

Mandatory vs. Contractual Disclosure in Securities Markets:
Evidence from the 1930s*

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ABSTRACT

This paper studies the effects of the initial mandatory disclosure documents filed under the Securities Exchange Act of 1934. Our sample companies are listed on the New York Stock Exchange (NYSE) and subject to its disclosure requirements at the time of the regulatory filings. To establish a baseline, we first document that listed firms experience abnormal volatility and trading volumes around the time of earnings announcements. When listed companies later make their initial Exchange Act disclosures, however, there is no similar market reaction, suggesting that the additional legally mandated disclosures—principally having to do with management compensation and ownership structure—did not contain value-relevant information as to companies already subject to NYSE disclosure rules.

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This paper studies the effects of the initial mandatory disclosure documents filed under the Securities Exchange Act of 1934. Our sample companies are listed on the New York Stock Exchange (NYSE) and subject to its disclosure requirements at the time of the regulatory filings. To establish a baseline, we first document that listed firms experience abnormal volatility and trading volumes around the time of earnings announcements. When listed companies later make their initial Exchange Act disclosures, however, there is no similar market reaction, suggesting that the additional legally mandated disclosures—principally having to do with management compensation and ownership structure—did not contain value-relevant information as to companies already subject to NYSE disclosure rules.

1. Introduction

Prior to the federal securities laws of the 1930s, stock exchanges in the United States imposed disclosure requirements on listed companies. Motivated by the belief that these requirements were both insufficient and laxly enforced, the Securities Exchange Act of 1934 (Exchange Act) required listed companies to file periodic disclosures with the newly-created Securities and Exchange Commission (SEC). More generally, mandatory disclosure laws are enacted because policy makers believe that managers lack sufficient incentive to make adequate disclosures voluntarily or pursuant to contracts with the exchanges on which their stocks are listed.

This paper studies traders' reactions to the first mandatory disclosure documents that New York Stock Exchange (NYSE) listed companies filed in 1935 pursuant to the Exchange Act. We first document that stock prices and trading volumes react sharply to company earnings announcements made pursuant to NYSE disclosure rules. We then ask

whether prices and volumes react similarly to the SEC-mandated disclosures. The market reaction to these filings should depend on whether they contain value-relevant information that was not disclosed under the NYSE rules. As we discuss in more detail below, whether the Exchange Act's disclosure system represented an improvement over the NYSE's is not obvious *a priori*.

Our tests of market reaction draw on prior studies by Bailey, Karolyi and Salva (2004) and Bhattacharya et al. (2000), which consider price volatility (proxied by the absolute value of abnormal returns) and abnormal trading volume following changes in the informational environment. We find no evidence that the SEC filings contained new, value-relevant information for firms already subject to the NYSE's disclosure rules. We also discuss possible alternative explanations for the results, including the possibility that listed companies voluntarily disclosed the SEC-mandated information in advance of the filings and the possibility that the initial filings were incomplete.

The empirical literature on the effects of mandatory disclosure laws is small and inconclusive. A few papers test whether the Securities Act affected the long-run returns realized by investors in new issues of stock (Stigler 1964; Simon 1989). Benston (1973) examines the effects of the periodic financial disclosures required by the Exchange Act. More recently, Greenstone, Oyer and Vissing-Jorgensen (2004) and Ferrell (2004) examine the effects of a 1964 statute that extended the Exchange Act's periodic disclosure provisions to most companies traded over the counter and Bushee and Leuz (2005) examine a further extension of Exchange Act reporting requirements in 1999. Daines and Jones (2005) also examine the long-run impact of the Exchange Act on bid-ask spreads.

Most of these papers take the enactment of a disclosure law as the relevant event. They typically observe long-run stock returns for affected companies before and after enactment or compare post-enactment returns, spreads, or liquidity for affected and unaffected companies. Our paper employs a different methodology for evaluating the effects of mandatory disclosure. We look for effects immediately around the filing of a disclosure document, using high frequency (daily) data.

The paper is organized as follows: Section 2 describes the main federal securities laws of the 1930s. Section 3 summarizes prior findings on the effects of mandatory disclosure laws. Section 4 describes our data and methodology. Sections 5 and 6 study market reactions to earnings announcements and Exchange Act filings, respectively, finding that only the former result in unexpectedly large volatility and trading volumes. Section 7 discusses possible explanations for these different results. Section 8 attempts to shed further light on potential explanations by comparing pre- and post-Exchange Act price reactions to earnings announcements. Section 9 suggests ways in which the results may be relevant to contemporary debates and Section 10 concludes.

2. The 1930s disclosure laws

Prior to 1933, listed companies' disclosure policies were largely determined by their managers and the stock exchange(s) that listed their shares. Most states had a "blue sky" law under which the sale of securities in that state triggered certain disclosure requirements. However, the blue sky laws of several states, including New York, did not require specific disclosures but merely prohibited fraud (Mahoney 2003). Thus, blue sky laws did not create a broadly applicable mandatory disclosure system. The Interstate

Commerce Act imposed some disclosure requirements on interstate railroads and other common carriers.

The Securities Act of 1933 (Securities Act) was the first generally applicable federal disclosure statute. It required companies issuing debt or equity securities to the public to file a registration statement containing substantial company-specific disclosures. Given the unfavorable financial climate of the early 1930s, there were very few new issues during the early years of the statute's existence.

The Exchange Act, enacted in June 1934, was the second major disclosure statute. It required each company with securities listed on an exchange to file an application (also called a registration statement but distinct from a Securities Act registration statement) and then to update the required information annually. The statute also created the SEC and made it the administering agency for both the Securities Act and Exchange Act. The SEC granted temporary Exchange Act registration until June 30, 1935 to companies already listed on an exchange. In December 1934, it adopted Form 10, the primary form for permanent registration. Most NYSE listed companies filed Form 10s during March through May of 1935.

Importantly, one cannot determine *a priori* whether the Exchange Act disclosures represented an improvement over the existing disclosure system for NYSE-listed companies. For the most part, Form 10 calls for information similar to the NYSE's disclosure requirements discussed in Meeker (1930), which include narrative descriptions of the company's business, management, properties, capital structure, terms of outstanding debts, and financial statements. The securities law forms, however, describe the required information in substantially more detail than do the listing standards. In

contemporary terminology, one would call the NYSE's disclosure system "principles-based" and the SEC's "rule-based." This is true both of the narrative and financial statement disclosures.

Form 10 also requires some notable disclosures not contained in the listing standards, such as management's compensation, interested-party transactions, and principal shareholders and their holdings. As Mahoney (1995) and La Porta, Lopez-de-Silanes and Shleifer (2005) note, such disclosures inform shareholders about potential sources of misappropriation and managers accordingly have an incentive not to make them unless required.

The benefits of disclosure rules depend not only on their substance but on their enforcement. Critics claimed that the NYSE did not vigorously enforce its requirements. This claim is difficult to assess empirically more than 70 years later, although Jiang, Mahoney and Mei (2005) and Mahoney (1999) provide evidence that another standard criticism of the pre-SEC NYSE—that it turned a blind eye to manipulative trading—was exaggerated. In any event, the SEC had an array of administrative, (civil) judicial and criminal sanctions at its disposal and carried out an aggressive enforcement program.

3. Prior studies of the securities laws

Stigler (1964) studies the Securities Act's impact on investors in new stock issues. He compares market-adjusted returns, excluding dividends, for samples of new issues during 1923-28 and 1949-55 and finds that two-year compounded annual returns are approximately the same for both groups. He finds differences over longer time periods but attributes them to specification error. Stigler also notes that the cross-sectional variance of these returns is lower for the post-Securities Act sample and concludes that

the Act drove out higher-risk securities. Jarrell (1981) carries out a similar study using a market- and risk-adjusted approach derived from the Capital Asset Pricing Model, with qualitatively similar results.

Simon (1989) also studies new issues before and after the Securities Act but partitions her sample based on assumed levels of pre-Securities Act informational asymmetry. In particular, she distinguishes initial public offerings from issues of seasoned companies, arguing that seasoned companies have a larger reputational incentive to provide high-quality voluntary disclosure. She similarly distinguishes companies listed on the NYSE, and therefore subject to its disclosure standards, from unlisted companies. Simon finds no evidence of a post-SEC change in average abnormal monthly returns, cumulated for up to 60 months, for companies that were seasoned or traded on the NYSE. The performance of unseasoned companies not traded on the NYSE, however, improves after 1933, leading Simon to conclude that mandatory disclosure provided useful information when neither reputation nor third-party bonding was available to provide appropriate incentives for voluntary disclosure. Simon also finds that the cross-sectional variance of long-run abnormal returns decreases after 1933. Contrary to Stigler, she interprets this as a reduction in forecast errors resulting from lower informational asymmetry rather than a reduction in risk.

