

NBER WORKING PAPER SERIES

THE INFORMATION CONTENT OF CORPORATE EARNINGS:
EVIDENCE FROM THE SECURITIES EXCHANGE ACT OF 1934

Oliver Binz
John Graham

Working Paper 29747
<http://www.nber.org/papers/w29747>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2022

We appreciate helpful comments and suggestions from Hami Amiraslani, Dan Bens, Thomas Bourveau, Greg Burke, Gavin Cassar, Scott Dyreng, Fabrizio Ferri, Elia Ferracuti, Robert Holthausen, Xu Jiang, Peter Joos, Matt Kubic, Mark Lang, Bill Mayew, Francine McKenna, Steve Monahan, Suresh Nallareddy, Katherine Schipper, Mani Sethuraman, Kevin Standridge, Robert Stoumbos, Rahul Vashishtha, Mohan Venkatachalam, Lauren Vollon, an anonymous reviewer and editor, and the workshop participants at Duke University, INSEAD, the European Accounting Symposium for Young Scholars (EASYS) webinar, the Early Insights in Accounting webinar, the Virtual Corporate Finance Fridays webinar, and the University of Miami. We thank Adarsh Abhinav, Sai Challa, Akshay Jituri, Maksym Kosachevskyy, Jairaj Joshi, Tyler Johnson, Rishi Badola, Vache Darbinyan, Darshit Parekh, Srushti Deshpande, and Sheilvy Soetanto for excellent research assistance. All errors remain our own. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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The Information Content of Corporate Earnings: Evidence from the Securities Exchange Act of 1934

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NBER Working Paper No. 29747

February 2022

JEL No. B26,G14,G18,K22,M41,M48

ABSTRACT

We examine whether the Securities Exchange Act of 1934 increased the information content of corporate earnings disclosures. Prior research questions whether the Act improved disclosure quality but generally relies on long-window tests and yields mixed results. We focus on whether the Act increased earnings informativeness, improving upon prior designs by focusing on short earnings announcement windows and employing a difference-in-differences design to control for potential contemporaneous structural changes. We document an increase in earnings informativeness following the Act, which is larger for treatment firms (which withheld disclosure before the Act) than for control firms. The increase in informativeness is more pronounced for firms that are subject to stronger enforcement.

Oliver Binz

INSEAD

Boulevard de Constance

Fontainebleau 77305

France

oliver.binz@insead.edu

John Graham

Duke University

Fuqua School of Business

100 Fuqua Drive

Durham, NC 27708-0120

and NBER

john.graham@duke.edu

1. Introduction

The Securities Exchange Act of 1934 (hereafter “the Act”) is the most expansive secondary market regulation enacted in the history of the United States.¹ The Act was the first federal law to mandate disclosure of audited financial statements, established the Securities and Exchange Commission (SEC) as an enforcement body, and is still the basis of most financial litigation.² Given that the Act changed the reporting environment from no federal mandatory reporting to mandatory reporting and increased enforcement, it provides an ideal setting to study whether its implementation improved the informativeness of accounting numbers. A report from the President and the Council of Economic Advisers (2003, pp. 96-97) indicates that this important issue is still unresolved: “whether SEC enforced disclosure rules actually improve the quality of information that investors receive remains a subject of debate among researchers almost 70 years after the SEC’s creation.” Some even argue that the Act did not improve the quality of information at all (Benston 1969, 1973).

To date, researchers have examined firms’ unconditional stock return performance around the Act and documented no change in returns but a decrease in return volatility. However, the scarcity of evidence and the sharp disagreement about interpretation led Easterbrook and Fischel (1984, p. 714) to conclude that “there is no good evidence that the disclosure rules are beneficial [but] there is [also] no good evidence that the rules are harmful, or very costly.” These prior studies generally rely on long-run returns tests, which measure the net benefit of the Act and cannot speak to specific costs and benefits (e.g., whether firms’ financial disclosures became more informative as a result of the Act). We hand-collect financial statement data and earnings announcement dates for the period around the Act

¹ Rajan and Zingales (2003) argue that the Securities Act of 1933 and the Securities Exchange Act of 1934 laid “the accounting, regulatory, and legal foundation [...] for today’s vibrant financial system in the United States,” which they identify as one of the main drivers of the unprecedentedly rapid economic growth observed over the past century.

² Of the core private class action securities fraud lawsuits filed in 2019, 87% were based on the Act’s Section 10b-5, which regulates security purchases and sales (Cornerstone Research 2020, Figure 9).

and directly analyze short windows of market activity to determine whether the Act provided a specific benefit: more informative financial reporting.

We examine two market outcome variables to measure the informativeness of earnings announcements: perfect-foresight hedge portfolio returns and earnings response coefficients (ERCs).³ Returns on perfect-foresight hedge portfolios (that are long in stocks of firms that report an earnings increase and short in stocks of firms that report an earnings decrease) measure the value of accounting to investors as the trading return investors could earn if they knew the accounting information before it was released to the public (Ball and Brown 1968). ERCs, the coefficients obtained from regressing returns on earnings news, measure how much investors bid up (down) stock prices upon the announcement of favorable (unfavorable) news. Thus, if the Act increased financial reporting informativeness, we would expect perfect-foresight hedge portfolio returns and ERCs to increase.

We begin our analysis of a large, representative sample of New York Stock Exchange (NYSE) firms by studying the cumulative returns of perfect-foresight hedge portfolios. Consistent with a large increase in the value of accounting information, hedge portfolio returns during earnings announcement windows are significantly higher after than before the Act.⁴ By calculating market

³ We examine the robustness of our results to alternative approaches, such as the one proposed by Ball and Shivakumar (2008), and generally find evidence confirming our inferences. However, we focus on perfect-foresight hedge portfolio returns and ERCs because they are based on earnings information, while these alternative approaches are not. Given that our research question is whether the Act increased the information content of corporate earnings, conditioning our inferences on earnings information is crucial because alternative approaches are generally reflective not only of the earnings information, but also of all other information released in the earnings announcement (e.g., Beaver, McNichols, and Wang 2020).

⁴ These findings raise the question of whether the net benefit of the Act was positive or negative. We are not able to answer this question because of a lack of data on the costs of the Act. If one were to accept the prior literature's conclusion that the Act did not net affect welfare, this suggests that the costs of the Act are approximately as large as the benefits we estimate. Phillips and Zecher (1981) estimate that the aggregate direct costs of complying with SEC regulation were \$1 billion in 1980. However, they also estimate that in the same year, firms incurred costs of approximately \$2 billion just by distributing voluntary disclosures to investors, which also suggests that firms would have incurred much of the compliance costs voluntarily even in the absence of regulation. Thus, direct compliance costs appear to be small. However, indirect compliance costs—such as proprietary costs arising from product market competition; increased noise arising from mandatory, uninformative disclosures; and the substitution of mandatorily disclosed, less useful information for voluntarily disclosed, more useful information—may be substantial but are hard to measure (Verrecchia 1983; Easterbrook and Fischel 1984).

responses conditional upon the underlying earnings news and over a very short window, our test offers the statistical advantage of clearly attributing the documented effect to earnings disclosure rather than to dissemination through alternative information channels, and it mitigates the effects of other confounding events. Still, it is possible that contemporaneous structural developments unrelated to the Act changed the informativeness of financial reports.

To address this concern, we take an additional step and employ a fully interacted difference-in-differences design. While all firms were affected by the provisions of the Act, some firms were treated with greater intensity. Specifically, we follow Benston (1973) and identify firms that were more affected by the Act as firms that released financial statements before the Act but did not report sales information. A determinant model based on Barton and Waymire (2004) reveals that older, more profitable, smaller, and more regulated firms that face control conflicts are less likely to disclose sales in the pre-Act period. Other potential disclosure determinants, such as membership in the technology industry, exposure to systematic risk, profitability, capital issuance, market share, and dividend policy, do not seem to play a prominent role. We define the control group as firms that released financial statements with sales information in their reports before the Act. The Act forced firms to disclose sales numbers, mandated a set of generally accepted accounting standards, and strengthened enforcement considerably (Daines and Jones 2012). If these measures resulted in more informative financial reporting, we would expect the informativeness of financial information to increase more for firms that were more affected by the Act (i.e., treatment firms) than for firms that were less affected by the Act (i.e., control firms).

Consistent with the Act increasing the value of mandatory accounting disclosures to investors, we find that perfect-foresight hedge portfolio earnings announcement returns increase for treatment firms but decrease for control firms as a result of the Act. Further, after controlling for firm-level ERC determinants identified in prior literature and firm and industry-year fixed effects, we document that

ERCs increase more for treatment firms than for control firms, which is consistent with the Act causing investors to react more to earnings news. Robustness tests indicate that an underlying parallel trends assumption is satisfied: ERCs do not significantly differ for treatment and control firms in the pre-Act period, and they increase more for treatment firms than for control firms in the year after the Act takes effect. While compliance with the Act's sales disclosure requirement increases gradually after 1934, the increase in ERCs is immediate and remains approximately constant thereafter, suggesting that other features of the Act (such as increased enforcement) affect previously opaque firms more than the disclosure of sales per se. That is, non-disclosure of sales before the Act acts in part as a proxy for other reporting deficiencies beyond the absence of sales information.

We conduct three sets of additional tests to strengthen the validity of our inferences. First, we examine an increase in enforcement as a potential channel for the ERC results. Specifically, we document that the Act increases ERCs more for firms whose industries and headquarter states experience greater SEC enforcement intensity. Second, we test whether the Act affected the degree of return synchronicity after corporate earnings announcements. Consistent with the Act increasing the proportion of firm-specific information incorporated into stock prices, we find some evidence that, following firms' earnings announcements, return synchronicity decreases as a result of the Act. Third, we confirm that our findings are robust to several alternative variable measurement approaches.

Our paper relates to several literatures. First, we contribute to the literature examining the effects of the Securities Exchange Act of 1934. Prior studies document that market value and the liquidity of affected firms did not change as a result of the Act, which the authors interpret as evidence that the Act did not provide much value to investors (Benston 1973; Mahoney and Mei 2006; Daines and Jones 2012; Bourveau, Breuer, Koenraadt, and Stoumbos 2020a). However, due to data limitations, these studies face two research design issues. First, with the exception of Mahoney and Mei (2006), who rely on high-frequency liquidity measures around Form 10 filing dates for their main

tests, these studies rely on long-run stock return and market microstructure data, which increases the likelihood that other events could confound their results. We address this issue by examining investor reactions to corporate accounting disclosures during short three-day earnings announcement windows, which mitigates concerns that investors are reacting to an information event other than the earnings announcement (Ball and Brown 1968; Fama, Fisher, Jensen, and Roll 1969). Our use of earnings announcement windows instead of Form 10 filing days also improves upon Mahoney and Mei (2006), who document no significant market reaction around Form 10 filing dates but, in supplemental analyses, a significant market reaction around earnings announcement dates. Second, previous research does not control for whether the accounting information disclosed by companies is favorable or not. If treatment firms systematically experienced weaker profits after the Act, the authors might unintentionally interpret the associated poor return performance as evidence that the disclosure requirements imposed by the Act did not provide value to investors. We improve upon prior research designs by examining market reactions conditional upon the underlying accounting information. Both our perfect-foresight hedge portfolio return and ERC analyses provide direct evidence that the Act made accounting disclosures more informative.

Second, our paper relates to the mandatory disclosure literature more generally. Prior research in this area examines, among other things, capital markets and real effects of the adoption of IFRS in the European Union, amendments to the Securities Exchange Act of 1934, Regulation Fair Disclosure, and the Sarbanes-Oxley Act. For a review of this literature, see Leuz and Wysocki (2016). We add to this body of research by examining the capital market effects of the Securities Exchange Act of 1934, which is arguably the most expansive piece of disclosure regulation released in the United States to date. One characteristic of most of the prior mandatory disclosure regulation research is that some form of mandatory disclosure regime was already in place before the regulation under consideration was implemented. For example, most EU countries mandated the use of some form of country-

specific generally accepted accounting principles (GAAP) before the adoption of IFRS in 2005. Studying the 1934 Act allows us to examine the effects of moving from a federally unregulated information environment to a regulated mandatory disclosure regime that prescribes GAAP-based accounting attributes such as comparability, full disclosure, transparency, and verifiability (Zeff 2005).⁵ However, our design allows us to speak to only one particular benefit of the Act, more informative accounting numbers, but not to its costs or other benefits. As a result, we are unable to draw inferences about social welfare effects.

