Research-Thesis: The long-term impact of financial fraud on U.S. public companies: a profitability analysis.

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THE LONG-TERM IMPACT OF FINANCIAL FRAUD ON U.S. PUBLIC COMPANIES:

A PROFITABILITY ANALYSIS

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Yasmine,

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List of Abbreviations

AAER	Accounting and Auditing Enforcement Releases
BTM	Book-to-Market ratio
COSO	Committee of Sponsoring Organizations of the Treadway Commission
DiD	Difference-in-Differences
FE	Fixed Effects
GAAP	Generally Accepted Accounting Principles
LSEG	London Stock Exchange Group
NPM	Net Profit Margin
ROA	Return on Assets
ROE	Return on Equity
SEC	U.S. Securities and Exchange Commission
TRBC	Thomson Reuters Business Classification
TWFE	Two-Way Fixed Effects

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1. Introduction

Background and Context

Financial fraud, defined as the deliberate misstatement or omission of financial information in violation of Generally Accepted Accounting Principles (Dechow et al., 1996), strikes at the core of what capital markets depend on: the ability of investors to trust what firms report. The collapses of Enron and WorldCom demonstrate that accounting fraud extends beyond individual firms, eroding billions in market value and undermining investor trust across the global financial system (Karpoff, Lee & Martin, 2008a; Palmrose, Richardson & Scholz, 2004).

Regulatory authorities, notably the U.S. Securities and Exchange Commission (SEC), continue to document hundreds of enforcement cases annually, underscoring the persistent and systemic nature of corporate fraud (Dechow, Ge, Larson & Sloan, 2011). In response, a substantial body of literature has examined the consequences of fraud revelation. Most of this research, however, focuses on immediate market reactions, finding that firms typically lose around 9% of market capitalization following restatement announcements (Palmrose, Richardson & Scholz, 2004) and experience sharp increases in the cost of equity capital (Hribar & Jenkins, 2004).

Yet, confining analysis to these short-term outcomes provides only a partial understanding of fraud's true economic cost. As Karpoff, Lee, and Martin (2008a) demonstrate, the most significant penalty firms face is reputational: customers, suppliers, and creditors all revise the terms of their dealings upward in response to the loss of trust, producing persistent revenue reductions, higher operating costs, and suppressed profitability that can last for years.

Building on this foundation, the present study broadens the analytical lens by examining the long-term operational implications of fraud. Specifically, it evaluates how core accounting indicators (ROA, ROE and NPM), reflect the sustained impact of reputational damage. Understanding these dimensions is crucial for determining whether firms can genuinely recover and restore stakeholder confidence after a major financial scandal.

Problem Statement and Research Gap

While accounting fraud has been extensively documented, existing research provides only a fragmented view of its overall economic impact. Most prior studies emphasize fraud detection models, such as identifying abnormal accruals (Beneish, 1999; Dechow, Ge, Larson & Sloan, 2011), or examine the short-term market shock that follows fraud disclosure (Palmrose, Richardson & Scholz, 2004). This heavy focus on immediate capital market reactions has left a significant empirical gap regarding how fraud affects a firm's long-term financial health.

The prevailing theoretical consensus in financial misconduct literature suggests that the most severe consequence of fraud is reputation loss, a non-monetary penalty that often outweighs regulatory fines and shareholder settlements (Karpoff, Lee & Martin, 2008a). This penalty forces customers, suppliers, and lenders to demand tighter and more costly contract terms. Firms also lose human capital through executive turnover and sanctions (Karpoff, Lee & Martin, 2008b; Desai, Hogan & Wilkins, 2006), and internal instability persists long after legal cases conclude (Farber, 2005). Collectively, these disruptions create a sustained drain on organizational resources and efficiency.

That said, empirical research has yet to quantify this operational impairment using long-horizon accounting measures. Stock price-based indicators, while informative, are influenced by market sentiment and fail to fully capture underlying performance fundamentals. By shifting attention to profitability metrics such as ROA, ROE, and NPM over a five-year pre- and post-fraud window, this study aims to provide the missing longitudinal evidence linking reputational damage to real, measurable declines in firm performance. This approach offers a more comprehensive perspective on the economic aftermath of accounting fraud and its enduring effects on corporate resilience.

Research Question and Objectives

The central question guiding this research is: Does financial fraud lead to a significant long-term decline in the profitability of U.S. publicly listed companies?

This inquiry builds directly upon the discussion in the preceding sections by moving beyond short-term market reactions to examine whether the reputational and operational consequences of accounting fraud are enduring. The study ultimately seeks to determine if these consequences result in a measurable and sustained deterioration in fundamental accounting performance.

To address this overarching question, the study pursues a set of specific, interlinked objectives. First, it aims to assess the impact of financial fraud on key profitability indicators, namely Return on Assets (ROA), Return on Equity (ROE), and Net Profit Margin (NPM). This assessment will be conducted through a longitudinal analysis, comparing firms' financial performance in the pre-fraud period (from five years to one year before exposure, T–5 to T–1) with the post-fraud period (one to five years after exposure, T+1 to T+5) in order to capture long-term performance dynamics. A critical component of this analysis is to identify whether the fraudulent firms experience a steeper and more persistent decline in profitability relative to a carefully constructed control group of non-fraudulent firms. Finally, the research will analyze whether profitability demonstrates signs of recovery in the years following fraud exposure or if the decline remains sustained over the long term.

By accomplishing these objectives, this research aims to provide robust empirical evidence on how corporate misconduct translates into enduring operational consequences, thereby offering a critical long-term perspective on the economic cost of financial fraud within U.S. capital markets.

Expected Contributions

This research offers significant theoretical, practical, and regulatory contributions to the understanding of how financial fraud affects the long-term profitability of U.S. public companies. By examining accounting-based profitability measures over an extended period, it provides new insights into the sustained financial consequences of corporate misconduct.

Theoretically, this study extends the literature on financial fraud and corporate reputation by shifting the focus from short-term market reactions to long-term accounting profitability. It empirically tests the theoretical concept of reputation loss, as discussed by scholars like Karpoff, Lee, and Martin (2008a), by investigating whether such damage translates into measurable and persistent impairment of a firm's financial performance. Furthermore, by analyzing profitability trends over a five-year pre- and post-fraud window, the study contributes to corporate finance and governance literature. This longitudinal approach helps clarify the extent to which firms can recover from major ethical and financial failures, thereby bridging a critical gap between event-based market studies and long-run economic outcomes reflected in accounting data.

From a practical standpoint, the findings hold valuable implications for key market participants. For investors, the long-term analysis of profitability indicators like ROA, ROE, and NPM offers a more reliable framework for evaluating a firm's recovery potential, moving beyond the noise of volatile short-term market reactions. From a corporate governance perspective, quantifying the extended economic costs of fraud can assist boards and executives in justifying and designing post-scandal reforms, cultural rebuilding, and enhanced internal controls. For managerial strategy, evidence on the presence or absence of profitability recovery provides a critical foundation for informed decisions regarding restructuring, reputational repair, and long-term financial planning in the aftermath of fraud exposure.

The study also provides relevant contributions for policymakers and regulators. The documentation of profitability trajectories after fraud offers crucial policy insight, serving to evaluate whether current enforcement and sanction mechanisms are sufficient to deter misconduct effectively. Additionally, the evidence generated can help shift the regulatory focus beyond fraud detection, a primary concern in studies like Dechow et al. (2011), and toward a longer-term view that emphasizes firm rehabilitation, financial resilience, and the rebuilding of sustained stakeholder trust.

In sum, this thesis provides comprehensive empirical evidence on whether financial fraud causes a lasting deterioration in firm profitability. By quantifying how reputational damage evolves into persistent profitability underperformance, an area previously underexplored in accounting and finance research, it enriches both theoretical and practical understanding of post-fraud corporate behavior.

Thesis Structure

This thesis is organized into six chapters that collectively examine the long-term impact of financial fraud on the profitability of U.S. public companies. Chapter 1 introduces the research by outlining the background, problem statement, research objectives, and expected contributions. Chapter 2 reviews the relevant literature and theoretical foundations related to financial fraud, reputation loss, and profitability, leading to the formulation of the main research hypothesis. Chapter 3 presents the methodological framework, including data collection, sample selection, variable definitions, and the econometric models used to evaluate the relationship between fraud and firm performance. Chapter 4 reports the empirical results, presenting and interpreting the findings from the main DiD estimation, robustness checks, and heterogeneity analyses. Chapter 5 discusses the main findings considering prior literature and outlines their theoretical and practical implications alongside the study's limitations. Chapter 6 concludes the thesis by summarizing the main insights and proposing directions for future research.

2. Literature Review and Theoretical Framework

2.1 Financial Fraud: Definition, Types and Motivation

Financial fraud represents one of the most severe breaches of corporate integrity, fundamentally undermining the credibility of financial reporting and the efficiency of capital markets. According to Jones (2011) in Creative Accounting, Fraud and International Accounting Scandals, financial fraud involves “the deliberate misrepresentation of a company’s financial position to mislead users of financial statements.” This definition emphasizes intentionality, fraud is not merely accounting error or judgment bias but a purposeful act to manipulate perceptions of performance, liquidity, or solvency.

Sherman and Young (2001), in Accounting Minefields, further distinguish between creative accounting and outright fraud, noting that while both distort reality, fraud involves clear violations of accounting standards and ethical principles, typically for personal or corporate gain. Such acts include revenue overstatement, premature revenue recognition, off-balance-sheet financing, and expense capitalization, tactics that inflate profitability metrics such as ROA and ROE, thereby misleading investors and regulators (Rezaee, 2005).

The U.S. Securities and Exchange Commission (SEC) defines financial reporting fraud as “intentional misstatements or omissions of amounts or disclosures in financial statements to deceive financial statement users” (SEC Staff Accounting Bulletin No. 99). This conceptualization highlights both manipulation and omission, consistent with empirical classifications found in enforcement releases and academic datasets like the Accounting and Auditing Enforcement Releases (AAERs).

Fraud mechanisms

Fraudulent financial reporting can occur through a range of mechanisms, including earnings management, revenue overstatement, expense deferral, off-balance-sheet financing, and improper asset valuation (Albrecht, Albrecht & Albrecht, 2008). Sherman and Young (2001) in Accounting Minefields emphasize that financial manipulation often arises from managerial attempts to meet market expectations or maintain financing conditions. These actions range from borderline “creative accounting”, practices that technically comply with GAAP but mislead users, to outright fraud, where managers intentionally falsify figures (Jones, 2011).

Jones (2011) further categorizes fraudulent accounting practices into intentional misstatements (e.g., falsifying transactions or inflating revenues) and creative accounting (e.g., exploiting accounting flexibility to distort results). Both share the goal of shaping financial narratives to meet short-term incentives, such as stock price targets or executive compensation thresholds (Healy &

Wahlen, 1999). Classic scandals such as Enron (2001) and WorldCom (2002) demonstrated how aggressive revenue recognition, off-balance-sheet vehicles, and misleading disclosures can mask underlying insolvency, ultimately resulting in systemic market failures.
Beyond managerial opportunism, pressure, rationalization, and opportunity, the three elements of the Fraud Triangle (Cressey, 1953), remain central theoretical explanations. Managers often face pressure to sustain growth or earnings benchmarks, rationalize misconduct as temporary or necessary, and exploit weak internal controls that provide the opportunity to manipulate results. Subsequent theoretical developments have expanded this framework to include a fourth element, capability, emphasizing that only individuals with sufficient authority, intelligence, and access can execute and conceal complex fraud schemes (Wolfe & Hermanson, 2004).

Theoretical foundations

Any study of financial fraud must choose its theoretical framework carefully, because that choice determines what consequences are predicted and over what time horizon. The existing literature draws on several traditions, Agency Theory (Jensen & Meckling, 1976), the Fraud Triangle (Cressey, 1953), and Information Economics, each of which captures a different aspect of corporate misconduct. However, these frameworks are not equally suited to every research question. Agency Theory, for example, offers a clear explanation of why fraud happens in the first place: the separation of ownership and control creates information asymmetry between managers and shareholders, and when executive compensation is heavily tied to short-term accounting targets, managers may find it personally advantageous to manipulate reported figures. But Agency Theory's explanatory power stops largely at the point where fraud is committed; it is a theory of incentives and origins, not of what follows.
This study asks a fundamentally different question. It does not seek to explain why fraud occurs, but whether its economic damage lasts, whether the harm done to a firm's profitability is temporary or long-lasting, and whether it can still be measured several years after the fraud becomes public. This requires a framework whose main focus is the aftermath of fraud, not its causes. This is precisely what Reputational Capital Theory, developed and tested empirically by Karpoff, Lee, and Martin (2008a, 2008b), provides. It is therefore this framework that serves as the main theoretical anchor of this thesis. Agency Theory and Information Economics are retained as supporting tools, they help explain how reputational damage is triggered and why it is so hard for firms to recover from, but they do not replace Reputational Capital Theory as the primary lens through which the hypothesis is built and interpreted.
To understand why fraud causes lasting financial harm, it helps to first understand what reputation actually means in economic terms. Karpoff et al. (2008a), building on the work of Klein and Leffler (1981), define reputational capital as the trust a firm has built up over time through consistent and honest dealings with its stakeholders, creditors, customers, suppliers, employees, and investors. This trust is economically valuable because it allows the firm to operate on favorable terms. Creditors lend at lower rates because they trust the firm's financial statements. Customers enter long-term contracts because they trust the firm to deliver. Suppliers offer flexible payment terms because they trust the firm to pay. Employees invest in firm-specific skills because they trust the firm's implicit promises about their careers. The key insight is that this relational network is not a side feature of the firm, it is a core part of its ability to generate profit. A firm's earnings depend not just on its physical and financial assets, but on the quality of the relationships surrounding those assets. Reputational capital is what sustains those relationships, and when it is destroyed, the effect shows up directly in the income statement.
When financial fraud is revealed, this asset is destroyed suddenly and on a large scale. What makes fraud so damaging, according to Karpoff et al. (2008a), is the nature of the signal it sends. A disclosed fraud does not simply tell stakeholders that management made a past mistake, it tells them that the firm's information was deliberately falsified. This is a fundamentally different message, because it implies intention. If management was willing to deceive investors and regulators through the firm's official financial statements, stakeholders have no strong reason to believe that commitments

made in other areas of the business are any more reliable. The damage therefore spreads far beyond the specific transactions that were manipulated. Karpoff et al.'s analysis of 585 U.S. enforcement cases shows that the reputational penalty, the stock price loss linked to trust erosion, after subtracting all direct legal costs, is on average 7.5 times larger than regulatory fines and legal settlements combined. This is the central finding that motivates the present study: the main economic cost of fraud is not the regulatory punishment, but the broad repricing of the firm's trustworthiness across all its stakeholder relationships at once.

