What If There Were No Annual Reports? Evidence from the Great Postal Strike of 1970

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April 2022

Abstract

Whether annual reports to shareholders provide valuable information for investors' portfolio decisions has been the subject of considerable research, but the findings are mixed. Furthermore, establishing causality is challenging given the endogenous nature of financial reporting and the possible confounding effect of concurrent firm disclosures. We exploit the 1970 postal strike as an exogenous shock to the distribution of annual reports using an event study framework to draw causal inferences. We predict that "treatment" firms unable to deliver the annual reports to their shareholders will experience a decline in trading volume during the strike due to increased information asymmetry and adverse selection problems. Consistent with this prediction, we find that trading volume declined by 28% for "treatment" firms, whereas "control" firms registered almost no change (–0.1%). Such stark differences in trading behavior are not observed weeks before or after the strike and in the two contiguous years of the strike. Treatment firms experience an increase in systematic risk by 23% during the strike relative to control firms. The non-delivery of annual reports affected corporate bondholders' trading activity similarly. Overall, our study provides causal evidence on the importance of annual reports to corporate stakeholders.

JEL: M40, M41, G14, D82, D83

Keywords: Annual Report; Trading Volume; Information Asymmetry; Information Content

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"Sometimes you will never know the value of a moment until it becomes a memory." Dr. Seuss

1. Introduction

Does the annual report distributed to shareholders matter? Firms spend considerable effort, time, and money preparing the annual report to shareholders, which should contain a substantial amount of information (e.g., Singhvi, 1972; Fernandes, 1975; Pease, 1976).[1] So, one might expect annual reports to be value-relevant and useful to investors in their portfolio decisions. However, researchers report mixed findings on the information content of annual report and 10-Ks in the pre-EDGAR era (i.e., before 1994). In the post-EDGAR era, several studies support the information content of annual reports and 10-Ks (see Section 2.3 for more details). But, recent evidence (e.g., Li and Ramesh, 2009) suggests that stock market reactions to SEC filings are more likely due to confounding events or time-clustering of filing dates. In addition, endogenous disclosures by firms surrounding the filings and possible information transfer resulting from contemporaneous filings by peers may explain the market reaction surrounding 10-K filings. Thus, the information content of annual reports remains an open empirical question, and establishing causality is even more challenging.

We exploit the postal strike in 1970 as an exogenous shock to the distribution of annual reports that allows us to test for the information content of annual reports using an event study framework along with a *de facto* difference-in-differences (DiD) research design. In mid-March

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[1] While the content of annual report to shareholders is governed by Section 14 of The Securities Exchange Act of 1934, the reporting requirements under Schedule 10-K are governed under Section 13 and 15(d) of The Securities Exchange Act of 1934. Although annual reports to security holders may be combined with the required information of Form 10-K, during our sample period very few exercised this option (Singhvi, 1972). For the purpose of our paper, we do not distinguish 10-K filings from annual reports to shareholders. However, accessing 10-K at that time was costly, for example, through a visit to the SEC or a request of the document for a fee. Therefore, at a minimum, the evidence in our study can be viewed as the lower bound of information content of the annual report and 10-K put together.

1970, starting with the Grand Central Post Office in New York City, post offices in 671 other locations across the United States followed suit to join the strike that has come to be known as the Great Postal Strike of 1970. The strike lasted over two weeks, crippling the mail system, and had significant ramifications for the stock market as transfers of securities and payments from settlements were adversely impacted, leading to a reduction in the number of trades (Wall Street Journal, 3/24/1970; Kennedy, 1970).

The postal strike of 1970 is an ideal setting to examine the value of annual reports for several reasons. First, the strike occurred during a time that fortuitously coincided with the distribution of annual reports and proxy statements to shareholders before a scheduled annual general meeting for a significant number of firms. Second, the timing of the "wildcat" strike was completely unexpected, and therefore, the strike represents a clear exogenous shock to the distribution of annual reports.[2] The unexpected nature of this shock is akin to a randomized experiment enabling us to draw causal inferences on the stock market effects of annual reports. Third, although the strike affected many firms, there were enough firms unaffected by this strike to result in a reasonable "control" sample to test our hypothesis. Thus, we classify firms that were expected to deliver the annual report during the strike period but were unable to do so as "treatment" firms and the remaining firms as "control" firms.

We hypothesize that information asymmetry and adverse selection problems are exacerbated for investors of treatment firms relative to investors of control firms during the strike period. We use the insights from theoretical models in Wang (1994) and Kim and Verrecchia (1991) and subsequent empirical evidence in Chae (2005) to posit that the trading volume should be muted

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[2] A work stoppage undertaken by unionized employees without the consent of their respective unions is commonly referred to as a wildcat strike. Oftentimes, these strikes occur unexpectedly and without prior notification. Postal workers, although unionized, like all federal employees, were forbidden to strike and advocate for the right to strike (https://aflcio.org/about/history/labor-history-events).

for firms that expect to deliver annual reports to shareholders but are unable to do so due to the postal strike. Theory suggests that prior to a scheduled disclosure event (e.g., earnings announcement or an SEC filing), some investors have incentives to gather private information and profit from trading on such information. Consequently, uninformed investors would be reluctant to trade immediately before the event, causing a reduction in trading volume.[3] In contrast, during the strike, some investors, particularly institutions, have incentives and the ability to obtain annual report information (e.g., by visiting or making phone calls to the firm's corporate office, requesting the firm to send annual report via courier service, etc.), thereby increasing information asymmetry and adverse selection for uninformed investors. To the extent that the annual report contains value-relevant information, we expect treatment firms to have relatively lower trading volume than control firms during the strike period.[4] Our identification strategy is analogous to that of prior studies that evaluate the value of information in the context of sudden stoppages of information flows (e.g., Peress, 2014; Koudijs, 2016) and those that evaluate the importance of corporate executives like CEOs and independent directors in the setting of sudden executive deaths (e.g., Hayes and Schaefer, 1999; Nguyen and Nielsen, 2010).

We test our hypothesis using a hand-collected sample of 274 firms and classify them into treatment and control groups depending on whether a firm was affected by the strike in delivering the annual report. We find that the stock trading volume during the strike period (March 15–April

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[3] To the extent that the annual report is not anticipated by uninformed investors, they will be more willing to trade with informed investors rather than delay their trade. Such a situation would serve to go against the grain of our hypothesis.

[4] We focus on trading volume, as opposed to price reactions, because we are attempting to discern market reactions in the absence, rather than the presence, of an expected disclosure. With thin trading, it would be difficult to draw meaningful inferences from price changes. Further, the direction of price change will depend on market expectations about good news or bad news as well as the precision of information in the reports, making it difficult to generate unambiguous predictions. In addition, Cready and Hurtt (2002) suggest the importance of volume-based analysis especially when sample sizes are small.

4, 1970) declined considerably (14%), on average.[5] More importantly, the decline in trading volume is observed for the treatment group (28%) but not the control group (0.1%). Multivariate analysis confirms this finding. In economic terms, treatment firms experienced a significant 29% decline in abnormal trading volume (as a percentage of shares outstanding) during the strike period relative to the control group. This speaks to the importance of annual reports to stakeholders.

We confirm the robustness of this finding by conducting two analyses. First, we perform two falsification tests to examine whether the differential trading volume pattern observed during the strike period is attributable to the annual report delay caused by the strike. Specifically, we examine the differences in trading volume between treatment and control firms for (i) two contiguous three-week periods before and after the strike (i.e., February 22–March 14 and April 5–April 25 of 1970), and (ii) the same three-week period as the strike period for two contiguous years (i.e., March 15–April 4 of 1969 and 1971). We do not find any significant differences in trading volumes between treatment and control groups in all alternative event windows, confirming that our finding of reduced trading volume is unique to treatment firms during the strike period. Second, we examine whether the reduced trading volume for treatment firms during the strike period is consistent with the adverse selection channel. Specifically, we find that the reduction in trading volume occurs primarily for treatment firms with considerable ex ante information asymmetry. In other words, when ex ante information asymmetry is low, investors are less incentivized to collect private information, mitigating the adverse selection problem.

We next investigate whether the change in information asymmetry during the strike period affects the firm's cost of equity capital. Levi and Zhang (2015) indicate that temporary increases in information asymmetry can lead to changes in expected returns. Consistent with the implication

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[5] Although the strike started on March 18, 1970, and ended on April 2, 1970, our beginning and end dates include the entire week to accommodate mail already in the postal system.

of their paper, we find that treatment firms experience a 23% increase in market beta relative to control firms during the strike period, but market betas are not statistically different between treatment and control firms before and after the strike. Furthermore, the market beta effect during the strike period also varies cross-sectionally with the level of ex ante information asymmetry. That is, the increase in market beta is more pronounced for treatment firms with high levels of ex ante information asymmetry.

Is it the case that the reduced propensity to trade is pronounced for retail investors who are more likely to face the adverse selection problem stemming from increased information asymmetry? The answer is affirmative. While retail investors reduce their willingness to trade, institutional investors, who are more likely to be better informed, do not. In our last set of tests, we examine whether the reduction in trading volume extends to bond trading activity. Our exploratory analysis suggests that the non-delivery of annual reports also affects bond trading.

Our paper contributes to the accounting and finance literatures in the following ways. First, our study provides causal evidence that annual reports are valuable to investors. As argued by Li and Ramesh (2009), disclosure confluence and interactions among events within a disclosure timeframe make it difficult to draw conclusions about information effects of annual report disclosure (see also Ecker, Francis, Olsson, and Schipper, 2006; Arif, Marshall, Schroeder, and Yohn, 2019). As a natural experiment, the postal strike enables us to address the disclosure confluence problem and quantify the usefulness of annual reports. Second, our paper contributes to recent evidence by Koudijs (2016) that documents a puzzling result that return volatility is still considerable in the absence of news. Koudijs (2016) conjectures that the higher return volatility during no-news days is due to order flow imbalances from the lower trading volume. By examining

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trading volume during the postal strike when information about firms' financials is temporarily unavailable, we offer support for this conjecture.

2. Institutional Background and Hypothesis Development

2.1 The postal strike of 1970 and disruptions in annual report delivery

On March 17, 1970, at the stroke of midnight, post office workers at New York Branch 36 that served Bronx and Manhattan voted to go on strike. This branch was the largest one in the National Association of Letter Carriers (a labor union for postal workers), with over 8,000 members. The reason for the strike was dissatisfaction with pay and working conditions (Shannon, 1978). The "wildcat" strike (i.e., a strike without union authorization) occurred on March 18, beginning with New York Branch 36, and spread quickly to other neighboring cities in New Jersey and Connecticut (New York Times, 3/19/1970). By March 19, the strike spread west and northeast to Akron, Boston, Colorado, Cincinnati, Detroit, Houston, and Pittsburgh, with strikers numbering more than 200,000 out of 739,000 postal workers in 671 post offices within five days.[6]

A court injunction ordering the postal workers back to work was met with resistance. By the end of that week, the strike had crippled mail service for over 30 major cities (Time, 3/30/1970), resulting in either disrupted service or a complete shutdown. Through an Executive Order, President Richard Nixon deployed military personnel to help move the mail, but this effort was largely ineffective due to their inexperience and sabotage by striking carriers. Collective bargaining negotiations began within the week, and although some postal workers returned to work during the negotiations, the strike officially ended on April 2, 1970, with a Memorandum of Agreement that included, among other things, a general wage increase both retroactively and prospectively (Kennedy, 1970).

