

THE INFORMATION CONTENT OF ANNUAL EARNINGS RELEASES:

A TRADING VOLUME APPROACH

DISSERTATION

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* * * * *


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CHAPTER I

INTRODUCTION

Accountants are interested in the usefulness of the information they produce. In the Statement of Financial Accounting Concepts No. 2 - Qualitative Characteristics of Accounting Information, the Financial Accounting Standards Board (FASB) states that:

The central role assigned here to decision making leads straight to the overriding criterion by which all accounting choices must be judged. The better choice is the one that, subject to considerations of cost, produces from among the available alternatives information that is most useful for decision making. (Paragraph 30).

Decision usefulness is a relative criterion. To whom must the information be useful? The investment community is one group which concerns the FASB: In Concept No. 1 - Objectives of Financial Reporting by Business Enterprises, the FASB suggests that "Financial reporting should provide information that is useful to present and potential investors...." (paragraph 34).

These authoritative pronouncements suggest that accountants should be interested in whether investors find accounting earnings releases useful in making their investment portfolio decisions. Historically, researchers have employed three different approaches to measure the impact of accounting information on investors:

- 1) examination of market prices,
- 2) tests of market trading volume, and
- 3) experimental investigations (primarily laboratory studies) of individual investors' behavior.

While the third method yields the most convincing evidence on how specific individuals process information (e.g. Libby, 1981; Uecker, 1980; Casey, 1980), this approach is hampered by the difficulty in generalizing from a specific task performed by a specific group of individuals to more general conclusions concerning how the typical investor analyzes the barrage of financial information produced by accounting reports. Further, (to varying degrees) these laboratory studies by definition sacrifice external validity or realism, because ethical and practical considerations preclude the researcher from designing experiments where investors actually assume the risks inherent in financial market investment.

The researcher can avoid these problems by studying the results of investors' information processing as evidenced by their portfolio decisions. The most readily available data concerning portfolio decisions consist of the summary market statistics: prices and trading volume.

In order to explain why a test of market volume, rather than an examination of market prices, was employed in this study, the next two sections of this chapter briefly discuss the advantages and disadvantages of each approach. Research on market trading volume is a relatively recent phenomenon; historically, most market research has focused on the price statistic (or more specifically, the changes in

price associated with the release of different types of information).

Presumably this emphasis was largely due to:

- 1) interest in aggregate market reactions and the sophisticated market-making investors, and
- 2) the existence of a theoretical underpinning, in the form of equilibrium asset pricing models.

The following section outlines the broadening of interest to include the individual investors, and discusses the different implications of price versus volume statistics. The next section briefly reviews the narrowing gap between the quantity and quality of price and volume theories. Subsequent sections of this chapter review the purpose, method of investigation, results, and contributions of the study.

Differences Between Price and Volume Statistics
and their Implications for Market
Behavior

Although market price and trading volume are both objective summary market statistics, they measure different phenomena which may reflect different underlying market characteristics. In other words, implications of unusual price movements may differ from implications of unusual trading patterns. As explained by Beaver (1968), price tests reveal changes in the overall or aggregate market's expectations regarding future security return distributions. Individual expectations are "averaged" or offset. For example, a piece of information may cause two investors to change their expectations in offsetting ways. If the rest of the market remains neutral with respect to this new information, the two investors may trade with each other at the equilibrium price. The investors use the information, but since they change their

expectations in offsetting directions, the market price may not change. Thus, the equilibrium market price reflects a type of averaging of all the investors' expectations. Price research, therefore, provides evidence on average market reactions. If one further assumes that the market is "made" (or driven) by a group of highly sophisticated investors, then price research could also provide a basis for drawing inferences concerning the market-makers' behavior.

In recent pronouncements, however, authoritative bodies such as the FASB and SEC have shown interest in the impact of financial statements on individual investors, as well overall market reactions. In paragraph 34 of Concept No. 1, for example, the FASB states:

Financial reporting should provide information that is useful to present and potential investors and creditors and other users in making rational investment, credit, and similar decisions. The information should be comprehensible to those who have a reasonable understanding of business and economic activities and are willing to study the information with reasonable diligence.