The question of why this damage persists over time is where Reputational Capital Theory makes its most important contribution to this study's design. Unlike financial capital, which can be raised quickly through new debt or equity, trust cannot be bought or restored through a single action, it builds slowly through repeated honest behavior and collapses quickly through one clear betrayal (Karpoff, 2012). What makes recovery even harder is that stakeholders face a genuine uncertainty problem after fraud is detected. They cannot directly observe whether the governance failures that allowed the fraud to happen have been truly fixed. Beasley (1996) shows that board composition weaknesses are systematically linked to financial statement fraud, and Jensen and Meckling (1976) demonstrate that information asymmetry between managers and outside parties is a built-in feature of corporate structures, not something that disappears simply because new managers are appointed. Because stakeholders cannot verify whether real change has occurred, they rationally continue to apply protective conditions to their dealings with the firm, higher interest rates, stricter contract terms, greater scrutiny, until the firm has demonstrated, over a long enough period, that it can be trusted again. The result is that the financial penalties of fraud do not fade quickly. They persist across the full post-fraud period, which is exactly the time horizon this study examines.

This persistence is further reinforced by the breakdown of the information environment around the firm. Ali et al. (2023) show that financial analysts, who normally play a key role in reducing information asymmetry between firms and capital markets, are most effective at processing broad, industry-level information. In the specific case of firm-level fraud, however, their ability to interpret and transmit reliable information about the firm is severely weakened, because the firm's own disclosures can no longer be taken at face value. Gande and Lewis (2009) document what this means in practice: following SEC enforcement announcements, bid-ask spreads widen significantly and return volatility increases, reflecting a general loss of confidence in the firm's information. As analyst coverage falls and institutional investors pull back, the firm faces a sustained increase in its cost of capital that operates on top of the direct stakeholder penalties described below.

These penalties flow through three channels, each of which is linked directly to one of the dependent variables in this study. In capital markets, Graham, Li, and Qiu (2008) show that firms involved in accounting misreporting face higher loan spreads, tighter financial covenants, and shorter debt maturities in the years after detection. These financing constraints raise interest expenses, reduce net income, and compress Return on Equity, both by lowering the earnings available to shareholders and by limiting the firm's ability to use leverage strategically. In product and supplier markets, fraud revelation leads to a deterioration in the firm's commercial relationships. Farber (2005) documents a contraction in business activity following fraud disclosure, and the COSO (2010) report finds an average 35% decline in sales growth within two years of detection among restating firms. Kedia and Philippon (2009) further document significant post-revelation contractions in operating margins, as customers either leave for more trustworthy competitors or demand price reductions, while suppliers tighten payment terms and raise input costs. The combined effect is a persistent compression of Net Profit Margin, the metric most directly sensitive to the quality of these commercial relationships. In the internal labor market, Desai, Hogan, and Wilkins (2006) find that more than 90% of executives implicated in fraud leave their positions in the years following detection. This turnover destroys accumulated firm-specific knowledge and forces remaining management to spend time and resources on legal defense, governance reform, and reputation rebuilding rather than on running the business. The result is a decline in the productive use of the firm's assets, which shows up as a reduction in Return on Assets.

Collectively, these three channels generate a clear and testable prediction: firms that experience a public fraud revelation should show a sustained decline in profitability across all three dimensions, ROE compressed through higher financing costs, NPM eroded through weaker commercial relationships, and ROA reduced through operational disruption. This prediction does not emerge from the data arbitrarily, it follows directly from the economic logic of trust destruction and the difficulty of rebuilding it, as described by Reputational Capital Theory. It is this prediction that the main hypothesis of this study, developed in Section 2.3, is designed to test empirically.

2.2 Consequences of Financial Fraud

Financial fraud is not merely an accounting anomaly; it represents a profound economic and institutional disruption. Beyond the immediate act of misrepresentation, fraud damages the informational foundations on which capital markets and stakeholder relationships rest. The consequences are multidimensional, encompassing short-term market reactions, long-term operational and financial deterioration, and persistent erosion of profitability, the ultimate indicator of a firm's capacity to create sustainable value.

Scholarly and regulatory investigations (e.g., Karpoff, Lee & Martin, 2008a; Beasley, 1996; COSO, 2010) converge on the view that fraud imposes a triadic cost: market-based, reputational, and organizational. Each of these channels interacts dynamically, amplifying the overall damage.

Market-Based Consequences (short-term market reactions)

When financial fraud becomes public, financial markets react quickly and severely. Investors who had been relying on the firm's reported figures to make decisions suddenly find that the information they trusted was false, and they revise their assessment of the firm's value downward almost immediately. Feroz, Park, and Pastena (1991), in one of the earliest studies to use SEC Accounting and Auditing Enforcement Releases as an identification tool, found that stock prices dropped by an average of 9.5% around the time of enforcement announcements. Later work by Dechow, Sloan, and Sweeney (1996) and Karpoff, Lee, and Martin (2008a) found that cumulative abnormal losses could reach 20 to 30% or more, particularly when the fraud involved core earnings figures or had been running for several years. Palmrose, Richardson, and Scholz (2004) further show that the severity of the market reaction depends on the nature of the restatement, frauds that touch revenue recognition or involve outright falsification of figures tend to trigger much sharper price corrections than technical accounting adjustments, because they signal a deeper and more deliberate breach of investor trust.

What makes these market reactions so severe is not just the financial magnitude of the misreporting but the informational collapse that the revelation triggers. Capital markets function efficiently only when investors can rely on the information that firms provide. When fraud is uncovered, it does not only invalidate the specific figures that were manipulated, it raises doubts about everything the firm has communicated. This dynamic is well illustrated by research on how firms use language and tone in their public communications. Henry (2008) shows that the tone of earnings press releases meaningfully influences how investors interpret financial results, with more optimistic language generating more positive market reactions even after controlling for actual performance. Building on this, Huang, Teoh, and Zhang (2014) demonstrate that managers actively use language to shape investor perceptions in ways that are not always justified by fundamentals, and that unusually optimistic communication tends to predict negative future returns as the gap between perception and reality eventually closes. When fraud is detected, investors are forced to reinterpret not just past financial statements but the entire history of the firm's public communications, a process that amplifies the initial price correction as the full extent of the informational distortion becomes clear.

The role of financial analysts in this process deserves particular attention. Luo et al. (2015) show that analysts serve as important intermediaries between firms and capital markets, processing and transmitting information in ways that shape how investors respond to corporate performance signals. Under normal conditions, favorable analyst coverage helps translate positive firm performance into higher stock valuations. When fraud is revealed, however, this intermediation mechanism breaks down in two directions simultaneously. Analysts who had been covering the firm find that the information they relied on was corrupted, which damages their own credibility with investors. At the same time, the firm loses access to the positive analyst attention that had been supporting its valuation, as coverage drops and remaining analysts adopt a more skeptical stance. The result is a compounding of the direct valuation loss with a loss of informational support that further depresses the firm's market standing.

The audit failure that typically underlies fraud revelation adds a further dimension to the market reaction. Bhaskar et al. (2019) demonstrate that auditors can be influenced to avoid last-minute audit adjustments when management has already pre-released earnings figures, reflecting how client-side pressure compromises the independence of the audit process. When fraud becomes public, investors learn not only that management deceived them but that the external audit process, their primary independent check on the reliability of financial reporting, also failed to catch or correct the problem. This dual failure, of management integrity and of audit oversight simultaneously, sends a particularly strong negative signal about the firm's entire governance environment, and helps explain why the initial market reaction to fraud is typically far more severe than the reaction to a comparable non-fraudulent restatement of equivalent financial magnitude.

The regulatory environment surrounding financial fraud in the United States changed significantly during the period covered by this study. Following the Enron and WorldCom scandals, the Sarbanes-Oxley Act (SOX) was enacted in 2002, introducing major reforms including the personal certification of financial statements by CEOs and CFOs under criminal liability, stricter auditor independence requirements, the creation of the Public Company Accounting Oversight Board (PCAOB), and heavier criminal penalties for securities fraud. These measures were designed to make fraud harder to commit and easier to detect. Although the evidence on whether SOX effectively reduced fraud occurrence is mixed, it represents the most important regulatory change in U.S. financial markets within the sample period (Dechow et al., 2011). Crucially, however, SOX does not eliminate the consequences of fraud once it becomes public. The profitability damage documented in this study occurs at and after the point of revelation; a process driven by reputational destruction and stakeholder responses that unfolds regardless of the regulatory framework in place.

These findings confirm that financial markets act swiftly and decisively when fraud is detected, imposing large and immediate valuation penalties that reflect not only the correction of past misreporting but the broader collapse of the informational and institutional trust on which the firm's market valuation depended. These short-term market consequences are important in their own right, but they also set the stage for the longer-term operational and financial deterioration examined in the following section, because the same loss of trust that drives the initial stock price decline is what subsequently reshapes the firm's relationships with creditors, customers, suppliers, and employees in ways that suppress profitability for years to come.

Operational Consequences and Long-Term Financial Performance

The reputational damage caused by fraud revelation does not stay abstract for long; it quickly translates into concrete operational and financial difficulties that affect how the firm runs its business on a daily basis. The empirical literature consistently shows that fraud-affected firms struggle with performance problems that last well beyond the initial disclosure, reflecting genuine and durable harm to their ability to operate and generate profit.

The most immediate operational impact is felt in the firm's relationship with capital providers. When fraud becomes public, it confirms to lenders and investors that the firm's governance environment failed, that those responsible for oversight did not do their job, and that management had both the incentive and the opportunity to act against the interests of outside stakeholders. Castro et al. (2020) show that creditors are highly sensitive to the quality of governance surrounding a borrower: when they perceive that executives are taking on excessive risk without adequate oversight, they respond by imposing tighter and more restrictive debt structures to protect their own position. A fraud event makes these concerns concrete rather than hypothetical, triggering a fundamental reassessment of the firm's creditworthiness that goes far beyond a temporary increase in interest rates. The firm's entire financial structure is repriced, reducing its room to maneuver and limiting its ability to invest in the activities needed to recover its competitive position. This financial rigidity becomes a compounding problem, the firm needs resources to rebuild, but the fraud has made those resources harder and more expensive to obtain, directly compressing Return on Equity as higher interest expenses reduce the earnings available to shareholders.

Banking relationships are disrupted in subtler ways as well. Ertan (2021) examines how financial institutions behave when they deal with borrowers they consider risky, and finds that banks under pressure to manage their own earnings actively adjust both the timing and pricing of loans extended to suspect borrowers, accelerating originations when it suits their own reporting needs and imposing pricing terms that reflect the heightened counterparty risk they associate with the firm. For fraud-affected firms, this dynamic is particularly damaging: rather than receiving the flexible and competitively priced financing that a recovering firm needs, they face a lending environment shaped primarily by their financial partners' self-interest. The result is a sustained increase in the cost and difficulty of obtaining the working capital and investment financing that normal business operations require, further suppressing net income and eroding profitability over the medium to long term.

A less commonly discussed but equally significant consequence is the deterioration of the firm's relationship with tax and regulatory authorities. Finley (2019) finds that firms with a history of financial misreporting face much closer scrutiny in subsequent regulatory interactions, as authorities rationally conclude that a firm willing to manipulate its public financial statements may also adopt aggressive positions in other reporting contexts. This leads to more frequent audits, stronger challenges to previously accepted accounting and tax positions, and significant administrative costs associated with managing these interactions. While these costs may not be large enough individually to appear prominently in any single line of the income statement, they accumulate over time and contribute meaningfully to the gradual erosion of Net Profit Margin in the years after fraud is detected, adding a layer of financial burden that operates entirely independently of the firm's commercial relationships.

The internal governance consequences are equally persistent and harder to resolve than they might initially appear. Beyond the executive turnover that follows fraud revelation, the firm faces the much harder challenge of rebuilding a credible governance structure from a position of severe reputational disadvantage. Fan and Yu (2022) show that effective monitoring relationships within firms take considerable time to develop and depend on a level of institutional trust and stability that fraud directly destroys. Rebuilding these relationships (with board members, regulators, and external auditors) and establishing reformed control systems, is a slow and expensive process that absorbs significant managerial time and organizational resources, diverting attention away from the operational and commercial activities that generate profit.

The audit relationship adds another layer of complexity to this picture. Bhaskar et al. (2019) show that audit quality is significantly shaped by the institutional context surrounding the engagement, and that strong audit committee oversight is what prevents auditors from making overly accommodating judgments under client pressure. In the post-fraud context, the firm almost always replaces its auditor as part of its reform effort, but the incoming auditor arrives with no established knowledge of the firm's operations and faces the difficult task of signaling independence and rigor to a market that is already skeptical. Francis et al. (2022) add to this by demonstrating that disruptions in the established auditor-client relationship tend to reduce audit quality, suggesting that the very governance reforms the firm undertakes to restore credibility may inadvertently sustain informational uncertainty in the short to medium term rather than resolve it.

What emerges from this body of evidence is a picture of compounding and mutually reinforcing operational difficulties. Tighter and more expensive financing limits the firm's ability to invest in recovery, while disrupted banking relationships make financial planning unpredictable. At the same time, heightened regulatory scrutiny adds compliance costs that quietly erode margins, and the effort required to rebuild governance structures absorbs managerial time and resources that would otherwise go toward generating revenue. The disruption of established audit relationships further sustains the informational uncertainty that keeps the cost of capital elevated long after the initial scandal. None of these effects is catastrophic in isolation, but together they create a prolonged period of operational inefficiency and financial pressure that shows up clearly in the firm's profitability metrics, not because historical figures are being corrected, but because the firm's genuine capacity to generate earnings from its ongoing activities has been seriously and durably weakened. This sustained deterioration across Return on Assets, Return on Equity, and Net Profit Margin is precisely what this study sets out to measure and quantify.

2.3 Hypothesis Development

Background and Conceptual Foundation

The literature reviewed in the preceding sections establishes two things clearly. First, fraud imposes large and immediate market penalties that are well documented and relatively well understood. Second, the theoretical logic of Reputational Capital Theory predicts that the damage does not stop there, it continues to reshape the firm's relationships with creditors, commercial partners, and employees in ways that should show up in profitability metrics for years after the initial disclosure. What the literature has not yet provided, however, is robust longitudinal empirical evidence that directly links this reputational destruction to sustained and measurable deterioration in accounting-based performance.