Benston (1973) considers the effects of the financial disclosures required by the Exchange Act. Like Simon, he partitions his sample based on a proxy for the company's pre-Exchange Act informational environment. Benston notes that prior to the Exchange Act, a large majority of NYSE companies disclosed the main financial statement line items later required by Form 10, except for sales. He therefore compares average and

cumulative abnormal market- and risk-adjusted returns over an approximately two-year period for firms that voluntarily disclosed sales prior to 1934 with those that did not and finds no significant difference.

Greenstone, Oyer and Vissing-Jorgensen (2004) study the effects of the 1964 extension of Exchange Act periodic disclosure to all OTC companies meeting certain size thresholds. They divide their sample firms into groups based on the extent to which the 1964 amendments altered their disclosure obligations. They find that the firms most affected by the statute, on average, earned positive cumulative weekly abnormal returns during the 20-month period beginning when the amendments were first proposed and ending when they were enacted and also over a 10-week period around the time they filed their first disclosure document.

Ferrell (2004) also studies abnormal returns for OTC companies around the time of the 1964 amendments. Using monthly data and a slightly longer event window, he obtains results qualitatively similar to those of Greenstone et al. Ferrell also finds that the cross-sectional variance of returns, as well as the average time-series variance, falls for OTC stocks after the 1964 amendments and concludes that investor forecast errors declined because of the mandated disclosures.

Bushee and Leuz (2005) find permanent increases in liquidity for firms traded on the over-the-counter bulletin board (OTCBB) that were required by a regulatory change in 1999 to register under the Exchange Act. Their sample firms differ from ours in having selected a low-disclosure market prior to the regulatory change. Daines and Jones (2005) test whether bid-ask spreads fall after the first half of 1935, when companies made

their first Exchange Act filings. Following Benston (1973), they partition their sample based on the informativeness of the companies' pre-SEC accounting statements.

Unlike most of the prior literature, we do not focus on changes in long-run (low frequency) trading returns or spreads following a policy change. Instead, we look at short-run (high frequency) changes in volatility and trading volume around the time of companies' disclosures. This approach may provide a more appropriate test of whether the securities laws improved investors' ability to value firms because the measurement of long-run, low frequency abnormal returns is vulnerable to specification error. This is surely a concern for any studies that include the 1930s, when the economy was in severe distress followed by recovery. This implies significant changes in companies' debt-equity ratios during the period as the value of equity shrunk continuously during the depression. This in turn implies that firms' betas, or systematic risk, also changed during the period. This could lead the market model, which assumes a stationary beta, to be misspecified when applied over a long time period. By using high frequency daily data on individual stocks, we can better exploit the data on both the time and cross-sectional dimensions, as our approach only assumes a constant beta over a 103 trading day period.

In addition, our research focus differs somewhat from that of the prior literature. We do not attempt to determine whether the 1930s securities laws were, on balance, beneficial to investors. Rather, we ask whether the mandated disclosures constituted information as to companies already listed on the NYSE. Alternatively, one might pose the question as whether the NYSE's disclosure requirements already produced information substantively identical to that mandated by the securities laws.

4. Data and Methodology

We obtain daily return and volume data from the Center for Research in Security Prices at the University of Chicago (CRSP). We identify Form 10 filing dates (that is, the date on which the company registered under the Exchange Act) from SEC press releases available in the LEXIS/NEXIS database, supplemented with news stories from the *New York Times*. Our sample begins with the 712 companies listed on the NYSE for which CRSP has daily data as of January 1, 1935. We eliminate interstate railroads (SIC code 4000) because they were already subject to mandatory disclosures under federal law. The SEC adopted a separate registration form for railroads incorporating the disclosures they were already required to make. Of the remaining 654 companies, we can identify Form 10 filing dates for 576, or 88%, prior to the July 1, 1935 deadline. Approximately 20 additional companies filed after the deadline, but we do not include these in the sample because they may not be comparable to the companies that filed on time. For example, they may be disproportionately financially distressed companies that considered delisting rather than bearing the cost of the new disclosure rules. We also exclude three companies that are missing return data for more than 25 days during the estimation and event windows, resulting in a sample of 573 companies.

Prior to the Exchange Act's disclosure system, the primary means by which the market learned of listed companies' financial results was the annual report prepared in accordance with NYSE rules. The NYSE required listed companies to mail the annual report to shareholders at least 15 days in advance of the annual meeting. Companies typically released their audited financial statements to the press at the time they mailed the annual report and the business press routinely reported company earnings. Moreover,

some companies announced preliminary unaudited earnings numbers some weeks prior to the release of the audited financial statements.

To compare the market's reaction to NYSE and SEC disclosures, we search the *New York Times* for the period January 1 to June 30, 1935 for earnings announcements for the companies in our sample. The search uncovers announcement dates for 458 of our 573 sample companies. Of these, 423 pre-date the Form 10 filing. We limit our study of earnings announcements to those 423 companies to avoid any contamination from data contained in the Form 10 filings. When the same company announces both preliminary and definitive results, we include only the first announcement.

Our basic methodology is to establish a baseline by measuring the market's reaction to earnings announcements and then compare the reaction to (subsequent) Form 10 filings. We focus on short-term volatility (as proxied by the absolute value of abnormal returns) and abnormal trading volume. We estimate a market model for returns using the standard methodology described in Brown and Warner (1985) and taking the CRSP equally-weighted index as the market proxy. Our estimation period is the interval from days -100 to -11, inclusive and day zero is the day on which earnings appear in the *Times*. We similarly estimate a trading volume market model and calculate abnormal turnover as suggested in Tkac (1999). We winsorize abnormal returns and turnover at the 1% and 99% levels prior to further analysis to reduce the influence of outliers.

For purposes of hypothesis testing, we use Corrado's (1989) nonparametric rank test. This has become common in the literature for situations in which the sign of the abnormal return induced by an event may vary across firms. The absolute value of abnormal returns is an obvious measure of the volatility induced by the event, but it is not

normally distributed, making a nonparametric test attractive (Bhattacharya *et al.* 2000; Bailey *et al.* 2004).

5. The benchmark case: 1935 earnings releases

We begin by measuring traders' reactions to earnings announcements. First, we establish that our earnings announcement sub-sample is comparable to the full sample of Form 10 filers. Table 1 provides descriptive statistics for the full sample of 573 Form 10 filers and the 423 companies in the earnings announcement sub-sample. The two are closely matched on basic measures of firm size, average trading volume and volatility. Table 1 also shows summary measures of daily absolute abnormal return and abnormal turnover during the estimation periods as a baseline against which to compare these measures during event windows.

As one measure of the market's reaction to earnings announcements, we determine the percentage of the 423 companies for which we can reject the null hypothesis of zero abnormal return for each day in the interval (-100, +2) in event time using standard event study methodology. The result, shown in Figure 1, is a clear spike in the number of nonzero abnormal returns at day zero, as we would expect if the earnings announcements are informative. Figure 2 shows the average abnormal turnover for the same sample companies for the same period. Once again, we see a spike in trading volume around day zero.

To determine whether absolute abnormal returns and abnormal turnover are statistically significant on average, we use Corrado's (1989) nonparametric rank test. Table 2 shows the means and medians of absolute abnormal return and abnormal turnover for the earnings announcement subsample for days -1 through +2 in event time

as well as averages for the 4-day period. Absolute abnormal returns and abnormal turnover are significantly different from their pre-event medians on days -1, 0 and 1 and for the entire 4 trading day period. Abnormal turnover is also significant on day 2. Comparison of the means and medians with the summary measures presented in Table 1 confirms that the differences are economically meaningful as well.

Finally, we undertake cross-sectional tests to determine whether the magnitudes of the price and volume reactions are related to explanatory variables that are common in the earnings announcement literature. Atiase (1985) shows that the price reaction to earnings announcements is on average smaller for larger firms. He argues that there is more private production of information (and therefore lower pre-announcement information asymmetry) with respect to larger firms. We accordingly use the market value of equity as an explanatory variable.

The price reaction to earnings announcements is also related to the absolute magnitude of the earnings surprise (Ball & Brown 1968). Unfortunately, we lack information about expected earnings for our period of interest. We therefore use prior year earnings as a crude proxy for investor expectations. The earnings surprise, then, is defined as the absolute value of the difference between 1934 and 1933 earnings, normalized by the absolute value of 1933 earnings (as reported in the *New York Times*). The reaction to earnings may also depend on their sign (Hayn 1995). We accordingly include a dummy variable indicating whether 1934 earnings are positive or negative. We also use the average bid-ask spread over the interval (-100, -11) in event time as an explanatory variable. We obtain closing bid and ask quotations from CRSP. The spread is a direct measure of trading costs (and should therefore be negatively related to trading

volume). It is also, however, a proxy for the dispersion in investor beliefs in advance of the announcement, and therefore potentially positively related to abnormal return and turnover. In some specifications, we also include 12 industry dummies using the categorizations suggested on Kenneth French's web page.