In the previous scenario, where the two investors responding to the release of information change their expectations in offsetting ways, a price test would indicate that the information was not useful, because the market price did not change. A trading volume test, however, would indicate that the information was useful since the investors traded. This simple example shows that price must be viewed as an indicator of the average market response, since the market-clearing price results from an offsetting or averaging of individual investors' expectations: the effect of one investor's changed expectations may be wiped out by other investors' changes. Market trading volume on the other hand, is not an average but a summation of all investors' trades. If a piece of

information changes the investor's expectations enough to cause him to rebalance his portfolio, the trade cannot be offset or averaged; it will increase the market trading volume statistic.

Policy-makers interested in the effect of information on individual investors (as opposed to the average market reaction) may prefer trading volume research to price research for the reason discussed above: price is an average statistic, while trading volume is a summation. If the policy-maker is interested in 1) whether the information has an impact on the investors, or 2) the magnitude of that impact, the volume statistic has a clear advantage over the price statistic. If, upon receiving a piece of information, the investor changes his expectations and rebalances his portfolio (i.e. trades), this trade will increase the market trading volume statistic. If other investors change their expectations in the opposite direction, and trade, their trades will also increase the market trading volume. The market trading volume will indicate that the information resulted in trading, supporting the proposition that the information was useful to the decision-makers. The ultimate effect upon market price is unclear--it may increase, decrease, or remain unchanged. If the changes in expectations are perfectly offsetting, price might not change at all, and if the research only tests for price changes, the policy-makers might be misled into believing that the information was not used by the investors. The fallacy of decomposition might occur: what was true for the whole (aggregate market did not change its expectations) was not true for the parts (individual investors did change their expectations). Thus, although the market trading volume is a summation of the trading

throughout the entire market, it provides a movement away from the strictly aggregate nature of the price statistic, towards a measure of individual investor behavior (see Figure 1). The differences between price and trading volume research will be discussed in more detail in the following chapter. At present, it is sufficient to recognize that policy-makers have shown an interest in individual investor behavior, a phenomenon which may not be completely captured by stock price tests which rely on the aggregate market expectations.

Theoretical Developments

Aside from the historical focus on the behavior of the aggregate market (as opposed to individual investors), price research also enjoyed another advantage over volume research. The equilibrium pricing models provided a theoretical framework for empirical price research, and to date, volume research has no single analogue to the most widely used equilibrium asset pricing model--the Sharpe (1964) - Lintner (1965) - Mossin (1966) capital asset pricing model (hereafter, CAPM). Currently, however, the gap between the quality and quantity of price and volume theories appears to be narrowing. Although the theoretical literature involving trading volume will be reviewed in the following chapter, two of the most recent and interesting developments will be highlighted here.

The first line of research, typified by Copeland (1976, 1977), Morse (1980), and Jennings, Starks, and Fellingham (1981), is based on the premise that publicly disclosed information arrives sequentially. Presumably, the more intelligent, more interested, and better informed

Summation of individual investors portfolio changes. More a test of individual behavior (than price tests) because individual trades are summed, not offset. Less a test of individual behavior (than lab experiment) because evidence is only available on the total number of trades in the market--there is no information on individual trades (or traders).

Aggregate Market Measure

Individual Investor Measure

Price Tests

Reveal changes in the market's aggregate expectations of future security return distributions. A result of offsetting individual investors expectations.

Trading Volume Tests

Laboratory Experiments

Detailed experiments in a controlled setting designed to reveal how specific individuals use information. Lack of realism and difficulty in generalization are the major problems with this approach.

FIGURE 1

THE AGGREGATE-INDIVIDUAL CONTINUUM

investors will receive and process information more quickly than less intelligent, disinterested investors. These models propose a sort of chain reaction: upon the release of the information, the first recipient processes it and trades (with uninformed investors) to rebalance his portfolio in accordance with his revised expectations. If his trade has changed the equilibrium market price, other investors may trade in response to their revised expectations, based upon the new market price. The second recipient then trades, possibly changing the market price and subsequently generating a new round of trading. This sequence continues until all investors have received the information. One consequence of this model is that trading increases upon the release of information. Jennings, Starks, and Fellingham (1981) extend this result to conclude that under certain circumstances, price changes and trading volume changes may be negatively correlated so that trading volume may be approaching a peak while price changes near a minimum! These results suggest that volume research is of interest since in some cases, price and volume research may yield different results.

