Does Securities Regulation Matter? Mandatory Disclosure, Excess Stock Volatility and the U.S. 1934 Securities Exchange Act*

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Abstract

We examine whether the U.S. Securities Exchange Act of 1934 significantly stabilized the market by introducing mandatory disclosure of information. We argue that mandatory information disclosure can curb stock manipulation by enhancing transparency, thereby reducing excess stock volatility. After a comprehensive assessment of the voluntary disclosure practices of NYSE-listed companies before 1934, we group the companies and find that those with poor disclosure practices experienced a significantly greater reduction in volatility after the implementation of the 1934 Act compared to those with good disclosure practices. Further analysis reveals that the liquidity of these poorly disclosing companies also improved significantly more than that of the better disclosing companies, and the improvement in liquidity was linked to the decrease in their volatility. Given that one of the key intentions of the legislators was to reduce excess market volatility through the Act, our findings provide empirical support for this legislative intent.

Keywords: Financial Regulation, Mandatory Disclosure, Securities Acts, Manipulation, Stock Volatility.

JEL Codes: K22, G28, N42

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1 Introduction

The Securities Act of 1933 and the Securities Exchange Act of 1934 (the Acts) are cornerstone legislations in U.S. financial regulation, fundamentally shaping the landscape of securities law by promoting transparency in the wake of the 1929 stock market crash. The 1933 Act established the principle of mandatory disclosure of information related to securities being offered for public sale, requiring firms to provide comprehensive and truthful information to potential investors. The 1934 Act extended the principle to all exchange-listed firms and instituted the enforcement agency, the Securities and Exchange Commission (SEC). These Acts laid the groundwork for subsequent regulations, serving as a model for securities law enforcement and market oversight globally. However, their effectiveness has been a point of controversy for nearly a century.

Undoubtedly, transparency in information disclosure is pivotal for the trading of securities. The focal point of the debate is whether mandatory disclosure is necessary. Rational external investors, aware of potential information asymmetry, would be deterred from investing in securities if the issuing company fails to disclose pertinent information considered material to the securities being offered for sale (Stigler, 1964). A certain amount of information would thus be disclosed voluntarily. The critical question is: Is the voluntarily disclosed information truthful, timely, and sufficient?

Ideally, the authenticity of information can be ensured by the law's deterrent function, provided that fraud, misrepresentation, concealment of defects, and similar offenses are adequately and effectively punished. In such a scenario, a free and competitive market would incentivize firms to timely disclose what investors need to know while concealing what they do not, leading to an optimal level of information disclosure at equilibrium (e.g. Bentham, 1830; Becker, 1968; Stigler, 1970; Grossman and Hart, 1980). Regulation on information disclosure is irrelevant in this context. However, in a real world with transaction costs, the ambiguity of legal interpretation and friction in law enforcement can hinder firms from voluntarily disclosing at this optimal level. For example, it is very difficult for a plaintiff to prove *ex post* that a loss incurred from a stock traded on an exchange is primarily due to misrepresentation or the untimely reporting of a material fact *ex ante* (Seligman,

1983; Pistor and Xu, 2002). Absent a mandated disclosure standard, managers' legal liabilities for misleading or fraudulent reporting were far more limited than they are today (Benston, 1973). From this perspective, a mandated disclosure system can help mitigate this problem by clearly specifying what type of information is considered "material", when disclosure is deemed "timely", and what kind of disclosure practices can be regarded as "misleading" or "fraudulent". A unifying standard of reporting is thus helpful. Another potential benefit of mandatory information disclosure is that it can enhance the credibility of the disclosed information through a robust enforcement system, as voluntarily disclosed information might be discounted in the absence of a clear and strong enforcement authority (Daines and Jones, 2012). Moreover, mandatory disclosure may be socially beneficial if there are externalities of disclosure (e.g. Easterbrook and Fischel, 1984; Coffee, 1984; Dye, 1990; Admati and Pfleiderer, 2000; Zingales, 2009; Leuz and Wysocki, 2016). For example, when a typical company reports an improvement in its operating margin, investors can infer that the operating margins of the entire industry may also be improving, thereby enhancing their estimates of the industry's overall profitability. When companies disclose information voluntarily, the overall social welfare benefits that arise from externalities may not be fully realized.

Of course, a mandated disclosure system is not without its costs, which can be quite substantial for some companies. Beyond the direct costs of enforcement, potential issues such as regulatory capture (e.g., Stigler, 1971; Peltzman, 1976), a one-size-fits-all approach (e.g., Barth et al., 2008; Ahmed et al., 2013; Christensen et al., 2015), and the disclosure of proprietary information to competitors (e.g., Hayes and Lundholm, 1996) can impose varying levels of pressure on different companies. Evidence suggests that relaxing disclosure and compliance obligations for small, "emerging growth companies" (EGCs) has increased their value, enhanced information exchange, and boosted IPO market activity (Dambra et al., 2015; Dharmapala and Khanna, 2016; Pinto, 2023). Based on these considerations, some authors argue that a unified government regulatory system is not optimal and that introducing regulatory competition at the state or exchange level should be considered (e.g. Mahoney, 1997; Romano, 1998).

The above discussion indicates that whether mandatory disclosure is beneficial, and under what circumstances, is not a simple question.[1] Modern literature typically explores whether tightening or relaxing specific disclosure requirements in particular markets is advantageous, which is generally a "what and how to disclose" issue. Looking back at the 1930s, the 1933 and 1934 legislation was driven by the widespread belief[2] that financial markets were plagued by significant omission, fraud, manipulation, and the resulting excess market volatility.[3] The disclosure rates for certain fundamental information that we now consider essential—such as sales, cost of goods sold, tax expenses, the value of intangible assets, the basis of accounting preparation, the occurrence of an

[1]The Economic Report of the President of the U.S. in 2003 stated, "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."

[2]After the crash of 1929, there was a consensus that fraud and manipulation were rampant during the early 1920s, a view shaped by congressional hearings, journalistic reports, and academic commentaries (Mahoney, 2021). The debate resurfaced in the 1960s. Benston (1969) argues that there was little evidence of widespread fraud or misrepresentation of financial disclosures before 1933, stating, "a search of the available literature in several libraries revealed only anecdotal reports of fraudulent or misrepresentative accounting" and "an extensive search has revealed not a single American case in which a public accountant has been held liable in a criminal suit for fraud." Although the absence of convictions does not necessarily indicate the absence of fraud, Benston's research inspired many subsequent studies. Mahoney (1999); Jiang et al. (2005) examine "stock pools", a form of manipulation discussed in the 1932 inquiry by the Senate Committee on Banking and Currency (the Pecora Hearings). Seligman (1983) compiles a series of historical evidence on various types of fraudulent trading and securities manipulation prior to 1933. Agrawal (2013) discusses anecdotal evidence related to these practices.

[3]The Act 1934 sought to establish a framework to curb market manipulation and fraud by prohibiting practices that undermine market integrity, as outlined in Sections 9(a) and 10(b) and further elaborated in SEC Rule 10b-5. In *Ernst & Ernst v. Hochfelder (1976)*, the U.S. Supreme Court distilled the Act's principle into "intentional or willful conduct designed to deceive or defraud investors by controlling or artificially affecting the price of securities", solidifying this definition as a fundamental pillar of the Act's approach to manipulation.

However, it is important to note that with the evolution of regulatory rules, whether a particular market operation constitutes manipulation today may differ from perspectives in the 1920s—partly because the Act 1934 established clearer legal boundaries. For instance, stock pools were a common speculative practice in the 1920s, where a group of investors or brokers coordinated trades to manipulate the price of a particular stock. The Pecora Hearings documented instances of stock pools exacerbating market volatility, notably exemplified by the renowned Radio Corporation of America (RCA) pool. This pool drove RCA's stock price from approximately $473 to a peak of $545 between March 8 and March 12, 1929, before reducing it to $435 by March 29, 1929—only for the price to plummet further in the aftermath of the pool's operations (United States Senate Committee on Banking and Currency, 1934a, pp. 377–790). However, the committee at the time failed to recognize the liquidity-providing role of stock pools (Mahoney, 1999). Today's regulated market-making systems and block trading mechanisms share a historical lineage with stock pools, aiming to provide liquidity while minimizing excessive price impact. Since the Act's primary objective was to curb excessive price fluctuations caused by manipulation, volatility serves as a natural proxy for what many at the time regarded as a consequence of manipulation. Accordingly, we use volatility as a measure of manipulation to investigate whether the Act reduced volatility and therefore whether the Act achieved the purpose of what its proponents claimed to be a consequence of manipulation. We henceforth use the term "manipulation" to mean "excess volatility." (We thank the editors for this clarification.) We explore this further in Section 3.

audit, and the identity of the auditing firm—were remarkably low (e.g. see Table 1 in Section 4.2). Additionally, changes in major shareholders' holdings were often treated as private matters, with few firms opting to disclose this information. For the lawmakers of that time, establishing the principle of mandatory disclosure was an unprecedented "yes or no" decision. Their intention was that this principle would effectively improve disclosure quality, curb fraud and manipulation, reduce abnormal market fluctuations, and increase investor confidence: "Frequently the prices of securities on such exchanges and markets are susceptible to manipulation and control, and the dissemination of such prices gives rise to excessive speculation, resulting in sudden and unreasonable fluctuations in the prices of securities …the Federal Government is put to such great expense as to burden the national credit." (Securities Exchange Act of 1934, Title I, Sec. 2, "Necessity for Regulation as Provided in this Title")

Therefore, a natural research subject is whether the law effectively enhances disclosure, reduces market manipulation, and decreases excess market volatility. However, most of the early influential literature focused on finding changes in stock returns, and the general conclusion is that such changes cannot be observed before and after the law (Stigler, 1964; Benston, 1973; Jarrell, 1981; Simon, 1989). Some studies (e.g., Stigler, 1964; King, 1966; Fisher and Lorie, 1970; Benston, 1973; Jarrell, 1981; Officer, 1973) have found that the variance of stock returns decreased after the Acts. But due to the lack of a well-accepted identification strategy, interpretations of these findings vary significantly. Stigler (1964); Officer (1973); Benston (1973); Jarrell (1981) do not view this as reflecting an improvement in market efficiency or an increase in investor welfare. On the contrary, they regard these findings as evidence that the Acts excluded small, risky firms from public listing and did not enhance welfare for less risk-averse investors. Friend and Herman (1964); Friend (1972); Friend and Westerfield (1975); Seligman (1983), among others, interpret the results positively and suggest that the Acts improved investors' valuation of securities and the allocation efficiency of financial markets.[4] Although a consensus has not yet been reached, the current literature seems to

[4]Some studies circumvent the difficult identification issue by studying other institutional designs. La Porta et al. (2006) and Djankov et al. (2008) conduct surveys in a number of nations in the early 2000s, and find cross-country

assume that these two laws have achieved their objectives.[5] We believe that as fundamental laws, their impact deserves to be examined in greater detail.

In this paper we (re)examine the important issue of volatility. First, we restate the legislators' argument using modern economic terminology and discuss the relationship between enhanced disclosure, manipulation, and volatility. We examine the volatility of a stock subjected to manipulation when insiders' information about the security's fundamental value is withheld and compare it to the volatility of a stock unaffected by manipulation under disclosure. Our findings suggest that the volatility of a manipulated stock is higher than the normal volatility of an unmanipulated stock.

We then empirically examine the potential causal relationship between the legislation and volatility. In our empirical analysis, we focus on the 1934 Act (the Act) because the change in volatility is comparable for firms already listed on exchanges prior to the legislation, whereas directly comparing newly listed IPO companies is difficult due to the differing circumstances of firms going public at different times. Our empirical strategy involves the following steps.

First, we construct an overall measure of voluntary disclosure quality based on Barton and Waymire (2004), which emphasizes the transparency and credibility of publicly disclosed annual financial statements. This measure draws on key attributes from the balance sheet, income statement, statement credibility, and reporting conservatism—attributes deemed important by knowledgeable critics of corporate reporting in the 1920s, thus avoiding hindsight bias (e.g. Ripley, 1927; Sloan, 1929). A determinant model similar to that of Barton and Waymire (2004) reveals that

evidence that mandatory disclosure is beneficial to the financial market in several dimensions. Greenstone et al. (2006) study the 1964 Securities Act Amendment, which extended the mandatory disclosure requirements to large over-the-counter (OTC) firms. They find that weekly returns of the OTC firms sampling from 1963 to 1966 significantly improved after the introduction of the amendment. Similarly, Ferrell (2007) find that mandatory disclosure requirement in the 1964 Amendment is associated with an increase in abnormal returns and a reduction in volatility of OTC stocks. Brüggemann et al. (2018) also find that lowering regulatory requirements in the OTC markets reduces market quality. In these studies, the identification problem is less severe. However, one concern is that the estimated effects may be localized, and the results might have limited implications for the impact of the 1933 and 1934 Acts.

[5] "The type of concerns that afflicted investors in the 1920s (lack of transparency and market manipulation) are not at the forefront of their concerns today. In part, this is the result of the success of the 1930s legislation in addressing those problems."—Zingales (2009).

older firms, firms with no recent capital issuance, firms not incorporated in Delaware, and firms experiencing control conflicts were more likely to exhibit poorer disclosure practices prior to the 1934 Act. Our approach differs from most previous literature, which typically classify firms into treatment and control groups based on voluntary disclosure of a single item, such as sales (Benston, 1973; Daines and Jones, 2012; Binz and Graham, 2022; Binz and Roulstone, 2022), cost of goods sold, or the credibility of financial reporting (Daines and Jones, 2012). Not disclosing a single item only indicates poor disclosure quality in that specific dimension; the company may still disclose well in other dimensions.[6] In comparison, a comprehensive indicator better reflects the overall disclosure quality of the company. We classify firms into high and low-quality groups based on this quality index, designating the lowest 10% firms as the treatment group and highest 10% firms as the control group, as low-quality firms are potentially more influenced by the law.

Secondly, in our main analyses we primarily use idiosyncratic volatility (*IVol*)——the standard deviation of the residuals from a factor model of stock returns——as our proxy for volatility. This decision is based on the understanding that stock returns are often influenced by common risk factors that account for systematic risk, which may not be directly associated with manipulation and firm fundamentals. For example, a company might choose not to disclose major shareholders' holdings or several pieces of accounting items because managers consider this information private and don't want competitors to have access to it. Therefore, using *IVol* can help filter out some of the overall economic fluctuations that are unrelated to firm-specific disclosure quality, which is particularly important during the period following the Crash of 1929. Our primary identification strategy involves employing a difference-in-difference and propensity score matched difference-in-difference to compare the differential changes in *IVol* between the low-quality and high-quality groups before and after the law. To avoid interference from stocks that delisted during the legislative process or those newly listed after the Act, our sample includes only stocks with at least one year of

[6]Friend and Westerfield (1975) comment that "all of the 193 stocks (in Benston (1973)) which did not disclose sales did disclose net income as well as balance sheet and other financial data, …this type of test tells us nothing about the relative quality of disclosure for both groups of firms before and after the 1934 Act …"

observations both before and after the Act's passage. By leveraging variations in the law's impact, this comparison delineates the net effect of the law in a relatively clear manner. We find that in various econometric specifications, the decline in *IVol* after the 1934 Act is more significant for the low-quality group than for the high-quality group. Economically, restricting the sample in a relatively short period from January 1932 to December 1936, the magnitude of the decline in the low-quality group is about 10% greater than in the high-quality group.

Admittedly, it's also possible the decision to disclose or not could also be related to economic conditions or systematic risk. For instance, during an economic downturn, when a company's revenue decreases, management may choose not to disclose "sales". In addition, since the factor model is not perfect, the results here may also be heavily influenced by the choice of factors. Given these considerations, in the robustness section, we also use *IVol* with an alternative factor model as well as raw volatility, which does not incorporate any factor structure. The results are qualitatively the same, but the magnitude of economic significance decreases when using raw volatility. This partially supports our hypothesis that differences in voluntary disclosure quality are more apparent in *IVol*.