Our dependent variables are the daily absolute abnormal return averaged over the interval (-1, 1) and the daily abnormal turnover averaged over the interval (-1, 2) in event time. Looking at Table 2, absolute abnormal returns are statistically significant on average on days -1, 0 and 1 while abnormal turnover is also significant on day 2. This is understandable as absolute returns measure changes in traders' beliefs, while turnover measures traders' actions (Beaver 1968). Even after newly released information has become fully reflected in price, traders may still wish to rebalance their portfolios to maximize expected utility conditional on the new valuations. The results, however, are not sensitive to whether we average over three or four trading days.

Table 3 provides summary statistics for our dependent and explanatory variables. Because many of our variables have some skewness, there is a risk that our estimates will be affected by extreme values. In particular, the market value of equity and earnings surprise variables are strongly right-skewed. However, their logarithms are approximately normally distributed, so we use the logs in our regression analysis. Absolute abnormal returns and bid-ask spreads are considerably less skewed and we use them without transformation. The abnormal turnover variable has two extreme outliers, which we eliminate in the reported regressions, although all results are qualitatively the same if they are included.

Table 4 shows the results of the cross-sectional analysis. The results for absolute abnormal returns are consistent with the literature on earnings announcements. Each of the four explanatory variables enters significantly. As expected, the price reaction is negatively related to company size and positively related to the earnings surprise. Unlike Hayn (1995), we find that losses generate larger price reactions than gains. As Sivkumar and Waymire (1993) observe, however, favorable news may be more likely than unfavorable news to leak out before formal announcements, leading to larger effects for negative news. The inclusion of industry controls does not affect the inference on the other variables and also does not add explanatory power.

The explanatory variables are less significantly related to abnormal turnover. In particular, the r-squared of the regression is dramatically lower. However, both bid-ask spread and (the log of) the earnings surprise are significantly related to abnormal turnover around the time of the earnings announcement. Of course, bid-ask spreads are relatively high in general in 1935, with an average in our sample of 6%. An above-average bid-ask spread, therefore, represents a significant barrier to trading. Thus, it is not surprising that there is a negative relation between abnormal turnover and average spread.

The time series and cross sectional evidence from earnings announcements in early 1935 shows that the market reacted promptly and measurably to releases of information contained in earnings announcements. Despite a substantial fall in liquidity and equity values during the Depression, prices responded in the expected way to the information contained in earnings announcements, providing a baseline against which we can assess the reaction to Form 10 filings.

6. Market reactions to Form 10 filings

We now carry out similar procedures with respect to Form 10 filings. As an initial matter, we can be confident that market participants promptly learned the contents of these filings. Beginning with the earliest filings in March, the *New York Times* ran stories listing the prior days' filings and, as to some of the firms, discussing facts discovered in those filings. It is therefore clear that the *Times*' Washington bureau had access to the forms on the same day they were filed with the SEC. We also note that SEC rules required filers to deliver their forms to the relevant stock exchange on the day of filing, suggesting that stock exchange members had essentially real-time access as well.

Figures 3 and 4 show the same information on the same scales as Figures 1 and 2, this time with Form 10 filings rather than earnings announcements as the relevant events. For consistency with our study of earnings announcements, we define day zero as the day after the filing, when newspaper accounts typically appeared. The two figures show little or no reaction to Form 10 filings, in contrast to the spikes in returns and turnover around the time of earnings announcements. There is no evidence at all of a rise in the number of firms with non-zero abnormal returns. Abnormal turnover is slightly above zero on day +2, but it is negative on average during the event window. The difference between the two pairs of time plots is striking. The time plot and the analyses below include all 573 firms for which we have filing dates, but the results are unchanged if we limit the sample to the 423 earnings announcement firms.

The rank test in Table 5 confirms that absolute abnormal returns are not significantly different from their pre-event medians on any day during the event window, nor is the cumulative total during the event window. Indeed, comparing Tables 1 and 5

we see that the average and median values of absolute abnormal returns during the event window are very close to their pre-event values. Surprisingly, abnormal turnover is significantly negative on average during the event window. This raises the possibility of a delayed reaction—traders may have required a few days to sort through the data in the Form 10 filing and assess its consequences for firm valuation before trading on their revised beliefs. To test this possibility, we expand the event window to include days +3 through +5, inclusive, and re-run the rank test. Absolute abnormal returns and abnormal turnover are both statistically equal to their pre-event medians on the three additional days, individually and cumulatively. The evidence therefore suggests that the Form 10 filings were not informative.

We next carry out cross-sectional analysis of the abnormal returns and turnover similar to that for the earnings announcements. We unfortunately have no way of determining which Form 10 filings contained “surprises.” As in the case of the earnings reports, we identify a crude proxy for deviation from expectations and ask whether it is related to the size of abnormal absolute returns or abnormal turnover. For 77 of our sample firms, data in the Form 10 filing generated a headline in the *New York Times* on the day after the filing. These headlines in almost all cases related to CEO salaries, share holdings by company officers and directors, or large block holdings.

We assume that the newsworthiness of these data was to some extent a function of whether they were unexpected. Not surprisingly, larger companies are more often the subject of a headline—the average market value of equity of the 77 companies that made the headlines is \$126 million, compared to \$38 million for the sample as a whole. Nevertheless, we assume that the decision to cover particular companies was also based

on the amount of surprise in the information. For example, in several instances the accompanying article states that the CEO salary reported was the largest among those disclosed on the prior day. We therefore include in our regression model a dummy variable indicating whether the filing generated a headline. We use the log of market value of equity and average bid-ask spread during the estimation period as additional controls and, in one specification, use 12 Fama-French industry dummies consistently with our analysis of the earnings announcement sample above.

The results are shown in Table 6. Smaller size and larger bid-ask spreads are associated with larger absolute abnormal returns. However, controlling for firm size, whether or not the filing generated a headline is unrelated to the size of absolute abnormal returns or abnormal turnover around the time of a Form 10 filing. This suggests that there was a release of information with respect to some of the sample firms around the time of their filings. However, using the limited information about investor expectations available to us, we find no evidence that the release is related to data contained in those filings. This supplements our previous finding that there is no market reaction *on average* across the sample firms.

7. Potential explanations

There are three distinct plausible explanations for the sharply different reactions to earnings announcements and Form 10 filings. One is that market participants did not view the additional SEC-mandated information as value-relevant for firms already subject to the NYSE's disclosure rules. A second is that firms began to comply voluntarily with the SEC's requirements in annual reports released in early 1935, even though compliance was not mandatory until July 1, 1935. If the Form 10 filings simply

repeated what was already in the annual reports, we would not expect the Form 10 filings to have any impact. A final possibility is that firms were not yet able to comply meaningfully with the SEC's new requirements in their inaugural Form 10 filings, which were therefore largely duplicative of the companies' annual reports to shareholders.

There is ample evidence against the second explanation—that companies anticipated the SEC's requirements in their annual reports. Listed companies complained that the new SEC rules meant that they would have to prepare two separate annual reports, suggesting that they would not be able to comply with the SEC's rules in time for their annual meetings in early 1935. On February 2, 1935, the *New York Times* reported that a group of listed companies asked the NYSE for permission to delay mailing their annual reports for 1934 until after they had prepared them in accordance with SEC standards. However, the NYSE denied the request. The *Times* reported:

For convenience and economy, some corporations had planned to delay their annual reports to stockholders this year until they could be drafted to meet the requirements of Form 10. This would have obviated the preparation of two financial statements differing only slightly. The Exchange's letter, however, suggested that listed companies submit reports in their customary form to stockholders and later revise them to conform to Form 10.

Further reporting in the press about the difficulty of preparing Form 10 filings suggests that few companies were able to conform their 1934 annual reports to Form 10 in advance of the actual filing.

To confirm that companies did not meet the SEC's accounting requirements at the time of their annual reports for 1934, we take advantage of Benston's (1973) observation that prior to 1934, roughly half of NYSE listed companies disclosed sales, while the remainder disclosed only net operating profits in their income statements. We accordingly compare our sample companies' income statements compiled in the 1934 and 1935 editions of Moody's manuals, which were published in June of those years. None of our sample companies changed their practices with respect to reporting sales in 1935. Those that failed to disclose sales in the 1934 edition also failed to disclose it in the 1935 edition. As the Form 10 accounting requirements mandated disclosure of sales, our sample companies' annual reports for 1934 were no closer to compliance than their 1933 annual reports.

A slightly different version of this concern is that while annual reports released in early 1935 may not have included more information than in prior years, the information may have been of higher *quality* because of the SEC's existence. The Exchange Act provided for civil and (in some instances) criminal sanctions for misleading disclosures, whether made to the SEC or directly to investors. The SEC, as an arm of the federal government, had investigatory and remedial powers beyond those of the NYSE.