Third, our paper relates to the large value relevance literature.⁶ Due to data limitations, value relevance studies are generally restricted to the post-1950 period. One exception is the work of Ely and Waymire (1999a), who find that the power of earnings and book value of equity to explain annual returns did not change significantly during the pre-Compustat period. Our difference-in-differences design allows us to add to this literature by providing causal evidence that the Act increased the value relevance of corporate earnings. Our results also inform the claims in Benmelech, Kumar, and Rajan (2020) and Rajan and Zingales (2003) that accounting quality and reliability of reported numbers improved sufficiently during the 1900s to facilitate the rapid development of capital markets in the United States in the early 20th century.

Fourth, our paper relates to a body of research that examines the effects of firms' accounting disclosures in the unregulated pre-Act period during which voluntary disclosures were the major channel of corporate information dissemination, a time period when information intermediaries such

⁵ We also provide a foundation on which to interpret the findings of Greenstone, Oyer, and Vissing-Jorgensen (2006), Ferrell (2007), and Battalio, Hatch, and Loughran (2011), who examine stock returns around the Securities Act Amendments of 1964 that imposed the Act's requirements on smaller OTC firms. Greenstone et al. (2006) interpret their findings that OTC firms experienced higher stock returns and operating performance than control firms after becoming subject to the Act's requirements as evidence that the Act "causes managers to focus more narrowly on maximizing shareholder value" (p. 399). However, Leuz and Wysocki (2016) note that these findings could also be explained by a decline in firms' cost of capital resulting from improved disclosure. Our finding that investors react more to a given amount of earnings news after the Act is consistent with Leuz and Wysocki's (2016) argument.

⁶ See, e.g., Collins, Maydew, and Weiss (1997), Lev and Zarowin (1999), Francis and Schipper (1999), and Barth, Li, and McClure (2019).

as security analysts and rating agencies played a relatively minor role.⁷ The evidence presented in papers studying this period is generally descriptive. Our results add to this literature by providing causal evidence that earnings disclosures become more valuable and more informative as a result of the Securities Exchange Act of 1934 and thereby answer Ely and Waymire's (1999a) call for more research on the impact of specific standards on the relevance of accounting data.

2. Background

Congress and the Roosevelt Administration passed the Securities Act of 1933 and the Securities Exchange Act of 1934 in reaction to the 1929 stock market crash. Legislators argued that the crash was caused by security markets manipulation and excessive speculation, which led to “sudden and unreasonable fluctuations in the prices of securities”; caused a contraction of “credit available for trade, transportation, and industry in interstate commerce”; prevented “a fair calculation of taxes owing to the United States and to the several States by owners, buyers, and sellers of securities”; prevented “the fair valuation of collateral for bank loans”; and obstructed “the effective operation of the national banking system and Federal Reserve System” (US Congress 1934, Section 3.a.4).

The Securities Act of 1933 regulates legal and disclosure requirements for initial public offerings (IPOs)⁸, and the Securities Exchange Act of 1934 governs secondary market securities transactions on regulated exchanges and over-the-counter (OTC) markets and the conduct of financial intermediaries, i.e., investment advisors, brokers, and dealers. The Act, which was among the first mandatory disclosure regulations in the United States, required firms to file audited financial

⁷ See, e.g., Sivakumar and Waymire (1993, 1994), Porter, Sivakumar, and Waymire (1995), Ely and Waymire (1999a), Ely and Waymire (1999b), Barton and Waymire (2004), Granja (2018), and Bourveau, Breuer, and Stoumbos (2020b).

⁸ Several papers examine whether the Securities Act of 1933 affected the stock returns of newly listed firms. See, e.g., Stigler (1964), Jarrell (1981), and Simon (1989). Similar to the findings for the Securities Exchange Act of 1934, their findings generally show that return volatility decreased while mean returns did not change.

statements with a newly established regulatory body, the SEC. While there had been multiple attempts by states and private institutions such as the NYSE⁹ to establish a mandatory disclosure system in the United States, these attempts were ineffective because disclosures were “purely voluntary, were easily avoided, and were not rigorously enforced” (Keller and Gehlmann 1988, p. 334). However, researchers disagree on whether fraud was pervasive before the Act. Benston (1969) searches the literature for references to financial fraud and misrepresentation but finds no evidence that fraud was more prevalent before the Act than after it. Seligman (1983) critiques Benston’s (1969) search methodology. His own literature review reveals that fraud was much more prevalent before than after the Act. Further, many forms of financial misconduct as we define it today, such as insider trading and price fixing, were not illegal in the early 1900s and thus would not have been classified as fraud by the contemporary literature (Sutherland 1949; Soltes 2016).

As a result of the prior efforts by states and private institutions to regulate disclosure, many firms had already reported on a voluntary basis before the Act passed in Congress (Benston 1969, 1973). Among other things, the Act required all firms trading on national exchanges to file audited balance sheets, income statements, and “any further financial statements which the Commission may deem necessary or appropriate for the protection of investors” (US Congress 1934, Section 12.b.1).¹⁰ The Act also strengthened the enforcement of security laws. Before the Act, legislation was enforced predominantly at the state level through “blue sky laws” or by private associations. However, enforcement was ineffective, because it was relatively easy for firms to move their legal presence to a state that promised them more favorable treatment, and because the states themselves were competing

⁹ Effective on July 1, 1933, the NYSE started to require newly listed companies to provide audited financial statements on a quarterly basis (Zeff 1971, p. 123). However, these regulations did not apply to the firms that already traded on the exchange. Their original listing agreements with the exchange were upheld until the Exchange Act of 1934 became effective (Mahoney 1997).

¹⁰ Daines and Jones (2012) document that 92% of firms that did disclose before the Act were audited even before the Act made audits mandatory in 1934.

for firms' business to secure tax revenues and employment for their citizens. Moreover, the government officials charged with enforcement lacked funding and expertise, and enforcement by private associations such as the NYSE was weak (Blough 1939; Seligman 1983; Keller and Gehlmann 1988).

For the first time in history, the SEC's newly established Office of the Chief Accountant and the Division of Corporate Finance prescribed "the methods to be followed in the preparation of [financial] reports," which later evolved into US GAAP (US Congress 1934; Zeff 1971, 2005). The original principles emphasized comparability, full disclosure, transparency, and historical cost accounting. The goal was to unify the plethora of accounting practices used for similar transactions prevalent at that time and to impede the issuance of misleading disclosures (Healy 1938). Prior to the Act, the NYSE and the American Institute of Accountants (AIA; now the American Institute of Certified Public Accountants, AICPA) had recommended that firms orient their disclosures on six "broad principles of accounting which have won fairly general acceptance" published by the American Institute of Accountants (1934) shortly before the Act took effect.¹¹ However, the application of these principles to financial reports, as well as the decision of whether or not to disclose in the first place, was at the disclosing firm's discretion (Zeff 1984, p. 452).

The Act's Section 18 established civil liability for corporate officers who concealed or misrepresented material facts in financial reports, put the burden of proof on the firm rather than the plaintiff, made firms liable for damages to investors who based their trading decisions on false or misleading information included in financial reports, and allowed courts to award attorney fees to

¹¹ These initial principles relate to the timing of profit realization, parent and subsidiary capital surplus allocation, treatment of dividends from investments, accounts receivable due from insiders, and treasury stock (American Institute of Accountants 1934). While the principles did not emphasize conservatism, Benston (1969, p. 526) writes: "[The SEC] chose to accept generally practiced accounting procedures, and to encourage and even enforce the conservative bias generally followed by public accountants. Most prominently, the Commission has been a very strong force in banishing from accepted accounting practice the writing up of assets above their original cost." Similarly, Rappaport (1963, p. 5.3) states that "there was a period ... when the SEC was conducting something in the nature of a campaign to eliminate goodwill from all balance sheets filed with it."

plaintiffs who would otherwise not be able to afford lengthy and costly lawsuits. Further, the Act equipped the SEC with broad powers to legislate, punish, and enforce securities law at the federal level. The effects materialized rapidly. Daines and Jones (2012, p. 12) note that within “the first four years, the SEC brought hundreds of civil proceedings, securing 288 permanent injunctions against 657 firms and individuals and the Department of Justice secured criminal convictions of 403 defendants.”

While the Act was approved on June 6, 1934 and took effect on July 1, 1934, firms could apply to the SEC for an extension to delay the first reporting date at which they had to follow the provisions of the Act until after July 1, 1935 (US Congress 1934, Section 12.e). While some firms started disclosing according to the new rules in spring 1935, with Western Auto being the first to disclose under the new regime on March 15, 1935, most waited until the final deadline (Benston 1969; Daines and Jones 2012).

Some studies examine the Act’s effects and find mixed evidence on whether it led to desirable outcomes. Benston (1973) compares how the betas and abnormal returns of firms that did not disclose sales voluntarily before the Act changed relative to those of firms that did, and finds that they did not change significantly. Jarrell (1981) finds that firms defaulted less on their bonds after the Act. Chow (1983) examines stock and bond market reactions to events during the deliberation period of the Act that made the passage of the Act appear more likely. He documents a negative stock market reaction but a positive bond market reaction. Mahoney and Mei (2006) and Daines and Jones (2012) find mixed evidence that the Act led to decreased information asymmetries measured as bid-ask spreads and increased trading volume. Bourveau et al. (2020a) document that the Act did not substantially affect the frequency, contents, and informativeness of audit reports.

The interpretations of this evidence vary widely. Some interpret the findings that stock returns remained unchanged while stock market volatility and default rates decreased after the Act as evidence that the Act did not increase shareholder welfare and systematically excluded small, risky firms from

public exchanges (Stigler 1964; Benston 1969, 1973; Jarrell 1981; Chow 1983; Easterbrook and Fischel 1984). Others attribute these findings to sample selection and interpret them as evidence that the Act induced the industry to take more self-corrective action, which led to reduced risk and agency costs, and improved investor protection and the allocational efficiency of financial markets (Friend and Herman 1964; Robbins and Werner 1964; Keller and Gehlmann 1988; Seligman 1983; Coffee 1984).

Subsequent studies have circumvented some of the research design issues plaguing this literature by examining two regulatory changes that extended the universe of firms subject to the Act's disclosure requirements: the 1964 Securities Amendments Acts, which extended the Act to over-the-counter (OTC) firms with more than \$1 million in assets and more than 500 shareholders, and the 1999 Eligibility Rule, which extended the Act to the remaining OTC firms. Greenstone et al. (2006), Ferrell (2007), and Battalio et al. (2011) study the 1964 Amendments Acts. Greenstone et al. (2006) and Ferrell (2007) provide evidence that the announcement of amendments increased (decreased) affected firms' stock returns (stock return volatility). They interpret their findings as evidence that mandatory disclosure incentivizes managers to focus more on creating value for their shareholders. Questioning this inference, Battalio et al. (2011) find no change in stock returns on days on which OTC firms announce that they want to list at the NYSE, and they provide evidence that most firms already filed financial statements with the National Association of Securities Dealers before the amendments. Further, the authors document that more OTC firms moved to the NYSE after the amendments, and they identify the NYSE as a beneficiary of the increase in regulation.

Bushee and Leuz (2005) and Burnett (2020) study the 1999 Eligibility Rule. Bushee and Leuz (2005) document that while the extension imposed significant costs on firms by forcing many to leave the OTC Bulletin Board, it also provided significant benefits in the form of positive stock returns and long-lasting increases in liquidity for the firms that remained listed. Burnett (2020) documents that the Eligibility Rule strengthened the value relevance of several financial statement accounts including total

assets; total liabilities; sales; cost of goods sold; sales, general and administrative expense; and research and development expense. However, inconsistent with the confirmatory role of mandatory disclosure (Gigler and Hemmer 1998), Burnett fails to find evidence that the increase in mandatory disclosure increased the credibility of firms' voluntary disclosures.

3. Research Design

The Act mandated that all firms use comparable, transparent, verifiable, and economically sensible accounting standards and strengthened the previously weak enforcement of securities legislation. If these measures increased the information contained in firms' accounting numbers, investors' trust in and reliance on firms' financial reports would increase, leading to stronger market reactions to a given amount of financial news around the disclosure date.

In contrast to prior studies that examine the Act's net effect, we seek to inform the debate by documenting a specific benefit of the Act. We test whether accounting numbers published in annual reports after the Act became more informative by examining short-window perfect-foresight hedge portfolio returns and earnings response coefficients (ERCs). Perfect-foresight hedge portfolio returns approximate the value of earnings disclosures by calculating how much investors could have earned if they had known the earnings information disclosed on the earnings announcement date before that date. We measure perfect-foresight hedge portfolio returns as the cumulative returns of a portfolio long in stocks of firms that report an earnings increase and short in stocks of firms that report an earnings decrease at the earnings announcement date. We compare these hedge portfolio returns before versus after the Act. If the Act increased the value of accounting information through increased disclosure or improved enforcement, we would expect perfect-foresight hedge portfolio returns to increase as a result of the Act.