The second major theoretical development in the volume area is clarified in Lev and Ohlson (forthcoming), but is also based on Ohlson and Buckman (1981), Hakansson, Kunkel, and Ohlson (forthcoming) and Kunkel (forthcoming). The earlier articles specify necessary conditions for information to have social value in both pure exchange and production and exchange economies, while the Lev and Ohlson article clarifies and expands these results and their implications for measuring social welfare. The thrust of the research is that under certain assumptions, trading may become a valid measure of changes in social

welfare. Of course, this new development is not without its critics (see Verrecchia, forthcoming).

Just as the analytical work on market trading volume is developing, criticism of the CAPM and its empirically testable ex post analogue, the market-model (Sharpe, 1964) is mounting (see Appendix A for a brief discussion of the CAPM and market-model). First, Roll (1977) suggests that none of the empirical research designed to test the validity of the CAPM (e.g. Fama and MacBeth, 1973; Black, Jensen, and Scholes, 1972; Blume and Friend, 1973) has accomplished its purpose. The thrust of his argument is that these studies have not tested the posited linear relationship between security returns and beta, but instead have tested whether the index used to proxy the market portfolio is an efficient portfolio itself.

The second line of attack argues that the CAPM is misspecified, that is, it may omit potentially important explanatory variables. Litzenberger and Ramaswamy (1979) find that security returns are associated with dividend yields and personal income taxes. Atiase (1979), Grant (1980), and Banz (1981) suggest that the CAPM should include a term for firm size. Atiase and Grant find that smaller firms have bigger price changes (than larger firms do) around the dates of earnings releases. Banz finds that smaller firms tend to have larger rates of return. He conjectures that little information is available for small firms, and that the observed size effect occurs because investors in the smaller firms require a higher rate of return to offset the increased risk in estimating security returns based on less information.

These critiques do not imply that the CAPM or market models have been useless theoretical developments; to the contrary, the models have encouraged a large body of empirical price research. Several results have consistently emerged from this research, suggesting that the results are robust with respect to methodological refinements and different sample populations (e.g. Beaver, op.cit; Grant, op.cit; Morse, 1981). Three findings from previous price research were investigated in this study, employing market trading volume (instead of price) as the measure of information usefulness. These hypotheses, suggested by the results of price research, are discussed in the following section.

Purpose of the Study

The study's overall objective is to continue the investigation of the information content of annual earnings releases. Prior research (e.g. Beaver, op.cit; May, 1971; Grant, op.cit; Morse, op.cit.) has firmly established the finding that aggregate market price responds to annual and interim earnings releases. Researchers have interpreted these price responses to indicate that the market has impounded information; the data are useful (or relevant) to investors; they have information content. Beaver, Clarke, and Wright (1979) have extended this basic result to find that the price reaction is positively correlated with the absolute value of unexpected earnings. They find that the bigger the surprise in the earnings release, the greater the information content (as proxied by changes in market prices). Grant (op.cit.) and Atiase (op.cit.) find larger price reactions for smaller firms; they suggest that this result may arise if there are fewer

competing alternative information sources available for the smaller firms.

The major purpose of this study is to investigate the information content of annual earnings releases using a different measure of information content--market trading volume. In particular, an important question is whether results from price research also hold true for volume research. As discussed earlier, Jennings, Starks, and Fellingham's (op. cit.) sequential information arrival model suggests that the results of price and volume research may conflict. The price statistic is an average measure which can provide evidence concerning the behavior of the aggregate market, or possibly, a group of sophisticated market-makers. The trading volume statistic, on the other hand, is a summation of all individual trades. One individual cannot cancel out another's action, or trade. Volume research, therefore, provides a movement away from the aggregate market response toward individual investor behavior. As such, market trading volume provides a different definition of information content: one that focuses on individual reactions as opposed to aggregate reactions. Further, Lev and Ohlson (op. cit.) suggest that in certain circumstances, trading volume may even provide a valid measure of social welfare!

A brief outline of the more specific research objectives follows:

- 1) To determine whether abnormally high trading occurs around the time annual earnings are released,
- 2) To determine whether the volume of trading is positively associated with the magnitude of the surprise in the earnings release,
- 3) To investigate the size effect; in particular, to ascertain whether firm size is negatively associated with abnormal trading. If trading is found to be

proportionately greater for the smaller firms, the study will also investigate whether this result may occur because the earnings releases of the smaller firms are more surprising than the releases of the larger firms,

- 4) To determine whether the results are sensitive to adjustments for market-wide and firm-specific average levels of trading,
- 5) To determine whether the type of news in the earnings release (good news or bad news) affects the research results, and
- 6) To determine whether the results hold across fiscal year-end dates and stock exchanges.