Thirdly, we further analyse how liquidity varies between firms in the low- and high-quality groups before and after the enforcement of the 1934 Act. From a theoretical point of view, Diamond and Verrecchia (1991) shows that disclosure improves liquidity and reduces the cost of capital for the firm because of the possible mitigation of information asymmetry. Empirically, Welker (1995) and Heflin et al. (2005) find that policies promoting higher disclosure quality of accounting information enhance firms' market liquidity. However, Daines and Jones (2012) find no significant improvement in liquidity between firms that disclosed certain individual accounting metrics and those that did not prior to the 1934 Act.

When volatility is also considered, the direct relationship between disclosure, liquidity and volatility is not clear. On one hand, a reduction in volatility might be due to decreased liquidity: In extreme cases, the near-total loss of liquidity could result in calculated volatility approaching

zero. If the implementation of the Act raises entry barriers and squeezes out some companies (Stigler, 1964; Officer, 1973; Benston, 1973; Jarrell, 1981), it could lead to reduced market liquidity. On the other hand, if the introduction of the Act leads to increased liquidity and simultaneously reduces volatility, it can be considered a sign of more active trading and fewer abnormal price fluctuations. The latter would be evidence of a more efficient and stable market. We find that liquidity significantly increases more in the low-quality group than in the high-quality group after the enforcement of the 1934 Act. This effect is more pronounced in liquidity measures that reflect information asymmetry and trading frequency, such as the bid-ask spread and the percentage of no-trade days. Furthermore, if we categorize firms into low- and high-liquidity groups, the decline in *IVol* predominantly occurs in the low-liquidity, low-quality groups when the time window of the sample is relatively short. These findings suggest that firms with inadequate voluntary disclosure practices and low liquidity levels experienced a greater decline in *IVol* and a more significant increase in liquidity. Our results are fundamentally different from those of Daines and Jones (2012), and one significant reason for this discrepancy could be the different grouping variables we use for the treatment and control groups. Taken together, these results suggest that the legislation effectively stabilizes the market by reducing excessive volatility and increasing liquidity, indicating decreased manipulation and improved disclosure quality.

It is important to note that when we discuss volatility, we are not debating the merits of high or low volatility in the abstract, as its advantages or disadvantages can vary greatly depending on the context. For example, volatility is naturally higher when prices accurately reflect new information on firm fundamentals——a sign of market efficiency——compared to when there is no new information and prices remain unchanged. The challenge lies in the empirical difficulty, if not impossibility, of separating volatility into a "reasonable" component driven by fundamental changes and an "excess" component fueled by manipulation. Therefore, other channels affecting volatility cannot be ruled out entirely. With this in mind, we consider the background context to be essential. As it is widely believed that manipulation was rife during the pre-SEC era, a significant reduction in volatility

after the Act's implementation likely signals a decrease in excess volatility caused by manipulation.

We check the robustness of our results via several approaches. Firstly, as discussed above, we use different measures of volatility and the results remain basically the same. Secondly, we are careful in selecting the testing windows, as the definition of the "pre-" and "post-periods" fundamentally determines the empirical results.[7] To eliminate the impact of legislative news on the market, we follow Benston (1973) and exclude the period from March 1934 to June 1935 as the "Act-in-progress" period.[8] Consequently, the pre-Act window is defined as the period on and before February 1934, and the post-Act period is defined as July 1935 or later. In addition, to avoid transient patterns in the data, in all econometric exercises we keep three samples of different lengths. The short-term window, spanning from January 1932 to December 1936, covers approximately two years before and after the enactment dates of the Act 1934. The medium-term window spans from November 1926, when the Fama-French four factors became available, to November 1941, just before the U.S. entered World War II. The long-term window also begins in November 1926 but continues through December 1963, ending just before the Securities Acts Amendments of 1964.[9] Due to the randomness of financial markets, different window selections may yield different results. Short windows help eliminate other confounding factors, while long windows help identify potential long-term effects. In all samples of different lengths we use a falsification test to observe changes in $IVol$, in order to confirm that these changes indeed occurred during the post-Act period.

Additionally, we conduct a difference-in-difference analysis on NYSE and OTC stocks. The

[7]Several key dates in the legislative process are: The congressional investigation on market manipulation began in April 1932. The 1933 Act was signed into law by President Roosevelt in May 1933, and the first IPO registration with the Federal Trade Commission (FTC) started in July 1933. As for the Act 1934, the first congressional hearing was held in February 1934, the law was enacted in June 1934, and the deadline for registration with the SEC was June 1935. Western Auto was the first to disclose under the 1934 Act on March 15, 1935, but most firms waited until the final deadline of June 30, 1935.

[8]In the falsification test, we reintroduce the "Act-in-progress" period to observe more detailed changes across different time intervals.

[9]A variety of sample windows have been adopted in the literature. For example, Stigler (1964) defines the pre-period to be 1923–1928, and the post-period to be 1949–1955. Daines and Jones (2007) choose January 1934–June 1935 as the pre-1934 Act period and July 1935–December 1936 as the post-1934 Act period. In Mahoney and Mei (2006), the test windows are 30 days before and after each company's filing date with the SEC. Binz and Graham (2022) define pre-1934 Act period to be 1930–1934, and post-1934 Act period to be 1935–1938.

OTC stocks provide a natural control group since they were exempt from the mandatory disclosure requirements prior to the 1964 Amendment. However, due to constraints in data availability, we are unable to compile essential control variables for these stocks. Despite this limitation, our findings reveal a notably larger reduction in volatility for NYSE stocks relative to OTC stocks, with the reduction differing by approximately 20% to 30%. Even accounting for the apparent omitted variable bias, this magnitude of decline is still quite remarkable.

Finally, we test the market-wide effect of the Acts via a "reverse engineering" approach. The market-wide influence of the Acts is important because one economic justification for regulation is that mandatory disclosure can stabilize the market by mitigating market-wide information asymmetry among participants (Leuz and Wysocki, 2016). As cited above, the decline in variance before and after the Acts has been documented in previous studies, but the drawback is that regressional analysis is clearly susceptible to various confounding factors. To circumvent this drawback, we employ a data-mining style testing strategy: Assuming we are unaware of whether there are any structural change points in the volatility series, and if so, how many such points exist. We utilize a structural break test to examine the existence and number of change points in short, medium and long historical dataset starting from the late 19th century. In a time series, identifying structural break points one by one will highlight the most significant changes in the data and overlook less noticeable ones. The 1933 and 1934 Acts are among the most important laws in the U.S. securities market, and if this blind search identifies structural break points closely aligned with the timing of these Acts, it would, to some extent, corroborate their impact on the entire market. In all our exercises, the statistically identified structural change dates are very close to the enactment dates of the 1933 and 1934 Acts.

Our results are limited in several aspects. First, we do not observe significant differences in *IVol* changes when classifying firms into treatment and control groups based on their disclosure of "sales". In the early 20th century, partly due to government hostility towards large corporations (as reflected in the 1890 Sherman Act and the 1914 Clayton Act), managers had a strong incentive

to suppress and manipulate information related to profitability (Sivakumar and Waymire, 2003). Therefore, classifying firms into treatment and control groups based on whether they disclose "sales" is reasonable, though not as comprehensive as our quality measure, and aligns with the methodology used in some studies (e.g. Benston, 1973; Daines and Jones, 2012; Binz and Graham, 2022; Binz and Roulstone, 2022). The inability to observe similar phenomena in the sales-based grouping may indicate that since all NYSE-listed firms are affected by the legislation, the different groupings reflect varying degrees of the law's impact from different perspectives. Essentially, the lack of a clear-cut treatment and control group is a common limitation in current studies examining the 1933 and 1934 Acts.

Secondly, the statistical significance of our main results declines if we use more lenient criteria for the low- and high-quality groupings, such as designating the lowest 20% as the treatment group and the highest 20% as the control group. This result suggests that the legislation's impact on volatility is only significantly evident when comparing the most extreme 100 or so NYSE firms in terms of overall disclosure quality.[10] Considering that the NYSE consisted of the best-disclosing companies of that era, this outcome is not particularly surprising.

Thirdly, due to the lack of consensus on the definition of "manipulation" in the pre-Act era and the scarcity of relevant data, we are unable to directly measure whether the law has reduced market manipulation. Instead, we can only infer indirectly through the volatility indicator, which is simultaneously influenced by other factors. Consequently, despite our theoretical discussions, the extent to which the reduction in volatility reflects a decrease in manipulation remains questionable.

We contribute to the extensive literature on the regulation of disclosure by solidifying one of the key building blocks of the Acts. We (re)focus on the volatility of stock returns and provide credible evidence that the reduction in volatility is caused by the mandatory disclosure requirement. We suggest that this reflects a decrease in manipulation and an improvement in financial reporting, thereby supporting one of the legislators' claimed objectives.

[10]The data cleaning procedure keeps 523 firms in our regression analysis. At 10% and 90% percentile levels the low-quality group contains 54 firms, and the high-quality group contains 53 firms.

The rest of the paper is organized as follows. Section 2 provides a brief overview of the background. Section 3 examines the mechanism connecting disclosure, manipulation, and volatility, followed by Section 4, which outlines our data and empirical design. We present our results and robustness checks in Sections 5 and 6, respectively, and conclude in Section 7. Appendix A offers additional information on the empirical exercises. The online appendix provides detailed technical discussions.

2 Background

In this section, we briefly discuss the background of securities regulations in the early 20th century. For a more detailed review, see e.g., Seligman (1983, 2003) and Mahoney (2015).

2.1 Blue Sky Laws and Exchange Rules

In the early 20th century, the United States experienced a wave of legislation aimed at the securities market. This legislative push was driven by a prevailing sentiment that voluntary information disclosure was insufficient, a concern amplified by a series of closely spaced stock market crashes in the late 19th century.[11] Between 1905 and 1914, twenty separate bills proposing federal incorporation or federal licensing were introduced. Although no federal licensing statute was adopted, between 1911 and 1933, forty-seven states and the territory of Hawaii enacted state laws known as "Blue Sky Laws". As the name suggests,[12] these state laws focused on addressing the significant

[11] These are a few quotes concerning the disclosure environment of the early 20th century: "As late at 1900, the amount of financial information presented to stockholders by the managers of most publicly owned American manufacturing corporations was meager." (Hawkins, 1963); "to prevent the organizers of corporations or industrial combinations from deceiving investors and the public, either through suppression of material facts or by making misleading statements, prospectuses…should be deemed fraudulent unless their promoters furnished 'full details regarding the organization, the property or services…and all other material information necessary for safe and intelligent investment' "(Industrial Commission, 1902); "the 'principal evils' of 'present industrial conditions' included 'secrecy and dishonesty in promotion', 'secrecy of corporation administration' and 'misleading or dishonest financial statements'," and "such views were widely held" (Commissioner of Corporations, 1904).

[12] The name gained popularity after one legislator remarked that "if securities legislation was not passed, financial pirates would sell citizens everything in the state but the blue sky" (Parrish, 1970). Another noted that "some securities swindlers were so brazen that they would sell building lots in the blue sky" (Seligman, 2003).

problem of fraud in the issuance of securities.

The effectiveness of blue sky laws is a matter of debate. Enforcement agencies claimed the laws were effective,[13] but Mahoney (2003) argues that these laws were the product of political progressives and interest groups and had no substantial impact on improving financial markets. At firm level, there is positive evidence of the effectiveness (e.g. Agrawal, 2013), but a significant issue that state legislation could not address was that securities were sold across state borders.[14] Additionally, these laws typically do not require (or result in) publicly available disclosure (Mahoney, 2003; Brüggemann et al., 2018). Due to these issues, blue sky laws faced several criticisms: "First, the statutes were riddled with exemptions. …Second, Blue Sky administrators often were inexperienced and, on occasion, corrupt. …Third, defrauded investors often entered into 'compounding' agreements under which the seller of fraudulent securities returned part or all of the money in return for immunity from criminal prosecution" (Seligman, 1983, p. 21).

Besides blue sky laws, exchange rules were an important component of securities regulation. The rules of the NYSE represented the highest standard for information disclosure at the time (Ripley, 1927). But the consideration that NYSE was unable to effectively promulgate or enforce effective disclosure rules played a key role in persuading the U.S. Congress to enact the 1933 and 1934 Acts (Seligman, 1983). The main problems with the NYSE's enforcement were: 1. Firms could circumvent NYSE's disclosure rules by trading as "unlisted" on the New York Curb Exchange or any other of the seventeen exchanges permitting such practices;[15] 2. The NYSE might not have seriously enforced its disclosure rules. As noted by Parrish (1970), the number of newly listed stocks

[13]The Michigan Securities Commission reported that "over $200 million of questionable securities have been prevented from sale in Michigan through the administration of Blue Sky law during the period covered by this report" (Michigan Securities Commission, 1918). Similar reports were issued by other states.

[14]In 1915, the Investment Banker's Association informed its members that "the blue sky laws could be easily evaded by operating across state lines. Promoters could sell their securities through the mails in other states, as long as the sale was finalized through an acceptance from the seller's office by mail or telegram" (Keller, 1988).

[15]"An unlisted security was one about which the corporate issuer had supplied no information whatsoever but which had been admitted to trading on an exchange based on an application of an exchange member who usually supplied a pro forma description of the issuing firm based on data appearing in a statistical manual such as Moody's. Although the New York Curb Exchange in 1933 was the second-largest exchange in the country, as of November 1933, 82 percent of all securities traded there were on an unlisted basis" (United States Senate Committee on Banking and Currency, 1934b, pp. 68–70, as summarized in Seligman (1983)).

surged from 300 in 1926, to 571 in 1928, and to 759 in the first nine months of 1929. However, the NYSE Listing Committee remained nearly unchanged, consisting of eight Exchange members and a small investigative staff. The competence of the committee was in doubt when the work load had increased significantly.[16] Self-regulatory organizations like the NYSE, especially when membership is voluntary, face common challenges: competitive pressure from similar institutions can lead to lenient standards; lack of self-interest in investigations can result in them being neither timely nor thorough; and the lack of enforcement power makes it difficult to compel unwilling companies to comply with their regulations (Seligman, 2003).

2.2 The 1933 and 1934 Acts

Partly due to the above discussions, legislators began implementing national-level legislation after the market turmoil of 1929. The 1933 Act aimed to address the lack of disclosure in public offerings. It required the disclosure of underwriters' and promoters' compensations as well as insiders' profits. Additionally, it regulated the process of conducting a public offering. The 1934 Act adopted many of the NYSE's disclosure rules and made important additions. It mandated the content, timing, and frequency of information disclosure, established the accounting principles and assumptions for financial statements (which later evolved into U.S. Generally Accepted Accounting Principles). Information that was previously considered proprietary and confidential, such as management compensation and the identities and holdings of major shareholders, was now required to be disclosed.

The Acts also imposed civil liability on corporate officers who concealed or misrepresented material facts in financial reports, placing the burden of proof on the firm rather than the plaintiff.

[16] As an example of the NYSE's inattentiveness, the Pecora Hearings documented testimony from Frank Altschul, chairman of the Exchange's Committee on Stock Lists. Altschul revealed that the NYSE had ceased conducting an independent investigation of Kreuger and Toll Company's application for a thirty-year debenture, which included a provision allowing the substitution of new pledged securities for existing collaterals. Although the committee was aware of this unusual substitution privilege, no due diligence investigation was undertaken. Consequently, when the company replaced French bonds worth at least $24.5 million with Yugoslavian debentures worth about $10 million as collaterals, Altschul could only testify that the NYSE had been deceived (United States Senate Committee on Banking and Currency, 1934b).

The Act granted the SEC expansive authority to enact, penalize, and uphold securities laws on a national scale. Its impacts became evident swiftly. By June 30, 1938, three years after all firms registered with the SEC, 290 cases had been initiated, resulting in 486 firms and individuals permanently enjoined from the acts and practices in question (Securities and Exchange Commission, 1938).