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[6] See https://www.nalc.org/news/the-postal-record/2020/march-2020/document/Strike.pdf.

This unexpected disruption had severe implications for financial markets. It paralyzed Wall Street as checks, stock certificates, and financial documents, which we take for granted in today's environment, never arrived, forcing the New York Stock Exchange to consider a market shutdown. During the strike, stock exchanges experienced lower trading volume (Wall Street Journal, 3/31/1970), and there were concerns about possible credit and operational disruptions for brokerage firms (Wall Street Journal, 3/19/1970). Furthermore, pertinent to our research question, some companies were considering postponing stockholder meetings because proxy statements and annual reports could not reach the shareholders (Wall Street Journal, 3/20/1970) (see Appendix 2 for examples).

2.2 Annual report mailing requirements during the 1970s

Sections 14(a) and 14(c) of the Securities Exchange Act of 1934 specify the reporting requirements for the annual report to stockholders. The annual report requirement is different from the SEC 10-K filing rule in that Form 10-K contains more detailed information about financials. The SEC requires that Form 10-K be filed within 120 days of the fiscal year-end, while the annual report to the shareholders is filed with the SEC no later than the date on which the report is mailed to shareholders (Section 14a-3(c)).[7] While the annual report is sent to the shareholders free of charge, an investor interested in obtaining the 10-K filing will have to visit the SEC office or a stock exchange.[8] The annual report to stockholders contains management's discussion and analysis of financial condition (MD&A), wherein the management is required to provide a description of matters that "will enhance a reader's understanding of the registrant's financial

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[7] Management has an option to file the annual report along with the 10-K although very few firms exercised this option during the sample period (Singhvi, 1968; Singhvi, 1972). In recent years, however, many public companies have combined or replaced the annual report to stockholders with the Form 10-K.

[8] Copies of the 10-K can be requested from the SEC for a fee, and the expectation at that time was such requests will be mailed with five days of the request (Singhvi, 1972).

condition, cash flows and other changes in financial condition and results of operations" (Item 303

of Regulation S-K). Given the proximity of the mailing requirements of the annual reports and 10-

K, it is difficult to distinguish the two from a valuation usefulness standpoint. Therefore, we view

our study as evaluating the information content of the annual report and 10-K put together.[9]

The annual report, along with proxy materials, is usually mailed to the shareholders at least

20 days before the annual general meeting" (Section 14a-3(b)).[10] Independent of the Securities

Exchange Act, the New York Stock Exchange (NYSE) recommends a minimum of 30 days be

allowed between the proxy record date and the annual meeting date, while the American Stock

Exchange (AMEX) suggests a period that is at least 20 days in advance of the meeting (Pease,

1976).

During our sample period, firms were evenly split when using first class or third/fourth

class (bulk) mail postal service for distributing the annual report and proxy materials (Pease, 1976).

Third-class mailing is considered desirable by many companies as it offers considerable cost

savings without significant time lags in delivery and satisfies the Rule 14 (a) mailing deadlines.

2.3 Information content of the annual report to shareholders

Considerable research (e.g., Foster and Vickrey, 1978; Firth, 1981; Stice, 1991; Cready

and Mynatt, 1991) examines the usefulness of SEC 10-K filings and annual reports to shareholders

in the pre-EDGAR era but finds mixed evidence.[11] However, in the post-EDGAR period, several

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[9] One could argue that proxy materials are also part of the package sent to shareholders, which could also contain valuation relevant information. However, we believe that the quality and quantity of information contained in the annual report is greater, on average. Furthermore, any important proxy solicitations such as mergers or share issuances are known to the shareholders in advance through media.

[10] In recent times, however, firms can substitute the mailing of proxy materials by posting the materials on a publicly accessible website.

[11] In the pre-EDGAR era, papers that provide support for information content of 10-K and annual reports include Foster and Vickrey (1978), Firth (1981), and Kwon and Wild (1994). In contrast, a significant number of papers do not find evidence in support of information content (e.g., Foster, Jenkins, and Vickrey, 1983; Foster, Jenkins, and Vickrey, 1986; Cready and Mynatt, 1991; Stice, 1991; Easton and Zmijewski, 1993; Asthana and Balsam, 2001).

papers document significant stock market reactions surrounding the 10-K filings supporting the valuation usefulness of the annual report.[12] Li and Ramesh (2009) question the results in these papers by documenting that the SEC 10-K filing dates are contaminated by concurrent earnings releases and clustering around calendar quarter-ends. In particular, they find that the trading volume around 10-K filings is indistinguishable between firms that filed 10-K forms with firms that did not. Although they are careful to conclude that their evidence does not suggest that "SEC filings have no economic or informational value," their findings point to the challenge with interpreting prior results due to the effects of disclosure endogeneity and the potential confluence of news events surrounding corporate disclosures (see also Ecker et al., 2006; Arif et al., 2019).

2.4 Hypothesis development

The coincidental timing of the unexpected postal strike of 1970 surrounding the annual report distribution of some public companies presents an ideal setting to examine the usefulness of annual reports. Unlike most disclosure studies that examine the effect of an exogenous shock on a particular disclosure, our setting focuses on the absence of an important financial report that should have been disseminated to shareholders. We examine how traders respond to this non-availability of annual reports as evidence of the reports' usefulness and information content. In particular, we determine whether the propensity to trade changed during the strike period for firms unable to deliver the annual report.

We appeal to the theoretical research on information asymmetry to develop a hypothesis about trading behavior during the strike period. Traditional asymmetric information models (e.g., Glosten and Milgrom, 1985) consider two classes of traders: informed and uninformed liquidity traders. Informed traders either are endowed with private information or have incentives to acquire

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[12] For example, Qi, Wu, and Haw (2000), Asthana and Balsam (2001), Griffin (2003), Asthana, Balsam, and Sankaraguruswamy (2004), You and Zhang (2009), and Doyle and Magilke (2013).

private information about an anticipated disclosure event, making uninformed investors reluctant to trade. If uninformed traders have discretion with the timing of their trades—that is, they do not have to trade for liquidity reasons and thus can postpone trade for a short period—information asymmetry can result in decreased trading volume (e.g., Admati and Pfleiderer, 1988; Foster and Viswanathan, 1990). Kim and Verrecchia (1991) build a model in which investors acquire private information before an anticipated news announcement and achieve profits through informed trading. While Kim and Verrecchia (1991) primarily focus on the volume effects at the announcement date, their model also has implications for volume effects prior to the announcement. Wang (1994) shows that when uninformed investors perceive the presence of private information with informed investors, they face the adverse selection problem of trading with a party with superior knowledge and, hence, will have fewer incentives to participate in the market (also see Milgrom and Stokey, 1982; Chae, 2005).

In the context of the postal strike, although the strike crippled the delivery of annual reports to all investors, the willingness of some investors, particularly institutions, to engage in information acquisition about the contents of the annual report may be heightened during the strike period. This is because institutional investors can obtain annual report information through a visit to the head office, making phone calls, requesting faxes, and so on. The extent of endogenous information acquisition will be a function of the perceived information benefits of the annual report and attendant trading profits from acquiring the information. Because the annual report delivery is anticipated, the uninformed investor expects high trading demand from informed investors and, hence, would be reluctant to trade if delivery does not occur (e.g., Kim and Verrecchia, 1991). In other words, the exogenous delay in annual report delivery is expected to exacerbate the information asymmetry between informed and uninformed investors and increase the adverse

selection problem during the strike period, leading to a lower trading volume. However, to the extent that the information in the annual report is perceived as lacking valuation usefulness, the incentives to collect private information prior to the actual delivery are lower. As such, we should observe no change in trading volume under this scenario.

3. Research design and sample selection

3.1 Research design

We use an event study design that compares the abnormal trading volume during the strike period for treatment firms relative to control firms. We estimate the following specification:

$$AVOL_{i,t} = \delta_0 + \delta_1 \, TREAT_i + \lambda Z_{i,t} + \mu_{i,t}, \tag{1}$$

where $AVOL_{i,t}$ is the abnormal trading volume defined as the difference between average trading volume in the dates matching the strike week (March 15–April 4) and that in the three-week period prior (February 22–March 14). Although the strike began on March 18 (Wednesday) and ended on April 2 (Thursday), we include the entire weeks surrounding the strike to accommodate mails already in the postal system. *TREAT* is an indicator variable for treatment firms; $Z_{i,t}$ represents a vector of control variables, including industry fixed effects. We consider the following control variables: firm size (*Log (MVE)*), proxied by the natural logarithm of market capitalization at the fiscal year-end, book-to-market ratio (*B/M*, computed as the ratio of the book value of common and preferred stock divided by market capitalization), and stock returns (*RET_YTD*) during the calendar year before the strike period (i.e., January 1–March 14). δ_1 captures the differential abnormal trading volume during the strike period for treatment firms. We expect δ_1 to be negative if the unavailability of annual reports deters uninformed investors from trading, resulting in lower trading volume.

3.2 Sample selection

To identify our sample firms, we download all available scanned copies of annual reports from the ProQuest Historical Annual Report database for three consecutive fiscal years, 1968, 1969, and 1970. We collect annual reports for two contiguous years of the strike to conduct falsification tests. We retain only public firms, as we require stock trading data and financial information, and discard firms that did not disclose annual meeting dates. We also remove firms that do not have returns and accounting data on Compustat and CRSP databases, resulting in a final sample of 274 unique firms.[13] Table 1 provides the step-by-step process of sample selection.

From this sample, we classify firms unable to deliver the annual report due to the strike as treatment firms. Nevertheless, it is not straightforward to determine whether a firm's annual report delivery is affected by the strike since the exact date of annual report mailing is unavailable. Therefore, we determine which firms' annual reports are most likely to be delayed due to the strike by examining the extent to which a firm's annual report "mailing period" overlaps with the "strike period." To accomplish this, we first ascertain the annual report mailing period for each firm. While firms do not specify the exact mailing date of the annual report, a significant number of firms mail the annual report along with the proxy materials to save mailing costs and specify an approximate proxy mailing date in the annual report (Pease, 1976).[14] Of the sample firms, 76 (28%) state the proxy mailing date in the annual report. For these firms, we use the proxy mailing date as the *beginning* of the mailing period. When the proxy mailing date is unavailable, we use the audit signature date or the chairman letter date, whichever is later, plus 23 days. We base the 23 days on

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[13] Note that this sample represents 13.5% of the total number of firms in the CRSP-Compustat merged database, with about 45% of the overall market capitalization. About 66% of the sample firms are part of the S&P 500.

[14] Section 14a-3(b) of the Securities Exchange Act, 1934, requires that the annual report is received no later than the proxy material. Therefore, the annual report is either sent along with the proxy materials or separately prior to the mailing of the proxy material. In addition, even if the mailing date is known, the time it takes to reach the shareholders will vary depending on whether the firm chooses first class or third and fourth (bulk) class mail.

the median time interval between chairman letter and proxy mailing dates for firms that report the proxy mailing date.[15]

The *end* of the mailing period is determined by the annual meeting date. We use the annual meeting date minus 30 (20) days for NYSE (AMEX) firms as the end date of the annual report mailing period.[16] If the estimated mailing period is shorter than one week (i.e., seven days), we adjust the mailing period to be at least one week long to reflect the time it takes for the mailing to occur and receipt by the shareholders. Specifically, for firms with an estimated mailing period shorter than a week, we change the ending date of the mailing period to seven days after the beginning date if the proxy mailing date is known; if the proxy mailing date is unknown, we change the beginning date of the mailing period to seven days before the ending date.