The first objective will be examined using nonparametric tests of difference in location. In particular, the tests will determine whether trading around the earnings release date exceeds the average level of trading throughout the year. Previous price and volume research has established that earnings releases do have information content (e.g., Beaver, (op. cit.), May (1971), Kiger (1972)); therefore, if this study's trading volume surrogates for information content are indeed valid, the study should find that the volume of trading increases around the earnings release dates. Such a result would also be consistent with the sequential information arrival models (i.e., information induces abnormally high trading). Finally, if the assumptions of the social welfare models (discussed in Chapter II) are met, such results would also suggest that the earnings releases increase social welfare by increasing at least one investor's ex ante expected utility without decreasing any investor's ex ante expected utility.

Assuming that the trading volume metrics used in this study are found to be valid proxies of information content (i.e., trading volume

increases upon the release of annual earnings), the second objective will be investigated via nonparametric correlation analyses. Several measures of unexpected earnings and trading volume will be employed, and Spearman rank order correlations will be calculated between the different measures of unexpected earnings and trading volume in order to determine whether larger surprises are associated with increased trading. This objective seeks to complement prior research by extending a previously documented dichotomous relationship (earnings releases generate additional trading) to an ordinal relationship (more surprising earnings releases generate more trading than less surprising releases). A positive association between trading volume and unexpected earnings would be consistent with Griffin and Ng's (1978) model (to be reviewed in Chapter II), which suggests that trading is higher for conflicting information or signals (e.g., surprising earnings releases) than for reinforcing signals (e.g., confirming releases).

The third objective will also be examined via nonparametric correlations. The firm size effect is investigated by correlating firm size with 1) trading volume around the earnings release date, and 2) the unexpected earnings or surprises in the annual earnings releases. Finding a proportionately greater trading volume reaction for smaller firms would be consistent with the Banz (op. cit.), Grant (op. cit.), and Atiase (op. cit.) studies which document a similar size effect in price research.

The fourth objective reflects the lack of research on the trading volume metric. Although the two dominant volume theories--sequential information arrival models and social welfare models--assume that

trading occurs only in response to firm-specific information releases, these theories give no specific guidance on just how to mitigate the effects of either market-wide factors or different firm-specific average levels of trading. The question reduces to an empirical one: does adjusting for market-wide or firm-specific average levels of trading help reduce the noise in the volume data, and if so, which types of adjustments are most effective? In order to provide some evidence on this question, all statistical tests will be performed on three sets of volume measures--1) unadusted percentage of shares traded, 2) trading volume adjusted for a firm-specific average level of trading, and 3) volume adjusted for market-wide trading variations.

The fifth research objective recognizes that humans are not necessarily symmetric information processers with symmetric utility functions (e.g. Tversky and Kahneman, 1979, 1980; Schacter and Hood, 1982). Investors may react differently to good news than to bad news; for example, Schacter and Hood (op.cit.) report that bull markets are accompanied by a euphoria characterized by feverish trading upon the receipt of good news. This observation seems to be a clear example of Tversky and Kahneman's (1980) theory of decision framing, where information is interpreted differently, depending upon the decision-maker's frame of reference.

Sixth, prior research has sometimes employed convenience samples of NYSE firms (e.g., Kiger, 1972), non-12/31 firms (e.g., Beaver, op. cit.) or 12/31 firms (e.g., Foster, 1973). To date, no published study has considered the impact of these factors on research results. The

final objective, therefore, attempts to determine whether the results of this study can be generalized across these different types of firms.

The fifth and sixth objectives essentially focus on the generality of the findings concerning the first three objectives. Statistical analyses of the last two objectives will be accomplished by classifying the firms into groups according to:

- 1) direction and magnitude of unexpected earnings,
- 2) fiscal year-end month, and
- 3) stock exchange.

The nonparametric statistical analyses will be replicated on each of these subgroups in order to determine whether the study's findings are affected by any of these three factors. Further, periods of varying length around the earnings release date will be examined in order to test the sensitivity of the research results of the chosen test period.

