***	0.433***	-0.376***	-0.476**	-0.097	0.085	-0.075	-0.016	-0.062
	(0.239)	(0.319)	(0.145)	(0.133)	(0.229)	(0.102)	(0.273)	(0.050)	(0.045)	(0.068)
Market to Book Ratio	-0.001	-0.005	0.001	-0.001	0.000	-0.000	-0.004**	-0.000	0.000	0.000
	(0.007)	(0.009)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.000)	(0.000)	(0.000)
Constant	18.527***	20.493***	2.966***	2.816***	0.845	6.076***	-0.068	0.629***	0.323**	0.836***
	(0.871)	(1.175)	(0.596)	(0.612)	(0.902)	(0.469)	(1.060)	(0.242)	(0.153)	(0.276)
Observations	10,099	6,678	6,678	3,267	3,267	3,267	1,926	6,678	6,619	6,365
R-squared	0.418	0.431	0.453	0.638	0.641	0.400	0.809	0.788	0.542	0.904
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Appendix C Benford's Law

Benford's Law is also called The First Digit Law as it is about the frequency of first digit occurrence from 1 through 9 – numbers with the first digit of 1 were observed more often than those starting with 2, 3, and so on.

The first discovery was in 1881. An astronomer Simon Newcomb noticed a mathematical property that the earlier pages in logarithms books were more worn than the latter pages. He inferred from the observation and calculated the probability that a number has a first digit d:

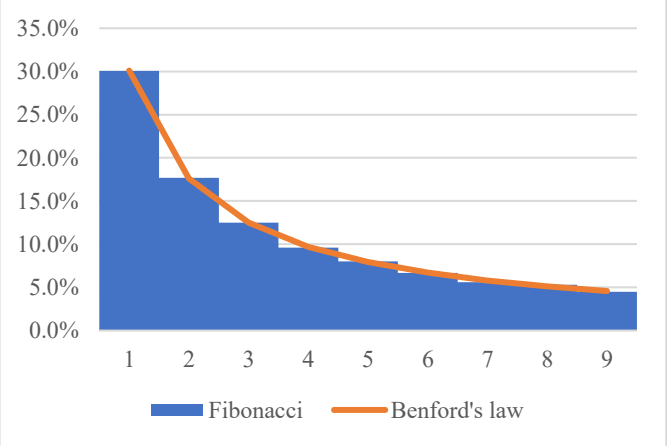
$$P(d) = \log_{10}\left(1 + \frac{1}{d}\right), \qquad where\ d = 1, 2, \ldots, 9$$

d	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
P(d)	0.301	0.176	0.125	0.097	0.079	0.067	0.058	0.051	0.046

Almost 50 years later, a physicist Frank Benford focused on the digit distribution and tested the mathematical property on a variety of dataset such as areas of rivers, atomic weights of elements, and numbers appearing in *Reader's Digest* articles. He found that the law held in each dataset and formulated the expected frequencies for the first and the second positions in a number together with their combinations forming a geometric sequence (Benford, 1938).

Benford's distribution is an empirically observable phenomenon. Sample calculation of Fibonacci Sequence ($F_n = F_{n-1} + F_{n-2}$, where $n>1$) is below and shows a good fit with the first 1,000 Fibonacci numbers to Benford's distribution.

Digit	**Occurrences**	**Empirical distribution**
1	301	30.1%
2	177	17.7%
3	125	12.5%
4	96	9.6%
5	80	8.0%
6	67	6.7%
7	56	5.6%
8	53	5.3%
9	45	4.5%
Total	**1,000**	



If distributions are selected at random and random samples are taken from each of these distributions, then the significant digital frequencies of the combined samplings are expected to converge to Benford's distribution, even though the individual distributions may not closely follow the Law (Grammatikos & Papanikolaou, 2020). In the context of this study, this means that if the digital frequency in a firm's annual financial statement data departs from the expectations of Benford's Law, then the financial reporting quality is low, and the firm's voluntary disclosure is also less-credible consequently.

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