These observations are important but largely unrelated to our analysis. There are good theoretical reasons to believe that effective prohibitions on fraud enhance the quality of company disclosures (Easterbrook & Fischel 1984) and it is entirely plausible that the Exchange Act's prohibitions on fraud improved the quality of disclosures made *outside* the mandatory disclosure system. Our primary objective is to ask whether the incremental substantive disclosures that the SEC required were meaningful to investors.

Nevertheless, in the next section we also consider whether the prohibitions on fraud affected the informativeness of annual reports or voluntary disclosures.

The third possibility—that the Form 10 filings were not complete—is a more serious concern. Several companies complained to the SEC and the NYSE that they would have great difficulty meeting the Form 10 filing deadline. The SEC therefore announced that it would permit companies to file a Form 10 that contained all the required information except for the financial statements, which could be filed by amendment no later than July 20. The SEC, unfortunately, did not announce the filing of amendments to Form 10s so we cannot be sure how many of our sample companies furnished their financial statements by amendment rather than in the initial filing. There is almost no press commentary on the new financial statement disclosures.

Form 10 also, however, called for substantial non-financial statement disclosures that are arguably important to investors and that had not previously been disclosed publicly. The most notable example is executive compensation. Form 10 filings had to disclose the salaries of the company's three most highly-compensated officers. They also had to disclose the stock ownership of all of the company's directors, top officers, and principal shareholders. Most companies had not previously disclosed this information, as is clear from press coverage.

However, it is also clear from press coverage that not all companies—particularly the earliest filers—complied in full with the new rules. Some of the early filings did not include salary information. Indeed, some companies insisted that it was improper for the SEC to request it and others filed a “confidential” addendum containing the salary data. The SEC eventually made it clear that it was unwilling to grant confidential treatment to

salary information, but it did not do so immediately. Our sample therefore includes some companies for which salary data was publicly available in the Form 10 filing and some for which it was not. By contrast, it appears clear from press reports that virtually all Form 10 filings disclosed officer and director stock ownership.

We accordingly divide our sample based on the information contained in press reports about the completeness of the sample firms' filings. We identify 28 companies that requested confidential treatment for salary information or simply did not disclose it. There are 223 companies whose salary data appeared in the news the day after the filing. For the remaining 322 companies, we cannot be sure how complete the Form 10 filing was because the press reports did not comment on salaries. We calculate absolute abnormal returns and abnormal turnover around Form 10 filing dates, limiting the sample to companies whose salary data appeared in the newspapers. The mean and median values are extremely close to (although slightly less than) those reported for the full sample in Table 5. We also re-run the regressions reported in Table 6 limiting the sample to the same 223 companies, then again with the full sample but substituting a dummy variable indicating that salary data appeared in the news in place of the "headline" dummy variable. In no case do the results indicate a stronger market reaction as to companies with reported salary data.

8. A pre-SEC comparison

As a method of estimating the impact of the Exchange Act's anti-fraud provisions, we ask whether the market reacted differently to earnings announcements in early 1935 in comparison to the pre-SEC era. By prohibiting fraud and creating a federal agency to enforce the prohibition, the Exchange Act could have improved the

informational environment even if the SEC's detailed disclosure rules produced nothing of value.

Drawing a line between the pre- and post-SEC eras is not entirely straightforward. By the time companies released their 1933 earnings in early 1934, the bill that became the Exchange Act had already been drafted and it was reasonably certain that the legislation would pass in some form. Looking at announced earnings for 1932, the depths of the Depression, seems problematic as well.

We accordingly go all the way back to 1927, a year we select because it is untainted by the Depression, the New Deal, or the sharp run-up in equity values of 1928-29. Following the procedure we used for 1935 earnings announcements, we search the *New York Times* for announcements of annual earnings during the first half of 1927. The search produces announcements for 384 firms, or 78% of 491 non-railroad companies listed on the NYSE as of January 1, 1927, in comparison to the 80% found for the 1935 sample.

We gather return data from CRSP for the sample companies and estimate a market model for returns using the same estimation and event windows as before. We omit analysis of turnover because we believe that, unlike returns, it cannot be meaningfully compared in the two years. Returns measure, in a sense, the *average* belief revision among investors, while trading volume measures the *aggregate* actions of investors (Kim & Verrecchia 1991). There were far fewer investors to aggregate in 1935 as compared to 1927, and trading volumes were correspondingly lower across the board.

Figure 5 shows a spike in the number of abnormal returns around the time of earnings announcements in 1927 that is qualitatively similar, although slightly smaller,

than that shown in Figure 1 for 1935 earning announcements. This is strong evidence against the extreme view that financial data were non-credible in the pre-SEC era.

We next verify that the same explanatory variables that are related to the magnitude of absolute abnormal returns around the time of 1935 earnings releases are also related to returns in 1927. Table 7 provides the same summary information for 1927 earnings as does Table 3 for 1935 earnings. The differences between Tables 3 and 7 are to be expected given the much healthier market conditions in 1927. The average and median NYSE firms have larger market capitalizations in 1927. The average absolute abnormal return, which as discussed above is inversely related to firm size, is lower. The number of firms with negative earnings is substantially lower. In 1935, however, the average earnings surprise is greater, reflecting the economic recovery of 1934-35.

Table 8 shows the same regression as Model 1 in Table 4, but estimated for the 1927 earnings sample. Comparing the two, in 1927 the constant is lower, the sensitivity to firm size is smaller, and the sensitivity to earnings surprises is greater. To determine which, if any, differences between the two years are statistically significant, we pool the two samples and include a dummy variable for 1935 along with interactions between the dummy variable and the other explanatory variables.

The coefficients on the dummy variable and interaction terms provide intuitively plausible tests of different hypotheses about the effects of the SEC and the mandatory disclosure system. If the federal securities laws' antifraud provisions made reported earnings more credible, we would expect a positive coefficient on the interaction between the 1935 dummy and the (log of the) earnings surprise. This would indicate that the market reacted more strongly to earnings surprises in 1935 than in 1927, reflecting

traders' greater confidence in reported earnings. Alternatively, the federal securities laws could have increased the quality (and perhaps quantity) of voluntary disclosure between annual reports. In that event, the unanticipated component of year-on-year changes in earnings should be smaller, leading to a negative coefficient on the same interaction term, consistent with the analysis in Kim and Verrecchia (1991). An improved information environment should also be reflected in a negative coefficient on the interaction term between the 1935 dummy and firm size coupled with a positive coefficient on the dummy variable itself. That result would indicate that the price reaction to earnings information was larger (smaller) for smaller (larger) firms in 1935 in comparison to 1927. The conventional explanation for the negative association between firm size and the price reaction to earnings announcements is that there is more private information available for larger firms and therefore their earnings announcements contain fewer surprises. If that effect is magnified post-SEC, it would suggest that these private disclosures were more informative.

Model 1 of Table 9 estimates the same regression as in Table 8, but for a pooled sample of firms with 1935 earnings announcement data and firms with 1927 earnings announcement data, and including the dummy variable indicating the 1935 sample. The estimated coefficient on the dummy variable is small and not statistically significant. The remaining models in Table 9 add interaction terms one at a time and then all together. None of the interaction terms enters significantly in any specification. The results in Models 4 and 6 suggest that the sensitivity of abnormal returns to the size of the earnings surprise does not vary significantly from the pre-SEC to the immediate post-SEC era. In short, there is no evidence that reported earnings are more credible in 1935.

The absence of statistically significant estimated coefficients on the dummy variable and interaction term in Models 2 and 6 is inconsistent with the hypothesis of more informative voluntary disclosure post-SEC. The lack of a result here, however, could be a consequence of noisy data. We note that the estimated coefficient on the dummy variable is large but the standard error is as well. Moreover, unlike the results previously discussed, this one is somewhat sensitive to transformations of the variables—in particular, of the bid-ask spread variable. The cross-section of the average bid-ask spread variable is right-skewed because there are a few illiquid stocks in the sample. The effect is greater for the 1927 sample (in which most firms have relatively low spreads but there are a few illiquid outliers). In Models 2 and 6, if we use the log of the bid-ask spread variable or remove outlying values, the estimated coefficients on the 1935 dummy variable and the bid-ask spread interaction term increase in absolute magnitude and gain in precision to become statistically significant or nearly so.

We must, of course, be cautious in concluding that this effect is real given that it is highly sensitive to the precise specification of the regression model. If it exists, however, it is clearly not a consequence of the formal disclosures required under SEC rules—recall that there is strong qualitative evidence that companies were not yet in compliance with SEC rules at the time of their early-1935 earnings announcements. Nor is it likely a consequence of increased reliability of audited financial statements, because the sensitivity of the price reaction to earnings surprises does not change from 1927 to 1935 under any specification. Any effect, therefore, is probably a consequence of the anti-fraud rules increasing the quality of *voluntary* disclosures by listed firms between

annual reports. This would be an unsurprising consequence of adding a federal regulator empowered to seek civil or criminal penalties for misleading disclosures.