We calculate the overlap between the estimated mailing period and the postal strike period (March 15–April 4, 1970) as a percentage of the overall length of the mailing period. Specifically, we divide the number of days of the mailing period that overlap with the strike period by the length of the mailing period. We classify treatment firms with an indicator variable *TREAT* that takes the value of 1 when the overlap percentage exceeds the sample median, and zero otherwise. Figure 1 illustrates the mapping between the mailing period and the strike period that ultimately determines the treatment and control groups. The figure provides a timeline for three sample firms, one of which falls in the treatment group (Chrysler) and two of which fall in the control group (General Motors and Caterpillar). The figure also contains a table with various dates obtained from the respective annual reports that help determine the mailing period.

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[15] While the audit signature date is available for all firms, the chairman letter date is missing for 73 firms. In these instances, we replace the missing chairman letter date with the audit signature date plus 14 days, which is based on the median time interval between audit date and letter date for all other firms that had both dates available.

[16] We chose the 30 (20) days to be consistent with the recommendations by NYSE (AMEX) (see Pease, 1976). Our sample consists of 270 (4) NYSE (AMEX) firms.

4. Empirical results

4.1 Descriptive statistics

Table 2 provides the descriptive statistics of abnormal trading volume and various control variables. In Panel A, we present descriptives for the strike period—March 15 to April 4, 1970. Note that the abnormal trading volume (*AVOL*) is significantly negative during the strike period (mean = –0.141) at the 1% level (*t*-statistic = –3.22; not reported). This evidence is consistent with the reduced pace of trading activity in stock exchanges (New York Times, 03/24/1970). The average firm size is $1,033 million, and the average book-to-market ratio is 0.81. The mailing period's mean (median) overlap with the strike period is 46.4% (37.5%). Panel B presents the correlation matrix. Noteworthy, the correlation between *TREAT* and *AVOL* is negative and significant, indicative of lower trading volume for treatment firms relative to control firms during the strike period.

Our treatment (control) firms comprise 134 (140) firms whose mailing period overlap with the strike period is above (below) the median overlap percentage of 37.5%.[17] When we examine differences in the characteristics of the treatment and control firms, we find no statistical difference in firm attributes such as size, book-to-market, and returns before the strike period (refer to Panel C of Table 2). The mean (median) abnormal trading volume for the treatment firms is significantly negative, –0.28 (–0.13), whereas that for the control firms is small, –0.01 (–0.03). The mean and median abnormal trading volume values are significantly different between the treatment firms and control firms (at the 1% level). Thus, while the average trading volume declined during the strike period due to the general lack of information in the market, treatment firms experienced a much greater decline in trading volume than control firms.

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[17] In robustness tests, we consider other alternatives to classify treatment and control firms. Our inferences are unchanged if we use the alternative classifications.

In Panel D of Table 2, we provide the industry composition for the treatment and control samples. We do not find a significant clustering in any one industry group, although the manufacturing industry has the most sample firms. More important, the matching between the treatment and control groups across each industry appears reasonable.[18]

4.2 Impact of strike on abnormal trading volume

Table 3 presents the results of estimate equation (1). In terms of other determinants of trading volume, we find that bigger firms and firms with higher returns during the pre-strike period have higher trading volume during the strike period (i.e., the coefficients on *Log (MVE)* and *RET$_{YTD}$*). More important, the coefficient of interest, *TREAT*, is negative and statistically significant (coefficient = –0.285; *p*-value < .01) after controlling for other determinants of trading volume.[19] This finding indicates that treatment firms that could not deliver their annual reports experienced a greater reduction in trading volume relative to their control counterparts. The effect is economically significant in that the coefficient represents a 29% reduction in trading volume (as a percentage of shares outstanding).

Is it possible that the treatment firms are somehow different from the control firms beyond the control variables in the empirical specification? We conduct two falsification tests. First, we examine the abnormal trading volume for the strike period in the year before and the year after the strike year (i.e., March 15–April 4 of 1969 and 1971). Suppose the abnormal trading volume is a manifestation of the particular time period during the year that happens to coincide with the strike period. We should then observe similar patterns for the treatment and control firms during the

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[18] As a robustness test, we exclude five industries in which either the treatment firms or the control firms comprise less than 40% of the total firms in the industry. Our results are similar (see Table 10).

[19] Our results are robust to several additional specification checks: (i) eliminating all non-December fiscal year-end firms and firms with earnings announcements during the strike, (ii) various clustering methods, and (iii) using raw trading volume instead of abnormal trading volume, while controlling for lagged trading volume.

same weeks in 1969 and 1971. Descriptive statistics presented in Panel A of Table 4 indicate that the difference between the treatment and control firms is not statistically different from zero in either 1969 or 1971. Panel A of Figure 2 depicts the trading volume effects for treatment and control firms with confidence intervals.[20] Next, we conduct multivariate analysis by estimating equation (1) for these two years.[21] Results in Panel B of Table 4 indicate that the coefficient on *TREAT* is not statistically different from zero. Thus, the differential trading volume patterns are unique to the three weeks of 1970.

Our second falsification test uses the three weeks before and after the strike period. If the trading volume behavior during the strike period is not specific to that event window in 1970, any random three weeks will show similar trading patterns. Descriptive statistics of abnormal trading volume for the weeks before (February 22–March 14) and the weeks after (April 5–April 25) indicate that the means are not statistically different from zero (see Panel A of Table 4). The one exception is that, for the median abnormal trading volume, the difference between treatment and control firms is significantly positive in the post-strike period. Panel B of Figure 2 depicts the results across contiguous periods surrounding the strike period pictorially.

The multivariate analysis presented in Panel C of Table 4 confirms the univariate findings. For completeness, we report regression results for the three weeks before and three weeks after the strike week for contiguous years. We find that the *TREAT* variable is not distinguishable from zero.

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[20] We conjecture that the large standard deviation for treatment firms in 1971 is due to two key changes that occurred in that year. First, the SEC required expanded disclosures in Form 10-K, and second, the SEC mandated quarterly reporting starting 1971 that is likely to have increased the number of firm disclosures during the event window (see the SEC annual report of 1971).

[21] Examining the two contiguous time periods separately tantamount to a *de facto* DID design where observations from the strike period and the two contiguous time periods are included in estimating equation (1) but augmented with *TIME* indicator for the strike period and interaction term *TREAT*TIME* as additional variables.

Thus, we can reasonably conclude that treatment firms' relatively lower abnormal trading volume is due to the non-delivery of annual reports during the strike.

4.3 Cross-sectional differences in the impact of the strike on trading volume

In this section, we examine whether the impact of the strike on trading volume varies cross-sectionally with the level of ex ante information asymmetry. As discussed earlier, in the presence of informed investors, uninformed investors should avoid trading due to adverse selection problems. Thus, the extent to which uninformed investors would be reluctant to trade will be a function of the ex ante level of information asymmetry (Chae, 2005). We modify equation (1) to test this hypothesis by interacting the *TREAT* variable with proxies for the ex ante level of information asymmetry. In particular, we estimate the following empirical specification:

$$AVOL_{i,t} = \alpha + \delta_1 \, TREAT_i + \delta_2 \, TREAT_i \times LOWINFASS_{i,PRE} + \lambda Z_{i,t} + \varepsilon_{i,t}, \tag{2}$$

where *LOWINFASS*$_{i,t}$ is a proxy for lower levels of information asymmetry three weeks prior to the event window. We expect the coefficient on δ_2 to be positive, because with lower levels of information asymmetry, the detrimental effects on trading volume are mitigated. Apart from the control variables included previously, we also include the main effect of *LOWINFASS*$_{i,PRE}$.

We consider two proxies for lower levels of ex ante information asymmetry.[22] First, we use an indicator variable (*LOWSPRD*$_{PRE}$) that equals one for firms in the bottom tercile of the average bid-ask spread during the three weeks before the strike (i.e., February 22–March 14). We compute the bid-ask spread using the highest ask and lowest bid prices each day because the closing bid and ask prices are not available during our sample period. As a result, our information asymmetry measure is susceptible to measurement error. For our second proxy, we use another

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[22] An arguably better proxy for information asymmetry would be the PIN measure advocated by Easley, Hvidkjaer, and O'Hara (2002). However, it is not feasible to estimate PIN during our sample period.

indicator variable (*LOWTVAR_PRE*) that equals one for firms in the bottom tercile of stock return volatility (i.e., the variance of stock returns) during the pre-strike period.

Table 5 reports the results from estimating equation (2) for the two proxies of ex ante information asymmetry, bid-ask spread, and return volatility. We provide results for the three-week strike window for all three years. For the year of interest, 1970, the results suggest that the coefficient on the interaction term (*TREAT×LOWINFASS_PRE*) is positive with a coefficient of 0.244 (0.348) and statistically significant at the 10% (1%) level for the bid-ask spread (return volatility) proxy. These results are consistent with our prediction that treatment firms with lower levels of ex ante information asymmetry have higher trading volume during the strike period. Furthermore, the sum of the main effect and the interaction effect for the treatment firms (i.e., $\delta_1 + \delta_2$) is not statistically different from zero (*F*-statistics = 1.28 and 0.26; not tabled). These results indicate that trading volume is lower during the strike period, predominantly for firms with high levels of ex ante information asymmetry (Chae, 2005).

When we estimate similar regressions for the year before and after the strike, reported in columns (3)–(6) of Table 5, neither the main effect, *TREAT*, nor the interaction term *TREAT×LOWINFASS_PRE* is statistically significant. These results further confirm that the effects obtained are due to the strike period in 1970 and not for any random years. Thus, we conclude that the annual report delay has a negative impact on trading volume, especially for firms with high levels of ex ante information asymmetry.

4.4 Did the non-delivery of the annual report increase systematic risk?

Easley and O'Hara (2004) and Hughes, Liu, and Liu (2007) suggest that information asymmetry can affect a firm's cost of capital. Further, the evidence in Levi and Zhang (2015) indicates that temporary increases in information asymmetry before information events such as

earnings announcements can also affect the cost of capital and expected returns. The argument is that traders have incentives to collect private information before scheduled events, thereby increasing the adverse selection problem that causes uninformed investors to ask for a higher cost of equity. Our evidence thus far indicates that an increase in information asymmetry will lead to a decline in trading volume (e.g., Chae 2005). In this section, we examine whether the increase in information asymmetry due to the non-delivery of the annual report increases a firm's cost of equity. Specifically, we examine whether the systematic risk changes during the strike period for the treatment firms relative to the control firms. An increase in systematic risk during the strike period would be consistent with investors demanding a higher expected return to compensate for greater information asymmetries arising from the non-delivery of the annual report.