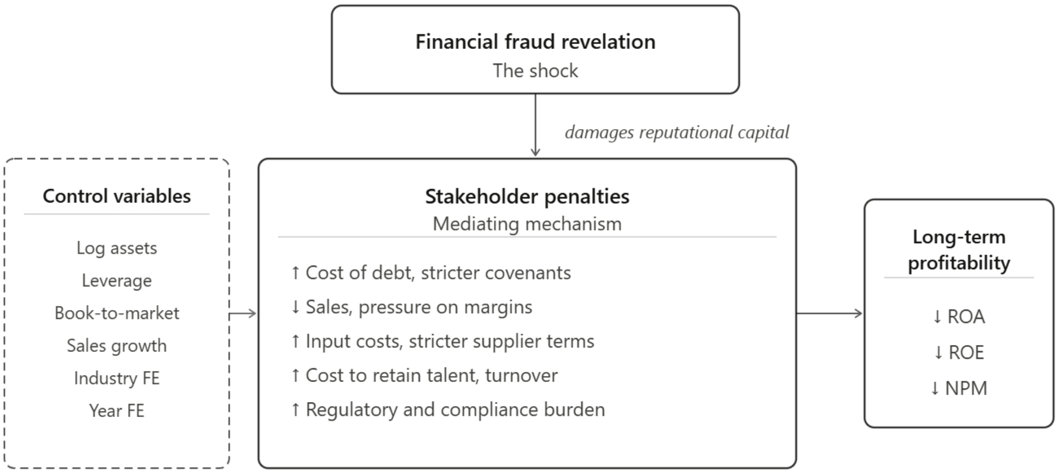
This gap is significant for two reasons. Studies that rely on stock price reactions capture the market's forward-looking expectations at a specific point in time, but expectations are shaped by many factors beyond the firm's operational reality, investor sentiment, market conditions, and uncertainty about the eventual severity of legal consequences all influence how prices adjust. Accounting-based measures such as Return on Assets, Return on Equity, and Net Profit Margin are different in nature: they reflect what the firm actually earned, from its actual operations, over a specific period. If the reputational damage predicted by theory is real and persistent, it should be visible not just in stock prices at the moment of disclosure but in the firm's income statement year after year in the post-fraud period. The existing literature has not systematically tested this, and the few studies that examine post-fraud operating performance do so over short windows or without an adequately constructed comparison group.

This study is designed to fill this gap directly. By following a sample of fraud firms and a carefully matched control group over a window of five years before and five years after fraud revelation, and by using a difference-in-differences framework to isolate the effect of fraud from firm-level and macroeconomic confounds, it provides the longitudinal accounting-based evidence that the literature currently lacks. The central question is whether the theoretical prediction of sustained profitability deterioration, grounded in Reputational Capital Theory and supported by the empirical channels documented in Section 2.2, is confirmed when subjected to rigorous empirical testing.

Development of the Main Hypothesis

The theoretical framework developed in Section 2.1.(Theoretical foundations) and the empirical evidence reviewed in Section 2.2 converge on a single, coherent prediction. Fraud revelation destroys a firm's reputational capital, triggering a coordinated response from all major stakeholder groups that raises financing costs, weakens commercial relationships, and disrupts internal operations. Because rebuilding trust is a slow and uncertain process, these pressures persist across an extended post-fraud horizon rather than resolving quickly. The cumulative effect is a sustained compression of the firm's profitability, not as a one-time correction of inflated past figures, but as a genuine and durable reduction in the firm's capacity to generate earnings from its ongoing operations.

This process is summarized in the illustration below, which shows the causal chain from fraud revelation through the destruction of reputational capital, the resulting stakeholder penalties, and the ultimate impact on the three profitability metrics examined in this study. Control variables: firm size measured as log assets, leverage, book-to-market ratio, sales growth, and industry and year fixed effects, are included to ensure that the observed performance differences between fraud and non-fraud firms reflect the effect of fraud rather than pre-existing structural differences between the two groups.



What makes this prediction theoretically grounded rather than simply intuitive is the precision with which Reputational Capital Theory maps the mechanisms of trust destruction onto specific financial outcomes. The elevated financing costs documented by Graham, Li, and Qiu (2008) flow directly into Return on Equity. The commercial relationship deterioration evidenced by Farber (2005) and Kedia and Philippon (2009) flows directly into Net Profit Margin. The operational disruption resulting from executive turnover documented by Desai, Hogan, and Wilkins (2006) flows directly into Return on Assets. These are not generic predictions of "worse performance", they are specific, channel-by-channel predictions that can be tested individually and collectively using accounting data.

Critically, the prediction is not that profitability will dip briefly and then recover. The theoretical logic of Reputational Capital Theory, as formalized by Karpoff (2012), implies that recovery is slow and uncertain, slow because trust must be rebuilt through repeated observable behavior over time, and uncertain because stakeholders cannot directly verify whether the governance failures that enabled fraud have been genuinely remediated. This means that the profitability effect should be observable not just in the first year after fraud revelation but across the full post-fraud window, which in this study runs from T+1 to T+5.

This leads to the following formal hypothesis:

H1: U.S. public companies that experience a public revelation of financial fraud exhibit a sustained decline in long-term profitability, as measured by Return on Assets, Return on Equity, and Net Profit Margin, relative to a matched control group of non-fraud firms.

The corresponding null hypothesis, which this study's empirical design is constructed to evaluate, is:

H0: Financial fraud does not lead to any significant difference in long-term profitability between fraud firms and comparable non-fraud firms.

To test these propositions, the empirical design detailed in Chapter 3 employs a difference-in-differences framework with entropy-balanced control firms, controlling for firm size, leverage, book-to-market ratio, and sales growth, alongside industry and year fixed effects, with standard errors clustered at the firm level to account for serial correlation within firms over time.

3. Research Design and Methodology

3.1 Research Design and Approach

This study adopts a longitudinal quasi-experimental research design to assess the causal impact of financial fraud on the long-term profitability of U.S. publicly listed companies. The empirical strategy is grounded in a Difference-in-Differences (DiD) framework, which compares the evolution of profitability metrics between firms that experienced a public revelation of financial fraud (treatment group) and a carefully constructed group of non-fraudulent firms (control group) over an eleven-year window spanning five years before and five years after the fraud revelation (T–5 to T+5).

The choice of a DiD design is motivated by its ability to isolate the causal effect of fraud exposure by controlling for time-invariant firm characteristics and common macroeconomic trends that affect all firms simultaneously. This approach is particularly well-suited to the research question, as it allows the study to distinguish the specific performance trajectory of fraud firms from broader industry and market dynamics. The treatment is defined as the public revelation of fraud through an SEC Accounting and Auditing Enforcement Release (AAER), and the event year T corresponds to the first AAER publication date associated with each firm.

To ensure the comparability of treatment and control groups prior to the fraud event, entropy balancing (Hainmueller, 2012) is applied as a preprocessing step. This reweighting technique ensures that the control group mirrors the treatment group on key pre-fraud characteristics, thereby strengthening the internal validity of the DiD estimates. The empirical analysis is complemented by a placebo test, an event study, five robustness checks on model specification, and a comprehensive set of heterogeneity analyses examining the moderating role of fraud severity, commercial momentum, pre-fraud financial performance, firm size, leverage, valuation profile, industry-relative performance, and market environment.

3.2 Data Collection

Fraud identification (AAER Data)

Fraud cases are identified using the SEC Accounting and Auditing Enforcement Releases (AAERs), a publicly available database maintained by the U.S. Securities and Exchange Commission that documents enforcement actions taken against firms and individuals for violations of securities laws and accounting standards. AAERs are widely used in the academic literature as a reliable and objective source for identifying instances of financial misreporting (Dechow, Ge, Larson & Sloan, 2011).

Because AAERs include enforcement actions against both firms and individuals, a cleaning procedure was applied to retain only company-level observations. Firm names were extracted using string-matching techniques and subsequently matched to company identifiers in Refinitiv Eikon. The fraud event year T is defined as the year of the first AAER publication date associated with each firm, consistent with the approach adopted in prior studies. After this cleaning process, a final sample of 167 fraud firms was identified.

Sample of U.S. public companies (Eikon)

Financial and accounting data for both fraud and control firms were collected from LSEG Workspace (Refinitiv Eikon) using TR() formula-based extraction. For each firm and each year within the study window, the following variables were retrieved: net income (TR.NetIncome), total assets (TR.TotalAssetsReported), total equity (TR.TotalEquity), total revenue (TR.Revenue), total debt (TR.TotalDebt), current assets (TR.CurrentAssets), current liabilities (TR.CurrentLiabilities), market capitalization (TR.CompanyMarketCap), and industry classification code (TR.TRBCIndustryCode). Industry codes are defined at the two-digit TRBC level and are used to construct industry fixed effects in the regression models, as recommended by the thesis supervisor.

The sample covers fraud revelations occurring between 2000 and 2023, with accounting data retrieved over the observation window T-5 to T+5, spanning calendar years 2000 to 2024.

Control sample construction and Entropy Balancing

The control sample was constructed in two stages. In the first stage, 133 non-fraudulent U.S. public firms were initially selected as candidate controls. To increase the size and representativeness of the control pool, an additional 174 large U.S. public firms were added, bringing the total candidate control pool to 307 firms. Of these 307 candidate control firms, 33 were excluded by the data sufficiency filter for having fewer than four non-missing observations on either side of the event window, leaving a final control sample of 274 firms. For firms in this expanded control group that lacked a natural fraud year reference, the post-fraud indicator was assigned based on the median fraud year of the treatment sample, which corresponds to 2011. While this assignment is necessarily arbitrary, year fixed effects in all specifications absorb any systematic differences in profitability trends across calendar years, mitigating the concern that the choice of pseudo-event year drives the results. This convention is standard in DiD designs where control units lack a natural event date, and ensures that the pre/post structure of the panel is preserved consistently across all observations.

A data sufficiency filter was then applied to retain only firms with at least four non-missing pre-fraud observations and at least four non-missing post-fraud observations for the primary outcome variable ROA. This filter ensures that the panel is sufficiently balanced to support reliable DiD estimation, firms observed for fewer than four years on either side of the event window would contribute insufficient information to identify meaningful pre- and post-fraud trends. After applying this filter, the final analytic sample comprises 89 fraud firms and 274 control firms.

To ensure pre-treatment comparability between the two groups, entropy balancing was applied using the WeightIt package in R. Entropy balancing is a preprocessing reweighting technique that adjusts the weights of control units such that the weighted moments of the control group match those of the treatment group (Hainmueller, 2012). Unlike propensity score matching, entropy balancing achieves exact moment balancing by construction, without discarding any observations, which makes it particularly attractive in settings where the treatment group is small. The balancing was performed on two pre-fraud firm characteristics: the average pre-fraud ROA and the average pre-fraud log of total assets, both computed over the pre-fraud period (T–5 to T–1). The decision to balance on only two variables was deliberate, including additional balancing variables reduced the number of eligible fraud firms from 89 to 63, representing an unacceptable loss of statistical power given the already limited sample size.

Under the entropy balancing scheme, fraud firms retain a weight of one, while control firms are reweighted to achieve distributional balance. The effective sample size of the reweighted control group is 41.04, reflecting the degree of concentration of the balancing weights among control firms, a lower effective N indicates that a smaller subset of control firms carries most of the weight in the reweighted sample, which is an expected and acceptable feature of entropy balancing when the treatment group is relatively small. For control firms for which entropy balancing weights could not be directly computed due to missing pre-fraud data, the mean weight of the remaining control firms was assigned to preserve their inclusion in the analysis. After reweighting, the mean pre-fraud ROA of the control group (0.036) closely matches that of the fraud group (0.034), and the mean pre-fraud LogAssets of the control group (22.2) closely matches that of the fraud group (22.4), confirming that the balancing procedure was successful.

It should be noted that while entropy balancing achieves exact balance on pre-fraud ROA and LogAssets, pre-existing differences in Leverage ($p = 0.002$) remain unaddressed by the weighting procedure. These differences are controlled for through the inclusion of Leverage as a time-varying covariate in all regression specifications.

3.3 Variables

Dependent (ROA, ROE, NPM)

Three accounting-based profitability measures serve as the dependent variables in this study, each capturing a distinct dimension of firm financial performance.

Return on Assets (ROA) is defined as net income divided by total assets and measures how efficiently a firm generates profit from its asset base. A sustained decline in ROA following fraud revelation would indicate that the firm's productive capacity has been impaired, consistent with the operational deterioration documented by Kedia and Philippon (2009).

Return on Equity (ROE) is defined as net income divided by total equity and captures the returns generated for shareholders. ROE is particularly sensitive to changes in the cost of debt and financial leverage, making it a natural candidate for detecting the financing channel through which reputational damage operates (Graham, Li & Qiu, 2008).

Net Profit Margin (NPM) is defined as net income divided by total revenue and reflects the firm's ability to convert sales into profit after accounting for all costs. A persistent decline in NPM would be consistent with rising operational costs, customer defection, and supplier pressure in the post-fraud period (Farber, 2005).

All three dependent variables are winsorized prior to analysis to limit the influence of extreme outliers on the regression estimates, which is standard practice in empirical accounting research (e.g., Dechow et al., 2011). The winsorization bounds are chosen to be wide enough to retain meaningful variation in the data while eliminating implausible values that are likely driven by data errors or exceptional firm circumstances unrelated to the fraud event: ROA is winsorized at [–1, 1], ROE at [–5, 5], and NPM at [–2, 2]. The same winsorization logic is applied to all control variables, with bounds selected on the same basis: Leverage at [0, 5], CurrentRatio at [0, 20], BookToMarket at [–5, 5], and SalesGrowth at [–1, 5].

Independent variables

The primary independent variable of interest is an interaction term between a fraud indicator and a post-fraud period indicator, which constitutes the core DiD estimator. The fraud indicator (fraud_status) takes a value of one for all firm-year observations belonging to fraud firms and zero otherwise. The post-fraud indicator (post_fraud) takes a value of one for years T+1 through T+5 and zero for the pre-fraud period. The interaction term fraud_status × post_fraud thus captures the differential change in profitability experienced by fraud firms in the post-revelation period, relative to the control group and relative to the pre-fraud period, the standard DiD estimator of the average treatment effect on the treated.

Control variables

Four time-varying firm-level control variables are included in the regression models to account for confounding factors that may influence profitability independently of fraud.

Firm size, measured as the natural logarithm of total assets (LogAssets), controls for the well-documented relationship between firm scale and profitability. Financial leverage, measured as total debt divided by total assets (Leverage) and winsorized at [0, 5], is included as higher leverage levels are associated with greater financial risk and lower profitability. The book-to-market ratio (BookToMarket), defined as total equity divided by market capitalization and winsorized at [–5, 5], controls for differences in firm valuation and growth opportunities. Higher values identify value firms whose market valuation is close to book value, while lower values characterize growth firms whose market valuation commands a significant premium over book value. Finally, sales growth (SalesGrowth), defined as the year-on-year percentage change in revenue and winsorized at [–1, 5], controls for the firm's commercial momentum, which may independently affect profitability trends. The current ratio (CurrentRatio), defined as current assets divided by current liabilities and winsorized at [0, 20], was considered but excluded from the main specification as its inclusion

reduces the analytic sample by approximately 35%, representing an unacceptable loss of observations.
All models additionally include industry fixed effects at the two-digit TRBC level and year fixed effects to absorb industry-specific and time-specific variation in profitability.