9. Implications

Our evidence suggests that the initial round of company disclosures made pursuant to the Securities Exchange Act of 1934 did not improve traders' ability to value stocks that were already subject to contractual disclosure obligations pursuant to their listing agreements with the NYSE. There are substantial risks involved in applying these results to current policy debates. As an initial matter, there is considerably less firm-specific accounting and analyst data available for our time period and none of it is in machine-readable form, making cross-sectional analysis challenging. The technology of trading, communications, and data storage and retrieval were all quite different in 1935 compared to today. Our results must be interpreted with those limitations in mind.

Nevertheless, some aspects of the change from an exchange-driven to a government agency-driven disclosure system may hold lessons for current policy debates. One clear distinction between the NYSE's and SEC's disclosure regimes is that the former is more "principles-based" while the latter is more "rule-based," as those terms are used today. The SEC's system had essentially the same substantive focus as the NYSE system but permitted less managerial discretion over how to present the required information. We find no evidence that the distinction mattered to investors.

It is also interesting to observe that traders do not appear to have reacted strongly to information about executive compensation. This may be the most interesting result in our study inasmuch as salary disclosures generated substantial press coverage in 1935. It is also interesting in light of the immense attention the SEC has paid to disclosure of

executive compensation in recent years. One might note, however, that executive compensation in 1935 was modest compared to current levels. Most of the salaries that dominated the headlines were in the range of \$100,000 (about \$1.4 million in 2007 dollars inflated at the consumer price index). The largest salaries we can find are the \$364,000 that IBM paid Thomas Watson and the \$250,000 that two steel company CEOs received. Moreover, it may have taken traders and analysts some time to determine precisely how salary and stock ownership affect the behavior and performance of CEOs and other top officers. Although difficult to establish empirically, this is at least a plausible explanation of why we fail to find a reaction to the initial filings. Nevertheless, the simplest explanation consistent with our results is that the NYSE's disclosure rules as of the early 1930s provided traders with as much useful information as the SEC's initial disclosure rules.

There are additional limitations when considering lessons for other markets. First, the NYSE had a dominant market position and was therefore able to induce companies to agree to disclosure standards that they may not have otherwise been willing to accept. In that sense, the NYSE's experience in enforcing a contractual disclosure system may be better than could be achieved by a securities exchange with less market power. Bushee and Leuz's (2005) results suggest that our findings would not carry over to markets that attract listings in part by offering a low-disclosure environment. However, it is interesting to note that in early 20th-century America, the dominant market had the highest disclosure standards (although the direction of the causation is unclear).

Second, the United States in 1930 had a well-functioning legal system capable of enforcing contracts and deterring fraud. The federal securities laws added another "cop

on the beat,” but (despite the claims of the laws’ proponents) the market was not completely lawless prior to the SEC’s arrival. In the context of an efficient and non-corrupt legal system, our findings suggest that a government regulator may not improve much on the results of voluntary and contractual disclosures. In a developing market context, by contrast, a regulator may be necessary simply to assure that contracts are enforced.

10. Conclusions

This paper attempts to measure market reactions to the filing of a company’s initial Form 10 with the SEC in the late spring of 1935. Our objective is to determine whether the SEC-mandated information, which included a few new substantive disclosures and also substantially increased the level of detail, generated unusual volatility or trading volume, as we would expect if the filings contained information. We first document that earnings announcements—which were made pursuant to NYSE disclosure requirements—generate non-zero abnormal returns and turnover. Form 10 filings, by contrast, do not.

Our results, like those of Simon (1989), suggest that the NYSE’s disclosure requirements already provided investors with information of equivalent content and reliability to that available under the federal securities laws. While Daines and Jones (2005) find that the Exchange Act did not make a measurable difference for companies that already met high standards of financial disclosure, we find no difference, on average, across all NYSE firms. These results may have some relevance to current debates over principles-based and standards-based disclosure systems. We also, however, sound some cautionary notes in applying the results to present-day policy issues.

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Table 1. Descriptive statistics

The table describes a sample of all non-railroad companies contained in the CRSP data set as of January 1, 1935 for which we know Form 10 filing dates in 1935 as well as a subset of those companies for which we also know the date of an annual earnings announcement during the first half of 1935 but in advance of that company's Form 10 filing date. Market value of equity is the monthly average for 1934 of share price times the number of outstanding shares. Beta is the slope coefficient of a market-model regression using the CRSP equally weighted index as the market proxy, estimated over the period (-100, -11), where day 0 is the relevant filing or announcement event. Monthly turnover is the average monthly trading volume divided by shares outstanding and monthly volatility is the standard deviation of monthly total returns, in each case for the calendar year 1934. Absolute daily abnormal return is the absolute value of the residual from the market-model regression, while abnormal daily turnover is the residual from the analogous volume market model regression; both are winsorized at the 99% and 1% levels. Summary statistics for both of these measures are calculated on a pooled basis for the relevant sample.

| | Form 10 filers (n=573) | | | Earnings announcements (n=423) | | |
|---|---------------------------|--------|--------------------|-----------------------------------|--------|--------------------|
| | Mean | Median | Standard deviation | Mean | Median | Standard deviation |
| Market value of equity (millions of \$) | 39.97 | 7.27 | 111.60 | 42.47 | 9.48 | 110.78 |
| Beta | 0.94 | 0.85 | 0.59 | 0.94 | 0.90 | 0.56 |
| Monthly turnover (%) | 3.64 | 1.94 | 5.63 | 3.63 | 1.94 | 5.79 |
| Monthly volatility (%) | 16.16 | 15.00 | 8.83 | 15.67 | 14.34 | 8.547 |
| Absolute daily abnormal return, estimation period (%) | 2.342 | 1.290 | 3.344 | 2.163 | 1.221 | 3.040 |
| Abnormal daily turnover, estimation period (%) | -0.003 | -0.010 | 0.156 | -0.003 | -0.010 | 0.174 |

Table 2. Absolute abnormal return and abnormal turnover around earnings announcements, 1935

The table reports cross-sectional means and medians of absolute abnormal return and abnormal turnover for a sample of 423 companies that announced annual earnings during the first half of 1935 and in advance of their Form 10 filings. Abnormal returns and turnover are prediction errors from market models estimated over the interval (-100, -11) and are winsorized at the 99% and 1% levels. Day zero is the day the *New York Times* reported the company's annual earnings. T-statistics are calculated using Corrado's (1989) nonparametric rank test.

| Day | Absolute abnormal return (%) | | | Abnormal turnover (%) | | |
|---------|------------------------------|---------|-------------|-----------------------|----------|-------------|
| | mean | median | t-statistic | mean | median | t-statistic |
| -1 | 2.840 | 1.569** | 4.28 | 0.034 | -0.005* | 2.18 |
| 0 | 2.612 | 1.507** | 2.96 | 0.045 | -0.003** | 3.51 |
| 1 | 2.518 | 1.521* | 2.10 | 0.024 | -0.004* | 2.43 |
| 2 | 2.238 | 1.316 | 0.98 | 0.025 | -0.005* | 2.14 |
| average | 2.552 | 1.478** | 5.16 | 0.032 | -0.004** | 5.13 |

*, ** indicate significance at the 5% and 1% levels, respectively

Table 3. Firm characteristics around earnings announcements, 1935

The table provides summary statistics for absolute abnormal returns (averaged over days -1 to +1 in event time) and abnormal turnover (averaged over days -1 to +2 in event time) around the time of earnings announcements for a sample of 423 firms. We also provide summary statistics for variables that we will use to attempt to explain firm-specific variation in abnormal returns and turnover. These are the market value of equity (price times shares outstanding) averaged over our estimation period (days -100 to -11); the percentage bid-ask spread (the closing ask minus the closing bid divided by their midpoint) averaged over the estimation period; the earnings surprise (the absolute value of 1934 earnings minus 1933 earnings divided by 1933 earnings); and an indicator variable that takes the value 1 if 1934 earnings are negative and zero otherwise.

| Variable | Mean | Median | Standard deviation |
|--------------------------------------|--------|--------|--------------------|
| Absolute abnormal return (%) | 2.65 | 1.85 | 2.58 |
| Abnormal turnover (%) | 0.032 | 0.001 | 0.169 |
| Market value of equity (\$ millions) | 41.36 | 8.62 | 107.76 |
| Bid-ask spread (%) | 6.17 | 3.26 | 8.01 |
| Earnings surprise (%) | 319.90 | 55.47 | 1725.19 |
| Loss (%) | 25.06 | 0.00 | 43.39 |

Table 4. Cross-sectional analysis of absolute abnormal return and turnover around earnings announcements, 1935.