ERCs measure the strength of the stock market reaction to a given increase in unexpected earnings. If the Act increases earnings persistence through higher accounting quality or stricter enforcement, ERCs will increase. This is because the precision of an earnings signal increases when earnings are more persistent and manipulated less, inducing investors to revise their future cash flow expectations and thereby their firm value estimates more strongly in the direction of the earnings signal. That is, investors bid stock prices up (down) more in response to an earnings increase (decrease), leading to larger ERCs (Kormendi and Lipe 1987; Fischer and Verrecchia 2000).¹² Following a large literature (for a summary, see Dechow, Ge, and Schrand 2010), we compute ERCs as the slope coefficient obtained by regressing cumulative three-day stock market returns around the earnings announcement on a measure of unexpected earnings.¹³

We employ a difference-in-differences design to isolate the effects of the Act and to further mitigate concerns that factors such as macroeconomic fluctuations confound our results. As in Benston (1973), we define our treatment group as firms that filed financial statements before the Act but strategically withheld important items—in particular, they did not report sales revenue. Sales disclosure was the principal reporting requirement imposed by the Act.¹⁴ Thus, while all firms were

¹² There are two alternative channels through which the Act could affect ERCs. First, if the Act decreases investors' perception of the firm's exposure to systematic risk through higher accounting quality, ERCs will increase. This is because higher accounting quality could reduce investors' perception of the covariance between the firm's and other firms' earnings, which would lower investors' perceptions about the firm's exposure to systematic risk and thereby the required rate of return investors demand for investing in the firm (Lambert, Leuz, and Verrecchia 2007). As a result, changes in expected future earnings caused by an earnings signal would be discounted less and investors' estimates of firm value change more in the direction of the earnings signal, leading to larger ERCs (Easton and Zmijewski 1989). Second, if the Act increases earnings growth by facilitating better corporate decision-making through improved monitoring resulting from higher accounting quality, ERCs will increase (Collins and Kothari 1989). This is because higher monitoring curtails managers' ability to extract rents via shareholder value-destroying actions, which changes investors' perceptions of the ratio of expected earnings to systematic risk. This change can reduce firms' cost of capital, leading to larger ERCs (Lambert et al. 2007).

¹³ Our results are robust to using a [-1, +7] instead of a [-1, +1] earnings announcement window.

¹⁴ By surveying financial executives, Graham (2022) provides evidence that for most firms sales revenue is the preeminent variable for corporate planning. This suggests that, at least in modern times, withholding sales information might give a firm a strategic advantage over its peers. It stands to reason that reporting sales provides valuable information to investors. Further highlighting the importance of sales, especially during our sample period, Benston (1973) finds that “except for the sales definition of financial data, none of the financial data variables in any of the expectations forms has a greater than minimal economic relationship to changes in stock prices” (p. 139). Notwithstanding these arguments, another

affected by the Act's provisions, firms that did not voluntarily disclose sales before the Act were relatively more affected. Following Benston (1973), we define our control group as firms that provided financial statements that included sales information even before the Act and thus were relatively less affected. Figure 1 plots the proportion of firms not disclosing sales over time. Consistent with strict enforcement, the percentage of firms that do not report sales falls rapidly after the Act: 1930 (35%), 1931 (40%), 1932 (42%), 1933 (41%), 1934 (40%), 1935 (31%), 1936 (22%), 1937 (13%), and 1938 (6%).

The change in investors' reactions to earnings disclosures as a result of the Act might differ for treatment and control firms for two reasons. First, the Act requires treatment firms to disclose an additional piece of information, sales, which might increase investors' understanding of the resulting earnings number (Keung 2010; Merkley, Bamber, and Christensen 2013). Second, investors might perceive relatively more opaque firms that did not disclose sales before the Act as more likely than other firms to deceive through creative accounting. The generally accepted accounting principles and the increased enforcement brought by the Act could discipline these firms and thereby increase investors' trust in the resulting earnings number (Shin 1994). While we are unable to distinguish between these two explanations, we note that both are consistent with the hypothesis that the Act increased the informativeness of accounting figures.

For our portfolio-level tests, we compute hedge portfolio returns for treatment and control firms in the pre- and post-Act periods. We estimate the Act's causal effect on both variables as the difference between the changes for treatment and control firms as a result of the Act. For our firm-level ERC tests, we estimate the following fully interacted equation:¹⁵

consideration is that non-reporting of sales before the Act could also proxy for a given firm being opaque more generally; we discuss this possibility below in the context of an enforcement interpretation of our results.

¹⁵ The main effects of *Post* and *Treat* are absorbed by the firm and industry-year fixed effects.

$$\begin{aligned}
Return_{i,t} = & \beta_0 + \beta_1 Treat_i \times Post_t \times \Delta E_{i,t} + \beta_2 Treat_i \times Post_t + \beta_3 Treat_i \times \Delta E_{i,t} \\
& + \beta_4 Post_t \times \Delta E_{i,t} + \beta_5 \Delta E_{i,t} + \Gamma_i + \phi_t + Controls + \varepsilon_{i,t}
\end{aligned} \tag{1}$$

where $Return_{i,t}$ denotes the cumulative three-day earnings announcement stock market return of firm i in year t , $Treat_i$ denotes an indicator that the firm files financial statements but does not disclose sales in 1932, $Post_t$ denotes an indicator for the post-Act period (i.e., 1935 and later), $\Delta E_{i,t}$ denotes unexpected earnings measured as the change in earnings relative to the last fiscal year scaled by average total assets (Collins and Kothari 1989), Γ_i denotes a firm fixed effect, and ϕ_t denotes an industry-year fixed effect based on the two-digit SIC industry classification.¹⁶ $Controls$ is a vector that contains firm-level determinants of ERCs documented in prior literature (Collins and Kothari 1989; Easton and Zmijewski 1989; Freeman and Tse 1992). Our main coefficient of interest is the slope coefficient β_1 , which measures the differential change in ERCs for treatment firms as a result of the Act.

In additional tests, we examine the robustness of our results in several analyses: employing a propensity score matched sample based on the disclosure determinant model proposed in Barton and Waymire (2004); examining enforcement as a potential channel; examining whether the Act led to less return synchronicity; and employing alternative earnings surprise measurement approaches. More details are provided below.

4. Data

We collect accounting data for NYSE firms from the Moody's Industrial Manuals using a double data entry procedure (Graham, Leary, and Roberts 2015; Graham and Leary 2018)¹⁷ and CRSP

¹⁶ Our results are robust to using abnormal returns as the dependent variable and to using year instead of industry-year fixed effects.

¹⁷ The Moody's Industrial Manuals do not cover financials (SIC codes 6000–6999) or utilities (SIC codes 4900–4999).

stock return data from 1930 to 1938. Maintaining a relatively tight window around the Act mitigates concerns that other structural changes might confound our results but nevertheless gives us enough data to test for parallel trends in the years preceding the Act. The majority of public firms were traded on the NYSE in the 1930s and voluntarily disclosed earnings before the Act (Benston 1973). Indeed, we verify that the Moody's sample comprises 85% of the non-financials, non-utilities CRSP universe in 1932 and can thus be regarded as large and representative.

Collecting *Wall Street Journal* (WSJ) earnings announcements is burdensome because it requires the researcher to search through each daily issue of the WSJ over the sample period. We hired 10 research assistants to collect the data from the WSJ archive on ProQuest. To ensure data quality and to boost morale, we reached out to each team member on a weekly basis, collected feedback on the data-gathering process, and provided individual performance metrics. Further, we met with the research assistants as a group every three months to discuss the project status and to collect additional feedback to improve process quality and efficiency. To improve data quality, we used double data entry, where each announcement was independently located by two different research assistants. Appendix B presents some examples of the WSJ earnings announcements published in the 1930s.

Table 1 presents the SIC two-digit industry composition of our sample. Our sample firms come from a wide range of different industries with concentrations in Food & Kindred Products (9.93%), Chemical & Allied Products (7.01%), Primary Metals (10.17%), and Transportation Equipment (10.29%). Table 2 shows the descriptive statistics. We winsorize all continuous variables at the 1st and 99th percentiles. On average, earnings changes reported in earnings announcements are approximately 0% of total assets, though there is heterogeneity in the change in earnings: 25th and 75th percentiles of -3% and 3%; 1st and 99th percentiles of -18% and 18%. *Return* has a mean and median close to zero but exhibits heterogeneity with a standard deviation of 7% and an interquartile range of 6%. *Treat*'s mean indicates that 38% of our sample firms do not disclose sales before the Act, which

is identical to the 38% documented in Benston (1973). *Post*'s mean of 0.46 indicates that our sample is approximately evenly distributed around the Act's effective date. Table 3 presents the correlation matrix. Pearson (Spearman) correlations are above (below) the diagonal. Unexpected earnings correlate positively with returns (Ball and Brown 1968; Beaver 1968). Investors bid up prices response to earnings news. Unexpected earnings are higher in the post-Act period.

We employ Barton and Waymire's (2004) voluntary disclosure determinant model to explore which factors drive firms' decisions to voluntarily disclose sales in 1932 by regressing *Treat* on measures for equity market information cost including a firm's age (the number of years since the first time the firm appeared in Moody's Manuals, *Age*), membership in the technology industry (an indicator that the firm is a member of the technology industry, *Tech*), earnings variability (the standard deviation of earnings scaled by average total assets calculated over the current and the previous four years, *Std(E)*), systematic risk (the slope coefficient obtained from regressing the firm's excess return on the market risk premium over the [-100, -10] and [+10, +100] day window around the earnings announcement, *Beta*), and capital issuance (an indicator that the number of shares outstanding increased by more than five percent over the fiscal year, *Issue*); measures for contractual and control conflicts including financial leverage (total debt scaled by total assets, *Leverage*), income conflicts (an indicator that the firm has income bonds, non-cumulative preferred stock, or another type of stock with participation rights, *Income Conflict*), control conflicts (an indicator that a voting trust or another company controls the firm, that the firm has a second class of outstanding voting common stock, or that outstanding preferred equity allows unrestricted voting even in the absence of financial distress, *Control Conflict*), and the state of incorporation (an indicator that the firm is chartered in Delaware, *Delaware*); measures for competitive and political costs including a firm's market share (total assets divided by the contemporaneous sum of total assets of all sample firms in the same 2-digit SIC code industry, *Market Share*) and size (the natural logarithm of total assets, *Size*); and measures for alternative

information including a firm's dividend policy (an indicator that the firm pays a dividend, *Dividend*) and membership in a regulated industry (an indicator that the firm is a member of a regulated industry, *Regulated*). The results are shown in Table 4.

Note that our dependent variable reflects firms' decision to withhold disclosure, while Barton and Waymire's (2004) dependent variable reflects firms' decision to disclose more. Thus, we would expect the slope coefficients in our regression to take the opposite sign of those in Barton and Waymire (2004). We generally find that this is the case. When regressing *Treat* on each group of determinants individually, we find evidence that older firms, firms with more variable earnings, more profitable firms, firms with lower leverage, and firms facing control conflicts are more likely to withhold sales disclosure. In the multivariate regression, the slope coefficients of *Std(E)* and *Leverage* become insignificant, and the slope coefficients of *Size* and *Regulated* become significant. None of the other slope coefficients are statistically significant at conventional levels.

5. Results

5.1. *Perfect-Foresight Hedge Portfolio Returns*

We first test whether the introduction of the Securities Exchange Act increased the amount of information released in earnings announcements. To start this analysis, we form hypothetical perfect-foresight portfolios and observe the return an investor could earn if she had the earnings information in advance.

In Figure 2, we plot the cumulative returns of stocks of firms that report an earnings increase (*Good News*) and the cumulative returns of stocks of firms that report an earnings decrease (*Bad News*) around the earnings announcement before (Panel A) and after (Panel B) the Act. Following Patell and Wolfson (1982) and Abdel-Khalik (1984), who document that the WSJ publishes most earnings

announcements within the three-day window of a firm's official press release, we shade the three-day earnings announcement window in gray.