3 Mechanism Discussion: Enhanced Disclosure and Excess Volatility

We briefly discuss the mechanism behind the legislators' claim (the necessity of legislation quoted previously) that enhanced disclosure leads to decreased volatility. The claim can be summarized as the following conjecture: Insufficient disclosure leads external investors to form false beliefs about a company's fundamentals, creating opportunities for manipulation that cause stock prices to abnormally deviate from their true value.

In the online appendix we formalize this claim with a model that builds upon Allen and Gale (1992). This model describes a scenario where a manipulator or insider can leverage information asymmetry to create the illusion of potential fundamental changes, attracting ordinary external investors to trade. Such an action triggers price fluctuations reflecting no real underlying economic information. We find that this manipulation-induced volatility is larger than the normal price movements that occur with genuine fundamental news changes. The reason for this amplified volatility is that speculative prices without fundamental support will inevitably revert to fair value. The substantial price swings emerge from the stark correction that occurs when false beliefs—carefully instilled by manipulators through strategic information exploitation—ultimately unravel.[17]

[17]When can manipulation be profitable? Many studies have considered cases of profitable manipulation under different model setups, all involving some form of information asymmetry. In Hart (1977), the manipulator "understands the way in which other traders in the market behave, at least in the aggregate…", which in reality likely means that the manipulator has learned some important information in advance and understands its impact on others who are unaware of it. In Allen and Gale (1992), the informed trader possesses private information affecting the fundamental value that is not disclosed to the public. Several other types of information asymmetry that incentivize

The establishment of accounting standards and improvements in financial statement disclosures can also lead to reduction of volatility unrelated to manipulation. Rajgopal and Venkatachalam (2011) document that deteriorating earnings quality is associated with higher idiosyncratic volatility over 1962–2001. Goldstein and Yang (2017) show that more transparent accounting information can reduce price volatility by allowing prices to more effectively reflect a company's operating conditions. On the other hand, contemporary literature offers a more detailed and nuanced discussion on what information should be disclosed and how, recognizing that in some cases, disclosure does not necessarily lead to increased market efficiency, reduced volatility, or enhanced social welfare (e.g. Gao and Liang, 2013; Goldstein and Yang, 2019).[18] However, as noted earlier, the state of voluntary disclosure in the 1930s was very poor for a subset of publicly traded companies, making their stocks susceptible to manipulation. Therefore, the marginal impact of mandatory disclosure on improving overall information transparency is likely to be significant, with its role in reducing volatility tied to a decrease in manipulation. We consider that assessing a company's overall disclosure quality in the pre-SEC era to distinguish the treatment group from the control group is a reasonable and practical approach.

manipulators are described in Van Bommel (2003); Aggarwal and Wu (2006); Goldstein and Guembel (2008). A suggestion made by Benabou and Laroque (1992) is that "more effective ways to prevent manipulation may be to require some types of insiders to disclose their trades promptly…", and Fishman and Hagerty (1995) argues that insiders have no incentive to disclose their trades voluntarily, so this kind of disclosure must be mandatory.

[18] In the extension, Goldstein and Yang (2017) show that public disclosure of information may crowd out the production of private information when the cost of information acquisition is considered. However, for insiders, the cost of acquiring information is likely quite low. Given the historical context of the 1934 Act, we do not delve further into this scenario here.

4 Empirical Design and Data Description

4.1 Empirical Design

4.1.1 Idiosyncratic Volatility

We use the following factor model to estimate idiosyncratic volatility (*IVol*):

$$r_{st}^{(i)} = \alpha_t^{(i)} + \beta_{1t}^{(i)} MKT_{st} + \beta_{2t}^{(i)} SMB_{st} + \beta_{3t}^{(i)} HML_{st} + \beta_{4t}^{(i)} MOM_{st} + \epsilon_{st}^{(i)}, \tag{1}$$

where $r_{st}^{(i)}$ is the excess return of stock i at date s in month t, *MKT* is the excess market return, *SMB* is the "Small Minus Big (market capitalization)" factor, *HML* is the "High Minus Low (book-to-market value ratio)" factor, and *MOM* is the momentum factor. *IVol* is defined as

$$IVol_{it} = \left(\frac{\#AvgTradeDays}{N_t^{(i)} - 1} \sum_{s=1}^{N_t^{(i)}} (\epsilon_{st}^{(i)})^2 \right)^{1/2}, \tag{2}$$

where $N_t^{(i)}$ is the number of trading days for stock i in month t, and $\#AvgTradeDays$ is a scaling number that equals the average trading days in a month. $\#AvgTradeDays = 25$ before September 1952 (as the NYSE traded 6 days a week at that time) and $= 21$ on or afterwards.

4.1.2 The Disclosure Quality Index

To observe the causal effect of the law on *IVol*, we construct an index that assesses the overall disclosure quality of firms prior to the Act, which effectively separates the treatment group from the control group. Based on Barton and Waymire (2004), this index evaluates attributes in annual reports that were considered crucial by critics of corporate reporting in the pre-SEC era, thereby avoiding hindsight bias introduced by modern accounting practices.[19] The data are collected by

[19] Before the SEC, accounting and auditing principles were not yet fully developed and were more akin to conventions emerging from customary business practices (Moonitz, 1970; Ely and Waymire, 1999). See Barton and Waymire (2004) for detailed discussions on why these attributes were highly valued pre-SEC.

reviewing firms' annual reports for the most recent fiscal year ending no later than December 1933 from *Moody's Manual of Investments, American and Foreign: Industrial Securities*, published in 1934 (Moody's Manual 1934).

Concretely, the index is comprised of the following transparency scores:

1. Income Statement Transparency (ISTRANSP): This measure is based on the separate disclosure of sales, cost of sales, depreciation expense, tax expense, and other operating expenses. ISTRANSP is coded 0–5 based on the count of separate items disclosed in the firm's income statement. The maximum value of 5 indicates that the firm disclosed all five items; a value of 0 indicates that the firm disclosed none of these five items.

2. Balance Sheet Transparency (BSTRANSP): This measure is based on separate disclosures about fixed assets, intangibles, surplus, and reserves. Similar to ISTRANSP, BSTRANSP is the sum of five indicator variables. The first indicator is set to 1 (0 otherwise) when the net value of property, plant, and equipment was disclosed (indicating that fixed assets had been subject to depreciation in some cases), while the second indicator is set to 1 (0 otherwise) when the depreciation reserve was also disclosed. The third indicator is set to 1 (and 0 otherwise) if intangible assets were reported as a separate line item. The fourth indicator is set to 1 (and 0 otherwise) if earned surplus was reported separately from capital surplus. The fifth indicator is set to 1 (and 0 otherwise) if reserves other than depreciation were reported separately.

3. Auditor (AUDITOR): This measure reflects the quality of the external audit. AUDITOR is coded 2 if the firm's financial statements were audited by one of the nine largest auditors at the time,[20] 1 if audited by a small auditor, and 0 if not audited.

4. Accounting Conservatism (CONSERV): This measure is based on firms' reported intangible

[20]The list of nine largest auditors at the time is based on Merino et al. (1994). They were (in descending order) Price Waterhouse; Ernst and Ernst; Haskins and Sells; Arthur Young; Peat, Marwick and Mitchell; Lybrand, Ross Brothers and Montgomery; Barrow, Wade and Guthrie; Delloite, Plenders and Griffin; and Touche Niven. If no auditor name is mentioned anywhere in the financial statements, the firm is considered as not issuing audited financial statements.

asset values. CONSERV takes on the value of 1 (or 0) if the firm reported intangible assets at nominal amounts on the balance sheet (e.g. 1$), indicating a more conservative approach.

The overall quality index is constructed by summing the four scores of transparency and standardizing (minus median and divided by standard deviation) the raw score within each industry.[21] The benefit of the standardization is that it considers industry-specific norms and conditions of disclosure while not changing the order of disclosure quality within each industry. It also rescales the quality index so that firms across different industries are comparable. The treatment group comprises firms that score below 10% in the disclosure quality index, while the control group consists of firms that score above 90%. Figures A.1 and A.2 in Appendix A provide examples of two firms' voluntary disclosure in Moody's Manual 1934.

4.1.3 Difference-in-difference

The difference-in-difference estimation is:

$$\log(IVol_{it}) = \mu_i + \tau_t + \beta_1 LowQlty_i \times PostAct34 + \gamma_0 \log(IVol_{i,t-1}) + \boldsymbol{\gamma}' Controls_{it} + u_{it}, \qquad (3)$$

where μ_i is firm fixed effect, τ_t is month fixed effect, $LowQlty_i$ is a dummy variable that equals 1 if the firm is in the treatment group (low disclosure quality) and 0 if the firm is in the control group (high disclosure quality). $Controls_{it}$ is the vector of the control variables including stock return, log of stock price, log of market capitalization, turnover ratio, idiosyncratic skewness, book-to-market ratio, leverage, and firm age. It has been documented that small firms tend to be more volatile, lower-priced firms have higher volatility, more liquid firms are less volatile, and there is a tendency for volatility to rise following negative returns (Campbell et al., 2001; Sias, 1996;

[21] Our construction at this step is different from Barton and Waymire (2004) in which a principal component of the four scores is used as the final quality index. The main reason is that we find in the 1933 reports the principal component of the four scores can only explain about 50% of the shared variation, while in the 1929 reports the principal component explains about 87% according to Barton and Waymire (2004). Even combining the first two components in the 1933 reports accounts for less than 70%. We thus use a more straightforward approach to construct the quality index.

Brandt et al., 2010). Idiosyncratic skewness is added to control the intercorrelation caused by speculators' preference to gambling-like stocks (Kumar, 2009; Barberis and Huang, 2008; Barberis and Xiong, 2009). We include the book-to-market ratio and leverage to ensure that the results do not merely reflect the relationship between future growth opportunities and idiosyncratic volatility (Xu and Malkiel, 2003; Cao and Zhao, 2008). We also incorporate firm age to control for the apparent time trend effect. In the idiosyncratic volatility literature, institutional ownership and retail trading intensity are important covariates (Brandt et al., 2010). We are not able to include these variables because the information is not available in our sampling period. However, we are not much concerned about firm-level differences in these two variables because retail investors play a major role in the entire market before 1960 (Evans, 2009; Zingales, 2009). We lag all control variables related to trading (i.e. except leverage and firm age) to filter their contemporaneous effect with *IVol*. We add the lagged value of *IVol* as an independent variable because of its autocorrelation. A complete list of variable definition is provided in Appendix A.

In all main regressions we apply the falsification test in the following form:

$$\begin{aligned}\log(IVol_{it}) = {} & \mu_i + \tau_t + \beta_1 LowQlty_i \times PreAct34_{-12m,-7m} + \beta_2 LowQlty_i \times PreAct34_{-6m,-1m} \\ & + \beta_3 LowQlty_i \times Act34InProgress + \beta_4 LowQlty_i \times PostAct34_{1m,6m} \\ & + \beta_5 LowQlty_i \times PostAct34_{7m,12m} + \beta_6 LowQlty_i \times PostAct34_{13m+} \\ & + \boldsymbol{\gamma}' Controls_{it} + \gamma_0 \log(IVol_{i,t-1}) + u_{it}, \end{aligned} \tag{3'}$$

where $PreAct34_{-jm,-km}$ is a dummy variable for the period from j to k months before March 1934 (inclusive, same below), $PostAct34_{jm,km}$ is a dummy for the period from j to k months after June 1935, and $PostAct34_{13m+}$ is a dummy for the period 13 months after June 1935. *Act34InProgress* equals 1 in March 1934 through June 1935, and 0 otherwise. If significant changes occur in the time dummy after the 1934 Act, our confidence is strengthened that the effect is caused by the Act.

We check the robustness of the results in several additional analyses: implementing the difference-

in-difference analysis with a propensity score matched sample based on the disclosure quality determinant model of Barton and Waymire (2004); examining the Act's effect through the liquidity channel; testing if the results still hold with alternative measures of volatility; comparing OTC stocks with NYSE stocks because OTC stocks were not directly influenced by the Act until 1964; and employing a structural break test on the aggregate volatility to search for signs of the impact of the Act. More details are provided in Section 5.

4.2 Data

The data on NYSE traded stocks are from the Center for Research in Security Prices (CRSP), and the four factors used in constructing the *IVol* series are downloaded from Professor Kenneth French's website. Financing firms (SIC code 6000–6999), utilities (SIC code 4800–4999), and railway companies (SIC code 4000–4099) are excluded from the sample because these firms were subject to other regulations and their reporting practices differed in fundamental ways from other industrials. We retain securities with share codes 10 or 11, exclude observations where the number of trading days within a month is fewer than 12, and require at least one year of trading records in both the pre- and post-1934 Act periods. The full sample covers November 1926 (when all factors in equation 1 are available) through December 1963, the year before the Securities Acts Amendments of 1964.

The disclosure quality index is constructed by reviewing firms' financial reports in Moody's Manual 1934. The other control variables that are related to accounting information are also from Moody's yearly manuals, and are kindly provided by Professor John Graham.

Table 1 provides the descriptive statistics of the components of quality index. By 1933, approximately 61.8% of all firms disclosed sales, and 45.5% disclosed the cost of sales. In the high-quality group, 90.6% disclosed sales and 79.2% disclosed the cost of sales, whereas in the low-quality group, these figures were 37% and 22.2%, respectively.[22] In general, firms in low quality group reported

[22] As a validation of data entry accuracy, Benston (1969) reported that the percentage of firms not disclosing cost of goods sold in 1934 is 46%; Binz and Graham (2022) reported that the percentage of firms not disclosing sales in

much less information on cost of sales, other operating expense, intangible assets, and earned surplus than the high quality group did. There is a significant amount (22.2%) of firms in the low quality group that did not have their financial statements audited, and most low quality firms (92.6%) chose a non-conservative approach when reporting intangible assets.

Following Barton and Waymire (2004), we explore which factors determined firms' voluntary disclosure quality by regressing *Quality* index on measures for equity market information cost including a firm's age (*Age*, the number of years since the firm's incorporation date), membership in the technology industry (*Tech*, an indicator that the firm is in the technology industry), earnings variation (*CVEarn*, the coefficient of variation in net income over the previous five years), systematic risk (*Beta*, the slope coefficient obtained from regressing the firm's excess return on the market risk premium with monthly data before December 1933), return on equity (*ROE*, net income divided by shareholders' equity), and capital issuance (*Issue*, an indicator if shares outstanding of the firm increased by more than 5% between January 1931 and December 1933); measures for contractural and control conflicts including leverage (*Leverage*, total debt divided by common shareholders' equity), income conflicts (*IncConf*, an indicator that the firm has income bonds, noncumulative preferred stock or another type of stock with participation rights), control conflicts (*ContConf*, an indicator that the firm is controlled by a voting trust or another company, 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), and whether the state of incorporation is Delaware (*Delaware*, an indicator that the firm is chartered in Delaware); measures for competitive and political costs including market share (*MktShr*, the firm's total assets divided by the sum of total assets of all sample firms in the same two-digit SIC code industry), size (*Size*, the logrithm of firm's total assets); and measures for alternative information including a firm's dividend policy (*Dividend*, an indicator that the firm paid dividends) and membership in a regulated industry (*Regulated*, an indicator that the firm is in a regulated industry). The results are shown in Table 2.

1933 is 41%.