The table reports results of regressions in which the dependent variables are absolute abnormal return and abnormal turnover around the time of earnings announcements, in each case expressed in percents. Explanatory variables are described in Table 3, except for Industry dummies, which are 12 industry dummy variables using the categorizations suggested on Kenneth French's web page. Heteroskedasticity-consistent standard errors are in parentheses.

| Variable | <u>Absolute abnormal return</u> | | <u>Abnormal turnover</u> | |
|-------------------------------|---------------------------------|---------------------|--------------------------|--------------------|
| | Model 1 | Model 2 | Model 3 | Model 4 |
| Constant | 7.133** (1.736) | 6.855** (2.937) | 0.068 (0.067) | -0.319 (0.166) |
| Log of market value of equity | -0.364** (0.098) | -0.376** (0.100) | -0.004 (0.004) | -0.004 (0.004) |
| Average bid-ask spread | 0.102* (0.044) | 0.099* (0.043) | -0.002* (0.001) | -0.001* (0.001) |
| Log of earnings surprise | 0.138* (0.055) | 0.158* (0.064) | 0.008** (0.003) | 0.008** (0.003) |
| Loss | 0.818** (0.313) | 0.840** (0.321) | -0.008 (0.017) | -0.007 (0.018) |
| Industry dummies? | No | Yes | No | Yes |
| Adjusted r-squared | 0.345 | 0.342 | 0.016 | 0.003 |
| N | 421 | 421 | 419 | 419 |

*, ** indicate significance at the 5% and 1% levels, respectively.

Table 5: Absolute abnormal return and abnormal turnover around Form 10 filings, 1935

The table reports cross-sectional means and medians of absolute abnormal return and abnormal turnover for a sample of 573 companies that filed Form 10s during March through June 1935. Abnormal return and turnover are prediction errors from market models estimated over the interval (-100, -11) and are winsorized at the 99% and 1% levels. Day zero is the day after the filing. T-statistics are calculated using Corrado's (1989) nonparametric rank test.

| Day | Absolute abnormal return (%) | | | Abnormal turnover (%) | | |
|---------|------------------------------|--------|-------------|-----------------------|----------|-------------|
| | mean | median | t-statistic | mean | median | t-statistic |
| -1 | 2.183 | 1.301 | -0.48 | -0.012 | -0.015* | -2.13 |
| 0 | 2.451 | 1.222 | -0.71 | -0.015 | -0.017 | -1.77 |
| 1 | 2.360 | 1.402 | 1.35 | -0.015 | -0.015 | -1.37 |
| 2 | 2.411 | 1.339 | 0.67 | 0.003 | -0.010 | -0.60 |
| average | 2.351 | 1.312 | 0.41 | -0.010 | -0.014** | -2.93 |

*, ** indicate significance at the 5% and 1% levels, respectively

Table 6. Cross-sectional analysis of absolute abnormal return and turnover around Form 10 filings.

The table reports results of regressions in which the dependent variables are absolute abnormal return and abnormal turnover around the time of Form 10 filings, in each case expressed in percents. Explanatory variables are defined consistently with Tables 3 and 4. Heteroskedasticity-consistent standard errors are in parentheses.

| Variable | <u>Absolute abnormal return</u> | | <u>Abnormal turnover</u> | |
|-------------------------------|---------------------------------|---------------------|--------------------------|-------------------|
| | Model 1 | Model 2 | Model 3 | Model 4 |
| Constant | 6.120** (1.286) | 7.370** (1.362) | -0.054 (0.085) | -0.068 (0.082) |
| Log of market value of equity | -0.287** (0.073) | -0.325** (0.074) | 0.002 (0.005) | 0.002 (0.005) |
| Average bid-ask spread | 0.109** (0.031) | 0.103** (0.031) | 0.001 (0.001) | 0.002 (0.001) |
| Headline | 0.141 (0.312) | 0.200 (0.330) | -0.007 (0.018) | -0.002 (0.017) |
| Industry dummies? | No | Yes | No | Yes |
| Adjusted r-squared | 0.274 | 0.302 | 0.000 | 0.000 |
| n | 572 | 572 | 573 | 573 |

*, ** indicate significance at the 5% and 1% levels, respectively

Table 7. Firm characteristics around earnings announcements, 1927

All variables are defined consistently with Table 3. The table provides summary statistics for these variables for a sample of 384 firms with earnings announcement dates in the first half of 1927.

| Variable | Mean | Median | Standard deviation |
|--------------------------------------|-------|--------|--------------------|
| Absolute abnormal return (%) | 1.64 | 1.16 | 1.60 |
| Market value of equity (\$ millions) | 60.31 | 17.24 | 147.00 |
| Bid-ask spread (%) | 3.06 | 1.53 | 5.59 |
| Earnings surprise (%) | 70.35 | 22.86 | 212.69 |
| Loss (%) | 8.33 | 0.00 | 27.67 |

Table 8. Cross-sectional analysis of absolute abnormal return around earnings announcements, 1927.

The table reports results of a regression identical to that of Model 1 in Table 4, estimated for a sample of earnings announcements in the first half of 1927 (n=370). The dependent variable is absolute abnormal return averaged over days (-1,1) in event time.

| | Estimated coefficient | (Heteroskedasticity-consistent) standard error |
|-------------------------------|-----------------------|--|
| Constant | 5.147** | 0.984 |
| Log of market value of equity | -0.263** | 0.055 |
| Average bid-ask spread | 0.095** | 0.027 |
| Log of earnings surprise | 0.173** | 0.045 |
| Loss | 0.870* | 0.431 |
| Adjusted r-squared | | 0.389 |

*, ** indicate significance at the 5% and 1% levels, respectively.

Table 9. Pooled cross-sectional analysis, absolute abnormal return around 1935 and 1927 earnings announcements

All variables are defined consistently with Table 4 for a pooled sample of 423 firms with 1935 earnings announcements and 384 firms with 1927 earnings announcements. Y1935 is a dummy variable that indicates 1935 firm-level data.

| Variable | Model 1 | Model 2 | Model 3 | Model 4 | Model 5 | Model 6 |
|------------------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
| Constant | 6.102** (0.824) | 5.167** (1.076) | 6.178** (1.049) | 6.082** (1.116) | 6.111** (1.010) | 5.147** (0.983) |
| Log of market value of equity | -0.317** (0.046) | -0.261** (0.062) | -0.319** (0.060) | -0.317** (0.061) | -0.318** (0.061) | -0.263** (0.055) |
| Bid-ask spread | 0.101** (0.011) | 0.100** (0.011) | 0.088** (0.027) | 0.101** (0.032) | 0.101** (0.032) | 0.095** (0.027) |
| Log of earnings surprise | 0.153** (0.039) | 0.153** (0.039) | 0.152** (0.038) | 0.160** (0.050) | 0.155** (0.038) | 0.173** (0.045) |
| Loss | 0.861** (0.188) | 0.838** (0.188) | 0.858** (0.260) | 0.859** (0.260) | 0.750 (0.439) | 0.870* (0.430) |
| Y1935 | 0.215 (0.132) | 1.917 (1.267) | 0.143 (0.176) | 0.254 (0.254) | 0.194 (0.109) | 1.987 (1.996) |
| Y1935 x log market value of equity | | -0.103 (0.076) | | | | -0.101 (0.112) |
| Y1935 x bid-ask spread | | | 0.018 (0.044) | | | 0.007 (0.052) |
| Y1935 x log earnings surprise | | | | -0.011 (0.075) | | -0.034 (0.071) |
| Y1935 x loss | | | | | 0.148 (0.522) | -0.052 (0.533) |
| Adjusted r-squared | 0.389 | 0.395 | 0.389 | 0.389 | 0.389 | 0.389 |
| n | 791 | 791 | 791 | 791 | 791 | 791 |

*, ** indicate significance at the 5% and 1% levels, respectively.