Cumulative returns for the good news portfolio build up before and after the Act, which is consistent with the findings of Sivakumar and Waymire (1993, 1994), who document that investors react to earnings numbers even in the discretionary disclosure environment of the pre-SEC period. However, while the cumulative returns for the bad news portfolio fall in the post-Act period, they increase in the pre-Act period. As a result, there is a clear spread to be earned from building hedge portfolios in the post-Act period but not in the pre-Act period. We thus find evidence that earnings announcements became more informative following the implementation of the Act.

Importantly, the inferences we can draw using this simple design are limited because the test does not control for other economic factors that might confound the results. It is also possible that corporate officers more frequently traded upon their inside information before the earnings announcement date before the passage of the Act made it illegal. If so, market responses during the earnings announcement period would increase after the Act not because financial reporting became more informative, but because the price already (partially) reflected the news before the earnings announcement in the pre-Act period. We address these weaknesses next. We use a more formal difference-in-differences analysis because it is unclear why treatment firms should be differentially exposed to macroeconomic fluctuations or why treatment firm officers would engage in more insider trading around earnings announcements than control firm officers.

Specifically, Table 5 computes perfect-foresight hedge portfolio earnings announcement returns for portfolios containing treatment and control firms before and after the Act. While returns increase for the treatment firm portfolio, they decrease for the control firm portfolio. Further, while control firm portfolio returns are higher than treatment firm portfolio returns before the Act, treatment firm portfolio returns are higher after the Act. As a result, the difference-in-difference

estimate is significantly positive. Consistent with the Act increasing the value of accounting information, perfect-foresight hedge portfolio earnings announcement returns increase by 1.09% as a result of the Act, which is 57% (103%) of the control (treatment) portfolio return in the pre-Act period.

5.2. Earnings Response Coefficients

Our tests so far incorporate only binary earnings information (i.e., earnings either increase or decrease) and thus do not allow us to infer how investors changed their reaction to a given amount of earnings information as a result of the Act. We now analyze ERCs to address this shortcoming. Table 6 Panel A presents the results of estimating Equation (1) under all possible firm and industry-year fixed effects combinations and with and without controls. Firm fixed effects control for time-invariant firm characteristics, and industry-year fixed effects control for other industry-wide developments that could confound our results. Standard errors are clustered by firm and year.

ERCs, measured as ΔE 's slope coefficient, are positive across all specifications (Easton and Zmijewski 1989). Most importantly for our hypotheses, the significantly positive interaction term $Treat \times Post \times \Delta E$ indicates that the earnings response coefficient increased more for treatment firms than for control firms as a result of the Act across all specifications. This is evidence that the earnings disclosures of firms that were forced by the Act to increase the extent of their disclosures became more informative relative to the earnings disclosures of firms that voluntarily disclosed before. In terms of economic significance, the results in Column (8) indicate that while non-treatment firms' ERCs did not change, treatment firms' ERCs increased by 41.57% ($= (0.243 - 0.077 - 0.083 + 0.152) / (0.243 - 0.077) - 1$) as a result of the Act.

We can interpret some of the slope coefficients relative to the results and arguments in prior literature. First, stock returns are on average lower for treatment than for control firms, but the difference in differences in stock returns for treatment relative to control firms, after relative to before

the Act as measured as the slope coefficient of the interaction term $Treat \times Post$, is insignificant. Therefore, our findings indicate that the Act does not affect treatment firms' unconditional stock returns. This has been interpreted by other authors as evidence that the Act did not increase shareholder welfare (e.g., Benston 1973). Second, consistent with investors discounting treatment firms' earnings disclosures in the pre-period (perhaps because they either lacked sales information or mistrusted treatment firms' pre-Act accounting practices), the estimated earnings response coefficients are smaller for treatment relative to control firms before the Act.

Figure 3 tests the underlying parallel trends assumption of our difference-in-differences research design by re-estimating Equation (1) on the panel data after replacing $Post$ with fiscal year indicators (Christensen, Hail, and Leuz 2016). The base year for this analysis is 1934, the year before the Act took effect. Consistent with the parallel trends assumption, there is no evidence that ERCs differed for treatment firms relative to control firms or systematically shifted upwards over time in the years before the Act. The by-year treatment effect turns significantly positive in 1935, indicating that treatment firms experience a relative increase in ERCs right after the Act's effective date, and it remains positive thereafter. Combined with Figure 1's finding that sales disclosure increased gradually after 1934, this evidence suggests that both the disclosure of sales and—to an even greater degree—other features of the Act such as increased enforcement affected previously opaque firms. We examine enforcement in more detail in Section 5.3.

Lastly, we examine the robustness of our results by using a propensity score matching approach that matches one control firm to each treatment firm based on propensity scores derived from the Barton and Waymire (2004) disclosure determinant model presented in Table 4. Table 6 Panel B presents the results. While the additional data requirements reduce the number of observations (4,151 vs. 952 observations), the magnitude of the documented effect increases

substantially and remains statistically significant across all models, demonstrating the potency of Barton and Waymire's (2004) determinant model.

5.3. Cross-Sectional Variation in Enforcement

As discussed above, the findings in Figures 1 and 3 that ERCs increase immediately after the Act and stay approximately constant thereafter, while sales disclosure increases more gradually, suggest that increased enforcement may be an important driver behind our results. To explore this hypothesis, we examine whether our results are pronounced for firms that are exposed to higher enforcement intensity as measured by SEC enforcement actions brought against 1) firms in their industry and 2) firms in their geographic location (headquarter state). To facilitate this analysis, we collect data on industry-level and state-level enforcement actions from the SEC's annual reports.

5.3.1. Enforcement Against Firms in the Same Industry

For the industry-based measurement, we read the description of each SEC enforcement action to determine whether the enforcement action was brought against a firm and the industry to which such a firm belongs (Schenck 2012). Of the 275 enforcement actions brought by the SEC during the 1935 to 1938 period, 170 involved firms. Of these 170 firms, we were able to assign an industry to 83 firms, with the largest two industries being mining (41 of the 83) and finance (29 of the 83). For the remaining firms, we were not able to assign an industry classification. Because CRSP and the Moody's Industrial Manuals do not cover financial companies during our sample period, our analysis focuses on nonfinancial firms only. Hence, given the large proportion of cases brought against firms in the mining industry among firms in our sample, we measure industry-based enforcement as an indicator that a given firm belongs to the mining industry (1-digit SIC code: 1) and interact the resulting *Enforcement* indicator with the key interactive variable $Treat \times Post \times \Delta E$. Our classification is consistent with Mahoney (2003) and Agrawal (2013) who argue that agency conflicts were pronounced in the

mining industry during the early 1900s. Their reasoning suggests mining firms may be prime targets for strict enforcement by the newly established SEC.

Table 7 Columns (1) and (2) present the results of estimating the fully interacted model before and after controls. As before, $Treat \times Post \times \Delta E$'s slope coefficient is significantly positive, indicating that the Act increased ERCs broadly. Moreover, as evidenced by the significantly positive and economically large $Treat \times Post \times \Delta E \times Enforcement$ slope coefficient, the effect is significantly stronger for firms the mining industry, suggesting that enforcement plays an important role in the post-Act increase in ERCs.

5.3.1. Enforcement in the Firm's Geographic Region

For the geographic-based measurement of enforcement intensity, we collect data on the location of company headquarters from the Moody's Manuals and examine whether the results are pronounced for firms that experience higher SEC enforcement intensity in their headquarter states (Kedia and Rajgopal 2011; Silvers 2016; D'Acunto, Weber, and Xie 2019). We measure state-level enforcement intensity as the number of 1935 to 1938 SEC enforcement actions in a given state scaled by the amount of 1935 IRS tax collections in the state.¹⁸ The scaling is intended to normalize relative to a measure of business activity in a given state. We collect data on state-level tax collections from the IRS's annual reports.

Table 7 Columns (3) and (4) replace the industry-based enforcement measure with an indicator that a firm's headquarter is located in a state with above-median enforcement intensity among sample states, as measured by the number enforcement actions against firms divided by the amount of corporate tax collections. Based on this headquarter-based enforcement measurement, the $Treat \times Post \times \Delta E \times Enforcement$'s slope coefficient is significantly positive, indicating that post-Act ERCs increase

¹⁸ We also use the number of sample firms with headquarters in a given state as an alternative scalar. Our inferences remain unchanged.

proportional to enforcement intensity, thus corroborating the industry analysis in Columns (1) and (2). Lastly, to examine the robustness of the headquarter-based measurement approach, we include enforcement activity against individuals in the computation; specifically, we measure enforcement intensity as the number of enforcement actions against firms and individuals, divided by the amount of tax collections from firms and individuals. Table 7 Columns (5) and (6) present the results. Our inferences remain unchanged.

As a whole, we find evidence that the informativeness of financial reports increases more for firms that experienced increased enforcement intensity in their industry or geographic region as a result of the Act. This complements the findings in Figures 1 and 3 and suggests that enforcement is a driver of our findings.

5.4. Return Synchronicity

Lastly, we examine another aspect of investors' processing of information released in earnings announcements: return synchronicity. Prior literature interprets the degree to which a firm's stock returns move with its industry or the market as a measure of the quality of the firm's information environment. The line of reasoning put forward by this literature posits that the Act can affect firms' return synchronicity with other firms following earnings announcements in at least two ways. On the one hand, if the Act improves the quality of a firm's information environment, the proportion of firm-specific information incorporated in stock prices after firms' earnings announcements will increase, reducing return synchronicity (Piotroski and Roulstone 2004). On the other hand, if the Act increases disclosure similarity by mandating that firms apply a common set of accounting rules when preparing their annual reports, return synchronicity will increase for two possible reasons. First, firms are not able to tailor their accounting practices to their individual needs anymore; this reflects a worsening of the quality of firms' information environment and thereby decreases the proportion of firm-specific information incorporated in stock prices (Kothari, Ramanna, and Skinner 2010). Second, investors

now learn more about other firms from a given firm’s report, which increases information spillovers (Dyer, Roulstone, and Van Buskirk 2020).

We examine the Act’s effect on disclosure similarity by estimating the following equation:

$$Return\ Synchronicity_{i,t} = \beta_0 + \beta_1 Treat_i \times Post_t + \beta_2 Treat_i + \beta_3 Post_t + \Gamma_i + \phi_t + Controls + \varepsilon_{i,t}, \quad (2)$$

where $Return\ Synchronicity_{i,t}$ denotes return synchronicity measured as the R-squared obtained from a firm-level regression of weekly returns on contemporaneous and lagged market and 2-digit SIC industry returns estimated over the 52 weeks following the earnings announcement (requiring a minimum of 45 weekly returns) and scaled by one minus that same R-squared (Piotroski and Roulstone 2004). All other variables are defined as previously.

Table 8 presents the results. Consistent with an increase in the proportion of firm-specific information incorporated in stock prices following the firm’s earnings announcements, Columns (1) to (4) display a reduction in return synchronicity following the Act. However, the effect is insignificant in Column (4) after we include controls, firm fixed effects, and industry-year fixed effects. Overall, like the hedge portfolio and ERC results, the return synchronicity results provide some additional supportive evidence to suggest that the Act improves firms’ information environments.

6. Robustness Tests

We conduct two additional tests to examine the robustness of our findings in the previous sections. First, we test the sensitivity of our inferences to alternative earnings surprise measures computed from AR(1) and Hou, Van Dijk, and Zhang (2012) earnings forecasting models estimated either for the full sample or by industry. Table 9 presents the results. The estimated post-Act increase in ERCs is larger for these alternative models, relative to ERCs based on the measure of earnings

surprise used earlier in the paper. For example, measuring earnings surprises via an AR(1) model estimated by industry increases the magnitude of the triple interaction term by 45.39% ($= (0.221 - 0.152)/0.152$, where 0.152 is from Column (8) of Table 6). Second, in untabulated analyses, we examine the robustness of our main tests to alternative winsorization levels, as well as alternative scalars such as beginning-of-year and end-of-year total assets. Our inferences remain unchanged.

7. Conclusion

The Securities Exchange Act of 1934 is the most expansive piece of secondary market regulation enacted in US history. Nonetheless, critics question whether the Act made accounting more useful. In this study, we are the first to use modern techniques to examine whether the Act's implementation of a mandatory disclosure system and/or the substantial increase in enforcement of accounting standards and financial regulation made the earnings news released in earnings announcements more informative to investors.

We employ short-window tests and difference-in-differences designs to examine how perfect-foresight hedge portfolio returns and ERCs change as a result of the Act. After the Act, perfect-foresight portfolios earn significantly higher returns and treatment firms (i.e., firms that did not voluntarily disclose sales before the Act) experience a lasting increase in ERCs, which is consistent with the Act making firms' financial reports more informative. Further, we document that the increase in ERCs is more pronounced for firms that are faced with the prospect of greater enforcement of the Act. Lastly, consistent with the Act improving accounting quality and increasing the proportion of firm-specific news impounded in stock prices, we find some evidence that firms' return synchronicity with their industry and the market decreases.