The Investigation

A sample of approximately 400 firms was randomly selected from those included in the 1979 Valueline Investment Survey. (hereafter, Valueline). Additional selection criteria follow:

- 1) earnings announcements appear in the Wall Street Journal Index, and
- 2) trading volume is available through Compuserve's Value securities data base.

Firms were selected from Valueline because this publication was the only available source of quarterly earnings forecasts for large numbers of firms (1600) traded on the New York, American, and over-the-counter stock exchanges for the period 1977-1979.

The experimental design used in the study permits direct consideration of the research objectives and statistical testing of the related hypotheses. The test results may be summarized as follows:

- 1) Trading around the date of the annual earnings release is significantly greater than the median trading during the calendar year of the release.
- 2) Trading volume is significantly positively correlated with the absolute value of unexpected earnings.
- 3) Around the time of the annual earnings release, trading is proportionately greater for smaller firms than for larger firms. However, unexpected earnings were also greater for the smaller firms.
- 4) Removal of market-wide and firm-specific trading effects did not change the direction of the results, but did weaken the magnitude and significance of the results.
- 5) The magnitude, but not the significance of certain results was also affected by different magnitudes and directions of unexpected earnings, fiscal year-end date, and stock exchange.
- 6) Trading volume was more highly correlated with naive (e.g., random-walk) and outdated forecasts than with more current and sophisticated Valueline analysts' earnings forecasts. This result suggests the possibility that a large group of traders may base their trading decisions on relatively uninformed expectation models.
- 7) The Valueline analysts' forecasts were remarkably accurate. The median error was about one percent, while the median absolute error was about five percent. Compare these figures to the accuracy of a simple random-walk forecast model which uses last year's EPS as the prediction of this year's EPS: twelve percent median error, with a nineteen percent median absolute error.

Contributions of the Study

The primary contribution of the study consists of the following empirical evidence:

- 1) the volume of security trading seems to provide a valid measure of the elusive concept of information content (to be discussed in more detail in Chapter III),
- 2) several well-established results from price research are also found to hold true for the trading volume metrics used in this study, and
- 3) the magnitude of certain results is indeed affected by factors such as stock exchange and fiscal year-end date.

As discussed above, replication of price research results using trading volume as the measure of information content may yield new insights into market behavior, because the two metrics, price and volume, do not capture the same phenomena.

The study incorporates several methodological improvements upon prior research. First, the comprehensive sample includes firms listed over-the-counter (32) and on the American exchange (16) as well as on the New York exchange (349). The largest sample in a previously published volume study was Beaver's (1968) work which investigated 143 NYSE firms. Morse (1980, 1981) performed the only volume studies which included AMEX (five firms) and OTC (20 firms) stocks.

This study is believed to incorporate the first attempt to construct separate trading volume indices for the three exchanges: New York, American, and over-the-counter. Construction of the indices and their characteristics will be discussed in more detail in Chapters III and IV, but it should be pointed out that the three indices exhibited very different behavior. In fact, the OTC (over-the-counter) index was negatively correlated with the NYSE (New York Stock Exchange) index!

The study's major substantive contribution lies in the investigation of the association between trading volume and the magnitude of earnings forecast errors. Previous research has simply

established that earnings releases are associated with abnormally high trading (e.g. Beaver, op.cit., Morse, 1981). This study finds evidence that more surprising earnings releases are associated with higher levels of trading. In operationalizing the concept of earnings surprises, the study improves upon past research (e.g. Beaver, Clarke, and Wright, op.cit.) by employing analysts' forecasts as the measure of market expectations. Crichfield, Dyckman, and Lakonishok (1978), Brown and Rozeff (1978), Collins and Hopwood (1980), and Fried and Givoly (1981) all provide evidence that financial analysts generate more accurate earnings per share (EPS) forecasts than those obtained from mathematical time-series models. To the extent that financial analysts' forecasts provide a better surrogate for market expectations, use of analysts' forecasts provides a sharper measure of the earnings surprises. Improved measurement of the research variables enhances the power of the statistical tests.

In addition to merely documenting the fact that earnings releases are informative, this study extends prior work by investigating potential explanations for why these releases are useful. The results of this study suggest that several factors may affect the amount of information content in an earnings release. First, the amount of surprise in the earnings announcement is positively correlated with abnormal trading. The bigger the surprise, the more abnormal trading. In addition, the level of trading is proportionately higher for smaller firms than for larger firms. Perhaps this observation occurs because there are fewer alternative sources of information about the smaller firms--evidenced by larger surprises in their earnings releases.