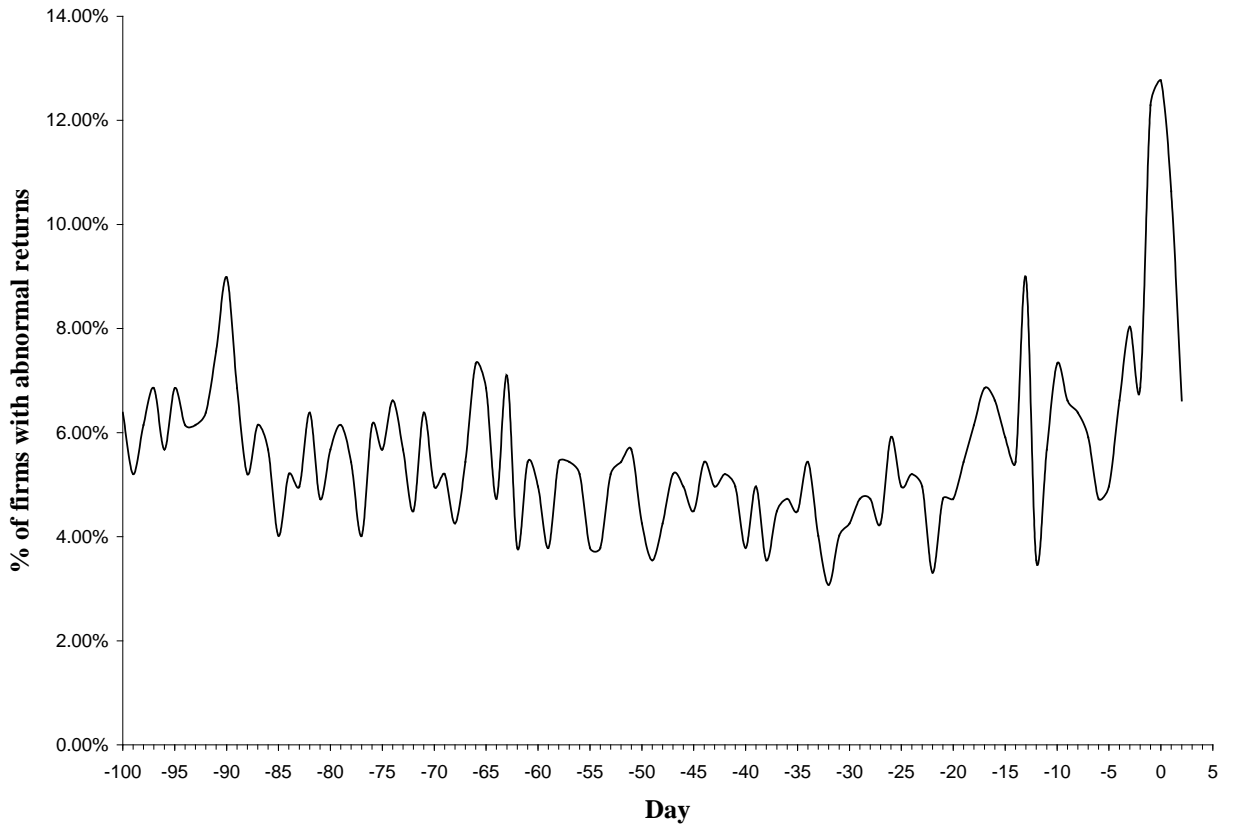


Fig. 1. Percentage of firms with statistically significant abnormal returns around the time of earnings announcements, 1935. The sample consists of 423 firms for which we could determine earnings announcement dates during the first half of 1935 that are also in advance of that company's Form 10 filing. Day zero for each firm is the day the *New York Times* reported the company's annual earnings. For each day from -100 to +2, the plot shows the percentage of the sample firms with positive or negative abnormal returns (based on a market model estimated over days -100 to -11) that are significantly different from zero at the 5% level.

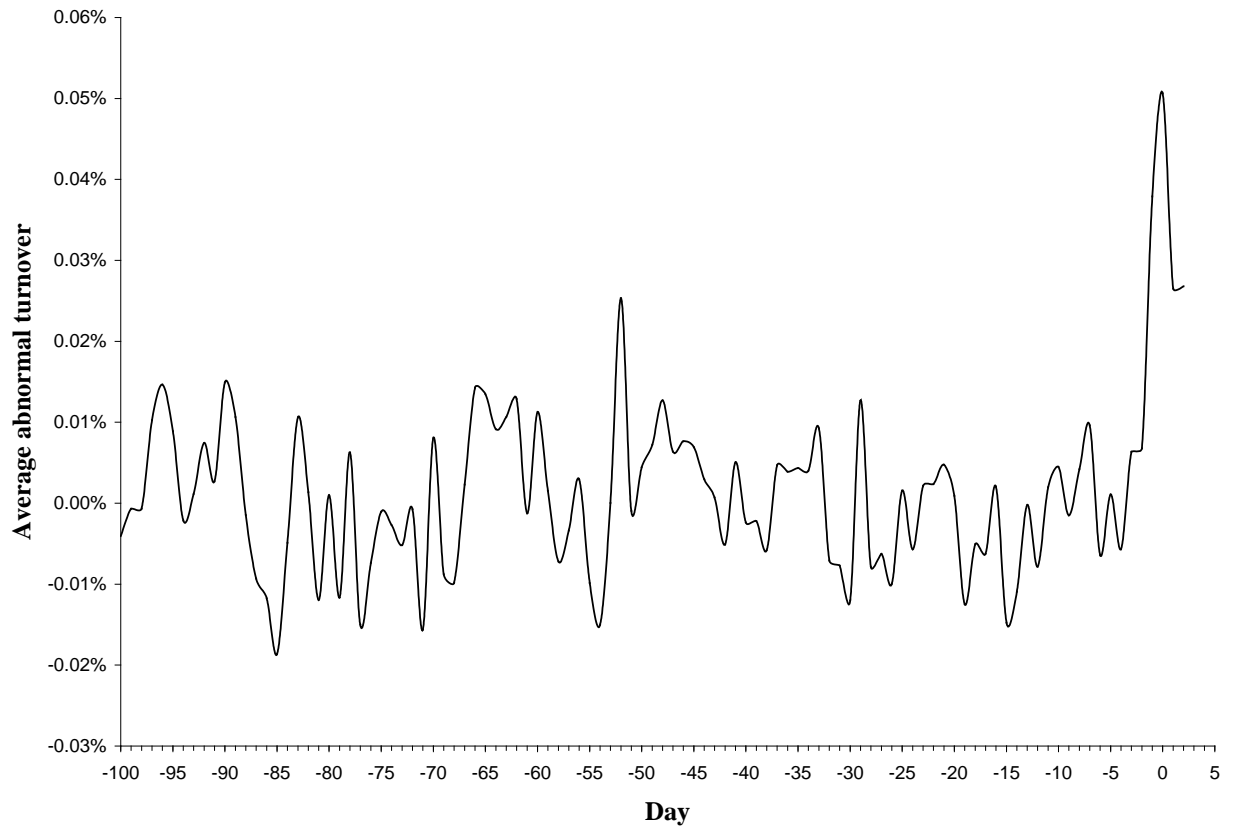


Fig. 2. Average abnormal turnover around the time of earnings announcements, 1935. The sample and interval are identical to those in Figure 1. For each day from -100 to +2, the plot shows the average abnormal turnover for the 423 sample firms (based on a trading volume market model estimated over days -100 to -11).