Table 1: Percentage of Firms Disclosing Components of the Quality Index

Panel A: percentage of firms disclosing selected financial statement items

Income statement item	All(%)	HighQlty(%)	LowQlty(%)	Balance sheet item	All(%)	HighQlty(%)	LowQlty(%)
Sales	61.8	90.6	37.0	Property, plant and equipment (net)	93.1	98.1	77.8
Cost of sales	45.5	79.2	22.2	Depreciation reserve	91.2	98.1	74.1
Depreciation expense	91.4	96.2	74.1	Reserves other than for depreciation	74.8	94.3	55.6
Other operating expense	57.7	84.9	33.3	Intangible assets	56.0	84.9	24.1
Tax expense	59.1	84.9	40.7	Earned surplus separately reported	50.1	77.4	33.3

Panel B: percentage of firms across levels of financial statement transparency, audit quality and conservatism

ISTRANSP	All(%)	HighQlty(%)	LowQlty(%)	BSTRANSP	All(%)	HighQlty(%)	LowQlty(%)
5 items	13.8	44.4	1.9	5 items	19.3	57.4	0.0
4	28.7	44.4	5.6	4	40.5	35.2	22.2
3	26.4	9.3	25.9	3	29.1	5.6	37.0
2	22.8	0.0	37.0	2	8.4	0.0	25.9
1	7.3	0.0	24.1	1	2.5	0.0	13.0
0	1.1	0.0	5.6	0	0.2	0.0	1.9
AUDITOR	All(%)	HighQlty(%)	LowQlty(%)	CONSERV	All(%)	HighQlty(%)	LowQlty(%)
2 (Large)	68.1	85.2	31.5	1 (Yes)	34.2	66.7	7.4
1 (Small)	26.0	11.1	46.3	0 (No)	65.8	31.5	92.6
0 (None)	5.9	1.9	22.2				

The full sample ("All") consists of 523 firms that met our data cleaning requirements. The "HighQlty" ("LowQlty") sample consists of 53 (54) firms that had the scores of transparency above 90% percentile (below 10% percentile) in their financial reports. Financial reporting variables are for the most recent fiscal year no later than December 1933 reported in Moody's Manual of 1934. The construction of ISTRANSP, BSTRANSP, AUDITOR, and CONSERV follows Barton and Waymire (2004). ISTRANSP(Income statement transparency)=the number of the following items disclosed: sales; cost of sales; depreciation expense; other operating expenses; income tax expense. BSTRANSP(Balance sheet transparency)=the number of the following items disclosed: property, plant and equipment (net of accumulated depreciation); intangible assets reported as a separate line-item; depreciation reserve; reserves other than for depreciation; earned surplus reported separately from capital surplus. AUDITOR= 2 if the firm's financial statements were audited by one of the nine largest auditors at the time (Merino et al., 1994); 1 if they were audited by other auditors; 0 if they were not audited. CONSERV= 1 if the firm reported intangible assets valued at nominal amounts (e.g., \$1), 0 otherwise.

Table 2: Quality Determinant Model

	Dependent Variable: *Quality*					
	(1)	(2)	(3)	(4)	(5)	Sign in BW
Equity market information costs						
Age	−0.009***				−0.007*	−**
	(0.003)				(0.004)	
Tech	0.158				0.169	+***
	(0.105)				(0.105)	
CVEarn	−0.000				−0.000	−***
	(0.000)				(0.000)	
Beta	−0.192***				−0.113	−
	(0.073)				(0.086)	
ROE	0.117***				0.132	+
	(0.040)				(0.161)	
Issue	0.300**				0.285**	+**
	(0.138)				(0.138)	
Contractual and control conflicts						
Leverage		−0.008**			0.004	+***
		(0.004)			(0.015)	
IncConf		−0.141			−0.145	+
		(0.181)			(0.179)	
ContConf		−0.209**			−0.160*	−***
		(0.092)			(0.093)	
Delaware		0.271***			0.220**	−
		(0.091)			(0.097)	
Competitive and political costs						
MktShr			−0.182		−0.252	−
			(0.286)		(0.334)	
Size			−0.010		0.024	−
			(0.035)		(0.038)	
Alternative information						
Dividend				0.198**	0.133	−**
				(0.086)	(0.106)	
Regulated				0.117	0.351	−***
				(0.263)	(0.283)	
Constant	0.442***	0.054	0.232	−0.035	−0.185	
	(0.132)	(0.059)	(0.581)	(0.060)	(0.598)	
R^2	0.043	0.036	0.001	0.010	0.068	
Observations	520	520	520	520	520	

The sample consists of 520 firms that met our data cleaning requirements in Section 4.2 and had complete records of the control variables used in the regressions. The financial reporting variables are for the most recent fiscal year ending no later than December 1933. The last column lists the signs and significance levels reported in Barton and Waymire (2004), which uses financial variables in fiscal years ending no later than June 1929. The definitions of the variables are listed in appendix A. *, **, *** indicate significance levels at 10%, 5% and 1% levels.

Table 2 contains 520 firms that met the data cleaning requirements and had complete records of the control variables in financial reports ending no later than December 1933.[23] For most of the observed estimates, the signs are consistent with those in Barton and Waymire (2004). For example, older firms, firms with more earnings variations, and firms with control conflicts tended to disclose less. Technology firms, firms with higher ROE, and firms had new issuance recently, tended to disclose more. These findings align with intuition. For instance, older firms might exhibit path dependence in their disclosure practices, firms with control conflicts might have unresolved agency problems, while tech companies and more profitable firms were more inclined to use increased disclosure to showcase their strengths.

There are some discrepancies as well. We find that firms incorporated in Delaware were more likely to disclose with higher quality, a result that is conceptually consistent—managers' broader decision rights under Delaware charter law could lead to more intensive monitoring by shareholders—but empirically inconsistent with findings in Barton and Waymire (2004). We also cannot find statistically significant results for firms that paid dividends or belonged to a regulated industry, whereas Barton and Waymire (2004) find negative effects for these firms. The discrepancy here may stem from changes in the disclosure environments due to the different time periods of the samples. The disclosure information in their paper is based on a sample from June 1929, prior to the stock market crash, whereas our sample is from December 1933. Another important observation is that firm size and market share are not significant factors influencing disclosure quality, which alleviates concerns about a close relationship between disclosure quality and firm size.

Table 3 shows the descriptive statistics of variables used in difference-in-difference analysis. We winsorize all continuous variables at the 0.1 and 99.9 percentiles. On average, both raw volatility (Vol) and idiosyncratic volatility ($IVol$) are higher in the low quality group, but the effect is most prominent in percentiles larger than 50%. Similarly, on average the stocks in the low quality group are less liquid than those in the high quality group in terms of bid-ask spread ($BidAsk$,

[23] In constructing the disclosure quality index used in the main difference-in-difference analyses, 523 firms are available. In this quality determinant model, three firms with missing data in the control variables are excluded.

monthly average of daily close relative spread, $2(ask - bid)/(ask + bid)$), percentage of no trade days (*PctNoTradeDays*, percentage of no trade days in a month) and Amihud (2002) illiquidity ($\text{Amihud}_t = \frac{1}{D}\sum_{t=1}^{D}\frac{|\text{ret}_t|}{\text{volume}_t}$ where D is the number of days in a month), and the main differences come from groups with percentiles larger than 50%. The average market capitalization (*MktCap*) and book-to-market ratio (*BM*) are larger in the low quality group, indicating that large firms and high value firms disclosed less transparently than small and low value firms. Firms in low quality groups are also older on average. For other variables, there are generally no significant differences between the two groups. These observations are consistent with the hypothesis that voluntary disclosure is more valuable for firms facing more severe information asymmetry issues, such as small, young, or less visible firms (e.g Daines and Jones, 2012).

5 Results

5.1 Changes in Idiosyncratic Volatility

We first implement difference-in-difference exercises to compare the change of idiosyncratic volatility in the low and high quality disclosure groups. Table 4 presents the results of estimating equations (3) and (3′). Odd number columns report baseline results and even number columns report parallel trend falsification tests. When samples are confined between 1932 and 1936, the estimation bias created by lag of the dependent variable becomes a concern because the time dimension is relatively short comparing to the cross-sectional dimention ($N = 107, T = 40$). Columns (3) and (4) thus present the Arellano and Bond (1991) type dynamic panel regressions. Further lags of *log(IVol)* are used as the instrument variables, but the lags are restricted to 3 to alleviate the weak instrument problem. For medium- and long-term regressions we only implement regular fixed effects models because the dynamic panel bias is less of a concern but the problem of weak instruments arises. Firm and time fixed effects are controlled and standard errors are clustered by firm and month.

The interaction term, *LowQlty*$\times$*PostAct34* is significantly negative in each of the sample periods

Table 3: Descriptive Statistics

Variable	Group	Mean	STD	P1	P25	P50	P75	P99
Volatility measures								
IVol	All	0.103	0.114	0.021	0.048	0.070	0.114	0.546
	HighQlty	0.102	0.108	0.023	0.050	0.072	0.113	0.514
	LowQlty	0.114	0.132	0.021	0.047	0.074	0.129	0.653
Vol	All	0.132	0.137	0.027	0.062	0.093	0.153	0.646
	HighQlty	0.132	0.128	0.029	0.065	0.096	0.153	0.626
	LowQlty	0.145	0.155	0.027	0.062	0.098	0.169	0.769
Liquidity measures								
BidAsk	All	0.033	0.061	0.000	0.010	0.016	0.029	0.302
	HighQlty	0.032	0.057	0.000	0.010	0.017	0.029	0.295
	LowQlty	0.039	0.078	0.000	0.009	0.016	0.035	0.396
PctNoTradeDays	All	0.186	0.260	0.000	0.000	0.045	0.300	0.957
	HighQlty	0.177	0.247	0.000	0.000	0.045	0.280	0.923
	LowQlty	0.219	0.280	0.000	0.000	0.080	0.381	0.960
Amihud	All	38.673	1029.828	0.011	0.285	1.192	5.460	569.504
	HighQlty	29.541	298.337	0.014	0.296	1.187	4.909	431.576
	LowQlty	90.864	2984.370	0.008	0.246	1.951	9.301	944.309
Control variables								
Price	All	11.345	21.721	0.026	1.727	5.431	13.412	86.500
	HighQlty	11.148	19.975	0.028	1.252	5.086	13.717	68.375
	LowQlty	10.384	15.624	0.079	2.428	6.067	12.760	67.149
Return	All	0.014	0.150	−0.324	−0.049	0.003	0.062	0.502
	HighQlty	0.014	0.146	−0.321	−0.051	0.003	0.064	0.500
	LowQlty	0.015	0.165	−0.333	−0.052	0.000	0.062	0.559
MktCap (in '000s)	All	131806	622589	332	5816	18554	67832	1850942
	HighQlty	86266	220865	512	6611	18270	62723	935809
	LowQlty	178733	608813	210	3750	12005	79350	3389295
IdioSkew	All	0.244	0.745	−1.599	−0.192	0.203	0.644	2.346
	HighQlty	0.263	0.749	−1.588	−0.180	0.221	0.661	2.404
	LowQlty	0.247	0.745	−1.558	−0.191	0.203	0.638	2.406
BM	All	2.012	6.015	0.152	0.658	1.078	1.822	17.925
	HighQlty	1.956	3.925	0.171	0.657	1.135	1.904	20.004
	LowQlty	2.999	11.054	0.172	0.708	1.266	2.263	35.378
Leverage	All	0.359	0.217	0.023	0.197	0.337	0.488	0.919
	HighQlty	0.368	0.204	0.021	0.225	0.353	0.498	0.871
	LowQlty	0.366	0.258	0.007	0.192	0.337	0.502	0.876
Age	All	32.175	17.805	2.583	19.167	30.417	42.250	84.000
	HighQlty	29.264	17.678	2.000	15.833	27.333	38.750	84.750
	LowQlty	34.499	18.868	3.333	20.917	32.417	44.250	94.417
Other information								
N of firms	All	523						
	HighQlty	53						
	LowQlty	54						
N of industries	All	40						
	HighQlty	19						
	LowQlty	24						

This table presents descriptive statistics for variables used in difference-in-difference analysis. A list of the definition of variables is presented in appendix A.

Table 4: Difference-in-difference: Low Quality versus High Quality

	Dependent Variable: *log(IVol)*							
	1932.01–1936.12				1926.11–1941.11		1926.11–1963.12	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
LowQlty × *PostAct34*	−0.112***		−0.112**		−0.068**		−0.074***	
	(0.041)		(0.047)		(0.029)		(0.028)	
LowQlty × $PreAct34_{-12m,-7m}$		−0.050		−0.032		0.017		0.041
		(0.054)		(0.048)		(0.045)		(0.044)
LowQlty × $PreAct34_{-6m,-1m}$		−0.035		−0.023		0.032		0.056*
		(0.043)		(0.051)		(0.029)		(0.031)
LowQlty × *Act34InProgress*		−0.075*		−0.058		−0.006		0.013
		(0.043)		(0.045)		(0.029)		(0.029)
LowQlty × $PostAct34_{1m,6m}$		−0.132**		−0.101*		−0.053		−0.025
		(0.050)		(0.056)		(0.034)		(0.032)
LowQlty × $PostAct34_{7m,12m}$		−0.137**		−0.117*		−0.062*		−0.031
		(0.055)		(0.061)		(0.032)		(0.030)
LowQlty × $PostAct34_{13m+}$		−0.118**		−0.124**		−0.064*		−0.066**
		(0.051)		(0.058)		(0.033)		(0.031)
Lag *log(IVol)*	0.127***	0.144***	0.109***	0.123***	0.285***	0.283***	0.314***	0.317***
	(0.025)	(0.020)	(0.031)	(0.026)	(0.018)	(0.017)	(0.014)	(0.014)
Lag *Return*	−0.027	−0.049	−0.045*	−0.068***	−0.080***	−0.089***	−0.107***	−0.119***
	(0.031)	(0.030)	(0.027)	(0.025)	(0.024)	(0.024)	(0.026)	(0.027)
Lag *log(Price)*	−0.044	−0.058	−0.015	−0.010	−0.006	−0.009	−0.014	−0.014
	(0.074)	(0.066)	(0.016)	(0.016)	(0.033)	(0.033)	(0.024)	(0.024)
Lag *log(MktCap)*	−0.305***	−0.292***	−0.249***	−0.247***	−0.271***	−0.271***	−0.197***	−0.199***
	(0.066)	(0.058)	(0.014)	(0.013)	(0.031)	(0.031)	(0.021)	(0.021)
Lag *Turnover*	0.043	0.034	−0.091	−0.121	0.032*	0.036**	−0.004	−0.004
	(0.047)	(0.046)	(0.090)	(0.104)	(0.018)	(0.018)	(0.024)	(0.024)
Lag *IdioSkew*	−0.006	−0.009	−0.022**	−0.023***	−0.011*	−0.012**	−0.004	−0.005
	(0.012)	(0.009)	(0.010)	(0.008)	(0.006)	(0.006)	(0.004)	(0.004)
Lag *BM*	−0.001**	−0.001***	0.000	0.000	−0.001	−0.001*	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Leverage	0.039	0.026	0.060	0.054	−0.010	−0.003	0.090*	0.094*
	(0.112)	(0.091)	(0.089)	(0.080)	(0.060)	(0.056)	(0.050)	(0.050)
Age	−0.023	−0.015	0.001	0.001	0.000	0.001	−0.005	−0.004
	(0.017)	(0.015)	(0.001)	(0.001)	(0.011)	(0.010)	(0.008)	(0.008)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.806	0.804			0.773	0.772	0.759	0.761
AB test of AR(1)			0.000	0.000				
AB test of AR(2)			0.649	0.913				
Hansen's overidentification test			0.872	1.000				
Number of instruments			153	215				
Observations	3879	5333	3879	5333	12287	13741	24453	25907