3.4 Econometric Model

Difference-in-Differences model

The main econometric specification takes the following form:
Profitability_it = α + β(fraud_status_i × post_fraud_t) + γX_it + δ_industry + λ_t + ε_it
Where **Profitability_it** denotes one of the three dependent variables (ROA, ROE, or NPM) for firm **i** in year **t**. The coefficient **β** is the parameter of primary interest and captures the average treatment effect of fraud on the treated firms in the post-revelation period, relative to the control group. **X_it** is a vector of time-varying firm-level controls including LogAssets, Leverage, BookToMarket, and SalesGrowth. **δ_**industry denotes industry fixed effects at the two-digit TRBC level, and **λ_t** denotes year fixed effects. Standard errors are clustered at the firm level throughout, following the recommendation of Bertrand, M., Duflo, E., & Mullainathan, S. (2004) to account for serial correlation in the error term within firms over time. All models are estimated using the fixest package in R, which supports high-dimensional fixed effects with clustered standard errors.
Two model specifications are estimated. Model 1 includes only LogAssets as a control variable alongside the DiD interaction term and fixed effects. Model 2 extends Model 1 by adding Leverage, BookToMarket, and SalesGrowth as additional controls. Both specifications are reported to assess the sensitivity of the findings to the inclusion of additional controls.

Identification challenges & solutions

The DiD design rests on the parallel trends assumption, which requires that, in the absence of fraud, the profitability of fraud firms and control firms would have followed the same trajectory over time. While this assumption is untestable in its strict form, several design choices and diagnostic tests are employed to support its plausibility.
First, entropy balancing ensures that the two groups are comparable on pre-fraud ROA and firm size, which are the strongest predictors of future profitability trends. Second, a placebo test is conducted by artificially shifting the treatment year backward and re-estimating the DiD model on the pre-fraud period only, to verify that no differential pre-trend exists between the two groups. Third, an event study is conducted to trace the year-by-year evolution of the treatment effect around the fraud revelation date, allowing for a visual inspection of pre-trend behavior and post-fraud dynamics. Fourth, standard errors are clustered at the firm level throughout to address the serial correlation concern raised by Bertrand, M., Duflo, E., & Mullainathan, S. (2004), which is particularly relevant in panel data settings with repeated observations per firm.

Sensitivity / Robustness tests

The robustness of the main findings is assessed through a comprehensive series of specification checks and heterogeneity analyses, designed to verify that the results are not sensitive to methodological choices and to illuminate the mechanisms through which fraud affects long-term profitability.
Placebo Test The parallel trends assumption is examined through a placebo DiD estimated exclusively on pre-fraud observations, in which the post-fraud indicator is artificially assigned to the second half of the pre-fraud window. A non-significant placebo coefficient would confirm that fraud and control firms did not follow divergent profitability trajectories prior to the fraud revelation, supporting the validity of the identifying assumption.
Event Study To complement the average treatment effect produced by the DiD estimator, an event study traces the year-by-year evolution of the profitability gap between the two groups around the revelation date. This allows for a visual inspection of pre-trend behavior as well as the timing and

persistence of any post-fraud effect, providing richer dynamic evidence than the pooled DiD coefficient alone.

Robustness Checks on Model Specification Five specification checks are conducted to assess whether the main findings hold under alternative methodological choices. Industry fixed effects are first replaced by firm-level fixed effects to absorb all time-invariant firm characteristics and impose a more stringent control for unobserved heterogeneity. The current ratio is then introduced as an additional control, despite its exclusion from the main specification due to the approximately 35% reduction in sample size it entails, to confirm that omitting this liquidity measure does not bias the estimates. The post-fraud window is subsequently narrowed to T+1 through T+3 to examine whether the effect is concentrated in the early post-revelation years or persists over the full five-year horizon. Heteroskedasticity-robust standard errors are further substituted for clustered standard errors to ensure that the significance of the results does not depend on the choice of inference method. Finally, an alternative winsorization scheme based on the 1st and 99th sample percentiles replaces the economic-bound approach used in the main specification, to verify that the findings are not driven by the treatment of extreme values.

Fraud Severity Analysis To examine whether the magnitude of fraud conditions its long-term profitability impact, a firm-level severity measure is constructed as follows:

FraudSeverity_i = (Avg NI_{T-2, T-1} - NI_{T+1}) / Avg TotalAssets_{T-2, T-1}

This measure captures the drop in net income immediately following fraud revelation, scaled by the firm's average pre-fraud asset base. Based on the sample median, firms are classified as high or low severity, yielding 45 and 44 firms respectively. Since this measure relies on net income, which also enters the profitability outcomes, the analysis is restricted to the post-fraud window T+2 through T+5 to avoid mechanical correlation between the severity measure and the dependent variables. Control firms are assigned a severity value of zero and serve as the baseline group in the regression.

<u>Growth Heterogeneity Analysis</u> Whether the profitability impact of fraud varies with commercial momentum is tested by splitting the sample based on average pre-fraud sales growth, computed over T-5 to T-1. The sample median of 5.27% serves as the classification threshold, yielding 39 high-growth and 39 low-growth firms, for each of which a separate DiD regression is estimated against the full control sample using the same specification as the main model. This analysis draws on the expectation that firms with stronger pre-fraud growth may be better equipped to rebuild stakeholder trust and absorb the costs of fraud revelation, while low-growth firms may face more persistent deterioration given their limited capacity to recover.

<u>DuPont Decomposition</u> A more granular view of the transmission channel is obtained by decomposing return on equity into its three constituent components, net profit margin, asset turnover, and the equity multiplier, and testing each separately as a dependent variable. This approach aims to determine whether the ROE effect is primarily driven by margin compression, reduced operational efficiency, or shifts in financial leverage.

<u>Speed of Recovery Analysis</u> To complement the DiD estimates with a firm-level perspective on persistence, the share of firms that have recovered to their average pre-fraud ROE baseline is tracked for each post-fraud year from T+0 to T+5. This approach provides a direct and intuitive measure of how durably fraud damages profitability and whether recovery, where it occurs, is gradual or non-linear across the observation window.

<u>Pre-Fraud Performance as a Moderator</u> Using the sample median of 11.49% as the threshold, the treatment sample is divided into high and low pre-fraud ROE subgroups based on average return on equity over T-5 to T-1, yielding two equally sized groups of approximately 45 firms each. A triple interaction between fraud status, post-fraud period, and the high pre-fraud ROE indicator is then introduced into the regression to test whether prior financial strength moderates the post-fraud impact. The underlying hypothesis is that firms entering the post-revelation period from a position

of financial strength possess a greater capacity to absorb reputational damage and sustain stakeholder confidence than those already operating from a position of weakness.

Size Heterogeneity Analysis Using the median pre-fraud log of total assets of 21.96 as the classification threshold, the sample is divided into large and small subgroups, and separate DiD regressions are estimated for each group against the full control sample. This analysis tests whether firm size, as a broad proxy for financial resources, market visibility, and access to capital markets, conditions the severity of the long-term profitability impact.

Leverage Heterogeneity Analysis The financial channel hypothesis is tested more directly by classifying the treatment sample into high and low leverage subgroups based on a median pre-fraud leverage ratio of 16.98% and estimating separate DiD regressions for each group. The underlying expectation is that firms already carrying significant debt before the fraud revelation are disproportionately exposed to the compounded financing constraints that arise post-fraud, as creditors and investors simultaneously price in existing indebtedness and newly revealed reputational risk.

Book-to-Market Heterogeneity Analysis A median pre-fraud book-to-market ratio of 0.168 is used to classify the sample into value firms, whose market valuation sits close to book value, and growth firms, whose market valuation commands a significant premium over book value. Separate DiD regressions are estimated for each group to examine whether the firm's valuation profile and its implied financial flexibility moderate the post-fraud profitability impact.

Industry-Relative Performance Analysis Rather than relying on absolute pre-fraud profitability, each firm's ROE is benchmarked against the average ROE of its industry peers in each year over T-5 to T-1. Based on the sample median of this relative measure, the treatment sample is split into 45 outperformers and 44 underperformers, and separate DiD regressions are estimated for each group against the full control sample. This provides a more refined test of the vulnerability hypothesis, controlling for sector-level profitability differences and isolating the role of relative competitive standing as a determinant of post-fraud resilience.

Pre/Post Financial Crisis Analysis As an exploratory complement to the main analyses, the sample is split according to whether the fraud revelation occurred before or after the 2008 financial crisis, yielding 32 pre-crisis and 68 post-crisis firms. The motivation is that revelations occurring in a more trusting pre-crisis environment may generate a sharper reputational shock than those occurring after 2008, when elevated investor scepticism and regulatory scrutiny had already altered the landscape. Given the limited size of the pre-crisis subsample, this analysis is presented as suggestive and intended to open avenues for future research rather than deliver a definitive empirical finding.

Two additional heterogeneity analyses were considered but ultimately not conducted. An industry-level split was deemed infeasible given that 89 fraud firms distributed across multiple two-digit TRBC industry codes would produce subgroups too small for reliable statistical inference. A combined low-growth and high-severity interaction was also explored but yielded only 18 fraud firms in the target subgroup, which was considered insufficient to support meaningful inference. Both decisions reflect a deliberate preference for analytical conservatism over the risk of reporting results driven by sampling noise.

3.5 Software & Statistical Tools (R)

The empirical analysis is conducted entirely in R. Data extraction from LSEG Workspace (Refinitiv Eikon) was performed using TR() formula-based queries in Excel, with manual cleaning applied where necessary. Panel construction, variable computation, winsorization, and entropy balancing are implemented in R using the readr, dplyr, and WeightIt packages. All DiD regressions and robustness tests are estimated using the fixest package, which provides efficient estimation of high-dimensional fixed effects models with clustered standard errors. Results are exported and formatted using the openxlsx and ggplot2 packages for tabular and graphical presentation respectively.

4. Empirical Results

This chapter presents the empirical results of the difference-in-differences analysis examining the long-term impact of financial fraud on firm profitability. Section 4.1 describes the characteristics of the sample. Section 4.2 reports the main estimation results. Section 4.3 presents the identification tests supporting the causal interpretation of the findings. Section 4.4 reports robustness checks. Section 4.5 presents additional analyses exploring the mechanisms underlying the main effect. Section 4.6 presents heterogeneity analyses examining the boundary conditions of the effect across firm subgroups.

4.1 Descriptive Statistics

Before turning to the regression results, we examine the characteristics of fraud and control firms in our sample. Panel A of Table 1 reports summary statistics for the 89 fraud firms and Panel B for the 274 control firms, over the full T-5 to T+5 observation window.

Table 1: Descriptive Statistics

Panel A: Fraud Firms (N=89)

Variable	P25	Median	P75	Mean	SD	Obs	Firms
ROA	0.008	0.036	0.083	0.035	0.101	1,022	89
ROE	0.049	0.110	0.182	0.088	0.325	1,020	89
NPM	0.021	0.063	0.129	0.075	0.227	884	89
LogAssets	20.720	22.213	23.998	22.453	2.511	1,049	89
Leverage	0.047	0.179	0.329	0.217	0.197	937	89
BookToMarket	0.083	0.197	0.446	0.392	0.658	992	89
SalesGrowth	-0.030	0.032	0.116	0.067	0.325	844	89

Panel B: Control Firms (N=274)

Variable	P25	Median	P75	Mean	SD	Obs	Firms
ROA	0.015	0.048	0.094	0.047	0.110	5,324	274
ROE	0.069	0.131	0.217	0.129	0.409	5,313	274
NPM	0.038	0.088	0.154	0.090	0.215	4,840	274
LogAssets	21.611	23.061	24.388	22.911	2.127	5,621	274
Leverage	0.124	0.247	0.376	0.266	0.202	4,798	274
BookToMarket	0.055	0.142	0.343	0.289	0.447	5,397	274
SalesGrowth	0.000	0.067	0.162	0.110	0.297	4,913	274

Note: This table reports descriptive statistics for fraud firms (Panel A) and control firms (Panel B) over the full sample period T-5 to T+5. ROA = Net Income / Total Assets. ROE = Net Income / Total Equity. NPM = Net Income / Total Revenue. LogAssets = natural logarithm of total assets. Leverage = total debt / total assets. BookToMarket = total equity / market capitalization. SalesGrowth = year-

on-year percentage change in revenue. All variables are winsorized as described in Section 3.3. The observation count exceeds 89 × 11 = 979 because fraud revelation dates are staggered across calendar years, resulting in variable panel lengths across firms. Differences across variables reflect missing values for specific accounting items.

The two groups exhibit broadly similar profiles across most dimensions, though several differences are worth noting. On profitability, fraud firms display a lower mean ROA of 0.035 compared to 0.047 for control firms, and a lower mean ROE of 0.088 versus 0.129. Net profit margins follow the same pattern, with fraud firms averaging 0.075 against 0.090 for control firms. These unconditional differences reflect the full observation window including post-fraud years, and should not be interpreted causally at this stage. On firm characteristics, fraud firms are modestly smaller, with a mean LogAssets of 22.5 relative to 22.9 for control firms, and carry lower leverage on average, with a mean ratio of 0.217 versus 0.266. The book-to-market ratio is somewhat higher for fraud firms (0.392 versus 0.289), suggesting that fraud firms trade at a lower market premium relative to their book value, consistent with the value firm profile identified in the heterogeneity analyses presented in Section 4.6.

Table 2 examines whether the two groups are comparable before the fraud revelation, which is the relevant pre-treatment window for the DiD design. Difference-in-means tests are restricted to pre-fraud observations to assess the degree of pre-treatment balance between fraud and control firms.

Table 2: Difference in Means - Pre-Fraud Period

Variable	Fraud Firms	Control Firms	Difference	t_stat	p_value
ROA	0.038	0.014	0.023	2.977	0.003
ROE	0.104	0.072	0.032	1.281	0.201
NPM	0.076	0.037	0.039	2.020	0.044
LogAssets	21.998	20.807	1.191	7.963	0.000
Leverage	0.228	0.275	-0.047	-3.140	0.002
BookToMarket	0.338	0.357	-0.020	-0.469	0.639
SalesGrowth	0.083	0.085	-0.002	-0.112	0.911

Note: This table reports difference-in-means tests comparing fraud and control firms during the pre-fraud period. t_stat reports the t-statistic and p_value the corresponding p-value.

The two groups are broadly comparable on the dimensions most relevant to the DiD design. The difference in pre-fraud ROE is 0.032 and statistically indistinguishable from zero ($p = 0.201$), providing reassurance that the two groups did not differ systematically on the primary outcome of interest before the fraud event. Similarly, no significant pre-treatment differences are detected for BookToMarket ($p = 0.639$) or SalesGrowth ($p = 0.911$). Statistically significant differences are however observed for ROA ($p = 0.003$), LogAssets ($p < 0.001$), and Leverage ($p = 0.002$), indicating that fraud firms tend to be larger and somewhat more profitable on an asset basis in the pre-fraud period. Net profit margin also shows a statistically significant pre-fraud difference ($p = 0.044$), with fraud firms averaging 0.076 compared to 0.037 for control firms, suggesting that fraud firms were somewhat more efficient at converting revenue into profit in the pre-fraud period. These pre-existing differences motivate the use of entropy balancing, as described in Section 3.2.c, which reweights the control group to match the fraud firms on pre-fraud ROA and LogAssets prior to estimation, ensuring that any post-fraud difference in profitability reflects the impact of fraud revelation rather than pre-existing compositional differences between the two groups.