Our study is subject to a number of limitations. First, while several of our results suggest that increased enforcement plays an important role, it is difficult to differentiate whether the

implementation of a broad mandatory reporting regime or increased enforcement (or both) is the mechanism that causes increased earnings informativeness. Our results should thus be interpreted as the joint effect of these two mechanisms in increasing the informativeness of earnings.

Second, while we document that firms' accounting reports became more informative, we are unable to determine whether the Act increased investor or social welfare. Nor do we study the costs and benefits of regulation broadly. To do so, one needs to consider all resulting costs and benefits jointly. Our setting and research design do not allow us to speak to these issues. Instead, we focus on one specific benefit: increased disclosure informativeness.

Third, following Benston (1973), we rely on firms that disclosed sales voluntarily before the Act as our control group. As noted by Leuz and Wysocki (2016), voluntary disclosure is a choice variable to the firm, which complicates the interpretation of results for our treatment versus control firms. While we address this concern by accounting for the determinants of this disclosure choice and by testing for parallel trends in our research design, this concern cannot be entirely eliminated and should be kept in mind when interpreting our results.

Fourth, one drawback of focusing on short-window earnings announcement returns to compute hedge portfolio returns or ERCs is that it does not allow us to speak to whether the joint informativeness of all corporate disclosures increases as a result of the Act. Specifically, the Act might induce treatment firms to disclose less earnings information outside of earnings announcement windows relative to control firms. As a result, the overall amount of information released by the firm during the year could decrease even though the amount of information released by the firm around the earnings announcement increases, resulting in a deterioration of the firm's overall information environment. Our study can only speak to whether the absolute amount of information released in the earnings announcement increases (see the earnings announcement hedge portfolio results in Figure 2 and Table 5) as a result of the Act. That is, our study examines the Act's effect on only one

particular form of disclosure, mandatory annual reports, but not the Act's effect on corporate disclosure more generally.

Fifth, our sample is limited to firms traded publicly on the New York Stock Exchange. Thus, we cannot directly speak to Stigler's (1964) conjecture that the Act deterred small firms from listing on a public exchange by raising compliance costs.

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Appendix A. Variable Definitions

Variable	Source	Definition
ΔE	Moody's	Change in earnings scaled by average total assets.
$ \Delta E $	Moody's	Absolute change in earnings scaled by average total assets.
<i>Age</i>	Moody's	Number of years since the firm appeared in Moody's Manuals for the first time.
<i>Beta</i>	CRSP & WSJ	Slope coefficient obtained from regressing the firm's excess return on the market risk premium over the [-100, -10] and [+10, +100] day window around the earnings announcement.
<i>Control Conflict</i>	Moody's	Indicator that a voting trust or another company controls the firm, that the firm has a second class of outstanding voting common stock, or that outstanding preferred equity allows unrestricted voting even in the absence of financial distress.
<i>Delaware</i>	Moody's	Indicator that the firm is chartered in Delaware.
<i>Dividend</i>	Moody's	Indicator that the firm pays a dividend.
<i>Income Conflict</i>	Moody's	Indicator that the firm has income bonds, non-cumulative preferred stock, or another type of stock with participation rights.
<i>Issue</i>	CRSP	Indicator that the number of shares outstanding increased by more than five percent over the fiscal year.
<i>Lag E</i>	Moody's	Lagged earnings scaled by lagged average total assets.
<i>Leverage</i>	Moody's	Total debt scaled by total assets.
<i>Market Share</i>	Moody's	Total assets divided by the contemporaneous sum of total assets of all sample firms in the same 2-digit SIC code industry.
<i>Market to Book</i>	CRSP & Moody's	Market value of equity scaled by book value of equity.
<i>Post</i>	Moody's	Indicator for post-1934 period.
<i>Regulated</i>	Moody's	Indicator that the firm is a member of a regulated industry (3-digit SIC codes: 481, 482, 489, 460, 419, 422, 440, 450, 474, 471, 491, 493, 492, 499).

<i>Return</i>	CRSP & WSJ	3-day cumulative earnings announcement return.
<i>Return Synchronicity</i>	CRSP & WSJ	R-squared scaled by $1 - R\text{-squared}$ where R-squared is obtained from a firm-level regression of weekly returns on contemporaneous and lagged market and 2-digit SIC industry returns (Piotroski and Roulstone 2004).
<i>Size</i>	Moody's	Natural logarithm of total assets.
<i>Std(E)</i>	Moody's	Standard deviation of earnings scaled by average total assets calculated over five years (the current year and the previous four years).
<i>Tech</i>	Moody's	Indicator that the firm is a member of the technology industry (3-digit SIC codes: 351-357, 363, 366, 369, 371, 372, 381, 383, 384, 387, 491, 493, 481, 482, 489, 781, 783, 791).
<i>Treat</i>	Moody's	Indicator that the firm did not report sales in 1932.

Panel A. Air Reduction's 1929 Earnings Announcement

AIR REDUCTION NETS \$7.75 IN 1929

**Earnings of \$5,972,995 Com-
pare with \$3,208,993, or
\$4.60 a Share, in 1928**

Report of Air Reduction Co., Inc., engaged in manufacture and sale of oxygen, acetylene, etc., for year ended December 31, 1929, shows net profit of \$5,972,995 after federal taxes, depreciation, etc., equivalent to \$7.75 a share on 770,402 no-par shares of stock. This compares with \$3,208,993 or \$4.60 a share on 696,793 shares in 1928.

For quarter ended December 31, 1929, net profit was \$1,680,039, after above charges, equal to \$2.18 a share on 770,402 shares, comparing with \$1,562,622, or \$2.05 a share on 761,863 shares in preceding quarter, and \$1,229,212, or \$1.76 a share on 696,793 shares, in fourth quarter of 1928.

Consolidated income account for year 1929 compares as follows:

	1929	1928	1927	1926
Gross inc ...	\$21,801,993	\$15,452,009	\$13,530,940	\$12,735,030
Oper exp ..	13,105,608	9,732,924	8,818,234	8,035,623
Oper Inc .	\$8,696,385	\$5,899,085	\$4,732,706	\$4,699,407
*Spl ded, etc	198,323	117,362	122,479
Depr res, etc	2,159,506	1,992,023	1,905,459	1,871,646
Fed tax	563,884	499,746	297,298	433,441
Net profit..	\$5,972,995	\$3,208,993	\$2,412,596	\$2,271,841
Dividends ..	3,228,059	2,050,398	1,532,741	1,118,462
Surplus ..	\$2,744,936	\$1,158,595	\$879,855	\$1,153,379

*Additional compensation to officers and employees.

Quarter ended December 31:

	1929	1928	1927	1926
Gross inc ...	\$5,819,923	\$4,584,556	\$3,459,471	\$3,400,474
Oper expenses	3,444,305	2,675,962	2,176,646	2,046,637
Oper Inc....	\$2,375,618	\$1,908,594	\$1,312,825	\$1,353,837
Depr, etc....	715,309	529,728	489,786	493,049
Fed'l taxes..	119,730	149,654
Net profit..	\$1,680,039	\$1,229,212	\$823,039	\$860,788

*Profit before federal taxes. †Profit.

Consolidated balance sheet of Air Reduction Co., Inc., as of December 31, 1929, compares as follows:

	1929	Assets 1928	1927	1926
Land, bldgs, eq, etc....	\$12,395,213	\$9,187,637	\$8,317,464	\$6,544,354
Cash	4,217,867	3,391,330	3,076,369	2,803,224
Accounts & notes rec...	3,078,459	2,492,121	1,929,404	1,827,642
Oth cur assets	5,653,327	4,306,192	2,877,239	1,882,107
Inventories...	1,639,610	1,354,026	1,376,117	1,306,436
Invest.....	4,127,708	3,562,296	3,533,221	3,893,337
Patents & Lic.	1	1	1	356,272
Prepaid ins, tax, etc....	149,340	135,424	110,708	98,251
Total.....	\$31,461,523	\$24,429,027	\$21,220,523	\$18,643,823
		Liabilities		
Cap stock...*	\$19,063,373	\$15,403,165	\$14,255,365	\$11,876,225
Min int N C	20,767
Corp	471,800
Accounts pay	520,206	463,622	414,048	471,800
Divs payable	577,799	348,395	280,744	261,228
Tax, ins res, etc	869,110	843,374	650,700	601,342
Cont Reserve.	1,599,042	1,283,434	881,224	684,495
Surplus	8,631,973	6,087,037	4,928,442	4,727,959
Total .	\$31,461,523	\$24,429,027	\$21,220,523	\$18,643,823

*Represented by 770,402 no-par shares. †After reserves.

Panel B. Gimbel Brothers' 1932 Earnings Announcement

GIMBEL NET LOSS \$1,791,352 FOR YEAR

Results Are in Contrast to
Profit of \$379,844 Previ-
ously—Assets \$59,365,427

Report of Gimbel Brothers, Inc., and subsidiaries for year ended January 31, 1932, shows net loss of \$1,791,352 after depreciation, interest, etc. This compares with net profit in 1930 of \$379,844, equivalent to \$2.19 a share on 173,000 shares (par \$100) of 7% preferred stock, in preceding fiscal year.

After payment of preferred dividends there was a deficit of \$2,972,777 for the year, comparing with deficit of \$870,181 in previous fiscal year.

Current assets as of January 31, 1932, including cash of \$6,420,736, amounted to \$28,300,128 and current liabilities \$5,659,880, comparing with cash of \$5,678,662, current assets of \$32,969,806 and current liabilities of \$6,632,491 on January 31, 1931.

Consolidated income account for year ended January 31, 1932, compares as follows:

	1932	1931	1930	1929
Net sales	\$87,982,853	113,222,850	124,636,273	121,109,386
Exp & costs	96,177,224	109,223,721	119,873,457	120,596,958
Oper profit	\$1,805,629	\$3,998,929	\$4,762,816	\$512,438
Depreciation	1,725,605	1,667,949	1,599,516	1,422,233
Other chgs	128,089			
Interest	1,843,907	1,951,136	2,358,816	
Net loss	\$1,791,352	\$379,844	\$804,484	\$909,795
Pfd divs	1,181,425	1,250,025	1,325,625	1,389,850

Deficit \$2,972,777 \$870,181 \$521,141 \$2,299,645
 *After deducting \$192,011 losses on sales of and reduction to market value of marketable securities of subsidiaries not consolidated and \$426,217 credit arising from repurchase of preferred stock at a discount. Losses from sales of and from reduction to market value of investments of subsidiaries not consolidated the reduction being limited for bonds to the basis allowed by the Banking Department of New York State amounted to \$208,838 less \$102,000 proportion of profit on sale of radio station received in cash and \$78,740 profit on repurchase of bonds of subsidiary company at a discount. †Net profit

Consolidated balance sheet of Gimbel Brothers, Inc., and subsidiaries as of January 31, 1932, compares as follows:

	1932	Assets 1931	1930	1929
*Land bldgs fx, etc.	\$27,901,472	\$27,591,547	\$28,081,861	\$30,169,552
Cash	6,420,736	5,678,662	1,272,860	2,806,417
Accts rec etc	8,787,549	11,103,572	13,586,950	13,320,027
Inventories	13,091,844	16,187,572	18,971,219	17,848,170
Treas stock	1,473,075	473,075		
Other assets	328,046			
Misc inv	1,431,086	1,347,950	1,444,819	2,002,820
Prepaid exp	928,134	728,297	874,092	885,045
Goodwill	1	1	1	1
Total	\$59,365,427	\$63,113,676	\$67,413,802	\$67,032,012
		Liabilities		
Pfd stock	\$16,280,000	\$17,300,000	\$18,420,000	\$19,740,000
Com stk&surp	33,562,397	139,861,196	136,712,739	28,576,237
Res for pfd stk red	2,413,500	2,595,000	2,767,000	2,961,000
Accts pay etc	5,173,755	6,329,742	8,418,197	7,773,898
Notes pay				6,500,000
Divs pay	288,125	302,750	322,350	345,150
Cont res	1,409,650	721,688	717,398	835,617
Total	\$59,365,427	\$63,113,676	\$67,413,802	\$67,032,012

*After depreciation, mortgages, etc. †Represented by 998,000 no-par shares. ‡Consists of 34,500 shares, at cost

Panel C. Alpha Portland Cement's 1933 Earnings Announcement

Alpha Portland Cement

Report of Alpha Portland Cement Co for 12 months ended September 30, 1933, shows net loss of \$971,485 after taxes, depreciation, minority interest, etc., comparing with net loss of \$1,432,285 for the 12 months ended September 30, 1932.