We use the market model to examine shifts in the market betas. Specifically, we estimate the following empirical specification:

$$RET_{i,t} = \alpha + \beta_1 \, RM_t + \beta_2 \, RM_t \times TREAT_i + \beta_3 \, TREAT_i + \lambda Z_{i,t} + \varepsilon_{i,t}, \tag{3}$$

where $RET_{i,t}$ is the daily stock return for firm i, and RM_t is the daily market return. β_1 is market beta. Unlike the previous equations, $Z_{i,t}$ represents only industry fixed effects. A positive β_2 during the strike period would be consistent with the systematic risk effects of increased information asymmetry stemming from delayed delivery of annual reports. Stated differently, the delay in annual reports propagates private information acquisition and results in higher information asymmetry among investors, which in turn increases systematic risk and expected returns.

Table 6 presents our findings, beginning with descriptive statistics of the return variables in Panel A. In Panel B, columns (1)–(3), we present our results of estimation equation (3) before, during, and after the strike period, including industry fixed effects.[23] We find that the coefficient

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[23] Our results are similar if we use firm fixed effects instead.

on market beta is positive and statistically significant across all estimations. More pertinent to the study, we find that the interaction term is positive and statistically significant for the strike period (coefficient = 0.231; t-statistic = 2.96 in column (2)). In other words, treatment firms that couldn't deliver the annual report experienced a 27% higher (0.231/0.842) market beta during the strike period. This is not the case, however, in either the pre-strike or the post-strike periods (see columns (1) and (3)), providing support as a falsification test.

Next, we examine whether the increase in systematic risk during the strike period varies with the extent of ex ante information asymmetry. To the extent that changes in information asymmetry during the strike period drive the shift in systematic risk, we expect the β_2 coefficient to vary with ex ante levels of information asymmetry. To test this hypothesis, we modify equation (3) and introduce an interaction term ($RM_t \times TREAT_i \times LOWINFASS_{i,PRE}$) that captures the incremental systematic risk for treatment firms with low levels of information asymmetry. We expect the coefficient on the interaction term to be negative because, for firms with low levels of ex ante information asymmetry, investors will have fewer incentives to collect private information. As before, we use two proxies for low information asymmetry, i.e., $LOWSPRD_{PRE}$ and $LOWTVAR_{PRE}$. We also include the interaction term, $RM_t \times LOWINFASS_{i,PRE}$, to capture cross-sectional average differences in systematic risk for low information asymmetry firms.

In the results presented in Table 7, we find evidence consistent with our prediction. We report results for both proxies of ex ante information asymmetry $LOWSPRD_{PRE}$ and $LOWTVAR_{PRE}$ in columns (1)–(3) and columns (4)–(6), respectively. We find that the coefficients on the triple interaction terms ($RM \times TREAT \times LOWSPRD_{PRE}$ and $RM \times TREAT \times LOWTVAR_{PRE}$) are both negative and statistically significant at the 1% level (coefficient = –0.501 and –0.373; t-statistic = –3.37 and –2.53) in columns (2) and (4). What is also noticeable is that the negative coefficient on

the triple interaction term is similar in magnitude to the positive coefficient for the $RM_t \times TREAT_i$ interaction term. The sum of these two coefficients is statistically indistinguishable from zero (*F*-statistic = 0.93 and 0.05), suggesting that the increase in systematic risk during the strike period is attributable primarily to firms with high levels of ex ante information asymmetry.

4.5 Did the non-delivery of the annual report affect retail investors more than institutional investors?

Our hypothesis on reduced trading volume for treatment firms during the strike period is predicated on the notion that some investors have incentives to get informed in the wake of an impending annual report disclosure, leading to exacerbated information asymmetry during the strike period. While it may be costly for retail investors to obtain private information during the strike period, institutional investors may have both the incentives and ability to become more privately informed during the strike period by (i) visiting the company's head office to obtain a physical copy of the annual report, (ii) making phone calls to obtain key information contained in the annual report, (iii) requesting reports to be sent through alternative modes such as courier service, or (iv) requesting copies via facsimile. As a result, although institutions want to trade during the strike period to generate arbitrage profits, retail investors anticipating the private information collection would be reluctant to trade except for liquidity reasons that cannot be postponed. Therefore, we expect that the decline in trading volume during the strike period is more attributable to lower retail trades than institutional trades.

Unfortunately, data on institutional ownership or institutional trades are unavailable for our sample period. Instead, we take advantage of the limited data available on odd-lot trades to provide explorative evidence on the extent to which the reduction in trading volume is due to retail trades. Odd-lot trades are trades of small sizes, usually trades of less than 100 shares. Considerable research attributes odd-lot trades to retail investors primarily because such investors are more

likely to use small orders due to capital constraints (e.g., Ritter, 1988).[24] We hand-collect odd-lot trading for NYSE firms from the SEC Statistical Bulletin that reports odd-lot trading for 100 NYSE stocks.[25] We are able to obtain odd-lot trades for 43 of the sample firms, of which 21 are treatment firms. See Panel A of Table 8 for descriptive statistics for the odd-lot trade sample.

We determine abnormal trading volume from retail investors ($AVOL_{RETAIL}$) as the average daily odd-lot trading volume during the strike week (March 15–April 4) minus that for the three weeks prior (February 22–March 14). Odd-lot trading volume is measured as the sum of odd-lot purchases and sales divided by the total number of shares outstanding. Note that odd-lot data is provided on a weekly basis, and we convert it to a daily basis by dividing the weekly trades by the number of trading days in the week. The average abnormal volume of retail investors is negative (–1.5%) during the strike period, consistent with the average reduction in volume reported in Table 2, Panel A, albeit much smaller in magnitude. We determine the abnormal institutional trading volume ($AVOL_{INST}$) as the residual from regressing total abnormal trading volume ($AVOL$) on retail abnormal trading volume ($AVOL_{RETAIL}$). As a result, the $AVOL_{INST}$ has a mean of 0.

While the average abnormal trading volume of retail investors declines during the strike period, the abnormal trading volume for the treatment group declines in a statistically significant manner (t-statistic = –2.04; not tabled) relative to the control group (t-statistic = –0.84; not tabled). After controlling for industry fixed effects, Panel B of Table 8 reports the results of estimating equation (1) for the strike period, separately for $AVOL_{RETAIL}$ and $AVOL_{INST}$ in columns (1)–(2) and columns (3)–(4), respectively. We report univariate regression results in columns (1) and (3) and

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[24] In recent times, however, with the advent of algorithmic trading and high-priced stocks such odd lots represent a considerable fraction of trades and, hence, cannot be easily categorized as coming from retail trades or uninformed trading (see O'Hara, Yao and Ye, 2014).

[25] The odd-lot series consists of 100 stocks listed on NYSE, furnished to the SEC by one odd-lot dealer firm. The list of stocks often includes stocks in the DJIA and other market leaders. The list of stocks changes from time to time (see the SEC Statistical Bulletins of 1970).

results with control variables in columns (2) and (4). Evident from columns (1) and (3), the coefficient on *TREAT* is negative and statistically significant for the $AVOL_{RETAIL}$ (coefficient = –0.028; *t*-statistic = –2.03) but not for $AVOL_{INST}$ (–0.111; *t*-statistic = –0.50). After including the control variables, the coefficient on $AVOL_{RETAIL}$ is still negative, but it is not statistically significant at conventional levels. The coefficient on $AVOL_{INST}$ is not significant after including the control variables (see column (4)).

Overall, the results in Table 8 suggest uninformed retail investors are more reluctant to trade during the strike period than informed institutional investors. This evidence offers supportive evidence for the role of the annual report in reducing information asymmetry for retail investors. This evidence augments Asthana, Balsam, and Sankaraguruswamy's (2004) finding that small investors differentially benefit from the 10-K filings disclosed through the electronic EDGAR system as they tend to trade more surrounding the 10-K release. However, given the small sample size of the odd-lot trades, we advise caution in interpreting findings based on this sample.

4.6 Did the non-delivery of the annual report affect corporate bond trading?

This section examines whether the non-delivery of annual reports during the postal strike affects corporate bond trading. Prior research by Easton, Monahan, and Vasvari (2009) finds that the incidence of bond trade increases during the days surrounding earnings announcements. Furthermore, recent research by Jiang and Sun (2015) documents that trading volume in corporate bonds spikes before the release of scheduled macroeconomic news but not before scheduled firm-specific news. Unlike other news dissemination such as earnings or dividends announcements that are publicly disclosed, the annual report to shareholders must be mailed to the equity shareholders, not to the bondholders. Furthermore, bondholders face an asymmetric payoff function, and hence, the information content beyond earnings may be less relevant unless there is bad news. Given prior

evidence on the bond trading effects of firm disclosures, if annual reports contain valuable information to bond traders, we would expect similar trading behavior for bonds as with equity.

We estimate equation (1) replacing equity *AVOL* with abnormal trading volume for bonds ($AVOL_{BOND}$). We use the historical Wall Street Journal (Microfiche) to hand-collect daily bond trading volume. There are important differences in how corporate bond trading activity is reported relative to equity trading. First, WSJ reports the dollar amount of bonds traded (in $000) instead of the number of bonds traded. Therefore, to compute $AVOL_{BOND}$, we use the average daily trading volume during the strike period minus the average daily trading volume in the previous three-week period, divided by the firm's total debt. This way, we can capture the abnormal bond trading activity adjusted for scale and mimic the abnormal trading volume variable for equity. Second, unlike equity trading, bond trading is more infrequent on average, and even when traded, the amount traded is not very large.

We can obtain bond trading data for 35% of the full sample (96 firms), of which 50% are treatment firms. The average daily trading activity for each firm during the strike period and three weeks before was $23,000 (results not tabled). In comparison, the average debt outstanding is $325m. Therefore, as mentioned earlier, the daily bond trading activity is much smaller in magnitude relative to equity markets. Panel A of Table 9 indicates that the abnormal bond trading volume during the strike period is 0.008. In economic terms, this change is minimal (1/1000th of 0.8%) because the variable has been multiplied by 1,000 for expositional convenience. More relevant to our study is whether the abnormal trading volume changes differentially for treatment and control firms.

Panel B of Table 9 suggests that while the bond trading declines for treatment firms on average (mean = –0.014), it increases for the control firms (mean = 0.031). However, the difference

is weakly significant ($p < .12$; not tabled). Multivariate regression results are presented in Panel C, with (column (2)) and without (column (1)), including the control variables. In both columns, we find an incremental decline in abnormal trading volume for treatment firms. In addition, both effects are statistically significant at the 10% level (two-tailed). Together, the evidence in Table 9 indicates that, like equity trading volume, bond trading volume exhibits a similar decline as the equity market for treatment firms relative to control firms. However, we caution the reader that the evidence is based on a smaller sample, with the economic magnitudes being relatively small.

4.7 Additional tests

In this section, we consider several additional tests to ensure the robustness of our empirical findings. First, Li and Ramesh (2005) point to the contaminating effect of annual reports clustering around calendar quarter ends. Is this problem salient to our study? While most treatment firms in our sample have December fiscal year-ends (98%) because the strike period coincides with when annual reports to shareholders are usually distributed by such firms, our control group also contains a good number of December year-end firms (54%). To the extent that non-December fiscal year-end control firms may have firm characteristics different from December year-end firms, it is possible that our control sample is biased. To ensure comparability across treatment and control samples, we eliminate the non-December fiscal year-end firms and estimate equation (1). Results presented in column (1), Panel A of Table 10, suggest that excluding non-December fiscal year-end firms does not alter the effects on trading volume. The magnitude of the effects is also unchanged, and hence, the economic significance is unaltered. Moreover, our comparison of the industry groups across the treatment and control samples presented in Table 2, Panel D, is indicative of common characteristics across the two samples. Lastly, if the patterns are unique to

the particular calendar quarter-end, we should have observed similar reductions in trading volume in contiguous years. This is not the case, however.