4.2 Main DiD Results

Table 3 presents the main difference-in-differences estimates of the effect of fraud revelation on long-term firm profitability. Two model specifications are reported for each outcome variable. Model 1 includes only firm size as a control variable alongside the DiD interaction term and fixed effects, while Model 2 extends this baseline by adding leverage, book-to-market ratio, and sales growth as additional controls. Both specifications include industry fixed effects at the two-digit TRBC level and year fixed effects, with standard errors clustered at the firm level throughout.

Table 3: Main DiD Results

Variable	ROA (1)	ROA (2)	ROE (1)	ROE (2)	NPM (1)	NPM (2)
LogAssets	0.0058* (0.0027)	0.0094* (0.0038)	0.0127* (0.0060)	0.0143 (0.0092)	0.0041 (0.0082)	0.0055 (0.0092)
Leverage		-0.0820*** (0.0221)		0.2180. (0.1250)		-0.0149 (0.0884)
BookToMarket		0.0006 (0.0085)		-0.0605* (0.0294)		0.0201 (0.0195)
SalesGrowth		0.0481*** (0.0129)		0.0906* (0.0385)		0.0808** (0.0290)
fraud_status x post_fraud	-0.0014 (0.0087)	-0.0015 (0.0091)	-0.0534. (0.0276)	-0.0549. (0.0311)	-0.0093 (0.0180)	-0.0035 (0.0200)
FE Industry	Yes	Yes	Yes	Yes	Yes	Yes
FE Year	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,033	1,499	2,039	1,502	1,762	1,471
R2	0.113	0.164	0.087	0.128	0.109	0.114
Within R2	0.017	0.070	0.010	0.047	0.001	0.012

*Note: This table reports DiD estimates of the effect of fraud revelation on long-term profitability. Columns (1) include only LogAssets as control variable. Columns (2) add Leverage, BookToMarket and SalesGrowth as additional controls. The dependent variables are ROA, ROE and NPM. fraud_status x post_fraud is the DiD estimator capturing the average treatment effect on the treated firms in the post-revelation period. All specifications include industry and year fixed effects. Standard errors clustered at the firm level in parentheses. Entropy balancing weights applied throughout. Differences in observation counts across outcome variables reflect missing data for specific accounting items. *** p<0.001, ** p<0.01, * p<0.05, . p<0.10.*

The results reveal a directionally negative effect of fraud revelation on return on equity. The DiD coefficient on fraud_status × post_fraud is negative across both specifications, equal to -0.053 in Model 1 and -0.055 in Model 2. While the significance level is marginal under clustered standard errors, it strengthens to conventional levels when heteroskedasticity-robust standard errors are applied, as reported in Section 4.4. The economic magnitude of the effect is meaningful, a coefficient of approximately -0.055 implies that fraud firms generate returns on equity that are on average 5.5 percentage points lower than comparable control firms in the five years following fraud revelation. Given that the median pre-fraud ROE of fraud firms stands at 11.0%, this represents a decline of approximately 50% relative to the median pre-fraud ROE of fraud firms, suggesting the economic magnitude is substantial.

The effects on ROA and NPM are consistently small and statistically indistinguishable from zero across both specifications. The ROA coefficient is -0.001 in Model 1 and -0.002 in Model 2, while the NPM coefficient is -0.009 in Model 1 and -0.004 in Model 2. The concentration of the significant effect on ROE rather than ROA or NPM is theoretically informative. ROE captures the returns generated for shareholders and is particularly sensitive to changes in financing costs and financial leverage. Fraud revelation damages firm reputation, which in turn raises the cost of external financing and compresses equity returns without necessarily affecting operational margins or asset productivity in the short run. This pattern is consistent with a financial channel mechanism in which reputational damage primarily manifests through increased financing constraints rather than through immediate operational deterioration.

Taken together, these findings provide partial support for the research hypothesis. The long-term profitability of fraud firms is negatively affected following public revelation, but the effect is concentrated in return on equity rather than being uniformly observed across all three profitability measures, suggesting that the financing channel is the primary transmission mechanism through which fraud affects long-term firm performance.

4.3 Identification Tests

Before interpreting the main results as causal, two identification tests are conducted to support the validity of the parallel trends assumption underlying the DiD design. The placebo test examines whether differential pre-trends exist between the two groups in the pre-fraud period, while the event study traces the dynamic trajectory of the effect year by year around the fraud revelation date.

Placebo Test

The validity of the DiD design rests on the parallel trends assumption, which requires that fraud and control firms would have followed comparable profitability trajectories in the absence of fraud revelation. To test whether this assumption holds empirically, a placebo DiD is estimated on pre-fraud observations only, artificially assigning the post-fraud indicator to the second half of the pre-fraud window. Under the parallel trends assumption, the placebo coefficient should be statistically indistinguishable from zero.

Table 4: Placebo Test - ROE

Variable	ROE
LogAssets	0.0158 (0.0117)
Leverage	0.4343** (0.1423)
BookToMarket	-0.0174 (0.0229)
SalesGrowth	0.1099 (0.1214)
fraud_status x placebo_post	-0.0091 (0.0318)
FE Industry	Yes
FE Year	Yes
Observations	611

Variable	ROE
Within R2	0.072

*Note: Placebo DiD on pre-fraud observations only. Non-significant coefficient confirms absence of differential pre-trends. *** p<0.001, ** p<0.01, * p<0.05, . p<0.10. Standard errors clustered at the firm level in parentheses.*

The placebo DiD coefficient on ROE is -0.009 with a standard error of 0.032, yielding a p-value of 0.775. This non-significant result confirms that fraud and control firms followed comparable ROE trajectories in the pre-fraud period, with no evidence of differential pre-existing trends between the two groups. This finding lends credibility to the identifying assumption of the DiD design and supports the interpretation of the main results as capturing the causal effect of fraud revelation on long-term profitability rather than reflecting a pre-existing divergence between the two groups.

Table 5: Placebo Test - ROA

Variable	ROA
LogAssets	0.0073 (0.0054)
Leverage	-0.0409 (0.0400)
BookToMarket	0.0195. (0.0112)
SalesGrowth	0.0429 (0.0323)
fraud_status x placebo_post	0.0086 (0.0101)
FE Industry	Yes
FE Year	Yes
Observations	609
Within R2	0.044

*Note: Placebo DiD estimated on pre-fraud observations only. The post-fraud indicator is artificially assigned to years T-2 and T-1. The dependent variable is ROA. A non-significant coefficient confirms the absence of differential pre-trends for return on assets. *** p<0.001, ** p<0.01, * p<0.05, . p<0.10. Standard errors clustered at the firm level in parentheses.*

Table 6: Placebo Test - NPM

Variable	NPM
LogAssets	0.0111 (0.0104)
Leverage	0.0508 (0.1177)
BookToMarket	0.0738* (0.0357)
SalesGrowth	-0.0395 (0.0677)
fraud_status x placebo_post	0.0388 (0.0247)

Variable	NPM
FE Industry	Yes
FE Year	Yes
Observations	602
Within R2	0.047

*Note: Placebo DiD estimated on pre-fraud observations only. The post-fraud indicator is artificially assigned to years T-2 and T-1. The dependent variable is NPM. A non-significant coefficient confirms the absence of differential pre-trends for net profit margin. *** p<0.001, ** p<0.01, * p<0.05, . p<0.10. Standard errors clustered at the firm level in parentheses.*

To ensure that the parallel trends assumption holds for all three outcome variables, placebo tests are additionally conducted for ROA and NPM. The placebo coefficient on ROA is +0.009 (p = 0.394) and on NPM is +0.039 (p = 0.116), both statistically indistinguishable from zero. These results confirm that fraud and control firms followed comparable pre-fraud trajectories across all three profitability dimensions, providing broad support for the parallel trends assumption underlying the DiD design. It should be noted, however, that the NPM placebo result (p = 0.116) is the least precisely estimated of the three, and caution is warranted when interpreting the NPM channel in subsequent analyses.

Event Study

To complement the average treatment effect estimated by the main DiD specification and to examine the dynamic trajectory of the fraud effect, an event study is conducted by replacing the single post-fraud interaction with year-by-year interaction coefficients spanning the full T-5 to T+5 window. Figure 1 presents the resulting coefficients for ROE, with T-1 serving as the reference year.

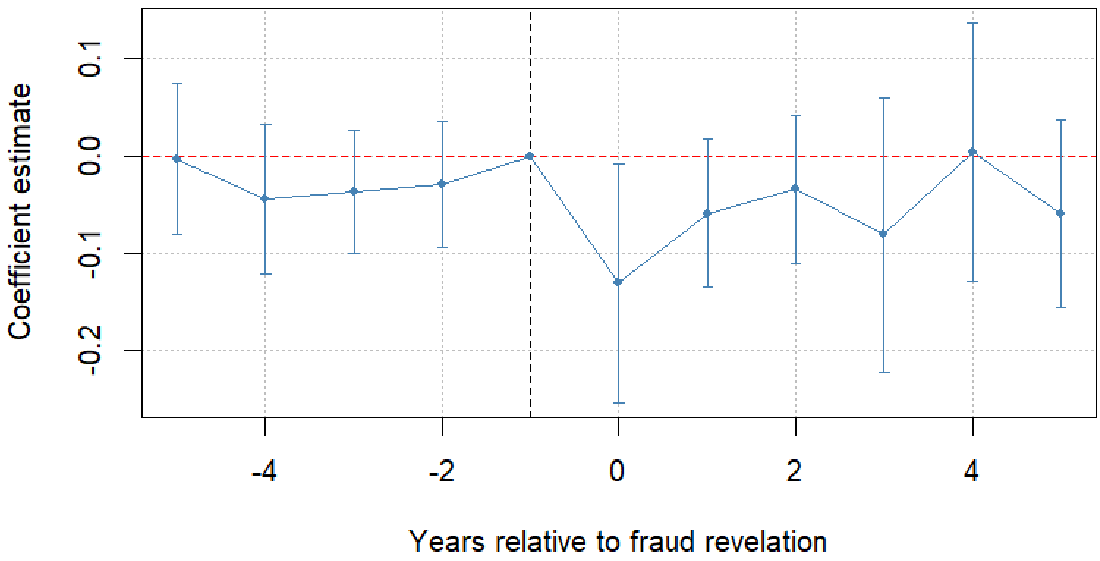


Figure 1: Event Study - Effect of Fraud on ROE

Note: This figure presents the dynamic difference-in-differences coefficients for return on equity (ROE) across the full T-5 to T+5 observation window. Each coefficient captures the differential ROE of fraud firms relative to control firms in each year, with T-1 as the reference year. The dashed *vertical line indicates the fraud revelation year T=0. The dashed horizontal line represents the zero baseline. Confidence intervals are at the 95% level. Standard errors are clustered at the firm level. Entropy balancing weights applied.*

The pre-fraud coefficients, corresponding to years T-5 through T-2, are all close to zero and statistically indistinguishable from zero, providing visual confirmation of the absence of differential pre-trends between fraud and control firms and reinforcing the placebo test results. A formal F-test of the joint significance of the pre-fraud coefficients (T-5 to T-2) yields a p-value of 0.986, confirming that pre-fraud trends are statistically indistinguishable from zero and providing strong support for the parallel trends assumption. A sharp negative shift is observed at T=0, the year of fraud revelation, followed by a persistent negative effect that extends through T+5. This pattern confirms that the profitability damage is not a transitory shock but rather reflects a sustained deterioration in firm financial performance that persists well beyond the immediate revelation year. Figures 2 (ROA) and Figure 3 (NPM) present the corresponding event study coefficients for ROA and NPM respectively. Both series remain close to zero throughout the full event window, consistent with the main DiD results showing no significant average effect on these two outcome variables and confirming that the effect of fraud is specifically concentrated in the equity return channel.

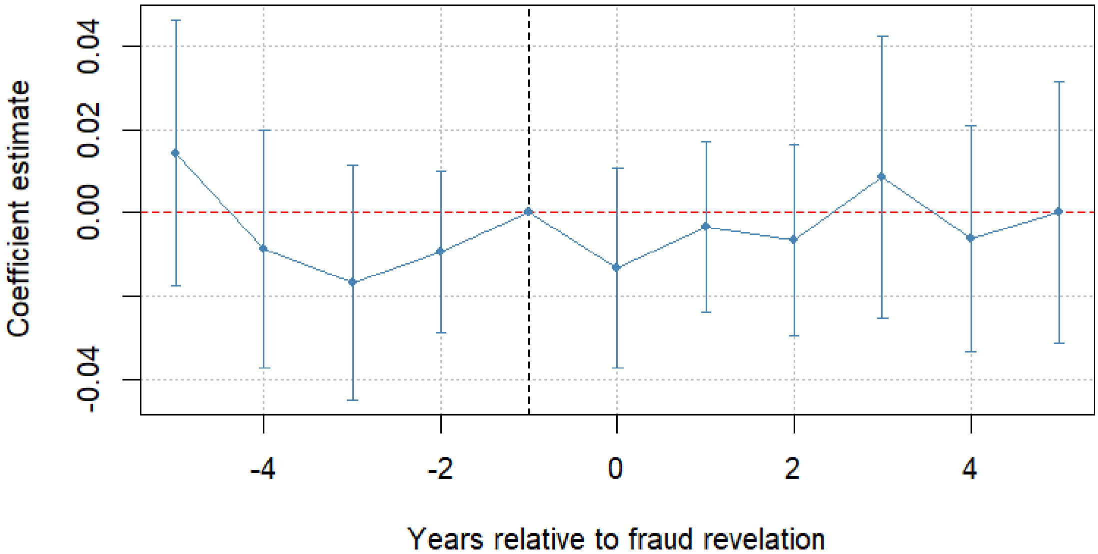


Figure 2: Event Study - Effect of Fraud on ROA

Note: This figure presents the dynamic difference-in-differences coefficients for return on assets (ROA) across the full T-5 to T+5 observation window. Each coefficient captures the differential ROA of fraud firms relative to control firms in each year, with T-1 as the reference year. The dashed vertical line indicates the fraud revelation year T=0. The dashed horizontal line represents the zero baseline. Confidence intervals are at the 95% level. Standard errors are clustered at the firm level. Entropy balancing weights applied.