Finally, the research follows the current emphasis on multi-method approaches (see Foster, 1980) by employing a measure of investor reaction which has received little exposure in the capital markets literature: trading volume. The research results provide evidence that trading volume provides a valid measure of information content which can supplement security prices in capital markets research.

Chapter Summary

In its Concepts Nos. 1 and 2 the FASB has evidenced a concern that accounting information be useful to individual investors making portfolio decisions. The usefulness of information to decision-makers has been investigated via three approaches: capital markets price research, capital markets trading volume research, and (laboratory) experiments focusing on how individuals use information to make decisions. The third approach would yield the best insight into specific use of information by specific investors, but the method is hampered by difficulty in generalizing the results to other information, tasks, and investors, as well as the lack of realism inherent in any laboratory study.

Use of the capital markets data base avoids the problems of the laboratory studies. Until recently, most of the accounting research in the markets area focused on price tests. Possible reasons for this emphasis include an interest in the response of the aggregate market or sophisticated market-making investors, and the existence of equilibrium capital asset pricing models. This emphasis on price research may be shifting for at least three reasons. First, the sequential information

arrival models show that price and volume research may not always yield identical conclusions. Second, the social welfare models suggest that if certain assumptions are met, it is trading volume which may serve as a measure of the social value of information releases. Finally, policy-making bodies are showing increasing concern for the plight of the individual investor (as opposed to the sophisticated market-makers or the aggregate market reaction), a phenomenon which may be better captured by trading volume research than by price research. Whether or not the factors favoring price research are diminishing, accounting researchers are beginning to examine other market variables such as trading volume (e.g. Morse, 1980, 1981) and options pricing (e.g. Patell and Wolfson, 1981).

One drawback of the trading volume measure is the absence of a single well-developed and accepted theory of trading in the financial markets. Although the sequential information arrival models and the social welfare models are not inherently conflicting, they have not yet been integrated into a single widely-accepted theory. Work is proceeding rapidly in the area, however, and these models can 1) impart additional meaning to empirical results, and 2) suggest that researchers isolate the effects of firm-specific information releases. They unfortunately are not yet sufficiently developed, however, to provide specific technical guidance concerning the appropriate techniques for removing the effects of market-wide and firm-specific average levels of trading. Because trading volume may possess a relative advantage in answering interesting questions about individual investor behavior,

however, this study pursues this relatively unexplored capital markets line of inquiry into individual investor behavior.

This research replicates several results of price research using trading volume as the measure (or proxy) of information content. The study includes the most comprehensive sample investigated in any accounting study published to date. The major methodological improvements consist of the construction of three market trading volume indices--one for each exchange (NYSE, AMEX and OTC) and the use of Valueline's financial analysts' forecasts of earnings per share as surrogates for the market's earnings expectations. Finally, the study attempts to seek explanations for observed patterns of trading behavior, instead of merely documenting the existence of the behavior.

Organization of the Study

The next chapter presents the literature review. An expanded discussion of the market price versus market trading volume metric is followed by a review of the analytical work which has attempted to provide a foundation for the future development of a comprehensive theory of trading in the capital markets. The subsequent section reviews the empirical trading volume studies. On the basis of the literature review, Chapter III specifies the research hypotheses, the experimental design, and the statistical tests used to analyze the results. Chapter IV presents the results of the research. Chapter V, the final chapter, summarizes the study in terms of the results of the hypothesis tests and their relation to the research objectives. The chapter concludes with a discussion of the limitations of the study, as well as areas for future research.

CHAPTER II

LITERATURE REVIEW

This chapter reviews the finance and accounting literature relevant to a study of the association of trading volume with unexpected earnings. The purpose of the chapter is to justify the research objectives, review previous work in related areas to enable the reader to gain a proper perspective for evaluating this study, and to identify problems in prior work which have been corrected in this study.

The chapter is organized into three sections. The first section identifies factors which lead to trading in the financial market, and then examines in more detail the reasons why one factor, the release of firm-specific information, might lead to increased trading. This discussion is followed by the development of three examples which extend and illustrate the introductory chapter's discussion of the differences between price and volume research, and the implications of each. This first section lays a foundation for understanding exactly 1) what phenomena the trading volume metric measures, and 2) what types of inferences may be drawn from the research results. This foundation is essential for understanding the literature reviewed in the remainder of the chapter as well as Chapter III's detailed discussion of this study's hypotheses and methodology.