Second, we exclude firms that announced earnings during the strike period because prior research by Li and Ramesh (2005) suggests that contaminating events, particularly earnings announcements, make it difficult to draw inferences about the usefulness of 10-K filings. However, the problem identified by Li and Ramesh (2005) pertains to quarterly earnings announcements surrounding the 10-K filings. This is less of an issue in our setting because the quarterly earnings requirement only became effective in 1971. Nonetheless, we hand-collect earnings announcement dates from the Wall Street Journal and find that only six firms announced earnings during the strike period. We exclude these observations and report our findings in column (2), Panel A of Table 10. Again, our results are robust.

Third, as indicated in Panel D of Table 2, we find a few industry groups—consumer non-durables, oil and gas, business equipment, wholesale and retail, and finance—in which the proportion of treatment or control firms is less than 40%; that is, there is an imbalance between the number of treatment and control firms. To examine whether this imbalance affects our findings, we remove firms in these five industries and report the results of estimating equation (1) in column (3). As before, the coefficient on *TREAT* is negative with a similar magnitude and statistical significance.

In our last set of robustness tests, we consider three alternative ways of classifying treatment firms. Given that we do not know the precise date firms mail their annual reports, we use the extent of overlap between the strike period and an estimated mailing period. In all our empirical tests, we classify firms above the median overlap between the strike period and estimated mailing period as belonging to the treatment group. For robustness, we consider three alternative

classification schemes: (1) classify firms with any overlap with the mailing period as treatment firms, (2) classify firms with at least 50% overlap with the mailing period as treatment firms, and (3) use the continuous variable, *OVERLAP*. Panel B of Table 10 reports the findings of the three alternative classifications in columns (1)–(3). In all three columns, we find that the coefficient on *TREAT* is both statistically and economically negative. It is comforting that, despite the potential measurement error in these alternative classification schemes, the effect sizes are similar to those obtained previously.

5. Conclusion

In this paper, we provide evidence that annual reports to shareholders have a causal impact on the trading behavior of capital market participants. We capitalize on the postal strike of 1970 that is exogenous to the market when annual report delivery was disrupted to provide evidence on the importance of annual reports. We find that this disruption reduced trading volume considerably during the strike period, primarily for treatment firms' shareholders who were expecting to receive annual reports but did not. This evidence is consistent with increased information asymmetry for investors of treatment firms. We conduct several falsification tests to ensure that our results are not obtained by chance.

We also find that retail investors are more likely to stay away from trading because they face the most adverse selection problems associated with the increased information asymmetry. The temporary surge in information asymmetry increases systematic risk substantially during the strike period, consistent with Levi and Zhang (2015). Lastly, we report that the decline in trading volume extends to bond traders as well.

Can our evidence apply to today's information environment in which firms and other information intermediaries release firm-specific news frequently? Given the ubiquity of

information and confounding disclosures that coincide with the release of annual reports to shareholders, parsing out the information content of annual reports during current periods will be an enormous empirical challenge, even if a wildcat postal strike were to occur now. Firms continue to spend a tremendous amount of time and money when preparing an annual report. While recent research (e.g., Li, 2010; Dyer, Lang, and Stice-Lawrence, 2017) finds that specific contents of the annual report (e.g., chairman's letter, MD&A) are valuable to market participants, the value relevance of annual reports in totality is not easy to discern, especially in today's complex information environment. Our paper represents an attempt at providing casual evidence on this important question.

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Appendix 1. Variable Definition

Panel A. The main sample

VARIABLE	DEFINITION	SOURCE
OVERLAP	Overlapped days between the postal strike period (March 15 – April 4) and the annual report mailing period divided by the length of the annual report mailing period.	ProQuest Historical AR
TREAT	An indicator that equals one if *OVERLAP* is above the sample median (i.e., 0.375), and zero otherwise.	ProQuest Historical AR
AVOL	Abnormal stock trading volume defined as average daily trading volume during the current 3-week period minus the previous 3-week period. Average daily trading volume is measured as a percentage of the total number of shares outstanding.	CRSP
Log(MVE)	Market value of equity at the most recent fiscal year end (Compustat: $PRCC\_F \times CSHO$). If Compustat data are missing, MVE is filled with CRSP market value of equity.	Compustat/ CRSP
B/M	Book-to-market at the most recent fiscal year end. Book value of equity is SEQ; if SEQ is missing book value of equity is (CEQ + PSTK); if CEQ is missing book value of equity is (AT – LT). If book value of equity is still missing, it is manually replaced by the value in the firm's historical annual report.	Compustat/ ProQuest
RET_{YTD}	Buy-and-hold stock returns between January 1 and March 14.	CRSP
$LOWSPRD_{PRE}$	An indicator that equals one for firms in the bottom tercile of average bid-ask spread (CRSP: $100 \times |ASKHI – BIDLO|/[(ASKHI + BIDLO)/2])$ in the 3-week period prior to the strike (February 22 – March 14) , and zero otherwise. The terciles are sorted by industry and year.	CRSP
$LOWTVAR_{PRE}$	An indicator that equals one for firms in the bottom tercile of stock return volatility (i.e., the variance of $RET \times 100$) in the 3-week period prior to the strike (February 22 – March 14), and zero otherwise. The terciles are sorted by industry and year.	CRSP

Panel B. The stock return sample

VARIABLE	DEFINITION	SOURCE
TREAT	An indicator that equals one if *OVERLAP* is above the sample median (i.e., 0.375), and zero otherwise.	ProQuest Historical AR
RET	Daily stock returns.	CRSP
RM	Daily market returns, calculated as the value-weight return on all NYSE, AMEX, and NASDAQ stocks.	Ken French website
$LOWSPRD_{PRE}$	An indicator that equals one for firms in the bottom tercile of average bid-ask spread (CRSP: $100 \times$ |ASKHI – BIDLO|/[(ASKHI + BIDLO)/2]) in the 3-week period prior to the strike (February 22 – March 14) , and zero otherwise. The terciles are sorted by industry and year.	CRSP
$LOWTVAR_{PRE}$	An indicator that equals one for firms in the bottom tercile of stock return volatility (i.e., the variance of $RET \times 100$) in the 3-week period prior to the strike (February 22 – March 14), and zero otherwise. The terciles are sorted by industry and year.	CRSP

Panel C. The retail vs. institutional trading sample

VARIABLE	DEFINITION	SOURCE
TREAT	An indicator that equals one if *OVERLAP* is above the sample median (i.e., 0.375), and zero otherwise.	ProQuest Historical AR
$AVOL_{RETAIL}$	Abnormal retail trading volume defined as average daily odd-lot trading volume during the current 3-week period minus the previous 3-week period. Average daily odd-lot trading volume is measured as the sum of odd-lot purchases and sales divided by the total number of shares outstanding. Since the original data is provided on a weekly basis, daily odd-lot trading volume equals weekly volume divided the number of trading days in the week.	SEC Statistical Bulletin – odd-lot trading for 100 NYSE stocks
$AVOL_{INST}$	Abnormal institutional trading volume defined as the residual from regressing total abnormal trading volume (*AVOL*) on abnormal retail trading volume ($AVOL_{RETAIL}$).	
Log(MVE)	Market value of equity at the most recent fiscal year end (Compustat: PRCC_F $\times$ CSHO). If Compustat data are missing, MVE is filled with CRSP market value of equity.	Compustat/ CRSP
B/M	Book-to-market at the most recent fiscal year end. Book value of equity is SEQ; if SEQ is missing book value of equity is (CEQ + PSTK); if CEQ is missing book value of equity is (AT – LT). If book value of equity is still missing, it is manually replaced by the value in the firm's historical annual report.	Compustat/ ProQuest
RET_{YTD}	Buy-and-hold stock returns between January 1 and March 14.	CRSP

Panel D. The bond trading sample

VARIABLES	DEFINITION	SOURCE
TREAT	An indicator that equals one if *OVERLAP* is above the sample median (i.e., 0.375), and zero otherwise.	ProQuest Historical AR
$AVOL_{BOND}$	Abnormal bond trading volume defined as average daily bond trading volume (in $ amounts) during March 15 – April 5 of 1970 minus the average daily volume in February 22 – Mar 14 of 1970, divided by the firm's total debt (Compustat: DLTT + DLC).	Historical WSJ – NYSE bonds
Log(MVE)	Market value of equity at the most recent fiscal year end (Compustat: PRCC_F × CSHO). If Compustat data are missing, MVE is filled with CRSP market value of equity.	Compustat/ CRSP
B/M	Book-to-market at the most recent fiscal year end. Book value of equity is SEQ; if SEQ is missing book value of equity is (CEQ + PSTK); if CEQ is missing book value of equity is (AT – LT). If book value of equity is still missing, it is manually replaced by the value in the firm's historical annual report.	Compustat/ ProQuest
RET_{YTD}	Buy-and-hold stock returns between January 1 and March 14.	CRSP

Appendix 2. Anecdotal Examples

Panel A. Key dates for the Standard Oil Company (Ohio) fiscal year 1969

THE STANDARD OIL COMPANY
(AN OHIO CORPORATION)

(SOHIO)   1969 ANNUAL REPORT

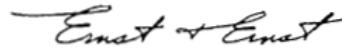(1) Auditor signature date and fiscal year end

Accountants' Report

Board of Directors
The Standard Oil Company
Cleveland, Ohio

We have examined the consolidated financial statements of The Standard Oil Company, an Ohio corporation, and subsidiaries for the year ended December 31, 1969. Our examination was made in accordance with generally accepted auditing standards, and accordingly included such tests of the accounting records and such other auditing procedures as we considered necessary in the circumstances. We previously made a similar examination of the consolidated financial statements for the preceding year.

In our opinion, the accompanying balance sheet and income, stockholders' equity, and source and application of funds statements present fairly the consolidated financial position of The Standard Oil Company and subsidiaries at December 31, 1969, and the consolidated results of their operations, changes in stockholders' equity, and the source and application of funds for the year then ended, in conformity with generally accepted accounting principles applied on a basis consistent with that of the preceding year.

Cleveland, Ohio
February 9, 1970

Ernst & Ernst

(2) Chairman letter date

To Stockholders and Employees:

Charles E. Spahr

March 6, 1970   Chairman

(3) Annual meeting date and proxy mailing date

The Annual Meeting of Stockholders

The Annual Meeting of Stockholders will be held at 2:30 p.m., Eastern Standard Time, Thursday, April 23, 1970, at Sohio's Home Office, Midland Building, Cleveland, Ohio. Stockholders unable to attend the meeting are urged to exercise their right to vote by proxy. Proxy cards and proxy statements will be sent to all holders of common stock, preferred stock, and special stock about March 17, 1970.