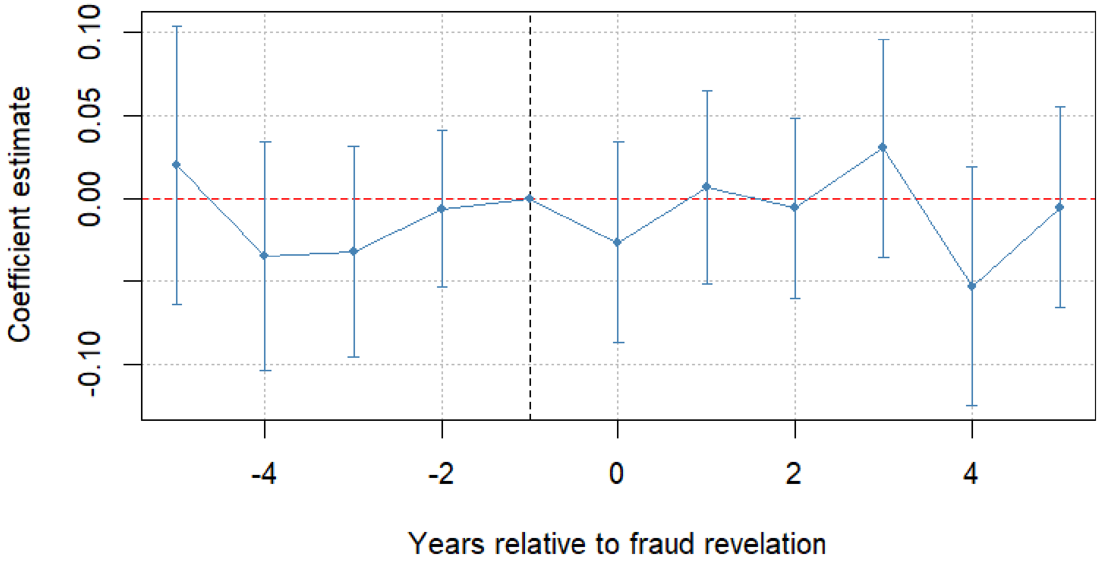


Figure 3: Event Study - Effect of Fraud on NPM

Note: This figure presents the dynamic difference-in-differences coefficients for net profit margin (NPM) across the full T-5 to T+5 observation window. Each coefficient captures the differential NPM of fraud firms relative to control firms in each year, with T-1 as the reference year. The dashed vertical line indicates the fraud revelation year T=0. The dashed horizontal line represents the zero baseline. Confidence intervals are at the 95% level. Standard errors are clustered at the firm level. Entropy balancing weights applied.

4.4 Robustness Checks

To assess whether the main findings are sensitive to methodological choices, five specification checks are conducted on the primary ROE specification. Table 7 reports the DiD coefficient on fraud_status × post_fraud across all five alternative specifications alongside the main model.

Table 7: Robustness Checks - ROE

Variable	Main	Firm FE	Current Ratio	T+1 to T+3	Robust SE	Winsorization 1/99%
fraud_status x post_fraud	-0.0549. (0.0311)	-0.0450 (0.0367)	-0.0338 (0.0283)	-0.0656. (0.0374)	-0.0549* (0.0278)	-0.0360 (0.0245)
FE Industry	Yes	No	Yes	Yes	Yes	Yes
FE Firm	No	Yes	No	No	No	No
CurrentRatio	No	No	Yes	No	No	No
SE Type	Clustered	Clustered	Clustered	Clustered	Robust	Clustered
Observations	1,502	1,500	1,183	1,244	1,502	1,502
Within R2	0.047	0.021	0.069	0.056	0.047	0.057

*Note: This table reports robustness checks on the main ROE specification. Column (1) is the main model. Column (2) replaces industry FE with firm FE. Column (3) adds CurrentRatio as control. Column (4) restricts the post-fraud window to T+1-T+3. Column (5) uses heteroskedasticity-robust SE. Column (6) applies 1%/99% winsorization. *** p<0.001, ** p<0.01, * p<0.05, . p<0.10. Standard errors clustered at the firm level in parentheses.*

The results are reassuring overall. Replacing industry fixed effects with firm-level fixed effects yields a coefficient of -0.045, which while no longer statistically significant at conventional levels, remains negative and of comparable magnitude to the main estimate, suggesting the direction of the effect is not an artifact of the fixed effects specification. The loss of significance with firm fixed effects is expected, as this specification absorbs all time-invariant firm-level variation and substantially reduces the identifying variation available. The R^2 jumps from 0.128 in the main model to 0.342 with firm fixed effects, confirming that firm-level heterogeneity explains a large share of the variation in ROE.

Adding the current ratio as an additional control yields a coefficient of -0.034, with the direction preserved but significance lost, consistent with the expected reduction in statistical power as the sample size falls from 1,502 to 1,183 observations due to missing current ratio data. This confirms that the exclusion of the current ratio from the main specification does not bias the main results. Restricting the post-fraud window to T+1 through T+3 produces a coefficient of -0.066 ($p \approx 0.08$), which is marginally stronger than the main estimate over the full T+1 to T+5 window, suggesting the effect is somewhat more concentrated in the early post-revelation years, with some partial recovery occurring between T+4 and T+5. Substituting heteroskedasticity-robust standard errors for clustered standard errors yields a coefficient of -0.055 that is statistically significant at the 5% level, confirming that the main result is not an artifact of the choice of inference method and holds under a more conservative approach to standard error estimation. Finally, applying a 1%/99% percentile-based winsorization in place of the economic-bound approach yields a coefficient of -0.036, which while no longer significant, remains negative and directionally consistent with the main estimate, suggesting the findings are broadly stable to the treatment of extreme values, though somewhat sensitive to the winsorization approach at the tails of the distribution.

Taken together, the robustness checks confirm that the negative effect of fraud revelation on ROE is directionally consistent and economically meaningful across all alternative specifications, lending further confidence to the main findings.

4.5 Additional Analyses

Beyond the main DiD results and robustness checks, two complementary analyses are conducted to shed further light on the mechanisms through which fraud affects long-term profitability. The DuPont decomposition examines which component of ROE is most affected by fraud revelation, while the speed of recovery analysis provides a firm-level perspective on the persistence of the profitability damage.

DuPont Decomposition

To identify the specific channel through which fraud revelation affects return on equity, Table 8 presents a DuPont decomposition in which each of the three constituent components of ROE is tested separately as a dependent variable: net profit margin, asset turnover, and the equity multiplier.

Table 8: DuPont Decomposition of ROE

Variable	NPM	AssetTurnover	EquityMultiplier
fraud_status x post_fraud	-0.0035 (0.0200)	0.0601 (0.0850)	-0.3655 (0.3865)
FE Industry	Yes	Yes	Yes
FE Year	Yes	Yes	Yes
Observations	1,471	1,508	1,508
Within R2	0.012	0.087	0.092

*Note: This table decomposes ROE into its three components: Net Profit Margin (NPM), Asset Turnover, and Equity Multiplier. None of the components is individually significant, suggesting the fraud effect operates through a combination of channels. *** p<0.001, ** p<0.01, * p<0.05, . p<0.10. Standard errors clustered at the firm level in parentheses.*

None of the three components yields a statistically significant DiD coefficient. The effect on net profit margin is -0.004 (p = 0.86), on asset turnover +0.060 (p = 0.48), and on the equity multiplier -0.366 (p = 0.34). The absence of significance at the individual component level does not imply the absence of an effect on ROE, but rather suggests that the fraud effect operates through a simultaneous combination of channels rather than a single dominant mechanism. Fraud revelation appears to simultaneously compress margins, leave asset efficiency largely unchanged, and alter the firm's leverage structure, but no single channel is strong enough to be detected individually given the sample size and the inherent heterogeneity across fraud firms. The positive coefficient on asset turnover, while insignificant, may reflect post-fraud asset divestitures that mechanically reduce the asset base and inflate turnover, rather than genuine operational improvement. This finding is consistent with the broad reputational damage hypothesis, under which fraud impairs multiple dimensions of firm performance simultaneously rather than targeting a specific operational or financial mechanism. It should be noted that the absence of significance at the component level does not contradict the aggregate ROE finding; decomposing ROE into three separate tests reduces statistical power further, and the large standard error on the equity multiplier (0.387) reflects noise at the channel level rather than absence of effect.

Speed of Recovery Analysis

To complement the DiD estimates with a more intuitive firm-level perspective on persistence, Figure 4 tracks the share of fraud firms that have recovered to their individual pre-fraud ROE baseline in each post-fraud year from T+0 to T+5.

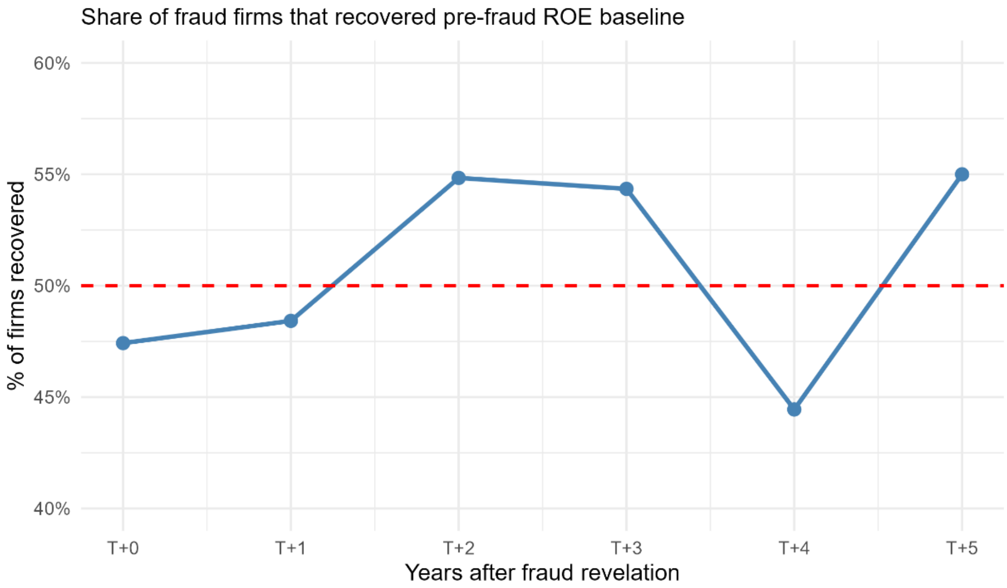


Figure 4: Speed of Recovery

Note: This figure tracks the share of fraud firms that have recovered to their average pre-fraud ROE baseline in each post-fraud year from T+0 to T+5. The pre-fraud baseline is defined as the average ROE of each fraud firm over the pre-fraud period T-5 to T-1. The dashed red horizontal line indicates the 50% threshold. A firm is classified as recovered in a given year if its ROE in that year equals or exceeds its individual pre-fraud baseline.

The results reveal a striking pattern. Immediately following fraud revelation, only 47.4% of fraud firms have recovered their pre-fraud ROE level at T+0, rising modestly to 48.4% at T+1. A partial recovery is observed between T+2 and T+3, where approximately 55% of firms exceed their pre-fraud baseline, before a temporary setback at T+4 reduces the share to 44.4%. By T+5, 55% of fraud firms have recovered, meaning that approximately 45% of fraud firms still have not returned to their pre-fraud ROE level five years after revelation. This non-linear and heterogeneous recovery pattern confirms that the long-term profitability damage from fraud is real and persistent for a substantial share of firms, and that recovery is neither automatic nor uniform across the sample.

4.6 Heterogeneity Analyses

The main DiD results establish an average treatment effect across all fraud firms. However, the impact of fraud revelation on long-term profitability may not be uniform and could vary systematically with firm characteristics. This section examines whether the effect is concentrated in specific subgroups, using a series of sample splits based on fraud severity, commercial momentum, pre-fraud financial performance, leverage, valuation profile, industry-relative standing, firm size, and market environment. Throughout, the dependent variable is ROE unless otherwise specified and each subgroup is estimated against the full control sample using the same specification as the main model.

Fraud Severity

Table 9 examines whether the magnitude of fraud conditions its long-term profitability impact. Given the mechanical correlation between the severity measure and ROE, this analysis is restricted to the post-fraud window T+2 through T+5.

Table 9: Fraud Severity Analysis

Variable	NPM_T2T5
LogAssets	0.0086 (0.0103)
Leverage	-0.0222 (0.0898)
BookToMarket	0.0253 (0.0188)
SalesGrowth	0.0634 (0.0398)
fraud_status x post_fraud	0.0407. (0.0242)
fraud_status x post_fraud x HighSeverity	-0.0847* (0.0363)
FE Industry	Yes
FE Year	Yes
Observations	1,147
Within R2	0.017

*Note: This table reports DiD estimates for the fraud severity analysis. The dependent variable is NPM restricted to T+2 through T+5 to avoid mechanical correlation. ROE was not used as the outcome for this analysis because net income enters both the severity measure and the ROE numerator, creating a mechanical link even when restricted to T+2-T+5; NPM was preferred as it captures margin-level effects while reducing this overlap. HighSeverity = 1 for firms above the median severity measure. *** p<0.001, ** p<0.01, * p<0.05, . p<0.10. Standard errors clustered at the firm level in parentheses.*

The DiD coefficient for high-severity firms on NPM is -0.085 and statistically significant at the 5% level, while the base effect for low-severity firms is positive and marginally significant at +0.041 (p ≈ 0.09). This result suggests that firms experiencing more severe frauds, those with the largest immediate drop in net income relative to their asset base, continue to suffer significant margin compression even two to five years after the revelation, well beyond the initial shock. Low-severity firms, by contrast, show no significant deterioration and may even experience a slight rebound, consistent with the view that less severe fraud revelations generate more contained reputational damage that firms can absorb and recover from more readily.

Growth Heterogeneity

Table 10 examines whether the profitability impact of fraud differs between firms with strong and weak pre-fraud commercial momentum.

Table 10: Growth Heterogeneity Analysis

Variable	LowGrowth	HighGrowth
fraud_status x post_fraud	-0.0822. (0.0457)	-0.0104 (0.0289)
FE Industry	Yes	Yes
FE Year	Yes	Yes
Observations	1,141	1,139
Within R2	0.041	0.074

*Note: This table reports separate DiD regressions for low-growth and high-growth fraud firms based on median pre-fraud SalesGrowth (5.27%). Dependent variable is ROE. *** p<0.001, ** p<0.01, * p<0.05, . p<0.10. Standard errors clustered at the firm level in parentheses.*

The ROE effect is marginally significant for low-growth firms (β = -0.082, p ≈ 0.07) and statistically indistinguishable from zero for high-growth firms (β = -0.010). This pattern suggests that firms without strong commercial momentum before the fraud are more vulnerable to the long-term profitability consequences of revelation. Low-growth firms have fewer resources and less commercial resilience to absorb the reputational shock and rebuild stakeholder confidence, making them more susceptible to sustained financing constraints in the post-revelation period. High-growth firms, by contrast, appear better positioned to leverage their commercial momentum to recover from the reputational damage.

Pre-Fraud Performance as Moderator

Table 11 tests whether pre-fraud financial strength moderates the post-fraud profitability impact, using a triple interaction between fraud status, post-fraud period, and an indicator for above-median pre-fraud ROE.