Current assets as of September 30, 1933, including \$5,861,813 cash and marketable securities, amounted to \$8,087,335 and current liabilities were \$238,546. This compares with cash and marketable securities of \$5,489,012, current assets of \$7,901,548 and current liabilities of \$306,860 on September 30, 1932.

Consolidated income account for 12 months ended September 30, 1933, compares as follows.

	1933	1932	1931	1930
Net sales	\$3,860,870	\$1,210,575	\$6,610,293	\$10,294,030
Oper exp	3,592,176	4,543,294	5,732,968	7,796,687
Depreciation	1,415,083	1,398,048	1,303,321	1,383,504
Oper loss	\$1,046,394	\$2,730,767	\$516,196	*\$1,293,769
Oth inc (net)	65,454	284,342	160,976	225,372
Loss	\$980,940	\$1,446,225	\$355,220	\$1,432,141
Fedl taxes	27,755	152,124
Min Int	19,455	213,910
Net loss	\$971,485	\$1,132,285	\$382,975	*\$1,280,017
Fid divs	140,000	140,000	140,000	140,000
Com divs	355,500	558,750	1,777,500
Deficit ..	\$1,111,485	\$1,927,785	\$1,411,725	\$637,483
*Profit	..	Income	..	Credit.

Consolidated balance sheet of Alpha Portland Cement Co., as of September 30, 1933, compares as follows:

Assets		1933	1932
Prop acct. after depr & dep:		\$17,503,915	\$19,198,793
Cash		2,010,864	2,772,220
Marketable securities		3,850,949	2,896,793
Work funds advances etc		132,090	128,557
Accounts and notes rec less reserve		749,927	780,175
Inventories		1,443,505	1,523,803
*Treasury stock		420,720	335,285
Miscellaneous investment, at cost		20,192	27,603
Deferred items		98,322	137,450
Total		\$26,430,485	\$27,533,719
Liabilities			
7% preferred stock		\$2,000,000	\$2,000,000
†Common stock		18,486,000	18,486,000
Accounts payable		171,370	229,772
Accrued taxes		86,898	77,089
Reserves		664,870	714,215
Minority interest in subsidiaries		68,759	78,214
Surplus		4,972,310	5,948,159
Total		\$26,430,485	\$27,533,719

*Consists of 45,700 common shares at cost, in 1933, and 36,300 common shares, at cost, in 1932. †Represented by 711,000 no-par shares.

ENDICOTT-JOHNSON'S EARNINGS INCREASE

**Net Income of \$4,312,064 in 1925 Equal
to \$8.47 on Common Against
\$8.04 in 1924**

Report of Endicott Johnson Corp. shoe manufacturers, for year ended December 31, 1925, shows net profit of \$4,312,064 after interest, depreciation, federal taxes, etc., equivalent after preferred dividends, to \$8.47 a share (par \$50) earned on \$20,268,000 outstanding common stock. This compares with \$4,175,644 or \$8.04 a share in 1924.

Consolidated income account for 1925 compares as follows:

	1925	1924	1923	1922
Sales . . .	\$69,346,931	\$66,378,177	\$66,565,812	\$63,659,076
Ex in dp etc	62,972,202	60,017,664	60,184,340	53,942,576
Oper prof.	\$6,374,729	\$6,360,513	\$6,381,472	\$9,716,500
Fed. tax . .	908,840	949,773	1,029,902	1,117,973
Emp prf sh	1,153,825	1,235,096	1,197,291	2,980,997
Net profit.	\$4,312,064	\$4,175,644	\$4,154,279	\$5,617,530
Pfd. divs..	876,228	914,874	932,517	974,990
Com. divs..	2,026,800	2,025,675	2,024,471	1,685,731
Surplus . .	\$1,409,036	\$1,235,095	\$1,197,291	\$2,956,809
P&L surp.	12,517,132	11,441,525	10,229,370	12,449,811

Consolidated balance sheet of Endicott Johnson Corp. as of December 31, 1925, compares as follows:

ASSETS				
	1925	1924	1923	1922
†Lnd bldgs. mach. etc.	\$13,836,395	\$13,860,618	\$14,262,924	\$13,887,014
E J pf stk.				100,100
E W W Co			400,593	374,989
Inventory..	20,584,014	15,891,862	19,895,794	18,420,180
Ac&nts rec	11,651,724	11,634,858	11,894,047	10,080,060
Investments	994,446	900,654		16,500
Sun dbt etc	219,308	169,476	153,953	99,039
Cash	3,639,712	4,810,007	5,015,413	4,243,233
Rec on contr	1,474,300	1,198,657	795,591	495,416
G'will	7,000,000	7,000,000	7,000,000	7,000,000
Def. chgs . .	4	4	120,455	158,125
Total . . .	\$59,399,903	\$55,466,136	\$59,038,770	\$54,874,676
LIABILITIES				
Pfd. stk . .	\$12,262,900	\$12,906,700	\$13,200,000	\$13,650,000
*Com. stk..	20,268,000	20,268,000	20,253,000	16,856,850
Notes pay..	10,050,000	6,000,000	10,650,000	5,000,000
Sun credit.	1,020,181	761,612	715,047	855,373
Divs pay . .		733,403	739,110	669,000
Ac pay etc..	784,522	739,048	657,425	1,099,601
Prof shr pl.	1,153,824	1,235,095	1,197,290	2,956,809
Fed tax res.	834,683	872,093	992,021	1,087,232
Ins res etc .	508,661	508,660	405,506	250,000
Surplus . .	12,517,132	11,441,525	10,229,370	12,449,811
Total . . .	\$59,399,903	\$55,466,136	\$59,038,770	\$54,874,676

†After depreciation. *Par value \$50.

Figure 1. Percentage of Sample Firms Not Disclosing Sales

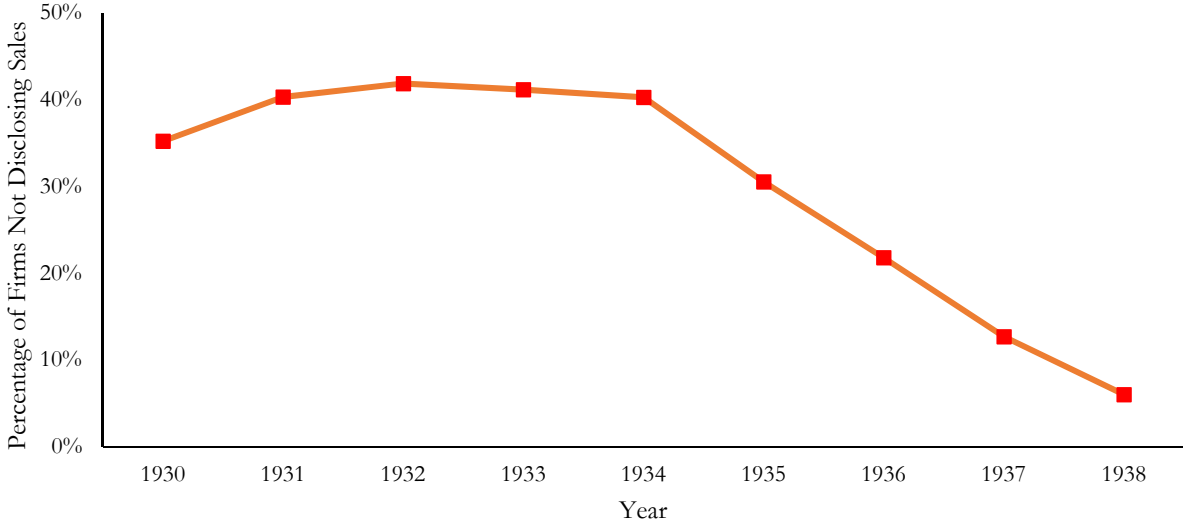


Figure 1 plots the percentage of sample firms that do not disclose sales by year from 1930 to 1938.

Figure 2. Cumulative Perfect-Foresight Hedge Portfolio Returns pre-Act and post-Act

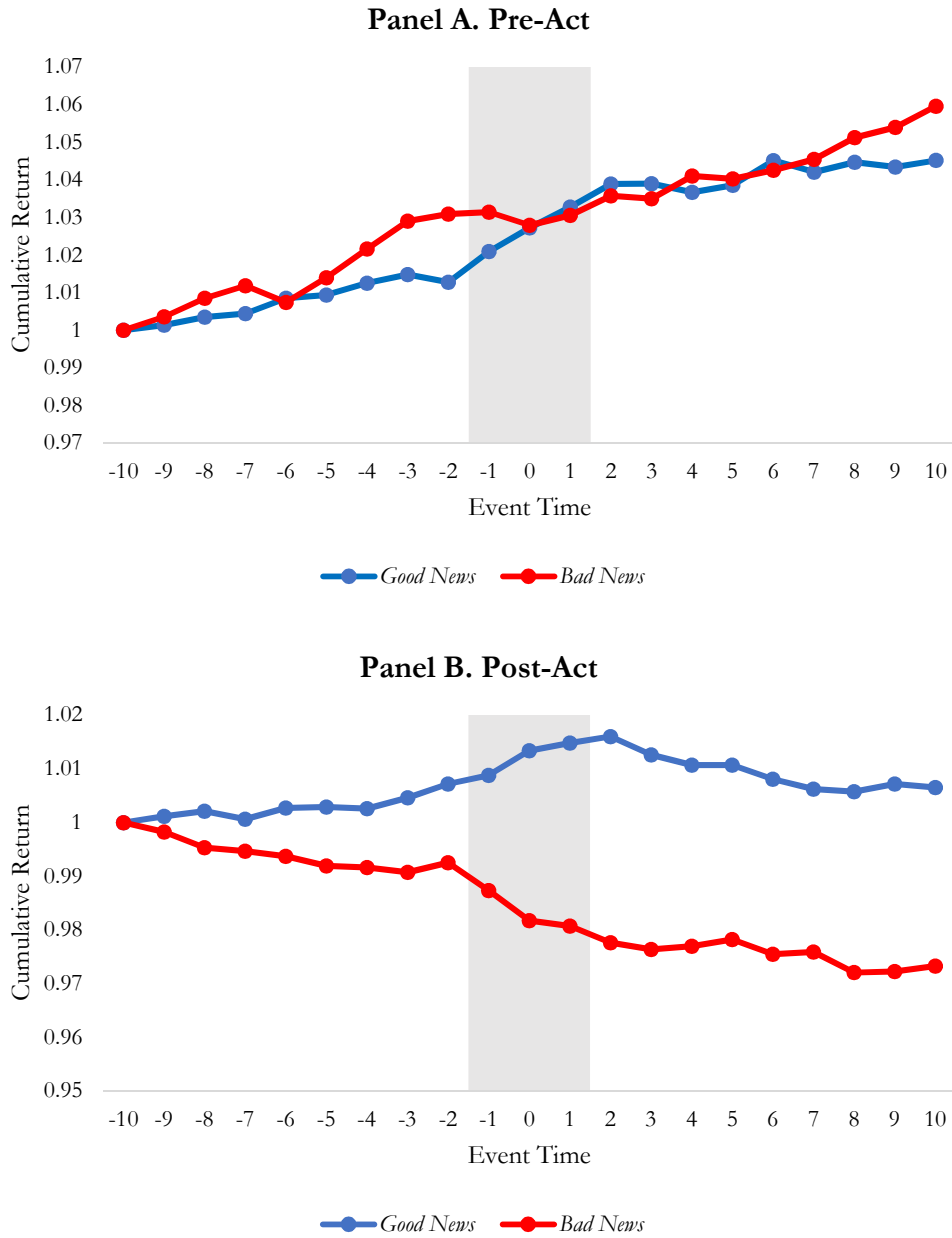


Figure 2 Panel A (Panel B) plots cumulative perfect-foresight portfolio returns around earnings announcements for the periods before (after) the Act. The [-1, +1] earnings announcement window is shaded in gray. The sample period spans 1930 to 1938.