Appendix 2 (Continued)

Panel B. Examples of annual meeting postponements

IMPORTANT NOTICE
Delayed Annual Meeting of
Puerto Rican Cement Company
April 21, 1970

Because of the recent mail strike and the resulting delay in posting annual report and proxy material, the annual meeting of Stockholders of Puerto Rican Cement Company, Inc., has been postponed until Tuesday, April 21, 1970. The meeting will be at 10:00 a.m., Atlantic Standard Time, at the Ponce Intercontinental Hotel, Ponce, P.R.

JORGE BARTOLOMEI, *Secretary*

Penn-Dixie Cement Meeting

NEW YORK — Penn-Dixie Cement Corp. said it had changed the date of its annual meeting to May 5 from April 28 because of delays due to the postal workers' strike and air controllers' slowdown and because of last-minute accounting adjustments.

Associated Brewing Meeting

By a WALL STREET JOURNAL *Staff Reporter*

DETROIT—Associated Brewing Co.'s management has asked for adjournment of its April 16 annual meeting in South Bend, Ind., to April 30 because the mail strike has delayed delivery of proxy statements to holders.

Gilbert Delaney, vice president and treasurer, said a number of holders in New York and New Jersey haven't received proxies that were mailed March 17.

Unexcelled Postpones Meeting

NEW YORK—Unexcelled Inc. said a special sharcholders' meeting will be postponed to April 9 from Thursday, primarily because the postal strike delayed the receipt of proxies.

The company said the meeting will be held at 3 p.m. at the offices of its Aero Spacelines Inc. subsidiary in Santa Barbara, Calif.

Panel C: Example of Wall Street Journal Article

THE WALL STREET JOURNAL,
Friday, March 20, 1970

Meetings May Be Postponed, Trading Halt Is Possible; Strike Enters Third Day

By a WALL STREET JOURNAL *Staff Reporter*

NEW YORK -- Chaos threatens as the nation's first postal strike moves into its third day in the New York metropolitan area.

Among the consequences:

- Stock markets in New York may be forced to close as early as Monday, according to securities industry officials.

- Dozens of companies may be forced to postpone stockholder and other meetings because proxies are bogged down in the mail crush.

...

The Wall Street Journal, prevented from mailing 60,000 copies to subscribers in New York City and suburban Long Island, increased its newsstand distribution in the metropolitan area to 110,000 copies of yesterday's edition from the normal 100,000, and said it would distribute 130,000 copies of today's edition.

...

Proxy-soliciting firms, poised at the brink of the annual meeting season, have felt the impact of the strike especially hard. A spokesman for D. F. King & Co., a major proxy solicitor, said: "Any company that's got a meeting in the next two or three weeks, with substantial stock with banks and brokers, will likely have problems getting a quorum, much less enough shares to pass proposals."

General Host Corp., among others, said it is considering delaying an annual meeting it had planned for late March because of the strike. "It would be nearly impossible to get out all the proxy statements in time for the meeting," said a spokesman.

Celanese Corp., which scheduled an annual meeting April 8, said it is considering taking newspaper ads telling stockholders to send in proxies by telegram, providing securities regulations would permit it.

Companies capable of providing any kind of substitute for mail service have been inundated with calls for help. REA Express Inc. reported business in New York is up about 100%, and United Parcel Service Inc., said all its offices were deluged with calls and orders as soon as they opened yesterday. Emery Air Freight

Figure 1. Timeline of Annual Report Mailing Periods

This figure plots the timeline of annual report (AR) mailing periods for Chrysler Corp., General Motors (GM), and Caterpillar Tractor after their fiscal year ends in 1970. The AR mailing periods are determined based on their audit signature dates, chairman letter dates, proxy mailing dates, and annual meeting dates (see Section 3.2 for details). The postal strike period is March 15 – April 4, 1970.
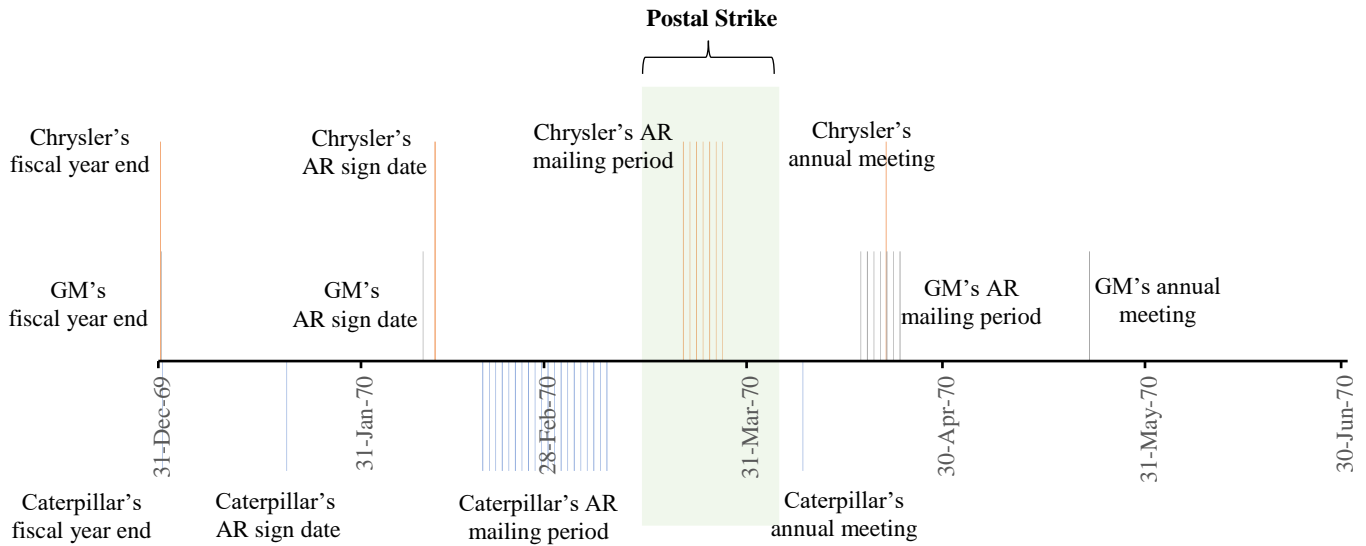
Fiscal 1969	Chrysler Corp.	General Motors	Caterpillar Tractor
Fiscal year end	December 31, 1969	December 31, 1969	December 31, 1969
Audit signature date	February 11, 1970	February 10, 1970	January 19, 1970
Chairman letter date	February 11, 1970	February 10, 1970	January 19, 1970
Proxy mailing date	March 21, 1970	April 17, 1970	February 18, 1970
Annual meeting date	April 21, 1970	May 22, 1970	April 8, 1970
Annual report mailing period	Mar 21–Mar 28, 1970	Apr 17–Apr 24, 1970	Feb 18–Mar 9, 1970

Figure 2. Abnormal Trading Volume across Treatment and Control Firms

This figure plots abnormal trading volume measured during the strike period for treatment and control firms along with two alternative benchmark periods. Figure in Panel A compares the average abnormal trading volume for the strike period with those in the same period for the year before (1969) and the year after (1971). Figure in Panel B uses the average abnormal volume for three weeks before and three weeks after the strike period as the benchmarks. It also plots confidence interval at the 10% level (two-tailed).

Panel A. Abnormal volumes in March 15 – April 4 of contiguous years
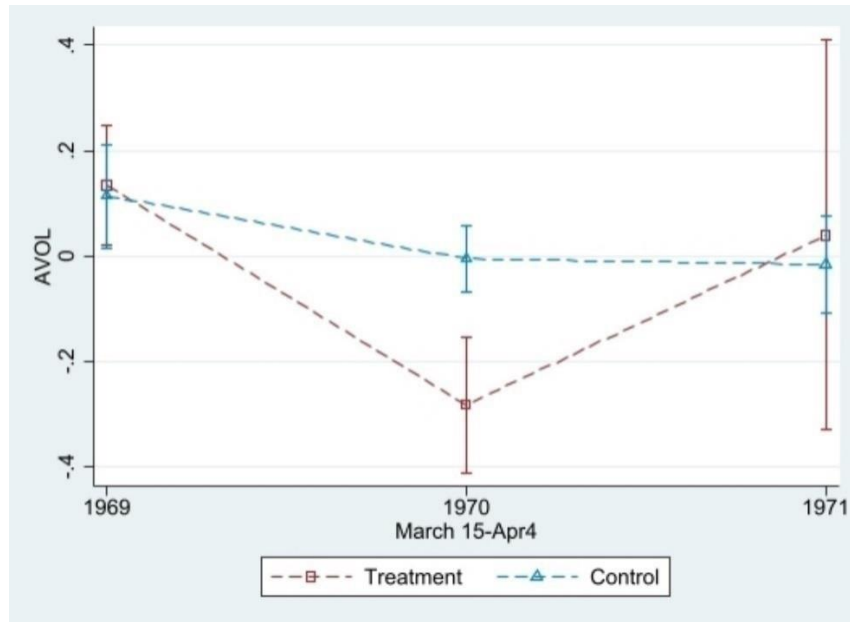
Panel B. Abnormal volumes in the 3-week periods before, during, and after the strike in 1970
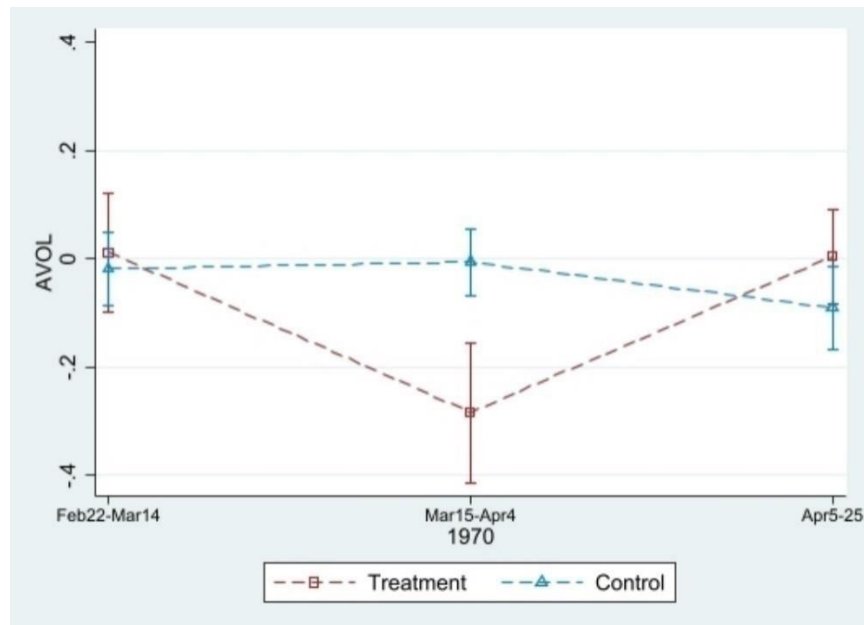
Table 1. Sample Selection

This table reports the selection process for the main sample, which consists of 274 unique firms between fiscal years 1968 and 1970, i.e., 822 firm-year observations.