Table 11: Pre-Fraud Performance as Moderator

Variable	ROE
fraud_status x post_fraud	-0.1157** (0.0443)
fraud_status x post_fraud x HighPreROE	0.1207** (0.0426)
FE Industry	Yes
FE Year	Yes
Observations	1,502
Within R2	0.055

*Note: HighPreROE = 1 for fraud firms with above-median pre-fraud ROE (median = 11.49%). The triple interaction tests whether pre-fraud financial strength moderates the post-fraud impact. *** p<0.001, ** p<0.01, * p<0.05, . p<0.10. Standard errors clustered at the firm level in parentheses.*

This analysis yields the strongest results in the entire heterogeneity section. The base effect, capturing the impact for firms with below-median pre-fraud ROE, is -0.116 and statistically significant at the 1% level. The interaction term for high pre-fraud ROE firms is +0.121 and equally significant at the 1% level, indicating that firms with above-median pre-fraud profitability are substantially more resilient to the post-fraud shock. Firms that were already underperforming before the fraud suffer a dramatic and persistent ROE decline, while firms entering the post-revelation period from a position of financial strength are able to absorb the reputational damage without experiencing a comparable deterioration. This finding underscores the critical role of pre-existing financial cushion in determining a firm's capacity to withstand the reputational and financing consequences of fraud revelation.

Leverage Heterogeneity

Table 12 examines whether pre-fraud indebtedness moderates the post-fraud profitability impact, testing the financial channel hypothesis more directly.

Table 12: Leverage Heterogeneity

Variable	HighLev	LowLev
LogAssets	0.0039 (0.0094)	0.0095 (0.0129)
Leverage	0.2428 (0.1539)	0.4166** (0.1513)
BookToMarket	-0.0286 (0.0217)	-0.0967. (0.0497)
SalesGrowth	0.1235* (0.0516)	0.0616 (0.0421)
fraud_status x post_fraud	-0.0852* (0.0429)	0.0139 (0.0346)
FE Industry	Yes	Yes
FE Year	Yes	Yes
Observations	1,177	1,103
R2	0.155	0.175
Within R2	0.043	0.075

*Note: Split based on median pre-fraud leverage ratio (16.98%). Dependent variable is ROE. *** p<0.001, ** p<0.01, * p<0.05, . p<0.10. Standard errors clustered at the firm level in parentheses.*

The ROE effect is statistically significant for high-leverage firms ($\beta = -0.085$, $p < 0.05$) and statistically indistinguishable from zero for low-leverage firms ($\beta = +0.014$). This result provides strong support for the financial channel hypothesis. Firms already carrying significant debt before the fraud revelation face compounded financing constraints post-fraud, as lenders and investors simultaneously price in existing indebtedness and newly revealed reputational risk. Low-leverage firms, by contrast, are largely insulated from this channel, likely because their limited dependence on external debt financing reduces their exposure to the increased cost of capital that follows fraud revelation.

Book-to-Market Heterogeneity

Table 13 examines whether the firm's pre-fraud valuation profile moderates the post-fraud profitability impact.

Table 13: BTM Heterogeneity

Variable	HighBTM	LowBTM
LogAssets	0.0139 (0.0114)	-0.0030 (0.0111)
Leverage	0.2640. (0.1503)	0.3495* (0.1367)
BookToMarket	-0.0645. (0.0338)	-0.0140 (0.0280)
SalesGrowth	0.0839* (0.0404)	0.1042* (0.0511)
fraud_status x post_fraud	-0.0924. (0.0513)	-0.0101 (0.0271)
FE Industry	Yes	Yes
FE Year	Yes	Yes
Observations	1,128	1,152

Variable	HighBTM	LowBTM
R2	0.151	0.179
Within R2	0.057	0.054

*Note: Split based on median pre-fraud BTM ratio (0.168). High BTM = value firms, Low BTM = growth firms. Dependent variable is ROE. *** p<0.001, ** p<0.01, * p<0.05, . p<0.10. Standard errors clustered at the firm level in parentheses.*

The ROE effect is marginally significant for high BTM firms (β = -0.092, p ≈ 0.07) and statistically indistinguishable from zero for low BTM firms (β = -0.010). High BTM firms are value firms whose market valuation is relatively close to their book value, while low BTM firms are growth firms commanding a significant market premium. The concentration of the effect among value firms suggests that firms with lower market valuations and fewer growth opportunities are more vulnerable to the financing constraints that follow fraud revelation. Growth firms, with their higher market valuations and greater financial flexibility, appear better positioned to absorb the reputational shock without experiencing a lasting profitability deterioration.

Industry-Relative Performance

Table 14 refines the pre-fraud performance analysis by benchmarking each firm's ROE against its industry peers rather than using absolute profitability levels.

Table 14: Industry-Relative Performance

Variable	Under	Out
LogAssets	0.0084 (0.0128)	-0.0010 (0.0102)
Leverage	0.2518. (0.1494)	0.3505** (0.1283)
BookToMarket	-0.0474. (0.0245)	-0.0491 (0.0403)
SalesGrowth	0.1015* (0.0462)	0.0858. (0.0450)
fraud_status x post_fraud	-0.1209* (0.0499)	0.0072 (0.0284)
FE Industry	Yes	Yes
FE Year	Yes	Yes
Observations	1,111	1,169
R2	0.193	0.147
Within R2	0.066	0.046

*Note: Split based on median pre-fraud industry-relative ROE. Underperformers had below-median ROE relative to industry peers. Dependent variable is ROE. *** p<0.001, ** p<0.01, * p<0.05, . p<0.10. Standard errors clustered at the firm level in parentheses.*

Industry underperformers, firms whose pre-fraud ROE was below the average of their industry peers, experience a significant ROE decline of -0.121 ($p < 0.05$) following fraud revelation, while industry outperformers show no significant effect (β = +0.007). This result is more nuanced than the absolute pre-fraud performance split, as it controls for industry-level profitability differences and isolates the role of relative competitive standing within the sector. Firms that were already perceived as weak players within their industry have the least capacity to absorb reputational shocks and face the most severe financing constraints post-fraud. Industry outperformers, by contrast, appear to possess sufficient competitive credibility to weather the reputational consequences of fraud revelation without a lasting profitability impact.

Size Heterogeneity

Table 15 examines whether firm size moderates the long-term profitability impact of fraud revelation.

Table 15: Size Heterogeneity

Variable	Large	Small
LogAssets	0.0021 (0.0135)	0.0069 (0.0136)
Leverage	0.2947. (0.1636)	0.2674* (0.1316)
BookToMarket	-0.0503. (0.0263)	-0.0691 (0.0418)
SalesGrowth	0.1160* (0.0532)	0.0739. (0.0410)
fraud_status x post_fraud	-0.0406 (0.0539)	-0.0592 (0.0377)
FE Industry	Yes	Yes
FE Year	Yes	Yes
Observations	1,123	1,157
R2	0.174	0.143
Within R2	0.053	0.049

*Note: Split based on median pre-fraud LogAssets (21.96). Dependent variable is ROE. *** p<0.001, ** p<0.01, * p<0.05, . p<0.10. Standard errors clustered at the firm level in parentheses.*

Neither large nor small firms show a statistically significant ROE effect, with coefficients of -0.041 and -0.059 respectively. The absence of a significant difference between the two groups suggests that firm size does not systematically moderate the fraud effect in this sample. Rather than concluding that size provides no protection, this null result may reflect that both groups are similarly affected on average, or that the subgroup sample sizes of approximately 45 fraud firms each are insufficient to detect a statistically meaningful difference. What the result does confirm is that the fraud effect is not exclusively concentrated in small, resource-constrained firms, even large firms with greater financial resources and market visibility are not immune to the long-term profitability consequences of fraud revelation.

Pre/Post 2008 Financial Crisis

As an exploratory complement to the main analyses, Table 16 splits the sample according to whether the fraud revelation occurred before or after the 2008 financial crisis.

Table 16: Pre/Post 2008 Analysis

Variable	Pre2008	Post2008
LogAssets	0.0086 (0.0126)	0.0050 (0.0106)
Leverage	0.2314 (0.1586)	0.3299* (0.1320)
BookToMarket	-0.0505 (0.0329)	-0.0577. (0.0331)
SalesGrowth	0.1304* (0.0655)	0.0681. (0.0374)

Variable	Pre2008	Post2008
fraud_status x post_fraud	-0.0909 (0.0551)	-0.0281 (0.0325)
FE Industry	Yes	Yes
FE Year	Yes	Yes
Observations	1,040	1,240
R2	0.190	0.135
Within R2	0.052	0.051

*Note: Split based on fraud revelation date relative to 2008 financial crisis. 32 pre-2008 and 68 post-2008 firms. Results should be interpreted with caution given limited pre-2008 subsample. Dependent variable is ROE. *** p<0.001, ** p<0.01, * p<0.05, . p<0.10. Standard errors clustered at the firm level in parentheses.*

The ROE effect appears stronger for firms whose fraud was revealed before the crisis ($\beta = -0.091$, $p \approx 0.10$) compared to those revealed after 2008 ($\beta = -0.028$, not significant). This pattern is suggestive of a moderating role of the broader market environment, with fraud revelations generating a sharper reputational shock in the pre-crisis period when investor trust was higher and market scrutiny of corporate misconduct was less elevated. However, given the limited size of the pre-2008 subsample of 32 firms and the marginal significance level, this result should be interpreted with considerable caution and is presented as exploratory rather than conclusive.

5. Discussion and Limitations

This chapter situates the empirical findings within the broader literature on financial fraud and long-term profitability, discusses their theoretical and practical implications, and provides a transparent account of the methodological limitations that should inform their interpretation.

Interpreting the Main Results

This study examined whether financial fraud leads to a lasting decline in firm profitability across three dimensions, return on assets, return on equity, and net profit margin, over a five-year post-revelation horizon. The results present a nuanced yet coherent pattern. The most consistent finding is a persistent negative effect on return on equity, with fraud firms generating ROE approximately 5.5 percentage points below comparable control firms in the years following fraud revelation. This effect is marginally significant under clustered standard errors and reaches conventional significance under heteroskedasticity-robust standard errors, holding directionally across all five robustness checks. Return on assets and net profit margin show no statistically significant average effect across the full sample, a pattern that is not a failure of the analysis but a substantively meaningful result in itself.

To understand why the three metrics behave differently, it is important to consider what each one captures and through which channel fraud affects it. ROE measures the returns generated for shareholders and is directly sensitive to the cost of financing. When fraud revelation forces firms to borrow at higher rates and under tighter conditions, a dynamic directly documented by Graham, Li, and Qiu (2008), net income falls relative to equity, compressing ROE persistently. ROA measures how efficiently the firm uses its asset base to generate profit, a metric more directly linked to internal operations and capital allocation than to financing costs. NPM measures how much profit the firm extracts from each dollar of revenue, and is most sensitive to the quality of commercial relationships with customers and suppliers. The theoretical prediction of reputational capital theory is that the financing channel activates first and most uniformly across all fraud firms, because creditors reprice the firm's trustworthiness immediately upon revelation, while operational channels are more gradual, more heterogeneous, and more dependent on fraud severity. The

empirical results confirm exactly this prediction: ROE is impaired on average across all fraud firms, while ROA and NPM are affected only in specific subgroups.
This interpretation is directly supported by the fraud severity analysis, which shows that high-severity fraud firms suffer a significant and persistent net profit margin compression of -0.085 ($p < 0.05$) over T+2 to T+5, well beyond the initial shock. This confirms that the commercial channel, customer defection, supplier pressure, rising operating costs, is real and active, but only when the fraud is severe enough to fundamentally damage the firm's commercial relationships. For less severe frauds, the commercial impact is more contained and may even be partially offset by the governance improvements that fraud revelation sometimes triggers, as reflected in the positive base effect for low-severity firms (+0.041, $p \approx 0.09$). The absence of a significant average NPM effect across the full sample therefore reflects the coexistence of these two very different types of fraud firms within the same dataset, not the absence of any commercial consequence. Similarly, the absence of a significant average ROA effect does not mean that asset productivity is unaffected in all cases, but rather that operational impairment is too heterogeneous across firms to be detectable as a statistically significant average in a sample of 89 fraud firms spanning a wide range of industries, fraud magnitudes, and market contexts.

What the Heterogeneity Analyses Add

The heterogeneity analyses consistently point to the same underlying pattern: the post-fraud profitability impact is most severe for firms that were already in a weak financial position before the fraud. It is important to note that this split is conducted within the fraud firm sample only, comparing fraud firms with below-median pre-fraud ROE against those with above-median pre-fraud ROE, both relative to the full control group. The finding does not contradict Table 2, which shows that fraud firms had higher average pre-fraud ROE than control firms overall. The strongest result in the entire study is the pre-fraud performance moderator (Table 11): firms already underperforming before the fraud suffer a dramatic and significant ROE decline, while above-median performers show no lasting deterioration. Firms that were already struggling before the fraud have no financial cushion to absorb the elevated financing costs that follow revelation, and they fall into a sustained profitability trap from which recovery is slow and uncertain. Firms entering the post-revelation period from a position of financial strength, by contrast, can absorb the shock without a lasting impact on their equity returns. The industry-relative performance split reinforces this finding by showing that industry underperformers (Table 14) suffer a significant ROE decline following fraud revelation, while outperformers show no significant effect, confirming that relative competitive standing within the sector is a meaningful determinant of vulnerability independent of absolute profitability level.
The leverage heterogeneity result provides the clearest direct evidence for the financing channel. As reported in Table 12, the fraud effect on ROE is entirely concentrated in highly leveraged firms, consistent with Castro et al. (2020) and Ertan (2021), while low-leverage firms show no significant effect. When a heavily indebted firm is hit by a fraud revelation, lenders simultaneously price in existing debt risk and newly revealed reputational risk, creating compounded financing constraints that compress ROE persistently. Firms with limited debt are largely insulated from this dynamic because they depend less on external financing.
The growth split and book-to-market results are more tentative and require careful interpretation. Low-growth firms show a marginally significant ROE decline ($\beta = -0.082$, $p \approx 0.07$), consistent with the idea that commercial momentum helps firms rebuild credibility with stakeholders after fraud, high-growth firms can demonstrate continued operational viability that reassures creditors and investors more convincingly. Value firms with high book-to-market ratios also show a marginally significant effect ($\beta = -0.092$, $p \approx 0.07$), most plausibly because they have fewer growth opportunities and less financial flexibility to absorb compounded financing constraints following fraud revelation, rather than because they have more intangible reputational capital to lose. Both results sit at $p \approx 0.07$ and should be treated as directionally consistent with theory rather than firmly established empirical findings.

The size split yields no significant result for either large or small firms, with coefficients of -0.041 and -0.059 respectively. This null result does not allow strong conclusions, it most likely reflects insufficient statistical power within subgroups of approximately 45 fraud firms each, rather than a genuine absence of any size effect. What it does confirm is that the fraud effect is not exclusively concentrated in small or resource-constrained firms.