Figure 3. Test of Parallel Trends Assumption

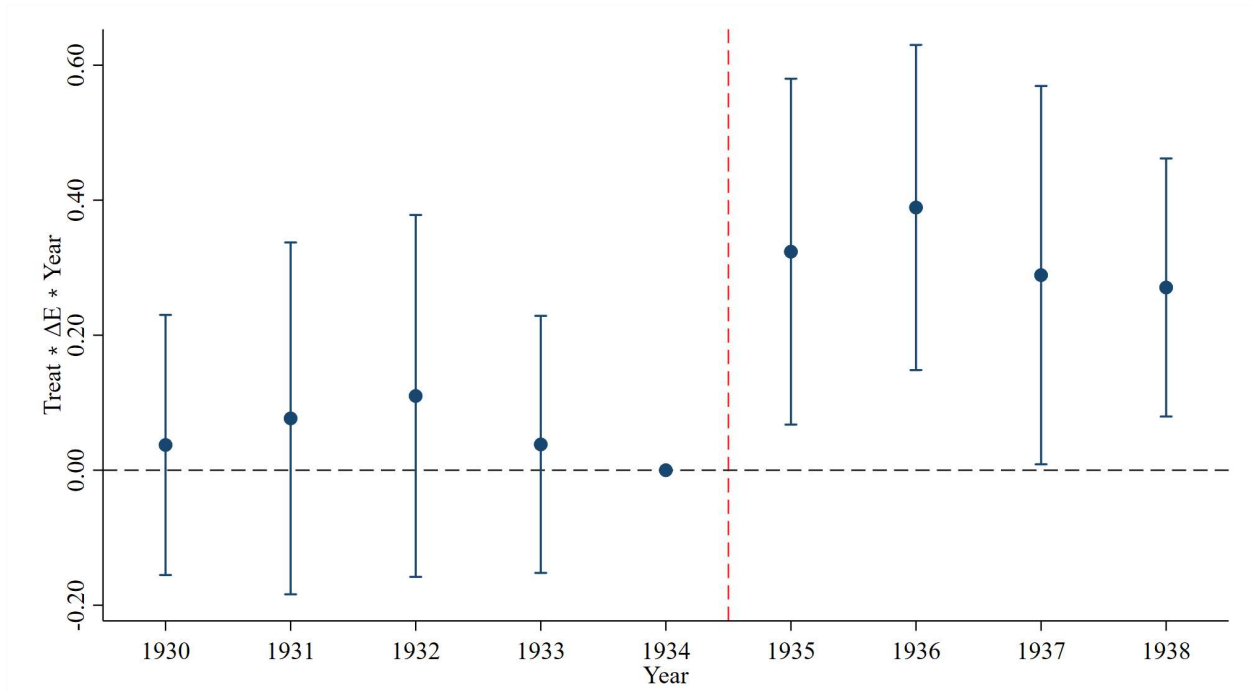


Figure 3 tests the parallel trends assumption of our difference-in-differences design by regressing three-day earnings announcement stock market returns on an indicator that the firm did not voluntarily report sales before the Act was enacted (*Treat*) interacted with the firm's unexpected earnings (ΔE) and with fiscal year indicators; controls; and fixed effects. 1934 constitutes the base year. The figure displays the slope coefficients and 90% confidence intervals for the interaction term between *Treat*, ΔE , and each of the fiscal year indicators. The red dashed line indicates the effective date of the Act, July 1, 1934. Standard errors are clustered by firm and year. The sample period spans 1930 to 1938. All variables are defined in Appendix A.

Table 1. Industry Composition

Industry	Observations	% of Total
Agricultural Production – Crops	17	0.41
Metal, Mining	83	2.00
Coal Mining	83	2.00
Oil & Gas Extraction	37	0.89
Nonmetallic Minerals, Except Fuels	16	0.39
General Building Contractors	14	0.34
Food & Kindred Products	412	9.93
Tobacco Products	125	3.01
Textile Mill Products	138	3.32
Apparel & Other Textile Products	47	1.13
Lumber & Wood Products	25	0.60
Furniture & Fixtures	15	0.36
Paper & Allied Products	94	2.26
Printing & Publishing	59	1.42
Chemical & Allied Products	291	7.01
Petroleum & Coal Products	240	5.78
Rubber & Miscellaneous Plastics Products	59	1.42
Leather & Leather Products	65	1.57
Stone, Clay, & Glass Products	107	2.58
Primary Metal Industries	422	10.17
Fabricated Metal Products	162	3.90
Industrial Machinery & Equipment	303	7.30
Electronic & Other Electric Equipment	151	3.64
Transportation Equipment	427	10.29
Instruments & Related Products	71	1.71
Miscellaneous Manufacturing Industries	52	1.25
Local & Interurban Passenger Transit	16	0.39
Trucking & Warehousing	13	0.31
Water Transportation	29	0.70
Transportation by Air	5	0.12
Transportation Services	18	0.43
Communications	9	0.22
Wholesale Trade – Durable Goods	53	1.28
General Merchandise Stores	198	4.77
Food Stores	64	1.54
Apparel & Accessory Stores	44	1.06
Furniture & Homefurnishings Stores	24	0.58
Eating & Drinking Places	54	1.30
Miscellaneous Retail	33	0.80
Personal Services	12	0.29
Business Services	9	0.22
Motion Pictures	50	1.20
Amusement & Recreation Services	5	0.12
Total	4,151	100.00

Table 1 presents our sample's 2-digit SIC code industry composition. The sample period spans 1930 to 1938.

Table 2. Descriptive Statistics

Variable	Observations	Mean	Std	P1	P25	Median	P75	P99
ΔE	4,151	0.00	0.06	-0.18	-0.03	0.00	0.03	0.18
<i>Return</i>	4,151	0.00	0.07	-0.15	-0.03	0.00	0.03	0.20
<i>Treat</i>	4,151	0.38	0.49	0.00	0.00	0.00	1.00	1.00
<i>Post</i>	4,151	0.46	0.50	0.00	0.00	0.00	1.00	1.00
<i>Size</i>	4,151	3.08	1.29	0.66	2.12	2.97	3.86	6.58
<i>Beta</i>	4,151	1.07	0.61	-0.15	0.59	1.04	1.50	2.55
<i>Market to Book</i>	4,151	0.94	0.77	0.14	0.44	0.70	1.14	4.21
$ \Delta E $	4,151	0.04	0.05	0.00	0.01	0.03	0.06	0.22
<i>Lag E</i>	4,151	0.05	0.08	-0.13	0.00	0.04	0.09	0.31
<i>Return Synchronicity</i>	4,151	1.83	2.98	0.04	0.33	0.84	2.09	19.46

Table 2 presents descriptive statistics. The sample period spans 1930 to 1938. All variables are defined in Appendix A.

Table 3. Correlation Matrix

Variable		1	2	3	4	5	6	7	8	9	10
ΔE	1	1.00	0.11*	0.01	0.09*	-0.05*	0.06*	0.14*	-0.12*	-0.39*	-0.02
<i>Return</i>	2	0.13*	1.00	-0.02	-0.03	0.00	-0.01	-0.01	-0.01	0.00	0.00
<i>Treat</i>	3	0.01	-0.02	1.00	-0.04	-0.07*	0.01	0.07*	0.02	0.07*	0.01
<i>Post</i>	4	0.13*	-0.01	-0.04	1.00	-0.01	0.12*	0.29*	-0.10*	0.20*	0.10*
<i>Size</i>	5	-0.06*	0.00	-0.06*	-0.01	1.00	0.08*	0.03	-0.27*	0.12*	0.43*
<i>Beta</i>	6	0.10*	-0.04	0.01	0.12*	0.07*	1.00	-0.09*	0.11*	-0.22*	0.24*
<i>Market to Book</i>	7	0.19*	0.03	0.04	0.35*	0.07*	-0.04	1.00	0.08*	0.54*	0.16*
$ \Delta E $	8	-0.09*	0.00	0.01	-0.11*	-0.25*	0.13*	0.03	1.00	-0.01	-0.03
<i>Lag E</i>	9	-0.35*	0.03	0.07*	0.22*	0.15*	-0.26*	0.50*	-0.03	1.00	0.16*
<i>Return Synchronicity</i>	10	-0.02	0.02	0.04*	0.18*	0.43*	0.43*	0.22*	0.02	0.21*	1.00

Table 3 presents our correlation matrix. * indicates significance at the 1% level. Pearson (Spearman) correlations are above (below) the diagonal. The sample period spans from 1930 to 1938. All variables are defined in Appendix A.

Table 4. Treatment Determinant Model

Variables	(1)	(2)	(3) <i>Treat</i>	(4)	(5)
Equity market information costs					
<i>Age</i>	0.013** (2.04)				0.013** (1.98)
<i>Tech</i>	-0.024 (-0.28)				-0.024 (-0.29)
<i>Std(E)</i>	2.102** (1.99)				1.525 (1.35)
<i>Beta</i>	-0.066 (-1.29)				-0.028 (-0.52)
<i>E</i>	0.879* (1.86)				0.959* (1.79)
<i>Issue</i>	-0.054 (-0.38)				-0.026 (-0.19)
Contractual and control conflicts					
<i>Leverage</i>		-0.534*** (-2.67)			-0.308 (-1.36)
<i>Income Conflict</i>		0.011 (0.14)			0.065 (0.80)
<i>Control Conflict</i>		0.132* (1.70)			0.142* (1.84)
<i>Delaware</i>		-0.025 (-0.40)			-0.037 (-0.59)
Competitive and political costs					
<i>Market Share</i>			-0.086 (-0.41)		-0.157 (-0.74)
<i>Size</i>			-0.031 (-1.41)		-0.047* (-1.78)
Alternative information					
<i>Dividend</i>				0.059 (1.00)	-0.003 (-0.04)
<i>Regulated</i>				0.323 (1.53)	0.513*** (2.61)
<i>Constant</i>	0.301*** (3.05)	0.429*** (7.99)	0.531*** (7.30)	0.397*** (10.24)	0.381*** (2.92)
Observations	290	290	290	290	290
Adjusted R-squared	0.017	0.026	0.002	0.003	0.047

Table 4 regresses our treatment variable (*Treat*) on the disclosure quality determinants identified in Barton and Waymire (2004) for all firms in 1932. Robust t-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10% levels. All variables are defined in Appendix A.

Table 5. Perfect-Foresight Earnings Announcement Hedge Portfolio Returns

	<i>Pre</i>	<i>Post</i>	
<i>Control</i>	1.91%***	1.60%**	-0.31%
<i>Treat</i>	1.06%	1.83%***	0.77%
	-0.85%*	0.23%	1.09%*

Table 5 displays perfect-foresight hedge portfolio returns earned over $[-1, +1]$ earnings announcement windows for control (*Control*) and treatment (*Treat*) firms in the periods before (*Pre*) and after (*Post*) the Act. Hedge portfolios go long (short) in stocks of firms that will report an earnings increase (decrease) in the earnings announcement. Standard errors are clustered by year. ***, **, and * denote statistical significance at the 1, 5, and 10% levels. The sample period spans 1930 to 1938.