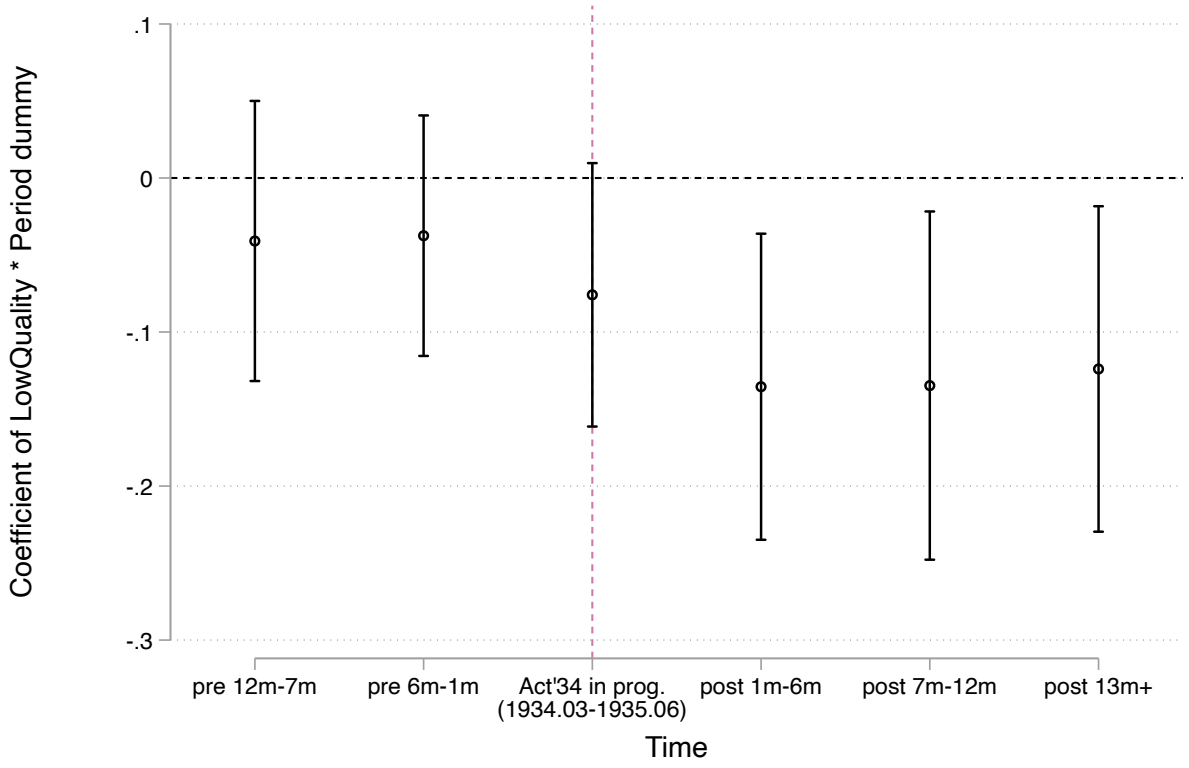
This table reports results of the difference-in-difference design in the short-term period (1932.01–1936.12, columns (1) through (4)), medium-term period (1926.11–1941.11, columns (5) and (6)), and long-term period (1926.11–1963.12, columns (7) and (8)). Columns (3) and (4) report Arellano and Bond (1991) type dynamic panel regressions because the time dimension is relatively short in samples covering 1932.01–1936.12 ($N = 107, T = 40$). Further lags of $\log(IVol)$ are used as the instrument variables and the lags are restricted at 3 so that the weak instrument problem is confined. For medium- and long-term regressions we only implement regular fixed effects models because the dynamic panel bias is less of a concern but the problem of weak instruments arises. Standard errors in parentheses are clustered by firm and year. *, **, *** indicate significance levels at 10%, 5% and 1% levels.

specified. This is evidence that the declines in idiosyncratic volatility were more prominent for firms that were forced by the Act to increase the transparency of their financial disclosures. In terms of economic significance, the result in column (3) (column (7)) suggests that in the short (long) samples the *IVol* of treatment firms decreased by about 12% (8%) more than the decrease in *IVol* experienced by the control firms. The falsification tests (columns (2), (4), (6), (8)) indicate that prior to the Act there is no much difference between the two group of firms as the coefficient estimates of $LowQlty \times PreAct34_{-12m,-7m}$ and $LowQlty \times PreAct34_{-6m,-1m}$ are not significantly different from zero. After the enforcement, the coefficient estimates of $LowQlty \times PostAct34_{1m,6m}$, $LowQlty \times PostAct34_{7m,12m}$, $LowQlty \times PostAct34_{13m+}$ are significantly negative. In samples covering 1932.01–1936.12, the treatment group observed about 12% more decline in *IVol* than the control group. In the 1926.11–1941.11 and 1926.11–1963.12 samples, the immediate effect of the Act within one year of enforcement is not very statistically significant (coefficient estimates of $LowQlty \times PostAct34_{1m,6m}$ and $LowQlty \times PostAct34_{7m,12m}$ in columns (6) and (8)). But the long-term effect is statistically significant at the 5% level as can be seen from the coefficient estimate of $LowQlty \times PostAct34_{13m+}$. The long-term economic magnitude of differences in reduction of *IVol* between the two groups is about 7%.

Figure 1 displays the parallel trends of *IVol* before and after the enforcement of the Act, which is the graphical representation of the regression in column (2) of Table 4. The base time is the Act-in-progress period, March 1934 through June 1935. Consistent with the parallel trend assumption, there is no evidence that the *IVol* differed significantly for treatment firms relative to control firms before the Act taking effect. After the enforcement, the treatment effect becomes significantly negative, indicating that the less transparent firms experienced relatively more decrease in *IVol*, and it remains negative thereafter.

Two potential concerns may arise when interpreting the results in Table 4. First, the effect is only significant on the measure of *IVol* specified, which is influenced by the choice of the factor model. In Section 6 we apply same tests using different measures of volatility, including the ordinary

Figure 1: Test of Parallel Trends Assumption



This figure tests the parallel trends assumption of the difference-in-difference design by regressing idiocyncratic volatility on an indicator that the firm disclosed with low quality interacted with time indicators; controls; and fixed effects, i.e. the regression in column (4) of Table 4. The sample covers the short period, 1932.01–1936.12. The slope coefficients and 90% confidence intervals for the interaction terms are displayed. Standard errors are clustered by firm and year.

standard deviation of raw returns and the standard deviation of residuals of a Fama-French three factor model. The slope coefficients of interest remain approximately unchanged. Second, the treatment and control firms were possibly fundamentally different in financial, contractual, or other aspects and thus the difference-in-difference results reflect discrepancies along these dimensions. We examine this hypothesis by using a propensity score matching approach that matches one control firm to each treatment firm on propensity scores generated by the same quality determinant model of Barton and Waymire (2004). Table 5 presents the results. With the matching samples, the results are similar.

5.2 Changes in Liquidity and Variation of Idiosyncratic Volatility across Levels of Liquidity

As is documented in the literature (Diamond and Verrecchia, 1991; Welker, 1995; Heflin et al., 2005), enhanced disclosure of information is generally connected with improvement in liquidity because information asymmetry is mitigated. However, Daines and Jones (2012) find that such effect is not found between group of stocks that are classified into treatment and control groups based on whether they disclose sales, cost of goods sold, depreciation, or audit status.[24] We explore if such effect exists in our specifications. In addition, if an increase in liquidity coincides with a decrease in volatility, the reduced volatility can be seen as a sign of a more stable and efficient market, rather than an indication of reduced, inactive trading.

We use three measures of illiquidity: relative bid-ask spread (*BidAsk*), percentage of no trading days (*PctNoTradeDays*), and Amihud's metric that measures volume-return impact (*Amihud*). We then explore how changes in idiosyncratic volatility may relate to levels of liquidity before the Act.

[24] Note that Daines and Jones (2012) separately tested the effects of not disclosing these individual items and did not attempt to integrate them into an overall disclosure quality measure.

Table 5: Difference-in-difference for Propensity Score Matched Sample: Low Quality versus High Quality

	Dependent Variable: *log(IVol)*							
	1932.01–1936.12				1926.11–1941.11		1926.11–1963.12	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
LowQlty × *PostAct34*	−0.116**		−0.113**		−0.067**		−0.076**	
	(0.043)		(0.048)		(0.030)		(0.029)	
LowQlty × $PreAct34_{-12m,-7m}$		−0.046		−0.023		0.022		0.047
		(0.057)		(0.050)		(0.046)		(0.046)
LowQlty × $PreAct34_{-6m,-1m}$		−0.032		−0.022		0.036		0.060*
		(0.047)		(0.053)		(0.034)		(0.035)
LowQlty × *Act34InProgress*		−0.072		−0.052		−0.002		0.018
		(0.046)		(0.047)		(0.030)		(0.030)
LowQlty × $PostAct34_{1m,6m}$		−0.133**		−0.099*		−0.051		−0.021
		(0.053)		(0.058)		(0.037)		(0.036)
LowQlty × $PostAct34_{7m,12m}$		−0.128**		−0.105*		−0.051		−0.020
		(0.058)		(0.062)		(0.034)		(0.032)
LowQlty × $PostAct34_{13m+}$		−0.131**		−0.132**		−0.063*		−0.067**
		(0.053)		(0.060)		(0.034)		(0.032)
Lag *log(IVol)*	0.125***	0.144***	0.114***	0.131***	0.285***	0.284***	0.314***	0.318***
	(0.025)	(0.020)	(0.033)	(0.026)	(0.019)	(0.018)	(0.015)	(0.015)
Lag *Return*	−0.024	−0.046	−0.037	−0.057**	−0.077***	−0.087***	−0.105***	−0.117***
	(0.031)	(0.030)	(0.031)	(0.026)	(0.024)	(0.024)	(0.026)	(0.027)
Lag *log(Price)*	−0.049	−0.065	−0.015	−0.010	−0.006	−0.009	−0.012	−0.012
	(0.074)	(0.067)	(0.017)	(0.016)	(0.034)	(0.033)	(0.024)	(0.024)
Lag *log(MktCap)*	−0.303***	−0.287***	−0.249***	−0.246***	−0.271***	−0.271***	−0.197***	−0.199***
	(0.065)	(0.058)	(0.015)	(0.014)	(0.032)	(0.031)	(0.021)	(0.021)
Lag *Turnover*	0.048	0.037	−0.090	−0.123	0.030	0.034*	−0.004	−0.004
	(0.046)	(0.045)	(0.089)	(0.105)	(0.018)	(0.018)	(0.024)	(0.025)
Lag *IdioSkew*	−0.006	−0.009	−0.024**	−0.025***	−0.011	−0.012**	−0.004	−0.005
	(0.012)	(0.009)	(0.010)	(0.008)	(0.007)	(0.006)	(0.004)	(0.004)
Lag *BM*	−0.001***	−0.002***	0.000	0.000	−0.001*	−0.001*	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Leverage	0.026	0.025	0.038	0.034	−0.016	−0.007	0.102**	0.107**
	(0.121)	(0.098)	(0.091)	(0.082)	(0.063)	(0.058)	(0.051)	(0.050)
Age	−0.016	−0.011	0.001	0.001	0.004	0.004	0.000	0.001
	(0.018)	(0.015)	(0.001)	(0.001)	(0.011)	(0.010)	(0.008)	(0.008)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.807	0.806			0.774	0.774	0.758	0.760
AB test of AR(1)			0.000	0.000				
AB test of AR(2)			0.620	0.849				
Hansen's overidentification test			0.156	0.969				
Number of instruments			115	167				
Observations	3727	5127	3727	5127	11831	13231	23343	24743

This table presents the same difference-in-difference analyses as in Table 4 for propensity matched sample. The matching model is a logistic regression in which the dependent variable is *LowQlty*, and the independent variables are the same as those in the quality determinant model in Table 2.

5.2.1 Changes in Liquidity

Table 6 presents the coefficient estimates of the DID changes in liquidity. The interaction term *LowQlty*×*PostAct34* is statistically significant in different specifications of liquidity measures except in one scenario (1932.01–1936.12 with *Amihud* index). On average, the *BidAsk* spread declines by approximately 2% to 3.5% more in the *LowQlty* group following the Act. The reduction in the *PctNoTradeDays* is about 6% to 10% greater in the *LowQlty* group after the Act. The magnitude of decline in the *Amihud* measure is larger (about -0.3), which is not surprising given its high standard deviation.

The falsification tests of liquidity are basically the same as those in Table 4 of *IVol*. The general trend of decline starts to be statistically significant after the enforcement of the Act. When *PctNoTradeDays* is used, the *LowQlty* group experienced a decline 12 to 7 months before the Act's first hearing. This period partially overlaps with the time following the implementation of the 1933 Act. Since our sample only includes stocks that have been trading for at least one year before and after the Act, the observed effect may partly reflect the 1933 Act's contribution to improving overall market liquidity. This assumes that the establishment of IPO regulations had spillover effects on listed stocks, such as bolstering investor confidence or reducing social waste across the entire market (e.g. Easterbrook and Fischel, 1984). However, the magnitude of this decline (approximately 6%) is less than that observed in the periods following the 1934 Act's enforcement (approximately 9%). The overall results are not as significant when using the *Amihud* measure compared to when using *BidAsk* and *PctNoTradeDays*, as can be seen from the samples covering January 1932 through December 1936. A natural question is: Why are there such differences across different measures of liquidity?

One explanation is that in the short term, the impact of the Act on changes of liquidity primarily concentrated on increasing trading activities which was a result of reduced information asymmetry. The *PctNoTradeDays* directly measures the frequency of trading and the *BidAsk* measures the discrepancy between buyers' and sellers' beliefs regarding the fair trading price. This discrep-

ancy may shrink gradually over time when buyers believe that sellers possess less informational advantages (e.g. Glosten and Milgrom, 1985; Kyle, 1985), such as when sellers are less likely to be manipulators. On the other hand, the *Amihud* measure is less direct on gauging trading frequency, as its emphasis is on price impact of trading volume. When there is no trading (but not a lack of information), *Amihud* is not defined. Our findings are consistent with the empirical literature which largely supports the positive association between liquidity and more disclosure (e.g. Welker, 1995; Healy et al., 1999; Heflin et al., 2005; Balakrishnan et al., 2014). Moreover, the current DID design provides new evidence under the earliest, most fundamental mandatory disclosure system.