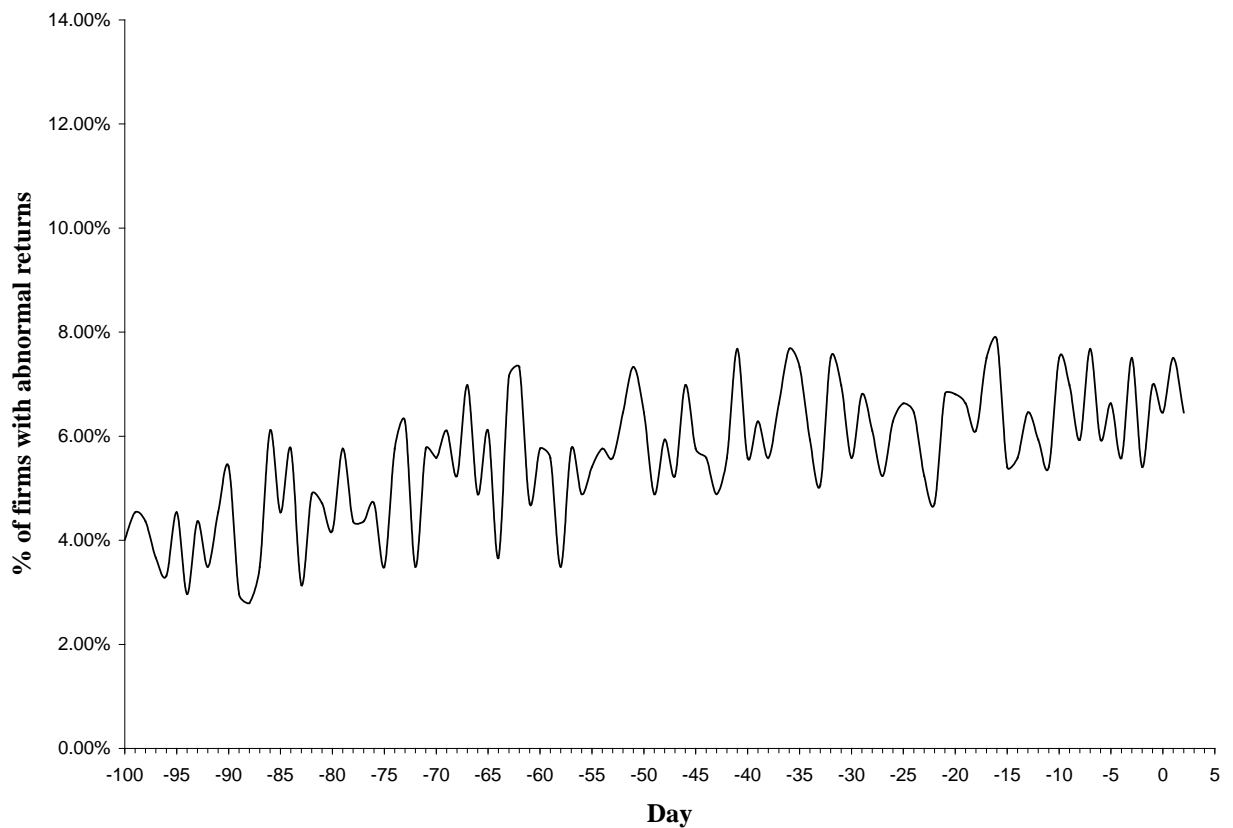


Fig. 3. Percentage of firms with statistically significant abnormal returns around the time of Form 10 filings. The sample consists of 573 non-railroad firms for which we could determine Form 10 filing dates prior to July 1, 1935. Day zero for each firm is the day after the Form 10 filing. For each day from -100 to +2, the plot shows the percentage of the sample firms with positive or negative abnormal returns (based on a market model estimated over days -100 to -11) that are significantly different from zero at the 5% level.

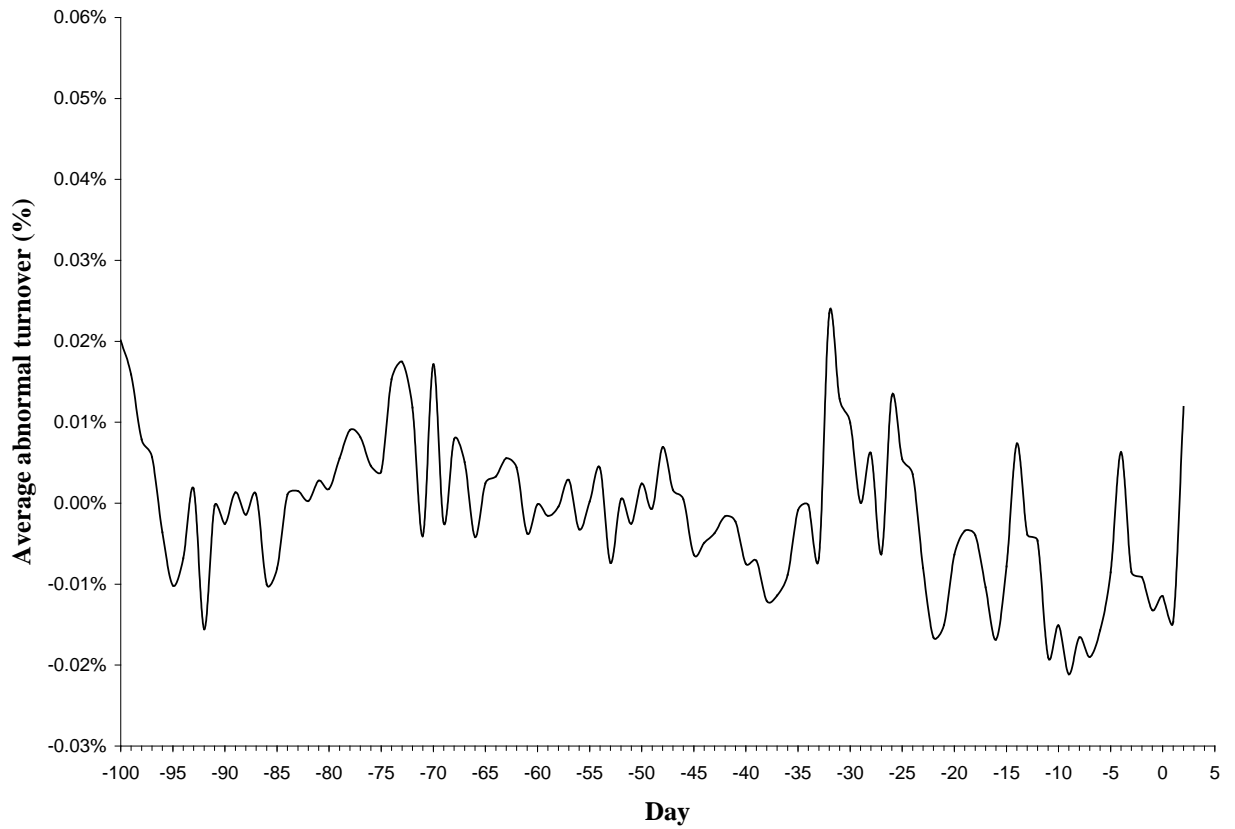


Fig. 4. Average abnormal turnover around the time of Form 10 filings. The sample and interval are identical to those in Figure 3. For each day from -100 to +2, the plot shows the average abnormal turnover for the 573 sample firms (based on a trading volume market model estimated over days -100 to -11).

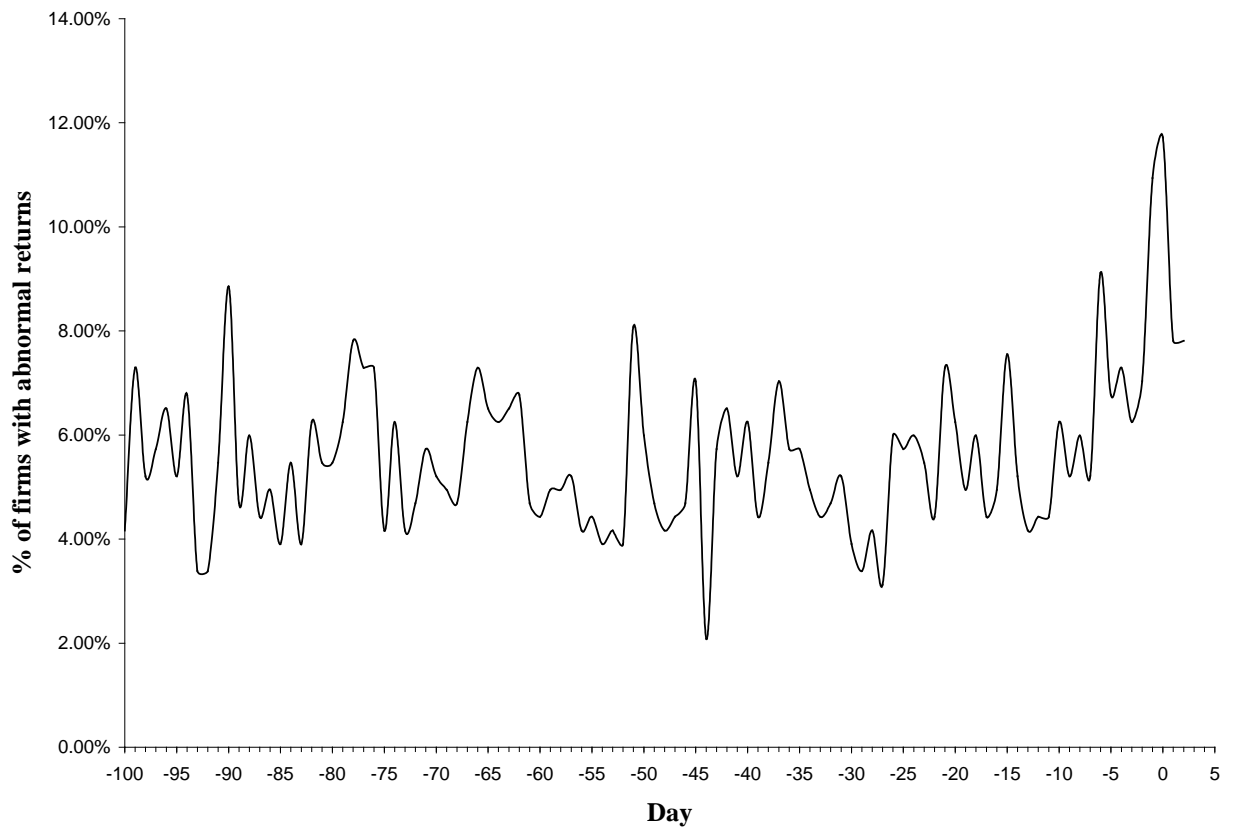


Fig. 5. Percentage of firms with statistically significant abnormal returns around the time of earnings announcements, 1927. The sample consists of 384 firms for which we could determine earnings announcement dates during the first half of 1927. Day zero for each firm is the day the *New York Times* reported the company's annual earnings. For each day from -100 to +2, the plot shows the percentage of the sample firms with positive or negative abnormal returns (based on a market model estimated over days -100 to -11) that are significantly different from zero at the 5% level.