Sample selection	Number of obs.
Step 1. Downloaded all available annual reports (scanned copies) from the ProQuest Historical Annual Report (HAR) database for fiscal year 1969.	594
Step 2. Read through the annual reports and removed firms that did not disclose their annual meetings dates for fiscal 1969.	(258)
	336
Step 3. Downloaded firms' annual reports (scanned copies) for fiscal 1968 and 1970 from the ProQuest HAR database.	593
	929
Step 4. Manually collected information about each firm's fiscal year end, audit signature date, chairman letter date, proxy mail date, and annual meeting date in each year from the annual reports. Removed non-public firms. Merged with Compustat/CRSP based on firm names.	(38)
	891
Step 5. Removed firms whose audit signature dates and chairman letter dates are both missing, firms without necessary return and accounting data, and firms that did not have all 3 years of data.	(69)
Step 6. Final sample consists of 822 firm-years observations, representing 274 unique firms in fiscal years 1968–1970.	822

Table 2. Summary Statistics

This table reports summary statistics for the main variables. Panel A shows descriptive statistics for the main sample, which consists of 274 unique firms. Panel B provides the correlation matrix. Panel C provides a comparison of stock trading volume and firm characteristics between the treatment and control samples in the postal strike year. Panel D provides industry composition for the two samples. ∗∗∗ indicates significance at the 1% levels for two-tailed *t*-tests (chi-square tests) for means (medians).

Panel A. Descriptive statistics for the main sample

VARIABLE	*N*	Mean	Std. Dev.	Min	5th	Q1	Median	Q3	95th	Max
AVOL (Mar 15 – Apr 4)	274	–0.141	0.724	–4.843	–1.376	–0.231	–0.066	0.093	0.521	4.013
OVERLAP	274	0.464	0.434	0.000	0.000	0.000	0.375	1.000	1.000	1.000
TREAT	274	0.489	0.501	0.000	0.000	0.000	0.000	1.000	1.000	1.000
Log(MVE)	274	6.051	1.206	3.494	4.201	5.191	6.108	6.779	8.195	10.632
B/M	274	0.811	0.530	0.048	0.154	0.452	0.718	1.029	1.791	4.571
RET_{YTD}	274	–0.030	0.107	–0.412	–0.203	–0.101	–0.030	0.055	0.136	0.220

Panel B. Correlation Matrix (Pearson/Spearman are below/above the diagonal)

	AVOL	*TREAT*	*Log(MVE)*	*B/M*	RET_{YTD}
AVOL		–0.253	0.041	0.046	0.113
		(0.00)	(0.50)	(0.45)	(0.06)
TREAT	–0.192		–0.009	0.076	–0.020
	(0.00)		(0.88)	(0.21)	(0.74)
Log(MVE)	0.104	–0.008		–0.394	–0.082
	(0.09)	(0.90)		(0.00)	(0.18)
B/M	0.050	0.098	–0.394		0.267
	(0.41)	(0.10)	(0.00)		(0.00)
RET_{YTD}	0.181	–0.011	–0.081	0.258	
	(0.00)	(0.86)	(0.18)	(0.00)	

Panel C. Differences in firm characteristics across treatment and control firms

VARIABLE	TREATMENT (*N* = 134)		CONTROL (*N* = 140)		Difference	
	Mean	Median	Mean	Median	Mean	Median
AVOL (Mar 15 – Apr 4)	–0.283	–0.129	–0.005	–0.027	–0.278***	–0.102***
Log(MVE)	6.041	6.127	6.061	6.087	–0.020	0.040
B/M	0.864	0.782	0.760	0.699	0.104	0.083
RET_{YTD}	–0.031	–0.031	–0.029	–0.029	–0.002	–0.002

Table 2 (Continued)

Panel D. Fama-French industry classification across the treatment and control firms

INDUSTRY	TREATMENT	CONTROL
Consumer Non-Durables	12	26
Consumer Durables	11	9
Manufacturing	49	41
Oil, Gas, and Coal	17	3
Chemicals and Allied Products	11	12
Business Equipment	5	9
Utilities	6	9
Wholesale, Retail, and Some Service	0	10
Healthcare and Medical	10	6
Finance	4	7
Other - Mines, Construction, etc.	9	8
Total	134	140

Table 3. The Impact of Annual Report Delivery on Stock Trading

This table presents the differential impact of annual report delivery on stock trading for the treatment and control firms during the strike period, March 15 – April 4 of 1970. *TREAT* is an indicator variable that equals one for treatment firms and otherwise zero. The model includes (Fama-French 12) industry fixed effects. *t-statistics* in parentheses are based on heteroscedasticity-robust standard errors. ∗, ∗∗, and ∗∗∗ indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively. Intercepts are suppressed for ease of presentation. Variable definitions are in Appendix 1.

VARIABLE	Mar 15 – Apr 4, 1970
TREAT	–0.285***
	(–3.14)
Log(MVE)	0.096**
	(2.13)
B/M	0.133
	(1.22)
RET$_{YTD}$	1.354**
	(2.10)
Industry F.E.	YES
N	274
Adj. R^2	7.3%

Table 4. Falsification Tests

This table presents the results of two falsification tests. Panel A provides summary statistics. Panel B provides regression results for abnormal trading volume measured in in March 15 – April 4 of 1969 and 1971. Panel C provides regression results for abnormal trading volume measured in February 22 – March 14 (i.e., 3 weeks before) or April 5 – April 25 (i.e., 3 weeks after) of 1969, 1970, and 1971. *TREAT* is an indicator variable that equals one for treatment firms and otherwise zero. The models include (Fama-French 12) industry fixed effects. *t-statistics* in parentheses are based on heteroscedasticity-robust standard errors. ∗, ∗∗, and ∗∗∗ indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively. Intercepts are suppressed for ease of presentation. Variable definitions are in Appendix 1.

Panel A. Differences in abnormal trading volume

VARIABLE	TREAT (N = 134)		CONTROL (N = 140)		Difference	
	Mean	Median	Mean	Median	Mean	Median
Year before and after						
AVOL (Mar 15 – Apr 4, 1970)	–0.283	–0.129	–0.005	–0.027	–0.278***	–0.102***
AVOL (Mar 15 – Apr 4, 1969)	0.134	0.011	0.114	0.005	0.020	0.006
AVOL (Mar 15 – Apr 4, 1971)	0.039	–0.021	–0.017	–0.006	0.056	–0.015
Weeks before and after						
AVOL (Mar 15 – Apr 4, 1970)	–0.283	–0.129	–0.005	–0.027	–0.278***	–0.102***
AVOL (Feb 22 – Mar 14, 1970)	0.012	0.017	–0.018	–0.006	0.030	0.023
AVOL (Apr 5 – Apr 25, 1970)	0.005	0.038	–0.090	–0.059	0.095	0.097***

Panel B: Regression results for contiguous years

VARIABLE	Mar 15 – Apr 4, 1969 (1)	Mar 15 – Apr 4, 1971 (2)
TREAT	0.014	0.043
	(0.14)	(0.18)
Log(MVE)	–0.017	–0.102
	(–0.34)	(–0.81)
B/M	–0.246**	–0.264
	(–2.39)	(–1.03)
RET_{YTD}	–1.376	–1.095
	(–1.56)	(–1.33)
Industry F.E.	YES	YES
N	274	274
Adj. R^2	2.7%	–2.6%

Table 4 (Continued)

Panel C: Regression results for the three weeks before and after the strike period

VARIABLE	Feb 22 – Mar 14, 1970 (1)	Apr 5 – Apr 25, 1970 (2)	Feb 22 – Mar 14, 1969 (3)	Apr 5 – Apr 25, 1969 (4)	Feb 22 – Mar 14, 1971 (5)	Apr 5 – Apr 25, 1971 (6)
TREAT	0.081	0.081	0.035	0.004	–0.016	0.277
	(1.04)	(1.17)	(0.56)	(0.05)	(–0.10)	(1.38)
Log(MVE)	–0.075*	0.034	0.036	–0.072*	0.076	0.136
	(–1.87)	(1.00)	(1.00)	(–1.71)	(1.44)	(1.38)
B/M	0.008	–0.092	–0.335***	0.216*	0.057	1.027***
	(0.09)	(–1.09)	(–3.04)	(1.94)	(0.44)	(2.91)
RET_{YTD}	–0.381	–0.040	–0.062	1.221	–2.129**	1.720
	(–0.56)	(–0.10)	(–0.08)	(1.05)	(–2.57)	(1.26)
Industry F.E.	YES	YES	YES	YES	YES	YES
N	274	274	274	274	274	274
Adj. R^2	2.8%	5.4%	2.1%	–1.0%	7.2%	13.2%

Table 5. Cross-sectional Tests based on Ex-ante Information Asymmetry

This table presents cross-sectional tests of the impact of annual report delivery on stock trading for treat and control firms in 1969, 1970, and 1971. The dependent variables are abnormal trading volume measured in March 15 – April 4 of each year. *TREAT* is an indicator variable that equals one for treatment firms and otherwise zero. The industry fixed effects are based on Fama-French 12 industries. We use two proxies for $LOWINFASS_{PRE}$, low bid-ask spread ($LOWSPRD_{PRE}$) and low return volatility ($LOWTVAR_{PRE}$). *t-statistics* in parentheses are based on heteroscedasticity-robust standard errors. *, **, and *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively. Intercepts are suppressed for ease of presentation. Variable definitions are in Appendix 1.

VARIABLE	Mar 15 – Apr 4, 1970		Mar 15 – Apr 4, 1969		Mar 15–Apr 4, 1971	
	$LOWSPRD_{PRE}$	$LOWTVAR_{PRE}$	$LOWSPRD_{PRE}$	$LOWTVAR_{PRE}$	$LOWSPRD_{PRE}$	$LOWTVAR_{PRE}$
	(1)	(2)	(3)	(4)	(5)	(6)
TREAT	−0.366***	−0.396***	−0.039	−0.027	0.082	0.129
	(−3.15)	(−3.24)	(−0.28)	(−0.19)	(0.22)	(0.36)
TREAT × LOWINFASS$_{PRE}$	0.244*	0.348**	0.146	0.113	−0.108	−0.236
	(1.66)	(2.34)	(0.94)	(0.66)	(−0.28)	(−0.68)
$LOWINFASS_{PRE}$	0.036	0.011	−0.085	−0.078	0.099	0.098
	(0.49)	(0.14)	(−0.78)	(−0.63)	(0.83)	(0.75)
Log(MVE)	0.100**	0.091**	−0.015	−0.010	−0.107	−0.100
	(2.21)	(2.02)	(−0.29)	(−0.18)	(−0.91)	(−0.90)
B/M	0.160	0.148	−0.248**	−0.238**	−0.261	−0.265
	(1.46)	(1.37)	(−2.37)	(−2.35)	(−0.95)	(−1.02)
RET_{YTD}	1.127*	1.156*	−1.428	−1.420	−1.091	−1.114
	(1.74)	(1.78)	(−1.58)	(−1.56)	(−1.25)	(−1.27)
Industry F.E.	YES	YES	YES	YES	YES	YES
N	274	274	274	274	274	274
Adj. R^2	8.2%	9.3%	2.1%	2.1%	−3.4%	−3.3%

Table 6. The Impact of Annual Report Delivery on Systematic Risk

This table presents the differential impact of annual report delivery on information risk for treat and control firms. Panel A shows descriptive statistics of daily stock returns and market returns for 274 unique firms in February 22 – April 25, 1970. Panel B shows market model estimations for February 22 – March 14 (i.e., before strike), March 15 – April 4 (i.e., during strike), and April 5 – April 25 (i.e., after strike). *TREAT* is an indicator variable that equals one for treatment firms and otherwise zero. The regressions include Fama-French 12 industry fixed effects. *t-statistics* in parentheses are based on heteroscedasticity-robust standard errors. ∗, ∗∗, and ∗∗∗ indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively. Intercepts are suppressed for ease of presentation. Variable definitions are in Appendix 1.