The remaining two sections review the research concerned with market trading volume. Since volume research is such a recent phenomenon, relatively few articles dealing with the subject have been published. As might be expected in the development of a new area, the initial research efforts appear to be loosely, if at all, related. However, it is possible to group the research into analytical and empirical categories. Of course, a few of the papers provide analytical models in conjunction with supporting empirical evidence, but the majority of the work falls cleanly into one category or the other.

Accordingly, the second section of this chapter traces the development of analytical work which has attempted to provide a framework for the future development of a more comprehensive theory of trading in the financial market. The analytical research is roughly divided into four segments. The first segment describing the early attempts to develop a volume model is followed by more detailed discussions of two recent, but disparate theoretical movements: sequential information arrival models and social welfare models. The fourth segment deals with two theories which seem unrelated to the other work discussed in this section, but which are nonetheless relevant to this particular study.

The third and final section reviews the previous empirical trading volume studies. The work is presented in chronological order, since it has not developed in a particularly orderly or logical fashion (i.e., the empirical articles are not particularly related to each other or to the theoretical work). The discussion illustrates the paucity of empirical evidence on market trading volume, and highlights weaknesses

of prior research which were corrected, or at least mitigated, in this study.

Price Versus Volume: An Expanded Discussion

Factors Underlying Investor Trading

Individuals may change their investment behavior (i.e., trade) for several reasons (Winsen, 1976; Foster, 1978):

- 1) to coordinate income earning and spending activities,
- 2) to rebalance the portfolio due to a change in the investor's risk/return tradeoff,
- 3) to obtain income tax advantages, or
- 4) to rebalance the portfolio due to revisions in relative probability assessments of return distributions or changes in assessment of relative security risk, based on information filtering into the market.

This study focuses on the fourth factor. For purposes of this research, the first three factors inject noise into the data. The problem is mentioned at this point to alert the reader that researchers are aware that trading may arise from a variety of reasons. Most volume research, however, ignores the first three factors and focuses on the fourth reason for trading: the release of firm-specific information. Aside from Chapter III's methodological discussion of empirical attempts to mitigate the impact of the first three sources of trading, this discussion will only concern itself with trading arising from the receipt of firm-specific information.

Why Firm-Specific Information Might Lead to Trading

Working in a world where information provides the only impetus for trading, Beaver (1968) was the first accounting researcher to point out

the different implications of price research versus volume research. He suggests that while security price tests reflect changes in the expectations of the aggregate market, volume tests reflect changes in individual investors' expectations. Of course, the released information may be interpreted differently by different investors. At least two documented psychological phenomena could explain this differential reaction to the same piece of information.

First, the notion of decision-framing suggests that humans interpret stimuli, rather than merely respond to them (Einhorn and Hogarth, 1981). One component of interpretation is the point of reference by which new stimuli are judged. In fact, the point of reference is so important for interpreting new information, that experimentally changing a person's reference point may even cause him to reverse his judgment or decision (Tversky and Kahneman, 1980). In this study, however, all that is required is that different investors have different reference points, which lead them to different decisions. Consider the following simplified example of decision framing in an investment setting. A plant manager in an auto-producing firm, investor A's opinion of the state of the economy is primarily related to unemployment and GNP, factors which are important to the growth of the auto industry. On the other hand, investor C, a manager of a computer producing firm, is more concerned about high interest rates which make it expensive for him to obtain capital for business expansion. If firm XYZ issues a mediocre earnings release in a period of declining GNP, employment and interest rates, A may interpret the release as good news relative to his perception that the economy is in trouble; his demand

for XYZ stock will rise. Since C thinks the economy is improving, he may interpret the earnings release as bad news, and his demand for XYZ stock will decline. *Ceteris paribus*, C will sell XYZ stock to A.