Table 6. Earnings Response Coefficients

Panel A. Full Sample

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	<i>Return</i>							
ΔE	0.159**	0.169**	0.148***	0.187***	0.236***	0.244***	0.192**	0.243***
	(3.15)	(2.80)	(3.51)	(3.95)	(4.03)	(4.06)	(3.08)	(3.81)
<i>Post</i>	-0.005		-0.005		-0.006		-0.004	
	(-1.70)		(-1.47)		(-1.62)		(-0.91)	
<i>Post</i> \times ΔE	-0.063	-0.045	-0.053	-0.077	-0.078	-0.056	-0.055	-0.083
	(-0.96)	(-0.60)	(-0.78)	(-1.05)	(-1.14)	(-0.76)	(-0.80)	(-1.15)
<i>Treat</i>	-0.003	-0.002**			-0.003	-0.003**		
	(-1.26)	(-2.33)			(-1.58)	(-2.73)		
<i>Treat</i> \times ΔE	-0.079***	-0.061***	-0.079***	-0.083**	-0.080***	-0.062***	-0.074***	-0.077**
	(-11.96)	(-6.05)	(-5.03)	(-2.79)	(-11.69)	(-7.47)	(-4.05)	(-2.54)
<i>Treat</i> \times <i>Post</i>	-0.001	-0.001	-0.003	-0.004	-0.001	-0.001	-0.003	-0.004
	(-0.22)	(-0.24)	(-0.56)	(-0.66)	(-0.20)	(-0.20)	(-0.57)	(-0.77)
<i>Treat</i> \times <i>Post</i> \times ΔE	0.125***	0.116**	0.136**	0.157**	0.121***	0.116**	0.129**	0.152**
	(5.40)	(2.86)	(3.28)	(2.80)	(5.75)	(3.32)	(3.08)	(2.59)
<i>Size</i>					-0.000	-0.000	-0.009	-0.014*
					(-0.11)	(-0.26)	(-1.65)	(-2.14)
<i>Beta</i>					-0.000	0.001	0.000	-0.000
					(-0.00)	(0.44)	(0.01)	(-0.03)
<i>Market to Book</i>					-0.008***	-0.009***	-0.008*	-0.010
					(-3.99)	(-4.42)	(-2.04)	(-1.72)
$ \Delta E $					0.023	0.013	0.042	0.016
					(0.67)	(0.40)	(1.16)	(0.58)
<i>Lag E</i>					0.102***	0.123***	0.064	0.101*
					(6.28)	(7.58)	(1.51)	(2.08)
<i>Constant</i>	0.005	0.003**	0.004	0.002**	0.008	0.005	0.033	0.049*
	(1.70)	(2.54)	(1.50)	(3.05)	(1.23)	(0.86)	(1.83)	(2.13)
Observations	4,151	4,151	4,151	4,151	4,151	4,151	4,151	4,151
Adjusted R-squared	0.013	0.036	0.026	0.044	0.019	0.043	0.028	0.047
Industry \times Year Fixed Effects	NO	YES	NO	YES	NO	YES	NO	YES
Firm Fixed Effects	NO	NO	YES	YES	NO	NO	YES	YES

Panel B. Propensity Score Matched Sample

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	<i>Return</i>							
ΔE	0.201*	0.274*	0.191*	0.243	0.269*	0.350**	0.193	0.267
	(2.28)	(2.22)	(2.09)	(1.76)	(2.29)	(2.35)	(1.38)	(1.54)
<i>Post</i>	-0.009*		-0.007		-0.007		-0.003	
	(-2.01)		(-1.14)		(-1.38)		(-0.33)	
<i>Post</i> \times ΔE	-0.087	-0.152	-0.091	-0.111	-0.106	-0.169	-0.084	-0.107
	(-0.69)	(-1.07)	(-0.60)	(-0.65)	(-0.76)	(-1.05)	(-0.53)	(-0.64)
<i>Treat</i>	-0.007	-0.006			-0.008	-0.007		
	(-1.40)	(-1.10)			(-1.46)	(-1.34)		
<i>Treat</i> \times ΔE	-0.262***	-0.282***	-0.275***	-0.314***	-0.272***	-0.299***	-0.281***	-0.319***
	(-5.13)	(-8.66)	(-4.00)	(-5.44)	(-5.21)	(-8.45)	(-4.17)	(-5.06)
<i>Treat</i> \times <i>Post</i>	0.004	0.004	0.004	0.003	0.004	0.003	0.002	0.001
	(0.60)	(0.56)	(0.48)	(0.44)	(0.48)	(0.41)	(0.24)	(0.14)
<i>Treat</i> \times <i>Post</i> \times ΔE	0.261**	0.219***	0.285*	0.226**	0.256**	0.225***	0.289*	0.230*
	(2.76)	(4.80)	(2.05)	(2.36)	(2.84)	(4.42)	(2.09)	(2.05)
<i>Size</i>					0.000	0.000	0.004	-0.011
					(0.19)	(0.23)	(0.35)	(-0.85)
<i>Beta</i>					-0.001	0.003	0.003	0.004
					(-0.17)	(0.86)	(0.59)	(0.60)
<i>Market to Book</i>					-0.008*	-0.009*	-0.007	-0.008
					(-1.96)	(-2.14)	(-0.80)	(-0.72)
$ \Delta E $					-0.049	-0.090*	-0.010	-0.036
					(-1.14)	(-1.87)	(-0.18)	(-0.70)
<i>Lag E</i>					0.079	0.101	-0.015	0.034
					(1.78)	(1.66)	(-0.17)	(0.35)
<i>Constant</i>	0.008**	0.004	0.003***	0.001	0.013***	0.008	-0.006	0.039
	(2.67)	(1.81)	(3.53)	(0.62)	(4.02)	(1.40)	(-0.14)	(0.70)
Observations	952	952	952	952	952	952	952	952
Adjusted R-squared	0.011	0.026	0.023	0.017	0.013	0.030	0.020	0.015
Industry \times Year Fixed Effects	NO	YES	NO	YES	NO	YES	NO	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES

Table 6 Panel A (Panel B) employs a difference-in-differences design for the full sample (propensity score matched sample) to examine the effects of the Securities Exchange Act of 1934 on firms' earnings response coefficients by regressing three-day earnings announcement stock market returns on controls, fixed effects, and an indicator that the firm did not voluntarily report sales before the Act was enacted (*Treat*) interacted with a post-Securities Exchange Act of 1934 indicator (*Post*) and with the firm's unexpected earnings (ΔE). Robust t-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, and * denote statistical significance at the 1, 5, and 10% levels. The sample period spans 1930 to 1938. All variables are defined in Appendix A.

Table 7. Cross-Sectional Variation in Enforcement

Enforcement Variable	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Against Mining Industry</i>		<i>Against Firms</i>	<i>Return</i>	<i>Against Firms & Individuals</i>	
ΔE	0.190*** (3.83)	0.248*** (3.75)	0.192*** (3.68)	0.250*** (3.65)	0.194*** (3.95)	0.252*** (3.86)
$Post \times \Delta E$	-0.070 (-0.89)	-0.078 (-1.00)	-0.086 (-1.07)	-0.093 (-1.18)	-0.093 (-1.22)	-0.099 (-1.34)
$Treat \times \Delta E$	-0.082* (-2.14)	-0.076* (-2.02)	-0.082** (-2.32)	-0.078* (-2.08)	-0.084** (-2.54)	-0.080* (-2.22)
$Treat \times Post$	-0.006 (-1.21)	-0.007 (-1.31)	-0.004 (-0.80)	-0.005 (-0.89)	-0.005 (-0.82)	-0.005 (-0.92)
$Treat \times Post \times \Delta E$	0.144** (2.34)	0.139* (2.21)	0.149** (2.51)	0.145** (2.33)	0.155** (2.72)	0.151** (2.52)
$\Delta E \times Enforcement$	-0.059 (-0.74)	-0.061 (-0.84)	-0.045 (-0.47)	-0.060 (-0.57)	-0.067 (-0.78)	-0.080 (-0.84)
$Post \times Enforcement$	0.000 (0.00)	0.000 (0.00)	-0.017 (-1.76)	-0.017 (-1.49)	-0.020** (-2.61)	-0.020* (-2.22)
$Post \times \Delta E \times Enforcement$	-0.252 (-1.09)	-0.227 (-0.96)	0.082 (0.70)	0.097 (0.77)	0.146 (1.29)	0.155 (1.29)
$Treat \times \Delta E \times Enforcement$	-0.355 (-1.11)	-0.386 (-1.33)	-0.112 (-0.86)	-0.102 (-0.74)	-0.089 (-0.91)	-0.081 (-0.73)
$Treat \times Post \times Enforcement$	0.073*** (5.66)	0.077*** (5.89)	0.014 (0.67)	0.013 (0.60)	0.017 (0.97)	0.016 (0.89)
$Treat \times Post \times \Delta E \times Enforcement$	1.898** (2.98)	1.682** (2.71)	0.455** (2.41)	0.480** (2.45)	0.390** (2.57)	0.421** (2.58)
Observations	4,151	4,151	4,151	4,151	4,151	4,151
Adjusted R-squared	0.046	0.049	0.043	0.047	0.044	0.047
Industry \times Year Fixed Effects	YES	YES	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES	YES	YES
Controls	NO	YES	NO	YES	NO	YES

Table 7 examines how the effects of the Securities Exchange Act of 1934 on earnings response coefficients around firms' earnings announcements vary with enforcement by regressing three-day earnings announcement stock market returns on controls, fixed effects, and an indicator that the firm did not voluntarily report sales before the Act was enacted (*Treat*) interacted with a post–Securities Exchange Act of 1934 indicator (*Post*), with the firm's unexpected earnings (ΔE), and with different enforcement intensity measures. Enforcement in Columns (1) and (2) is measured as an indicator that the firm is a member of the mining industry. Enforcement in Columns (3) and (4) ((5) and (6)) is measured as an indicator that the firm's headquarter is located in a state with high SEC enforcement intensity against firms (against firms and individuals). Robust t-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, and * denote statistical significance at the 1, 5, and 10% levels. The sample period spans 1930 to 1938. All variables are defined in Appendix A.

Table 8. Comparability around the Exchange Act

Variable	(1)	(2)	(3)	(4)
	<i>Return Synchronicity</i>			
<i>Post</i>	0.381 (0.84)	0.529 (1.06)		
<i>Treat</i>	0.294 (1.38)		0.194 (1.30)	
<i>Post × Treat</i>	-0.412*** (-4.13)	-0.299** (-2.46)	-0.127* (-1.99)	-0.149 (-1.60)
<i>Size</i>	0.957*** (7.05)	0.214 (1.03)	1.041*** (6.81)	-0.056 (-0.39)
<i>Beta</i>	1.115*** (4.13)	0.764*** (4.08)	0.941*** (4.86)	0.735*** (5.86)
<i>Market to Book</i>	0.391 (1.72)	-0.076 (-0.27)	0.624*** (3.82)	0.263* (2.11)
$ \Delta E $	3.354* (1.87)	1.162 (0.54)	1.405 (1.06)	-0.560 (-0.79)
<i>Lag E</i>	3.644* (2.05)	4.658** (2.42)	1.093 (1.02)	1.927** (2.92)
<i>Constant</i>	-3.211*** (-6.02)	-0.032 (-0.04)	-3.136*** (-4.96)	0.932* (1.89)
Observations	4,151	4,151	4,151	4,151
Adjusted R-squared	0.269	0.714	0.442	0.794
Industry × Year Fixed Effects	NO	NO	YES	YES
Firm Fixed Effects	NO	YES	NO	YES

Table 8 employs a difference-in-differences design to examine the effects of the Securities Exchange Act of 1934 on firms' return synchronicity by regressing the return synchronicity measure proposed in Piotroski and Roulstone (2004) (*Return Synchronicity*) on controls, fixed effects, and an indicator that the firm did not voluntarily report sales before the Act was enacted (*Treat*) interacted with a post–Securities Exchange Act of 1934 indicator (*Post*). Robust t-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, and * denote statistical significance at the 1, 5, and 10% levels. The sample period spans 1930 to 1938. All variables are defined in Appendix A.

Table 9. Alternative Earnings Surprise Measures

Variable	(1)	(2)	(3)	(4)
	Estimation Sample			
	Full Sample		By Industry	
	Earnings Forecasting Model			
	AR(1)	Hou et al. (2012)	AR(1)	Hou et al. (2012)
	<i>Return</i>			
<i>Surprise</i>	0.275*** (6.05)	0.251*** (5.88)	0.280*** (5.90)	0.264*** (6.45)
<i>Post</i> × <i>Surprise</i>	-0.118* (-2.21)	-0.067 (-1.16)	-0.125** (-2.34)	-0.094 (-1.60)
<i>Treat</i> × <i>Surprise</i>	-0.125* (-2.15)	-0.107 (-1.71)	-0.130* (-2.22)	-0.120* (-2.06)
<i>Treat</i> × <i>Post</i>	-0.001 (-0.24)	-0.001 (-0.25)	-0.001 (-0.22)	-0.002 (-0.29)
<i>Treat</i> × <i>Post</i> × <i>Surprise</i>	0.218** (2.74)	0.167* (2.01)	0.221** (2.93)	0.218** (2.75)
Observations	4,151	4,020	4,151	4,020
Adjusted R-squared	0.047	0.047	0.047	0.048
Industry × Year Fixed Effects	YES	YES	YES	YES
Firm Fixed Effects	YES	YES	YES	YES
Controls	YES	YES	YES	YES

Table 9 employs a difference-in-differences design to examine the effects of the Securities Exchange Act of 1934 on firms' earnings response coefficients by regressing three-day earnings announcement stock market returns on controls, fixed effects, and an indicator that the firm did not voluntarily report sales before the Act was enacted (*Treat*) interacted with a post–Securities Exchange Act of 1934 indicator (*Post*) and with the firm's unexpected earnings (*Surprise*). We measure unexpected earnings in Columns (1) and (2) (Columns (3) and (4)) as realized earnings minus the earnings forecast obtained from an AR(1) and a Hou et al. (2012) model forecast estimated in the cross-section (by industry), respectively. Robust t-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, and * denote statistical significance at the 1, 5, and 10% levels. The sample period spans 1930 to 1938. All variables are defined in Appendix A.