Table 6: Difference-in-difference of Liquidity: Low Quality versus High Quality

	Dependent Variable: *BidAsk*						Dependent Variable: *PctNoTradeDays*						Dependent Variable: *Amihud*					
	1932.01–1936.12		1926.11–1941.11		1926.11–1963.12		1932.01–1936.12		1926.11–1941.11		1926.11–1963.12		1932.01–1936.12		1926.11–1941.11		1926.11–1963.12	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
LowQlty × *PostAct34*	−0.035**		−0.025***		−0.021**		−0.069*		−0.099***		−0.063*		−0.258		−0.338**		−0.360**	
	(0.016)		(0.009)		(0.010)		(0.035)		(0.030)		(0.032)		(0.170)		(0.169)		(0.180)	
LowQlty × $PreAct34_{-12m,-7m}$		−0.027		−0.000		0.006		−0.059*		−0.061**		−0.053**		0.061		−0.002		0.014
		(0.020)		(0.014)		(0.014)		(0.034)		(0.025)		(0.024)		(0.093)		(0.124)		(0.128)
LowQlty × $PreAct34_{-6m,-1m}$		−0.033		−0.005		0.002		−0.048		−0.050		−0.038		−0.029		−0.122		−0.086
		(0.020)		(0.011)		(0.011)		(0.041)		(0.033)		(0.033)		(0.129)		(0.141)		(0.147)
LowQlty × *Act34InProgress*		−0.040**		−0.014		−0.008		−0.057		−0.054*		−0.043		−0.053		−0.106		−0.077
		(0.020)		(0.010)		(0.010)		(0.037)		(0.030)		(0.030)		(0.152)		(0.145)		(0.149)
LowQlty × $PostAct34_{1m,6m}$		−0.052**		−0.025**		−0.018*		−0.087*		−0.086**		−0.077**		−0.167		−0.217		−0.192
		(0.022)		(0.010)		(0.009)		(0.047)		(0.033)		(0.032)		(0.189)		(0.163)		(0.164)
LowQlty × $PostAct34_{7m,12m}$		−0.054**		−0.027**		−0.021**		−0.096**		−0.094***		−0.083***		−0.325		−0.378**		−0.344*
		(0.022)		(0.010)		(0.009)		(0.047)		(0.032)		(0.030)		(0.195)		(0.175)		(0.174)
LowQlty × $PostAct34_{13m+}$		−0.047**		−0.025***		−0.019*		−0.104**		−0.111***		−0.067*		−0.260		−0.364*		−0.365*
		(0.022)		(0.009)		(0.010)		(0.050)		(0.034)		(0.035)		(0.216)		(0.186)		(0.195)
Lag *log(Vol)*	0.015*	0.013**	0.016***	0.016***	0.018***	0.019***	0.002	0.000	−0.024*	−0.021*	−0.035***	−0.031***	0.104**	0.099**	0.189***	0.191***	0.190***	0.204***
	(0.008)	(0.006)	(0.004)	(0.004)	(0.003)	(0.003)	(0.012)	(0.011)	(0.013)	(0.012)	(0.012)	(0.011)	(0.050)	(0.042)	(0.068)	(0.065)	(0.041)	(0.041)
Lag *Return*	−0.036***	−0.033***	−0.023***	−0.024***	−0.020***	−0.021***	−0.072***	−0.074***	−0.045***	−0.048***	−0.027*	−0.029**	0.008	−0.041	−0.187**	−0.211**	−0.223**	−0.257***
	(0.008)	(0.007)	(0.005)	(0.005)	(0.005)	(0.005)	(0.018)	(0.017)	(0.016)	(0.015)	(0.014)	(0.013)	(0.086)	(0.091)	(0.084)	(0.084)	(0.093)	(0.094)
Lag *log(Price)*	0.017	0.007	0.002	0.001	−0.003	−0.004	−0.011	−0.017	0.041	0.038	0.050*	0.046	−0.076	−0.091	0.115	0.108	0.053	0.047
	(0.046)	(0.044)	(0.008)	(0.008)	(0.006)	(0.006)	(0.077)	(0.065)	(0.034)	(0.032)	(0.029)	(0.029)	(0.213)	(0.180)	(0.117)	(0.112)	(0.093)	(0.090)
Lag *log(MktCap)*	−0.058	−0.050	−0.036***	−0.035***	−0.016***	−0.016***	−0.094	−0.088	−0.133***	−0.131***	−0.130***	−0.130***	−1.510***	−1.487***	−1.449***	−1.456***	−1.311***	−1.309***
	(0.045)	(0.043)	(0.010)	(0.009)	(0.006)	(0.006)	(0.067)	(0.055)	(0.030)	(0.029)	(0.024)	(0.025)	(0.184)	(0.150)	(0.096)	(0.090)	(0.082)	(0.079)
Lag *Turnover*	−0.015	−0.015	−0.007	−0.007	−0.019**	−0.020**	−0.048	−0.045	−0.067**	−0.066*	−0.112**	−0.112**	−1.125***	−1.181***	−1.330***	−1.343***	−1.611***	−1.627***
	(0.010)	(0.011)	(0.006)	(0.006)	(0.008)	(0.009)	(0.036)	(0.039)	(0.032)	(0.034)	(0.046)	(0.049)	(0.375)	(0.387)	(0.282)	(0.281)	(0.380)	(0.381)
Lag *BM*	0.001***	0.001***	0.002***	0.002***	0.002***	0.002***	−0.000	−0.000	−0.000	−0.000	0.001	0.001	0.003*	0.003**	0.002	0.001	0.007**	0.007**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Leverage	−0.034	−0.024	0.015	0.017	0.026**	0.027**	−0.174*	−0.114	−0.141**	−0.129**	−0.115**	−0.112**	−1.224***	−0.989**	−0.562**	−0.471**	−0.314	−0.279
	(0.055)	(0.044)	(0.018)	(0.017)	(0.012)	(0.012)	(0.096)	(0.076)	(0.061)	(0.057)	(0.052)	(0.053)	(0.436)	(0.381)	(0.253)	(0.226)	(0.192)	(0.190)
Age	−0.003	−0.003	0.001	0.000	0.000	0.000	−0.004	0.003	0.001	0.003	−0.004	−0.002	−0.010	0.001	0.004	0.009	0.006	0.009
	(0.005)	(0.004)	(0.002)	(0.002)	(0.001)	(0.001)	(0.010)	(0.008)	(0.005)	(0.005)	(0.004)	(0.004)	(0.039)	(0.033)	(0.021)	(0.020)	(0.013)	(0.013)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.611	0.614	0.602	0.603	0.558	0.560	0.737	0.751	0.682	0.689	0.657	0.658	0.939	0.939	0.921	0.922	0.902	0.903
Observations	3836	5281	12369	13814	24506	25951	3879	5333	12460	13914	24626	26080	3832	5277	12363	13808	24497	25942

This table reports results of the difference-in-difference analyses for illiquidity measures: Effective bid-ask spread (*BidAsk*, columns (1) through (6)), percentage of no trade days (*PctNoTradeDays*, columns (7) through (12)), Amihud (2002) measure (*Amihud*, columns (13) through (18)). Standard errors in parentheses are clustered by firm and year. *, **, *** indicate significance levels at 10%, 5% and 1% levels.

5.2.2 Variation of Idiosyncratic Volatility across Levels of Liquidity and Disclosure Quality

We further examine this explanation by checking cross-sectional variations of *IVol* in different levels of liquidity groups in Table 7. We classify a firm as less liquid (or highly illiquid, $HighIlliq = 1$) if its average illiquidity measure before the Act exceeds the median, and as more liquid if it falls below the median ($HighIlliq = 0$). We observe from Table 7 that if the illiquidity measure is *BidAsk* or *PctNoTradeDays* and if the sample period is January 1932 through December 1936, the triple interaction term $LowQlty \times PostAct34 \times HighIlliq$ is significantly negative no matter how the regression model is specified (columns (1), (2), (5), (6)). Economically, the joint group of $LowQlty \times HighIlliq$ experienced about 16% to 20% more decline in *IVol* than other groups after the enforcement of the Act. This number is much higher than the coefficient estimate of $LowQlty \times PostAct34$ in Table 4, which is around 11%. Meanwhile, this effect is invisible if the sample covers November 1926 through November 1941 or November 1926 through December 1963, or if the measure of illiquidity is *Amihud*. In addition, coefficient estimate of $LowQlty \times PostAct34$ becomes insignificant in Table 7. The observation is that the majority of the post-Act reduction in *IVol* was concentrated within the $LowQlty \times HighBidAsk$ or $LowQlty \times HighPctNoTradeDays$ group. When this group is controlled, the overall effect of the decline in *log(IVol)* is absorbed. These pieces of evidence further confirm that the change in *IVol* in the treatment group is associated with a short-term increase in liquidity levels.

Overall, we find evidence that liquidity levels, especially indicators reflecting information asymmetry and trading frequency, significantly improved for the *LowQlty* group of stocks following the Act's enforcement. Additionally, most of the decline in *IVol* is concentrated in stocks with these improved indicators. This suggests that the change in *IVol* is driven by an improvement in information asymmetry, which is closely related to probability of stock manipulation and quality of financial reporting.

Table 7: Variation of Idiosyncratic Volatility across Different Levels of Liquidity and Disclosure Quality

	Dependent variable: *log(IVol)*											
	Illiquidity measure: *PctNoTradeDays*				Illiquidity measure: *BidAsk*				Illiquidity measure: *Amihud*			
	1932.01–1936.12		1926.11–1941.11	1926.11–1963.12	1932.01–1936.12		1926.11–1941.11	1926.11–1963.12	1932.01–1936.12		1926.11–1941.11	1926.11–1963.12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
LowQlty × *PostAct34* × *HighIlliq*	−0.156**	−0.183**	−0.047	−0.038	−0.160**	−0.203**	−0.068	−0.050	−0.061	−0.101	0.011	0.020
	(0.075)	(0.089)	(0.052)	(0.046)	(0.076)	(0.094)	(0.053)	(0.046)	(0.078)	(0.094)	(0.050)	(0.043)
LowQlty × *PostAct34*	−0.020	0.004	−0.038	−0.043	−0.015	0.016	−0.026	−0.027	−0.062	−0.035	−0.057*	−0.059**
	(0.049)	(0.058)	(0.038)	(0.031)	(0.050)	(0.050)	(0.035)	(0.029)	(0.055)	(0.056)	(0.032)	(0.028)
LowQlty × *HighIlliq*	0.000	0.016	0.000	0.000	0.000	0.073	0.000	0.000	0.000	−0.014	0.000	0.000
	(0.000)	(0.083)	(0.000)	(0.000)	(0.000)	(0.080)	(0.000)	(0.000)	(0.000)	(0.084)	(0.000)	(0.000)
PostAct34 × *HighIlliq*	−0.032	−0.060	−0.042	−0.105***	−0.019	0.014	−0.022	−0.102**	−0.097*	−0.058	−0.109**	−0.157***
	(0.052)	(0.064)	(0.043)	(0.039)	(0.052)	(0.071)	(0.045)	(0.039)	(0.053)	(0.067)	(0.043)	(0.034)
Lag *log(IVol)*	0.119***	0.109***	0.282***	0.306***	0.120***	0.112***	0.283***	0.306***	0.119***	0.107***	0.279***	0.304***
	(0.024)	(0.032)	(0.019)	(0.014)	(0.024)	(0.031)	(0.019)	(0.014)	(0.024)	(0.031)	(0.019)	(0.014)
Lag *Return*	−0.028	−0.055**	−0.078***	−0.105***	−0.030	−0.065**	−0.079***	−0.106***	−0.033	−0.062**	−0.078***	−0.107***
	(0.031)	(0.028)	(0.023)	(0.024)	(0.031)	(0.028)	(0.023)	(0.024)	(0.031)	(0.028)	(0.023)	(0.024)
Lag *log(Price)*	−0.025	−0.009	−0.014	−0.023	−0.059	−0.012	−0.017	−0.027	−0.056	−0.016	−0.020	−0.028
	(0.076)	(0.017)	(0.033)	(0.024)	(0.069)	(0.015)	(0.034)	(0.024)	(0.070)	(0.015)	(0.034)	(0.024)
Lag *log(MktCap)*	−0.315***	−0.230***	−0.265***	−0.189***	−0.283***	−0.207***	−0.260***	−0.186***	−0.284***	−0.210***	−0.258***	−0.184***
	(0.070)	(0.018)	(0.031)	(0.022)	(0.062)	(0.016)	(0.032)	(0.022)	(0.062)	(0.016)	(0.032)	(0.022)
Lag *Turnover*	0.052	−0.058	0.041**	0.020	0.051	−0.053	0.039**	0.019	0.059	−0.047	0.048***	0.023
	(0.042)	(0.075)	(0.017)	(0.019)	(0.043)	(0.073)	(0.017)	(0.019)	(0.042)	(0.077)	(0.017)	(0.018)
Lag *IdioSkew*	−0.007	−0.024**	−0.011*	−0.004	−0.007	−0.023**	−0.011*	−0.003	−0.005	−0.024**	−0.011*	−0.003
	(0.012)	(0.010)	(0.006)	(0.004)	(0.011)	(0.010)	(0.006)	(0.004)	(0.012)	(0.010)	(0.006)	(0.004)
Lag *BM*	−0.001**	0.000	−0.001	0.000	−0.002***	0.000	−0.001*	0.000	−0.002***	0.000	−0.001*	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Leverage	0.106	0.102	−0.001	0.099**	0.098	0.091	0.003	0.103**	0.082	0.041	0.004	0.094*
	(0.114)	(0.087)	(0.059)	(0.049)	(0.113)	(0.084)	(0.060)	(0.049)	(0.111)	(0.086)	(0.058)	(0.049)
Age	−0.021	0.001	0.000	−0.005	−0.021	0.001	0.000	−0.005	−0.023	0.001	0.000	−0.005
	(0.017)	(0.001)	(0.011)	(0.008)	(0.017)	(0.001)	(0.011)	(0.008)	(0.017)	(0.001)	(0.011)	(0.008)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.807		0.773	0.760	0.807		0.773	0.761	0.807		0.774	0.761
AB test of AR(1)		0.000				0.000				0.000		
AB test of AR(2)		0.667				0.654				0.691		
Hansen's overidentification test		0.589				0.587				0.631		
Number of instruments		157				157				157		
Observations	3879	3879	12287	24453	3879	3879	12287	24453	3879	3879	12287	24453

This table reports results of the triple difference analyses of idiosyncratic volatility when firms are grouped by their average liquidity levels prior to the Act and their disclosure quality levels. When the sample is restricted in 1932.01–1936.12, Arellano and Bond (1991) type dynamic panel regression is conducted (columns (2), (6), (10)). Further lags of log(*IVol*) are used as the instrument variables and the lags are restricted at 3. For medium- and long-term regressions we only implement regular fixed effects models because the dynamic panel bias is less of a concern but the problem of weak instruments arises. Standard errors in parentheses are clustered by firm and year. *, **, *** indicate significance levels at 10%, 5% and 1% levels.

6 Robustness

6.0.1 Other Model Specifications

We conduct several tests to examine the robustness of our findings. First, we use the standard deviation of monthly returns as a measure of volatility. This metric does not account for the influence of common risk factors, but the advantage is that it is not affected by any specific factor model. Table 8 presents the results. The overall results are similar to those in Table 4 and Table 5, with the most statistically significant effects observed in the short-term samples from January 1932 to December 1936. Interestingly, the overall magnitude of the change in *Vol* is less than that observed when using *IVol*, as the number is around -0.09 (columns (1) and (3)) now but in Table 4 and Table 5 the number is around -0.11. This suggests that differences in voluntary disclosure quality are more apparent in *IVol*. Secondly, in untabulated analyses, we use Fama-French three factor model to construct the *IVol* series and find all results qualitatively similar. Other factor models are not available because of data constraint. For example, the Fama-French five factor returns start from July 1963, which is near the end of our sample period.

Thirdly, we apply the difference-in-difference method to NYSE and OTC stocks. OTC stocks, which were largely exempt from disclosure requirements until the 1964 Amendment, naturally form a control group. However, the quality of OTC data imposes several limitations. From *Global Financial Data*, we have access only to split- and inflation-adjusted price and return data for 110 non-financial stocks at a monthly frequency. There is no information on other trading activities or firm characteristics, such as trading volume, firm size, or book equity value. The econometric analysis is therefore susceptible to issues arising from omitted variables. Despite these shortcomings, the difference-in-difference results suggest that during the post-Act period, NYSE stocks experienced a decline in *IVol* approximately 18% to 38% greater than that of OTC stocks, depending on the specified sample period. The detailed results are not included here but are available upon request.