Panel A. Descriptive statistics

VARIABLE	N	Mean	Std. Dev.	Min	5th	Q1	Median	Q3	95th	Max
RET	11,782	–0.002	0.020	–0.198	–0.033	–0.012	0.000	0.008	0.030	0.157
RM	11,782	–0.002	0.007	–0.016	–0.011	–0.005	–0.003	0.002	0.010	0.022

Panel B. Market model results

VARIABLE	Feb 22 – Mar 14, 1970	Mar 15 – Apr 4, 1970	Apr 5 – Apr 25, 1970
	(1)	(2)	(3)
RM	0.928***	0.842***	0.986***
	(13.93)	(15.81)	(11.53)
RM × *TREAT*	0.131	0.231***	0.031
	(1.20)	(2.96)	(0.23)
TREAT	0.000	0.000	–0.001
	(0.55)	(0.27)	(–0.74)
Industry F.E.	YES	YES	YES
N	3,836	3,836	4,110
Adj. R^2	9.8%	15.7%	6.2%

Table 7. Cross-sectional Tests of Systematic Risk

This table presents cross-sectional tests of the differential impact of annual report delivery on information risk for treat and control firms. The sample consists of daily stock returns for 274 unique firms in February 22 – March 14 (i.e., before strike), March 15 – April 4 (i.e., during strike), and April 5 – April 25 (i.e., after strike) of 1970. *TREAT* is an indicator variable that equals one for treatment firms and otherwise zero. We use two proxies for $LOWINFASS_{PRE}$, low bid-ask spread ($LOWSPRD_{PRE}$) and low return volatility ($LOWTVAR_{PRE}$).. The regressions include Fama-French 12 industry fixed effects. *t-statistics* in parentheses are based on heteroscedasticity-robust standard errors. ∗, ∗∗, and ∗∗∗ indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively. Intercepts are suppressed for ease of presentation. Variable definitions are in Appendix 1.

VARIABLE	$LOWSPRD_{PRE}$			$LOWTVAR_{PRE}$		
	Feb 22 – Mar 14, 1970	Mar 15 – Apr 4, 1970	Apr 5 – Apr 25, 1970	Feb 22 – Mar 14, 1970	Mar 15 – Apr 4, 1970	Apr 5 – Apr 25, 1970
	(1)	(2)	(3)	(4)	(5)	(6)
RM	1.118***	0.897***	1.234***	1.169***	0.871***	1.085***
	(11.87)	(12.41)	(11.22)	(12.23)	(11.76)	(10.20)
RM × TREAT	0.142	0.396***	0.025	0.157	0.351***	0.028
	(0.92)	(3.90)	(0.16)	(1.02)	(3.37)	(0.19)
RM × TREAT × $LOWINFASS_{PRE}$	−0.066	−0.501***	−0.027	−0.153	−0.373**	−0.020
	(−0.35)	(−3.37)	(−0.17)	(−0.84)	(−2.53)	(−0.12)
RM × $LOWINFASS_{PRE}$	−0.533***	−0.152	−0.696***	−0.662***	−0.077	−0.271*
	(−4.54)	(−1.50)	(−5.01)	(−5.70)	(−0.77)	(−1.88)
$LOWINFASS_{PRE}$	−0.000	−0.000	−0.003***	−0.000	−0.000	−0.000
	(−0.42)	(−0.75)	(−3.14)	(−0.48)	(−0.25)	(−0.02)
TREAT	0.000	−0.000	−0.001	0.000	0.000	−0.001
	(0.60)	(−0.02)	(−0.75)	(0.54)	(0.26)	(−0.74)
Industry F.E.	YES	YES	YES	YES	YES	YES
N	3,836	3,836	4,110	3,836	3,836	4,110
Adj. R^2	10.7%	16.4%	7.1%	10.9%	16.0%	6.4%

Table 8. The Impact of Annual Report Delivery on Retail vs. Institutional Trading

This table presents the differential impact of annual report delivery on retail vs. institutional trading for treat and control firms. Panel A shows summary statistics for the sample and Panel B shows the regression results. The dependent variables are abnormal retail trading volume and abnormal institutional trading volume, both measured during the strike period. *TREAT* is an indicator variable that equals one for treatment firms and otherwise zero. The models include Fama-French 12 industry fixed effects. *t-statistics* in parentheses are based on heteroscedasticity-robust standard errors. ∗, ∗∗, and ∗∗∗ indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively. Intercepts are suppressed for ease of presentation. Variable definitions are in Appendix 1.

Panel A. Descriptive statistics

VARIABLE	N	Mean	Std. Dev.	Min	5th	Q1	Median	Q3	95th	Max
TREAT	43	0.488	0.506	0.000	0.000	0.000	0.000	1.000	1.000	1.000
$AVOL_{RETAIL}$	43	−0.015	0.039	−0.185	−0.056	−0.022	−0.005	0.001	0.008	0.080
$AVOL_{INST}$	43	0.000	0.501	−1.617	−0.617	−0.102	−0.034	0.161	0.490	1.915
Log(MVE)	43	6.960	1.413	4.362	4.964	5.889	6.866	7.727	9.179	10.632
B/M	43	0.846	0.573	0.093	0.127	0.451	0.753	1.140	1.878	2.592
RET_{YTD}	43	−0.048	0.104	−0.236	−0.190	−0.143	−0.038	0.037	0.116	0.155

Panel B. Retail vs. institutional trading

VARIABLE	Dependent Variable = $AVOL_{RETAIL}$ Mar 15 – Apr 4, 1970		Dependent Variable = $AVOL_{INST}$ Mar 15 – Apr 4, 1970	
	(1)	(2)	(3)	(4)
TREAT	−0.028*	−0.019	−0.111	−0.061
	(−2.03)	(−1.47)	(−0.50)	(−0.22)
Log(MVE)		0.000		−0.027
		(0.02)		(−0.27)
B/M		−0.027		−0.071
		(−1.17)		(−0.24)
RET_{YTD}		0.152**		1.287
		(2.34)		(1.49)
Industry F.E.	YES	YES	YES	YES
N	43	43	43	43
Adj. R^2	6.2%	24.3%	−9.2%	−13.1%

Table 9. The Impact of Annual Report Delivery on Bond Trading

This table presents the differential impact of annual report delivery on bond trading for treat and control firms. Panel A shows summary statistics for the sample. Panel B provides a comparison of abnormal bond trading volume during the strike period between the treatment and control samples. Panel C shows the regression results. The dependent variable is abnormal bond trading volume ($\times 1000$) measured in March 15–April 4 of 1970. *TREAT* is an indicator variable that equals one for treatment firms and otherwise zero. The model includes (Fama-French 12) industry fixed effects. *t-statistics* in parentheses are based on heteroscedasticity-robust standard errors. *, **, and *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively. Intercepts are suppressed for ease of presentation. Variable definitions are in Appendix 1.

Panel A. Descriptive statistics

VARIABLE	N	Mean	Std. Dev.	Min	5th	Q1	Median	Q3	95th	Max
$AVOL_{BOND}$ ($\times$ 1000)	96	0.008	0.142	–0.356	–0.177	–0.028	–0.005	0.012	0.220	0.950
TREAT	96	0.500	0.503	0.000	0.000	0.000	0.500	1.000	1.000	1.000
Log(MVE)	96	6.332	1.140	3.494	4.326	5.659	6.413	6.984	8.195	9.179
B/M	96	0.907	0.500	0.137	0.276	0.540	0.812	1.184	1.878	2.592
RET_{YTD}	96	–0.041	0.113	–0.347	–0.236	–0.124	–0.037	0.056	0.136	0.195

Panel B. Differences in $AVOL_{BOND}$ across treatment and control firms

VARIABLE	TREATMENT ($N = 48$)		CONTROL ($N = 48$)		Difference	
	Mean	Median	Mean	Median	Mean	Median
$AVOL_{BOND}$ ($\times$ 1000)	–0.014	–0.003	0.031	–0.005	–0.045	0.002

Panel C. Bond trading regressions

VARIABLE	Mar 15 – Apr 4, 1970	
	(1)	(2)
TREAT	–0.065*	–0.066*
	(–1.90)	(–1.82)
Log(MVE)		–0.010
		(–0.79)
B/M		–0.054**
		(–2.38)
RET_{YTD}		0.180
		(1.23)
Industry F.E.	YES	YES
N	96	96
Adj. R^2	7.4%	7.5%

Table 10. Robustness Tests

This table presents results of two robustness tests. Panel A reports results after removing non-December fiscal year end firms, removing firms with earnings announcement during the strike, or removing industries with treatment/control firm imbalance. Panel B reports results using alternative measure of treatment. The dependent variables are abnormal trading volume measured in March 15–April 4 of 1970. In Panel A, *TREAT* is an indicator variable that equals one for treatment firms and otherwise zero as with other tables. In Panel B *TREAT* is an indicator when *OVERLAP* > 0 (column 1), *OVERLAP* ≥ 0.5 (column 2), and *TREAT* is the continuous variable *OVERLAP* (column 3). The model includes (Fama-French 12) industry fixed effects. *t-statistics* in parentheses are based on heteroscedasticity-robust standard errors. *, **, and *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively. Intercepts are suppressed for ease of presentation. Variable definitions are in Appendix 1.

Panel A. Removing firms with non-December fiscal year end, firms with earnings announcement during the strike, or industries with treatment/control firm imbalance

VARIABLE	Removing Non-December fiscal year end firms	Removing firms with earnings announcement during the strike period	Removing industries with imbalance in treatment and control firms
	(1)	(2)	(3)
TREAT	–0.284***	–0.299***	–0.287***
	(–2.85)	(–3.35)	(–2.89)
Log(MVE)	0.116**	0.089**	0.137**
	(2.07)	(2.00)	(2.47)
B/M	0.173	0.106	0.169
	(1.31)	(0.98)	(1.27)
RET$_{YTD}$	1.130	1.510**	1.482*
	(1.35)	(2.32)	(1.94)
Industry F.E.	YES	YES	YES
N	208	268	201
Adj. R^2	4.6%	8.3%	7.9%

Panel B. Alternative treatment classifications

VARIABLE	Mar 15 – Apr 4, 1970		
	TREAT = 1 if *OVERLAP* > 0	*TREAT* = 1 if *OVERLAP* ≥ 0.5	*TREAT* = *OVERLAP*
	(1)	(2)	(3)
TREAT	–0.180**	–0.227**	–0.190**
	(–2.27)	(–2.44)	(–2.06)
Log(MVE)	0.103**	0.098**	0.099**
	(2.17)	(2.13)	(2.14)
B/M	0.134	0.132	0.127
	(1.19)	(1.22)	(1.17)
RET$_{YTD}$	1.273*	1.370**	1.326**
	(1.94)	(2.10)	(2.01)
Industry F.E.	YES	YES	YES
N	274	274	274
Adj. R^2	5.0%	6.0%	4.9%