The DuPont decomposition finds no individually significant effect on any of the three ROE components, net profit margin, asset turnover, or the equity multiplier. This suggests that fraud simultaneously and diffusely impairs all three dimensions of ROE, with no single channel strong enough to be detected individually given the sample size. The combined effect of these simultaneous impairments manifests as the significant ROE decline in the main specification, consistent with the broad reputational damage hypothesis of Karpoff et al. (2008a).

The speed of recovery analysis adds an important firm-level perspective on persistence. Approximately 45% of fraud firms had still not returned to their pre-fraud ROE level five years after revelation, with a non-linear recovery pattern featuring partial improvement at T+2 and T+3, a setback at T+4, and a slight recovery at T+5. This confirms that recovery from fraud is neither automatic nor uniform, some firms rebuild relatively quickly while others remain persistently below their pre-fraud baseline for years, consistent with Karpoff's (2012) theoretical prediction that reputational recovery requires extended periods of demonstrably honest behavior.

The pre/post 2008 split is presented purely as exploratory. With only 32 pre-crisis firms and a p-value of approximately 0.10, the stronger ROE effect for pre-crisis revelations ($\beta = -0.091$) compared to post-crisis ones ($\beta = -0.028$) cannot support firm conclusions and is best treated as a direction for future research.

Theoretical and Practical Implications

At the theoretical level, this study contributes to the reputational capital literature by providing longitudinal accounting-based evidence that fraud revelation generates a persistent and differential impact across the three dimensions of firm profitability. The finding that ROE is the primary average channel while ROA and NPM are affected only in specific subgroups advances the understanding of how reputational damage transmits into financial performance, and highlights the value of examining multiple profitability metrics simultaneously rather than relying on a single indicator. The consistent pattern across heterogeneity analyses, vulnerability concentrated in financially weak, highly leveraged, and industry-underperforming firms, supports an integration of reputational capital theory with financial distress and capital structure theories, suggesting that the severity of post-fraud profitability damage is jointly determined by the magnitude of the reputational shock and the firm's pre-existing financial capacity to absorb it.

For investors, the results suggest that short-term stock price reactions underestimate the full cost of fraud. ROE compression driven by elevated financing costs can persist for years, particularly for firms entering the post-fraud period with high leverage or weak prior performance relative to peers. Monitoring ROA, ROE, and NPM over a multi-year horizon gives a more complete picture of fraud's true economic cost than market-based indicators alone. For creditors, the leverage result provides direct practical guidance: post-fraud credit risk assessments should explicitly account for the interaction between debt levels and reputational exposure, as highly leveraged fraud firms face compounded financing constraints that persist long after revelation. For boards and governance professionals, the pre-fraud performance result carries a clear message: maintaining strong financial buffers before any fraud event is not just good governance practice but meaningful protection against the lasting profitability consequences of reputational shocks that no governance framework can fully prevent.

Limitations

Several methodological limitations should be acknowledged transparently, as they directly affect the interpretation and generalizability of the findings.

The most significant limitation is sample size. With 89 fraud firms after applying the data sufficiency filter, statistical power is limited and subgroup analyses contain approximately 39 to 45 fraud firms each. This restricts the ability to detect moderate-sized effects in heterogeneity analyses and explains why several theoretically motivated results achieve only marginal significance. The entropy balancing was restricted to two variables, pre-fraud ROA and LogAssets, precisely because including additional balancing variables would have reduced the eligible fraud firm count from 89 to 63, further compromising statistical power. A related concern is that entropy balancing, while achieving exact distributional balance on the two chosen variables, results in an effective control sample size of only 41.04 firms out of the 274 available, meaning that a small subset of control firms carries most of the weight in the reweighted comparison. This concentration of weights is an expected and acceptable feature of entropy balancing when the treatment group is small, but it implies that the control group's contribution to the estimates is less diversified than the raw sample size suggests.

The study conducts 8 heterogeneity analyses and multiple subgroup regressions, raising potential multiple-comparison concerns. P-values are unadjusted throughout, as the analyses are pre-specified based on theory rather than data-driven. Nevertheless, results from marginally significant subgroup analyses should be interpreted with appropriate caution, as the probability of at least one false positive increases with the number of tests conducted.

Survivorship bias is a fundamental constraint of this research design. The data sufficiency filter requires at least four non-missing observations on each side of the event window, which mechanically excludes firms that failed, were acquired, or were delisted shortly after fraud revelation, precisely the cases where the impact on ROA, ROE, and NPM was likely most catastrophic. Firms like Enron and WorldCom, which collapsed entirely following fraud revelation rather than experiencing a sustained but recoverable decline, fall outside this sample. The estimates in this study are therefore conservative lower bounds on the true average impact across all three profitability metrics, applicable specifically to firms that survived the post-revelation period with sufficient data. This survivorship bias likely understates the true magnitude of the effect, particularly for the operational channels captured by ROA and NPM, where the most severe cases would be expected to show the most dramatic deterioration.

The AAER publication date used to define the fraud event year typically lags the underlying misconduct by several years. For long-running frauds, the pre-fraud period in this study may already be partially contaminated by ongoing misreporting, meaning the profitability baselines for ROA, ROE, and NPM may be partially inflated by the fraud itself. This would cause the estimated post-fraud declines to understate the true deterioration relative to what performance would have been in the complete absence of fraud. While the placebo test confirms the absence of systematic differential pre-trends on average, it cannot rule out this contamination at the individual firm level.

The current ratio was excluded from the main specification because its inclusion reduces the analytic sample by approximately 35%, from 1,502 to 1,183 observations for ROE. The robustness check confirms directional stability of the main result, but at substantially reduced observations and with loss of significance, reflecting the combined effect of reduced power and the additional liquidity control rather than a sign of instability in the underlying relationship. The winsorization sensitivity test shows that the main ROE result loses statistical significance under 1%/99% percentile-based winsorization, a sensitivity worth acknowledging when interpreting the precision of the main estimate, though the direction and approximate magnitude are preserved across both approaches.

Two additional heterogeneity analyses that were theoretically motivated could not be pursued. An industry-level split was deemed infeasible because 89 fraud firms distributed across multiple two-digit TRBC codes would produce subgroups too small for reliable inference. A combined low-growth high-severity interaction was explored but yielded only 18 fraud firms in the target subgroup, insufficient to support meaningful estimation. Both decisions reflect a deliberate preference for analytical conservatism over the risk of reporting results driven by sampling noise. Finally, the pre/post 2008 analysis is severely limited by the concentration of fraud revelations in the post-2008 period, leaving only 32 pre-crisis firms and preventing reliable cross-period comparisons. The assignment of the median fraud year of 2011 to control firms without a natural event date, while standard practice in DiD designs, introduces a further approximation in the pre/post structure of the panel that may affect the precision of subgroup analyses conducted around this temporal threshold.

The enactment of SOX in 2002 creates a structural break within the sample period that may affect the comparability of pre- and post-SOX fraud cases. This heterogeneity is partially absorbed by the year fixed effects included in all specifications, but is not explicitly modelled.

A further methodological consideration concerns the staggered nature of fraud revelations across the sample period which raises the concern that TWFE estimates may be biased due to heterogeneous treatment timing, as documented by Goodman-Bacon (2021) and Callaway and Sant'Anna (2021). While the clean pre-trends observed in the event study and the formal F-test ($p = 0.986$) mitigate this concern, future research could apply staggered DiD estimators to formally address this issue.

Despite these limitations, the core finding stands: financial fraud generates a persistent, differential, and theoretically coherent impact on firm profitability. ROE is the primary channel through which the average effect manifests, reflecting the immediate and uniform repricing of financing conditions following fraud revelation. ROA and NPM are affected in specific subgroups, most notably high-severity cases, but not on average, consistent with the heterogeneous and gradual nature of the operational consequences of fraud. Together, the three metrics tell a complete and internally consistent story about how reputational damage from fraud translates into lasting profitability deterioration in U.S. public companies.

6. Conclusion

This thesis set out to answer a deceptively simple but empirically underexplored question: does financial fraud cause a lasting decline in the profitability of U.S. publicly listed companies, and if so, through which channels and for which types of firms? The existing literature had established that fraud imposes large and immediate market penalties, but the long-term accounting-based consequences across multiple profitability dimensions had received comparatively little rigorous empirical attention. By following a sample of 89 fraud firms and 274 carefully selected control firms over an eleven-year window spanning five years before and five years after fraud revelation, and by employing a difference-in-differences framework with entropy balancing to isolate the causal effect of fraud from firm-level and macroeconomic confounds, this study provides the longitudinal evidence that the literature had been missing.

The central finding, detailed in Chapter 4, is a persistent and economically meaningful ROE decline, roughly half the pre-fraud baseline, that holds directionally across all five robustness checks and reaches conventional significance under heteroskedasticity-robust standard errors. Return on assets and net profit margin show no statistically significant average effect, a pattern that is not a null result but a theoretically coherent one: ROE is the metric most directly sensitive to the financing channel through which reputational damage operates, while ROA and NPM are affected through operational channels that are more heterogeneous, more gradual, and more dependent on fraud severity. The fraud severity analysis confirms this interpretation directly, showing that high-severity fraud firms suffer a significant net profit margin compression of -0.085 ($p < 0.05$) over T+2 to T+5, well beyond the initial shock, while low-severity firms show no lasting deterioration. The differentiated pattern across the three profitability metrics is itself a contribution: it reveals that fraud's long-term cost is not uniform across dimensions of performance but is channeled through financing first and operations only in the most severe cases.

Overall, H1 is partially supported: fraud revelation leads to a sustained decline in ROE, but no statistically significant average effect is detected for ROA or NPM, with operational damage concentrated in high-severity cases only.

The heterogeneity analyses reveal that the average treatment effect masks substantial variation across firm subgroups that is consistent with theory and practically meaningful. The most striking result is that firms with below-median pre-fraud ROE suffer a dramatic decline of -0.116 ($p < 0.01$), while above-median performers show no significant deterioration, confirming that pre-existing financial resilience is the single most powerful moderator of post-fraud vulnerability. Industry underperformers show a similarly significant effect of -0.121 ($p < 0.05$), demonstrating that relative competitive standing within the sector matters independently of absolute profitability. The leverage heterogeneity result directly corroborates the financing channel: the fraud effect is entirely concentrated in highly leveraged firms, consistent with the compounded financing constraints that arise when lenders simultaneously price in existing indebtedness and newly revealed reputational risk. The fraud severity result adds the operational dimension, showing that commercial channels are activated specifically when the magnitude of the fraud is large enough to generate lasting damage to customer and supplier relationships. Together, these results paint a coherent picture: fraud imposes lasting profitability damage through the financing channel on average, through the operational channel in severe cases, and most severely on firms that were already financially vulnerable before the revelation.

The speed of recovery analysis reinforces the persistence narrative. Approximately 45% of fraud firms had not returned to their pre-fraud ROE baseline five years after revelation, with a non-linear recovery pattern that confirms the durability and heterogeneity of the long-term damage. Recovery is uneven and protracted, consistent with the theoretical expectation that trust, once destroyed through deliberate deception, cannot be rebuilt rapidly.

This study makes three contributions to the existing literature. First, it provides robust longitudinal accounting-based evidence that reputational damage from fraud translates into measurable and persistent profitability deterioration across multiple dimensions, extending the largely market-based evidence of Karpoff, Lee, and Martin (2008a) into the domain of long-run accounting performance. Second, it identifies the financing channel, as captured by ROE, as the primary mechanism through which this damage operates on average, while demonstrating that operational channels captured by NPM are activated specifically in high-severity cases, providing a more complete and channel-specific account of how fraud costs accumulate over time. Third, it shows that the severity of the post-fraud profitability impact is systematically determined by pre-existing firm characteristics, particularly financial leverage, pre-fraud profitability relative to peers, fraud magnitude, and commercial momentum, providing a framework for identifying which firms face the greatest long-term risk from fraud revelation and why.

Several directions for future research emerge naturally from this study. The sample limitation of 89 fraud firms restricts statistical power and prevents definitive conclusions in several of the heterogeneity analyses. Future research with access to larger samples, whether through alternative fraud identification strategies such as securities class action lawsuits or restatement databases, or through longer data collection windows, would be better positioned to detect the full range of heterogeneous effects and to examine additional moderating variables such as institutional ownership structure, board composition, and auditor quality. The exploratory pre/post 2008 result, suggestive of a moderating role of the broader market environment, deserves more rigorous investigation with sufficient sample sizes to draw reliable conclusions across market regimes. The survivorship bias inherent in this study's design, which excludes the most extreme fraud cases that led to firm failure, represents a fundamental limitation that future research might partially address by incorporating survival analysis methods that model the censoring process explicitly and provide estimates that account for the full distribution of fraud outcomes including delisting and bankruptcy.

Financial fraud is costly, and the evidence presented in this thesis suggests that its costs are deeper, more persistent, and more unequally distributed across firms than the existing market-based literature has been able to reveal. The firms most damaged by fraud revelation are not necessarily those that committed the most visible frauds, but those that were already financially vulnerable when trust was destroyed. Understanding this asymmetry is essential for investors assessing post-fraud recovery potential, for creditors pricing reputational risk, and for boards designing governance frameworks robust enough to limit the lasting damage that fraud, when it occurs, inflicts on firm profitability and stakeholder relationships alike.

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EXECUTIVE SUMMARY

Financial fraud represents one of the most severe breaches of corporate integrity, yet its long-term consequences for firm profitability remain empirically underexplored. This study examines whether fraud revelation leads to a lasting decline in the profitability of U.S. publicly listed companies, as measured by return on assets (ROA), return on equity (ROE), and net profit margin (NPM), over a five-year post-revelation window.

The empirical strategy relies on a difference-in-differences design applied to a sample of 89 fraud firms identified through SEC Accounting and Auditing Enforcement Releases (AAERs) and 274 control firms, covering fraud revelations between 2000 and 2023. Entropy balancing ensures pre-treatment comparability, and the parallel trends assumption is validated through a placebo test, an event study, and a formal F-test ($p = 0.986$). Five robustness checks and eight heterogeneity analyses complement the main estimation.

The results reveal a persistent negative effect on ROE, with fraud firms generating returns approximately 5.5 percentage points below comparable control firms, equivalent to roughly 50% of the pre-fraud baseline. This effect is directionally consistent across all robustness checks and reaches conventional significance under heteroskedasticity-robust standard errors. ROA and NPM show no statistically significant average effect, a theoretically coherent pattern: the financing channel activates first and most uniformly, while operational channels are heterogeneous and concentrated in high-severity cases. Heterogeneity analyses show that the effect is most severe among highly leveraged firms, industry underperformers, and firms with weak pre-fraud profitability. The speed of recovery analysis confirms that approximately 45% of fraud firms had not recovered their pre-fraud ROE baseline five years after revelation.

Overall, H1 is partially supported. These findings extend the largely market-based evidence on fraud costs into the domain of long-run accounting performance and identify pre-existing financial resilience as the single most powerful moderator of post-fraud vulnerability.

KEYWORDS: financial fraud, long-term profitability, difference-in-differences, reputational capital, ROE, entropy balancing, SEC enforcement

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