Table 8: DID of raw volatility: Low Quality versus High Quality

	Dependent Variable: *log(Vol)*							
	1932.01–1936.12				1926.11–1941.11		1926.11–1963.12	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
LowQlty × *PostAct34*	−0.095***		−0.091**		−0.049*		−0.066**	
	(0.034)		(0.042)		(0.028)		(0.026)	
LowQlty × $PreAct34_{-12m,-7m}$		−0.099**		−0.094**		−0.006		0.013
		(0.046)		(0.046)		(0.037)		(0.037)
LowQlty × $PreAct34_{-6m,-1m}$		−0.079*		−0.072		0.021		0.039
		(0.046)		(0.047)		(0.037)		(0.038)
LowQlty × *Act34InProgress*		−0.112***		−0.104**		−0.015		−0.001
		(0.040)		(0.044)		(0.028)		(0.028)
LowQlty × $PostAct34_{1m,6m}$		−0.135***		−0.100*		−0.033		−0.011
		(0.043)		(0.055)		(0.033)		(0.033)
LowQlty × $PostAct34_{7m,12m}$		−0.138***		−0.125**		−0.039		−0.015
		(0.044)		(0.053)		(0.030)		(0.029)
LowQlty × $PostAct34_{13m+}$		−0.133***		−0.135***		−0.051		−0.063**
		(0.044)		(0.052)		(0.033)		(0.030)
Lag *log(Vol)*	0.134***	0.153***	0.124***	0.157***	0.306***	0.303***	0.340***	0.341***
	(0.027)	(0.021)	(0.032)	(0.025)	(0.021)	(0.020)	(0.017)	(0.017)
Lag *Return*	−0.029	−0.043	−0.041	−0.074***	−0.091***	−0.094***	−0.118***	−0.124***
	(0.033)	(0.030)	(0.031)	(0.025)	(0.025)	(0.024)	(0.026)	(0.026)
Lag *log(Price)*	−0.019	−0.049	−0.008	−0.007	−0.011	−0.016	−0.017	−0.018
	(0.064)	(0.061)	(0.017)	(0.016)	(0.034)	(0.034)	(0.024)	(0.024)
Lag *log(MktCap)*	−0.259***	−0.236***	−0.182***	−0.182***	−0.213***	−0.213***	−0.151***	−0.152***
	(0.058)	(0.056)	(0.014)	(0.013)	(0.031)	(0.030)	(0.020)	(0.020)
Lag *Turnover*	0.138***	0.126***	0.073**	0.057	0.097***	0.099***	0.081***	0.081***
	(0.036)	(0.026)	(0.029)	(0.035)	(0.019)	(0.019)	(0.018)	(0.018)
Lag *IdioSkew*	−0.005	−0.010	−0.021**	−0.025***	−0.008	−0.010*	−0.002	−0.003
	(0.010)	(0.008)	(0.009)	(0.007)	(0.006)	(0.005)	(0.004)	(0.004)
Lag *BM*	−0.001**	−0.001***	0.001	0.001	−0.001*	−0.001**	0.000	0.000
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Leverage	0.163	0.120	0.110	0.090	0.059	0.062	0.123***	0.124***
	(0.097)	(0.080)	(0.086)	(0.077)	(0.048)	(0.045)	(0.046)	(0.045)
Age	−0.023	−0.020	0.000	0.000	−0.006	−0.006	−0.006	−0.006
	(0.017)	(0.014)	(0.001)	(0.001)	(0.010)	(0.009)	(0.008)	(0.008)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.803	0.799			0.775	0.773	0.776	0.777
AB test of AR(1)			0.000	0.000				
AB test of AR(2)			0.631	0.874				
Hansen's overidentification test			0.229	1.000				
Number of instruments			144	207				
Observations	3879	5333	3879	5333	12287	13741	24453	25907

This table reports the difference-in-difference analyses using the raw volatility measure. All the setups are the same as those in Table 4.

6.0.2 Test of Structural Breaks

Finally, we apply a "reverse engineering" technique to find the statistical answer to this question: "What events have the greatest impact on volatility?" In other words, without knowing the timing of the Act, can we identify significant mechanism transition points purely from the characteristics of the data itself? If the answer derived from data mining roughly coincides with the timing of the enforcement of the Act 1934 (within the confidence interval), this can be seen as evidence supporting the hypothesis that the Act is effective and is one of the most influential events. Of course, since 1933 and 1934 were a period of major financial system reforms (e.g., the Bank Holiday of 1933 and the dollar devaluation relative to gold), this part of the analysis cannot clearly isolate the impact of these changes on volatility.

To this end, we employ the structural break estimator developed by Bai and Perron (1998) (henceforth BP) to test for multiple structural breaks in the mean levels of stock volatility. The BP algorithm sets an upper bound on the number of breaks and find the breakpoints that best fit the data progressively. A structural change model with m breaks ($m+1$ regimes) can be written as

$$log(Vol_t) = \mu_j + \boldsymbol{\gamma}' Controls_t + log(Vol_{t-1}) + u_t, \quad t = T_{j-1}+1, \ldots, T_j \quad \text{for} \quad j = 1, \ldots, m+1, \tag{4}$$

where $(T_1, \ldots, T_m)$ represents the breakpoints for the different regimes, Vol_t is the volatility of market returns in month t and μ_j is the mean level of stock volatility in regime j after controlling other covariates. $Controls_t$ is a vector of the macro-control variables including lag of $\log(Vol)$, the growth of volatility of money base, growth of PPI inflation, growth of industrial production.[25] The confounding effect of the Great Depression is a crucial issue that must be addressed when the sample period includes both the Great Depression and periods outside of it. Due to the algorithm

[25] Schwert (1989) argues that in a simple discounted present value model of stock prices, if macroeconomic data provide information about the volatility of future cash flows or future discount rate, they might explain some of the variations in stock market volatility. He finds that these macroeconomic variables can partly explain the overall stock market volatility in a sample covering 1857 through 1987.

for estimating the breakpoints, time dummies cannot be directly added to equation (4).[26] To address this issue, we first regress *log(Vol)* on a constant and on the Great Depression dummy (= 1 if $t \in$ [1929.10, 1939.12] and = 0 otherwise), and use the residual as the dependent variable in estimating equation (4). If we do not consider the Great Depression effect, the identified breakpoints would be close to the start and end dates of the Great Depression.

The estimation of the number of breaks and their corresponding dates follows the sequential procedure outlined by Bai and Perron (1998) and implemented in Bai and Perron (2003a).[27] First, the algorithm locates the initial break and test its significance against the null hypothesis of no break with a Double Maximum statistic, UDmax.[28] If the null hypothesis is rejected, it then find and test for the existence of the second break against the null hypothesis of only one single break with the supF(2|1) statistics. The process continues reporting supF($l+1|l$) until it reaches the specified maximum number of breaks, which is set to 5. The trimming parameter, representing the fraction of initial samples where break dates cannot occur to ensure feasible estimation, is set at 0.15, as suggested by Bai and Perron (2003b). The number of breaks is determined at the 5% significance level, and we construct 90% confidence intervals. Table 9 reports the results. Since data for market return and other control variables extend back to 1891, we adjust the medium- and long-term sample periods accordingly. The statistically identified break dates align closely to the enactment dates of the Acts at May 1933 and June 1934.[29] For the short sample period spanning from January 1932 to December 1936, the estimated breakpoints are located at October 1933 and August 1934. The confidence intervals for these estimates are [1933.11, 1934.10], and [1934.06,

[26]The algorithm essentially estimates multiple regressions with different selections of breakpoints. In this process, a set of dummy variables is added to estimate different μ_j. If the control variables include any time dummies, at some point the estimation cannot proceed because of collinearity.

[27]We implement the procedure using the Matlab program available from Pierre Perron's homepage, http://people.bu.edu/perron/

[28]The statistic is derived based on the supF statistic of no structural break versus a set of fixed numbers of break dates. We also implemented a WDmax test, which applies different weights to the individual supF statistics so that the marginal p-values are equal across the values of the number of breaks. The results are similar. Please refer to Bai and Perron (1998) for the estimation details.

[29]Without considering the Great Depression effect, the estimated break dates are October 1928 and August 1934 (1926.01–1941.11), and October 1928 and November 1948 (1891.01–1963.12).

Table 9: Tests of Multiple Structural Breaks of Unknown Dates

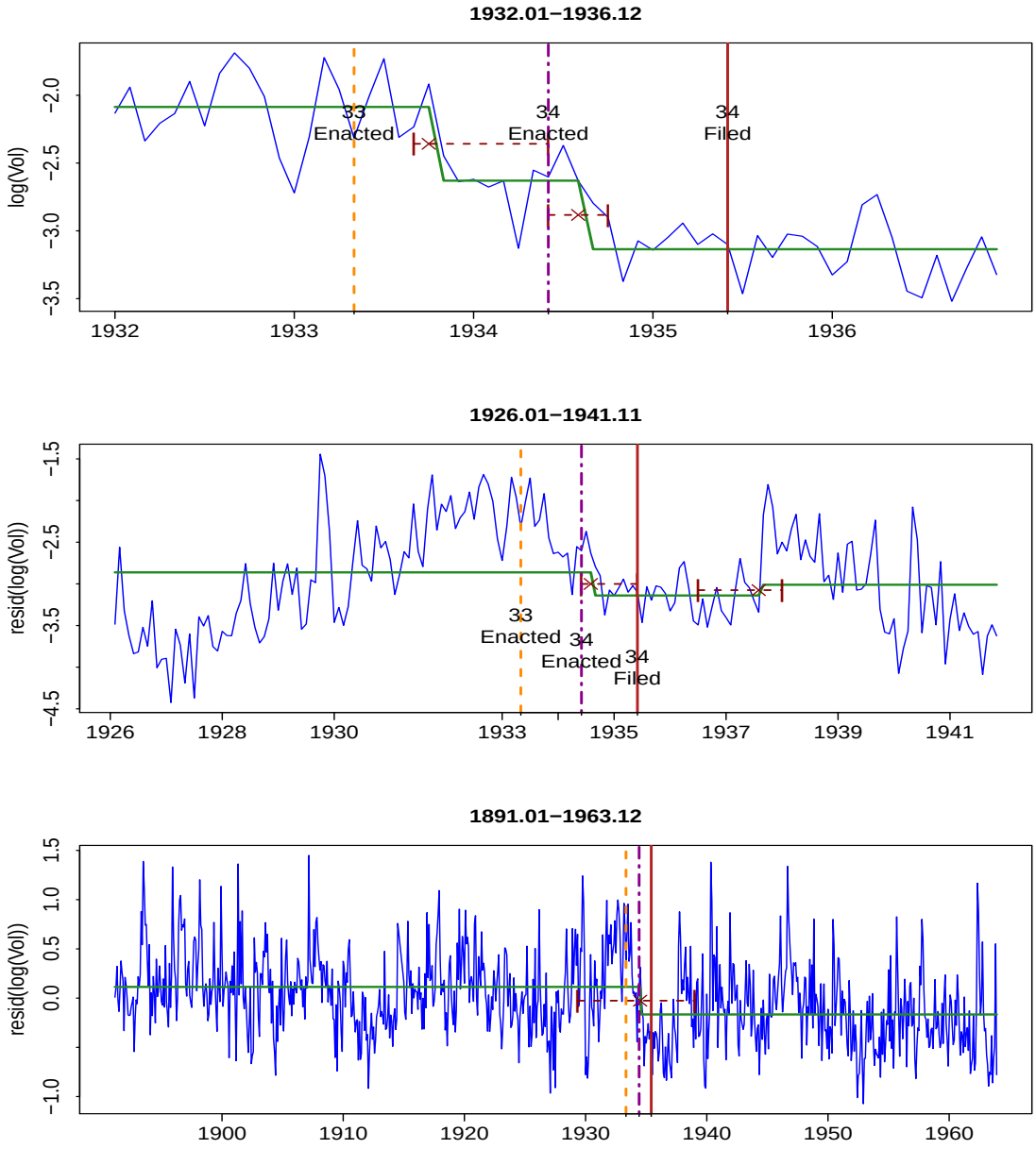
	1932.01–1936.12	1926.01–1941.11	1891.01–1963.12
UDmax	20.83***	16.18***	15.99***
supF(2 | 1)	18.75***	10.30**	4.88
supF(3 | 2)	4.13	3.34	3.15
supF(4 | 3)	4.13	1.20	4.88
supF(5 | 4)	0.19	0.00	0.00
Number of breaks selected	2	2	1
Regime 1 end date	1933.10	1934.08	1934.07
90% Conf. Int.	[1933.09, 1934.06]	[1934.06, 1935.06]	[1929.05, 1939.01]
Regime 2 end date	1934.08	1937.08	
90% Conf. Int.	[1934.06, 1934.10]	[1936.07, 1938.01]	

This table reports Bai and Perron statistics for tests of multiple structural breaks at unknown dates in the mean level of the volatility series. The number of breaks, the dates for each of the structural breaks, and their 90% confidence intervals (Conf. Int.) are selected by the sequential method described in Bai and Perron (1998). The column titles indicate the time span of the tests. The dependent variable for 1932.01–1936.12 is the logarithm of stock volatility. The dependent variable for 1926.01–1941.11 and for 1891.01–1963.12 are the residual series from regressing log volatility on a constant and the Great Depression dummy (1929.10–1939.12). Control variables include the lagged dependent variable, the growth rate of trading volume, the logarithms of the volatilities of PPI inflation growth, of money base growth (except 1891.01–1963.12 because the data is not available until May 1908), and of industrial production growth. The trimming parameter is set at 0.15. *, **, and *** respectively indicate significance at 10%, 5%, and 1%, when the largest number of breaks allowed is 5.

1934.10], respectively. Two breakpoints divide the time series of stock market volatility into three regimes: mean volatility fell substantially from 0.128 in regime 1 (1932.01–1933.10) to 0.073 in regime 2 (1933.11–1934.08), and then fell further to 0.044 in regime 3 (1934.09–1936.12). For samples covering January 1926 through November 1941, the estimated break dates are at August 1934 and August 1937, with 90 percent confidence intervals at [1934.06, 1935.06] and [1936.07, 1938.01]. The mean level of volatility fell from 0.072 in regime 1 to 0.044 in regime 2, and rised to 0.058 in regime 3. For the longest sample (1891.01–1963.12), the break date is estimated at July 1934, with a 90 percent confidence interval [1929.05, 1939.01]. The mean levels of volatility in the two regimes are 0.044 and 0.035, respectively. Figure 2 visualizes the information above.

The results here indicate that, from the perspective of the entire market, the establishment of the 1933 and 1934 Acts had a foundational impact on volatility, as this "guesswork" style of data mining in various samples consistently points to the time around the establishment of the 1933 and 1934 Acts.

Figure 2: Volatility and Structural Breaks



The top panel plots the time series of *log(Vol)*, while the bottom two panels display the residuals obtained from regressing *log(Vol)* on a constant and the Great Depression dummy. The green horizontal line represents the mean value across different regimes. The "|- - x - -|" symbols specify the break dates and their corresponding 90% confidence intervals. The three vertical lines denote the key dates of the Acts: the month of enactment for the 1933 Act (33 Enacted), the month of enactment for the 1934 Act (34 Enacted), and the deadline for the first filing with the SEC (34 Filed).

7 Conclusion

Our research question centers on whether the Act 1934, by enforcing transparency, has effectively contributed to a stabilization of market fluctuations. The findings of our analysis indicate a significant reduction in stock volatility, especially among companies with previously poor disclosure practices. This outcome aligns with the legislative intent to curb market manipulation and enhance the integrity of financial markets.

Our theoretical framework suggests that enhanced disclosure can mitigate the risk of market manipulation and reduce excess volatility. Empirical evidence supports this theory, showing that the volatility of stocks with lower initial disclosure quality experienced a more pronounced decrease post-Act.

Moreover, our examination of liquidity indicators reveals a marked improvement in trading activity and a reduction in information asymmetry for the affected companies. This suggests that the Act has fostered a more efficient market by enhancing the quality of information available to investors, thereby reducing the cost of capital and encouraging greater investor participation. Additionally, the liquidity effect interacts with the volatility effect, particularly over short time periods and is concentrated on measures related to information asymmetry and trading frequency. This means that the reduction in volatility does not come at the expense of reduced trading but rather indicates a more stable market.

The robustness of our results is further validated through various tests, including a difference-in-difference analysis with propensity score matching, alternative volatility measures, a comparison of NYSE and OTC stocks, and a structural break test. These additional analyses reinforce the conclusion that the 1934 Act has had a profound and lasting impact on market dynamics.

It is also important to acknowledge the limitations of this study. The inability to directly measure the reduction in market manipulation and the reliance on indirect inferences from volatility indicators are among the constraints. Furthermore, the generalizability of our findings may be limited by the specific historical context of the 1934 Act.

In conclusion, our research shows that the U.S. Securities Exchange Act of 1934 effectively reduces market volatility through its mandatory disclosure requirements. This study adds to the conversation about how financial regulations influence market behavior and provides meaningful message for policymakers and regulators working to ensure market stability and protect investors.

A Appendix: Variable Definitions and Disclosure Examples before the Act 1934

A.1 Variable Definitions

Table A.0: Variable Definitions

Variable	Source	Definition
IVol	CRSP	Monthly standard deviation of residuals of Fama-French 4 factor model multiplied by the square root of a scaler. The factors are Market Excess Return, SMB, HML, and Momentum. The scaler equals the average number of trading days within a month, which is 25 before September 1952, and 21 afterwards.
Vol	CRSP	Monthly standard deviation of stock returns multiplied by the square root of a scaler. The scaler equals the average number of trading days within a month, which is 25 before September 1952, and 21 afterwards.
Quality	Moody's	A score is obtained by first taking the sum of ISTRANSP, BSTRANSP, AUDITOR, CONSERV. The score is then standardized within each industry by substracting the median and divided by the standard deviation.
Age	Moody's	Number of years since the firm's incorporation date.
Tech	Moody's	An indicator that the firm is in the technology industry (three-digit SIC codes: 351–357, 363, 366, 369, 371, 372, 381, 383, 384, 387, 491, 493, 481, 482, 489, 781, 783, 791).
CVEarn	Moody's	The coefficient of variation in net income over the previous five years.
Beta	CRSP	The slope coefficient obtained from regressing the firm's excess return on the market risk premium with monthly data before December 1933.
ROE	Moody's	Net income divided by shareholders' equity.
Issue	CRSP	An indicator if shares outstanding of the firm increased by more than 5% between January 1931 and December 1933.
Leverage	Moody's	Leverage used in the quality determinant model of Barton and Waymire (2004): total debt divided by common shareholders' equity. Leverage used in the DID analysis is based on Brandt et al. (2010): total debt divided by total assets.

Variable	Source	Definition
IncConf	Moody's	An indicator that the firm has income bonds, noncumulative preferred stock or another type of stock with participation rights.
ContConf	Moody's	An indicator that the firm is controlled by a voting trust or another company, 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.
Delaware	Moody's	An indicator that the firm is chartered in Delaware.
MktShr	Moody's	Ahe firm's total assets divided by the sum of total assets of all sample firms in the same two-digit SIC code industry.
Size	Moody's	The logrithm of firm's total assets.
Dividend	Moody's	An indicator that the firm paid dividends.
Regulated	Moody's	An indicator that the firm is in a regulated industry (three-digit SIC codes: 481, 482, 489, 460, 419, 422, 440, 450, 474, 471, 491, 493, 492, 499).
BidAsk	CRSP	Monthly average of the daily close relative bid-ask spread. $(Ask - Bid)/((Ask + Bid)/2)$.
PctNoTradeDays	CRSP	Percentage of no trade days in a month.
Amihud	CRSP	The Amihud (2002) illiquidity measure in a month. $Amihud_t = \frac{1}{D}\sum_{t=1}^{D} \frac{\lvert \text{ret}_t \rvert}{\text{volume}_t}$, where D is the number of days in a month.
Price	CRSP	Month end close price of the stock.
Return	CRSP	Monthly stock return.
MktCap	CRSP	Month end close price multiplied by share outstanding.
Turnover	CRSP	Monthly trading volume divided by share outstanding.
IdioSkew	CRSP	Monthly skewness of residuals of regressing stock return on market excess return and square of market excess return.
BM	Moody's & CRSP	Book value of equity divided by market capitalization.
LowQlty	Moody's	Indicator that equals to 1 if the firm's Quality score is in the lowest 10%, or 0 if the score is in the highest 90%.

Variable	Source	Definition
PostAct34		Indicator that the date is July 1935 or later.
$PreAct34_{-12m,-7m}$		Indicator that the date is between March 1933 and August 1933 (inclusive, same below).
$PreAct34_{-6m,-1m}$		Indicator that the date is between September 1933 and Feburary 1934.
Act34InProgress		Indicator that the date is between March 1934 and June 1935.
$PostAct34_{1m,6m}$		Indicator that the date is between July 1935 and December 1935.
$PostAct34_{7m,12m}$		Indicator that the date is between January 1936 and June 1936.
$PostAct34_{13m+}$		Indicator that the date is July 1936 or later.

A.2 Examples of Voluntary Disclosure in Moody's Manual 1934

Below are examples of financial statements and related information from Moody's Manual 1934 for two firms in our sample: The American Can Company, which we classify as having lower quality financial reporting, and The Procter & Gamble Company, which we classify as having higher quality financial reporting.

Figure A.1: Income and Balance Sheet Statements of The American Can Company (Low Quality) in Moody's Manual 1934

The transparency scores: ISTRANSP = 2, BSTRANSP = 3, AUDITOR = 0, CONSERV = 0

Comparative Income Account, Years Ended Dec. 31

	†1933	1932	1931	1930	1929	1928
Net earnings from all sources..	*$20,157,048	$14,657,295	$19,729,579	$27,883,941	$27,599,803	$24,863,326
Depreciation	2,000,000	2,000,000	2,000,000	2,000,000	2,000,000	2,000,000
Balance	18,157,048	12,657,295	17,729,579	25,883,941	25,599,803	22,863,326
Reserve for Federal taxes	2,800,000	1,700,000	2,200,000	3,000,000	2,875,000	3,000,000
Net income	15,357,048	10,957,295	15,529,579	22,883,941	22,724,803	19,863,326
Preferred dividends	2,886,331	2,886,331	2,886,331	2,886,331	2,886,331	2,886,331
Common dividends	9,895,992	9,895,992	12,369,990	12,369,990	10,514,492	8,040,493
Surplus	$2,574,725	(d)$1,825,028	$273,258	$7,627,620	$9,323,980	$8,936,502

* Includes $575,587 dividends and interest received. † Consolidated statement.

NOTE: 1933 sales were approximately 14% greater than in 1932 and 1932 sales were about 17% less than in 1931.

Comparative Condensed Balance Sheet, as of Dec. 31

ASSETS:	‡‡1933	1932	1931	1930	1929	1928
Plants, real estate, patents	*$139,491,817	$140,729,949	$142,202,041	$136,843,848	$127,274,907	$121,090,042
Miscellaneous investments	†297,737	4,527,974	4,032,224	4,405,656	4,139,271	850,871
Employees' annuity fund	‡3,124,947	2,841,422	2,441,022	2,090,547	1,712,427	1,326,589
Time loans	.....	.....	.....	.....	*5,000,000*	*5,000,000*
Cash	*8,592,553*	*13,690,322*	*6,309,522*	*16,286,885*	*16,973,215*	*22,017,491*
Accounts and bills receivable	§17,955,718	*16,332,733*	*18,655,571*	*18,130,442*	*13,308,817*	*10,601,174*
Inventory	**31,332,392	*14,568,788*	*21,768,606*	*20,664,982*	*22,926,977*	*22,376,257*
Deferred charges	2,209,386	.....	.....	.....	.....	.....
Total	$203,004,551	$192,691,188	$195,408,986	$198,422,360	$191,335,614	$183,262,424
LIABILITIES:						
Preferred stock	$41,233,300	$41,233,300	$41,233,300	$41,233,300	$41,233,300	$41,233,300
Common stock	61,849,950	61,849,950	61,849,950	61,849,950	61,849,950	61,849,950
Accounts and bills payable	*10,091,943*	*4,715,239*	*4,764,785*	*7,187,797*	*8,506,040*	*8,090,930*
Preferred dividend payable	*721,583*	*721,583*	*721,583*	*721,583*	*721,583*	*721,583*
Common dividend payable	*2,473,998*	*2,473,998*	*2,473,998*	*2,473,998*	*2,473,998*	*4,329,496*
Reserve for Federal taxes	*2,800,000*	*1,700,000*	*2,200,000*	*3,000,000*	*2,875,000*	*3,000,000*
Deferred credits	299,528	.....	.....	.....	.....	.....
Employees' annuity fund res	3,134,797	2,880,106	2,540,813	2,140,126	1,746,340	1,374,920
Conting. insurance reserve	5,000,000	5,000,000	5,000,000	4,974,311	4,786,043	4,534,749
Conting. Federal tax reserve	412,055	251,094	209,626	508,516	438,199	746,316
Conting. inventory reserve	3,254,040	3,254,040	3,431,271	3,622,379	3,622,379	3,622,379
Miscellaneous conting. reserve	970,930	424,176	970,930	970,930	970,930	970,930
Surplus	††70,762,427	68,187,702	70,012,730	69,739,470	62,111,851	52,787,871
Total	$203,004,551	$192,691,188	$195,408,986	$198,422,360	$191,335,614	$183,262,424
Current assets	*$57,880,663*	*$44,591,843*	*$46,733,699*	*$55,082,309*	*$58,209,009*	*$59,994,922*
Current liabilities	*16,087,524*	*9,610,820*	*10,160,366*	*13,383,378*	*14,576,621*	*16,142,009*
Working Capital	*$41,793,139*	*$34,981,023*	*$36,573,333*	*$41,698,931*	*$43,632,388*	*$43,852,913*

* Less depreciation.
† At less than market.
‡ Investments for employees annuity fund, at cost.
§ Less reserves and write-offs for bad debts.
** Normal stock of tin plate at pre-war prices; other items at lower of cost or market.
†† Earned surplus.
‡‡ Consolidated statement.

Figure A.2: Income and Balance Sheet Statements of The Procter & Gamble Company (High Quality) in Moody's Manual 1934

The transparency scores: ISTRANSP = 5, BSTRANSP = 5, AUDITOR = 2, CONSERV = 1

Comparative Consolidated Income Account, Years Ended June 30

	1933	1932	1931	1930	1929	1928
‡Gross sales	$102,463,645	$142,421,660	$190,523,237	$203,365,610	$202,213,831	$179,622,844
Net sales	94,211,927	128,391,775	176,157,043	192,352,590	193,296,720	172,425,270
Cost of sales, etc.	57,475,877	79,982,848	115,895,414	133,868,305	137,501,610	124,994,477
Operating expenses	22,071,378	26,995,750	32,293,603	31,932,678	31,794,545	27,833,661
Depreciation	2,970,609	2,641,091	2,557,650	2,515,450	2,371,813	1,982,152
Net earnings	11,694,063	18,772,086	25,410,375	24,036,157	21,628,752	17,614,980
Margin of profit	*11.41%*	*13.18%*	*13.34%*	*12.50%*	*11.19%*	*10.22%*
Other income	1,050,537	1,113,698	975,796	1,485,533	530,740	855,370
Total income	12,744,600	19,885,783	26,386,171	25,521,690	22,159,492	18,470,350
Interest on bonds	470,250	474,750	479,250	483,750	488,250	461,000
Times Interest Earned	**27.10**	[illegible]	**55.06**	**52.76**	**45.39**	**40.07**
Miscellaneous charges	.....	‡‡8,910,489	.....	.....	.....	.....
Net income	12,274,350	10,500,545	25,906,921	25,037,940	21,671,242	18,009,350
Income tax	1,463,025	1,367,999	3,256,103	2,827,622	2,522,308	2,430,015
Balance	10,811,325	9,132,546	22,650,818	22,210,318	19,148,934	15,579,335
Minority interest		3,250	3,268	3,250	3,250	3,250
Preferred dividends (8%)	1,040,755	180,000	180,000	180,000	180,000	180,000
Preferred dividends (5%)		857,845	843,907	625,000	429,214	.....
Preferred dividends (6%)	.....	.....	.....	.....	.....	328,011
Times charges and preferred dividends earned	**4.29**	**1.69**	**5.54**	**6.19**	**6.12**	**5.43**
Common dividends	11,217,160	15,383,404	15,383,369	12,114,294	9,998,870	10,935,819
Surplus for year	*(d)$1,446,590	††(d)$7,291,953	§§$6,240,274	**$9,287,774	$8,537,600	$4,132,255

‡ Does not include inter-company sales.
§ Before good-will written down to $1 ($2,883,054).
** Before $240,282 credit adjustment of prior years' tax reserve.
§§ Before debiting $826,415—good-will written down to $1 (including good-will acquired in purchase of properties of James S. Kirk & Co.; and shares of Thos. Hedley & Co., Ltd., England).
‡‡ Inventory adjustments.
†† Before following debits: Adjustment of investments to market

value, $2,539,292; difference in exchange arising on conversion of accounts of foreign subsidiaries, $214,906; good-will written down to $1, $97,500; total, $2,851,698.
* Before crediting $399,235 net proceeds of claims collected, $767,983 reversal of reserve for investments at June 30, 1932 no longer required, and $272,152 difference in exchange arising on conversion of accounts of foreign subsidiaries, and before debiting $364,930 good-will written down to $1.

NOTE: Depreciation formerly computed at average rates was computed in 1933 upon each unit of property, plant, etc., to provide for complete extinction during its average life.

Comparative Consolidated Balance Sheet, as of June 30

ASSETS:	1933	1932	1931	1930	1929	1928
Property account	$86,849,863	$84,819,041	$81,590,562	$67,700,169	$57,783,482	$54,185,948
Good-will, etc.	1	1	1	1	1	2,883,055
§*Inventories*	*28,927,600*	*31,254,921*	*49,055,496*	*45,922,757*	*44,964,716*	*39,508,176*
Accounts and notes receivable	*8,347,923*	*7,511,078*	*12,340,947*	*14,451,721*	*13,188,243*	*12,526,285*
Investments	*‡22,386,065*	*17,807,419*	*9,192,678*	*10,566,723*	*2,629,349*	*2,507,641*
**Secured loans*	.....	.....	*9,841,369*	*10,870,243*	*7,690,573*	*6,925,148*
Cash	*5,823,567*	*7,055,671*	*6,373,482*	*5,835,003*	*6,633,307*	*4,493,571*
Deferred charges	689,248	504,067	633,095	531,937	348,476	330,976
Other assets	††7,173,300	8,096,501	.....	.....	.....	.....
Total	$160,197,567	$157,048,698	$169,027,631	$155,878,554	$133,238,147	$123,360,799
LIABILITIES:						
*Preferred stock	$19,462,785	$19,467,785	$19,472,260	$14,815,000	$14,815,000	$10,595,800
Common stock	†25,640,000	†25,640,000	†25,640,000	†25,640,000	25,000,000	25,000,000
Funded debt	10,400,000	10,500,000	10,600,000	10,700,000	10,800,000	10,900,000
Accounts payable	*4,036,456*	*3,664,880*	*4,042,686*	*3,399,187*	*3,333,890*	*3,443,997*
Accrued taxes	*1,831,842*	*1,464,733*	*3,104,459*	*3,053,308*	*3,165,542*	*3,969,641*
Depreciation reserve	33,137,045	30,397,668	28,286,526	26,107,570	23,840,977	22,960,502
Other reserves	2,408,407	2,260,449	4,084,866	3,780,513	3,587,818	3,450,485
Capital surplus	19,618,950	19,618,950	19,618,950	19,618,950	9,458,950	9,458,950
Earned surplus	43,662,082	44,034,233	54,177,885	48,764,026	39,235,970	33,581,424
Total	$160,197,567	$157,048,698	$169,027,631	$155,878,554	$133,238,147	$123,360,799
Current assets	*$65,485,155*	*$63,629,089*	*$86,803,973*	*$87,646,447*	*$75,106,188*	*$65,960,820*
Current liabilities	*5,868,298*	*5,129,613*	*7,147,144*	*6,452,495*	*6,499,432*	*7,413,638*
Working Capital	*$59,616,857*	*$58,499,476*	*$79,656,829*	*$81,193,952*	*$68,606,756*	*$58,547,182*

* Includes $55,885 5% preferred stock of subsidiaries in 1933, $60,885 in 1932, $65,360 in 1931 and $65,000 prior years.
† Represented by 6,410,000 no par shares.
§ At lower of cost or market.
‡ Represented by U. S. Government securities at market, $16,292,065; other Government securities, at market, $750,090; municipal securities at market, $1,563,142; $1,879,000 Procter & Gamble Co. deb. gold 4½s, $1,862,457; 2,944 shares Procter & Gamble Co. 5% cum. preferred, $294,400; 35,942 shares Procter & Gamble Co. common, $1,078,260; other securities at market, $545,651; total, $22,386,065.
** Loans against security to employees for stock acquisition.
†† Loans to employees, less reserve, $4,801,905 for stock acquisition (secured); other loans to employees, less reserve, $1,455,222 (secured); loans against mortgages, etc., less reserve, $916,173; total, $7,173,300.

NOTE: Accounts certified by Deloitte, Plender, Griffiths & Co.

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