The second psychological phenomenon, cognitive dissonance reduction, could also explain different perceptions of the same information (see Festinger, 1957, 1964; Aronson, 1976; Hollander, 1981). Once a person makes a decision, he tends to seek, use, and recall information consonant with the decision (Jecker, 1974; Ehrlich, et al., 1957). If the concept can be extended to beliefs as well as decisions, it may explain why some investors act on information while others ignore it. Consider the following simplified example of cognitive dissonance reduction in an investment setting. J has recently purchased XYZ stock, pursuant to a hot tip from his most reliable broker. K also owns some XYZ stock, but is not so certain that XYZ is a "sure-bet." XYZ issues a mediocre interim earnings release. J ignores the release, rationalizing that all firms have occasional bad quarters. K, however, feels that this release confirms his feeling that XYZ is beginning to decline, and so his demand for XYZ stock declines. *Ceteris paribus*, K will sell XYZ stock to J.

This discussion is meant to provide an illustrative, rather than exhaustive list of well-documented psychological phenomena which could explain why different investors might interpret the same information in different ways. Verrecchia (1981) shows, however, that even if they interpret the information in the same way, investors may still respond differently (i.e., trade) due to nonhomogeneous or nonlinear risk preferences. That is, lack of investor consensus on the meaning of the

information is not a necessary condition for trading to occur.^{1,2} However, lack of consensus does seem to provide an intuitively appealing explanation for different reactions to the same piece of information, so this rationale will be used in the following explanation of why information can generate either price reactions, or volume reactions, or both. (Use of the nonhomogeneous or nonlinear risk function arguments would only complicate the story without changing the results).

Three Scenarios

Consider the three following examples in a two-person world where the number of shares of XYZ firm's stock is fixed, and trading occurs only in response to the release of information (adapted from Winsen, 1976).

As shown in Figure 2, let:

D_j^t = investor j's demand for XYZ stock at time t

S_j^t = investor j's supply of XYZ stock at time t

E^t = equilibrium at time t

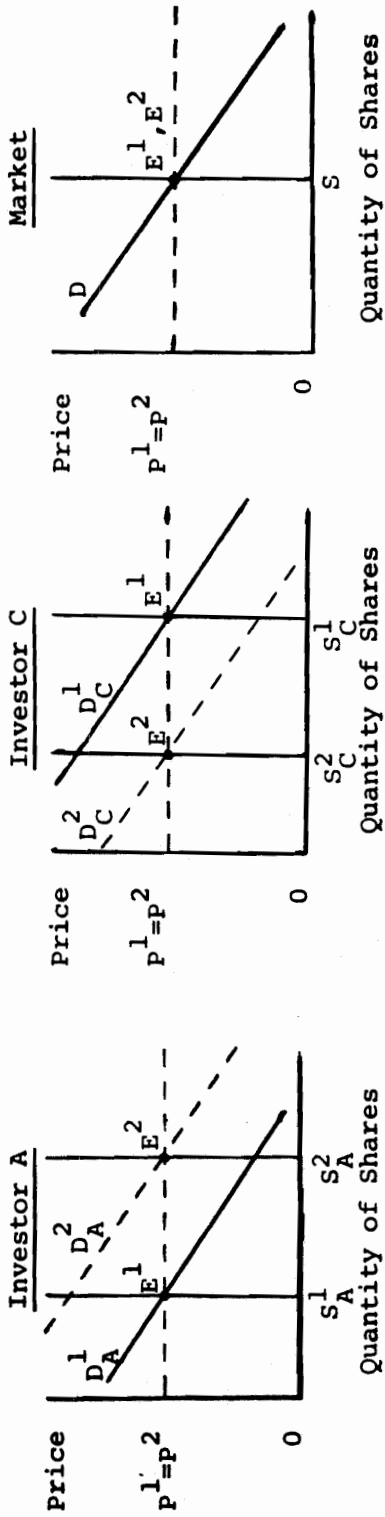
P^t = equilibrium price at time t

S = market supply of XYZ stock

$$\sum_j S_j^t = S^t, \forall t$$

$$\sum_j S_j^t = \sum_j D_j^t, \forall t$$

Let investor A perceive the release of XYZ's annual earnings as good news, while investor C feels it is bad news (perhaps due to different decision frames or cognitive dissonance reduction as discussed



Where:

D_j^t = individual j's demand for XYZ shares at time t

S_j^t = individual j's supply of XYZ shares at time t

P^t = equilibrium price of XYZ shares at time t

E^t = equilibrium point at time t

S = market supply of XYZ shares (fixed supply)

$$\sum_j S_j^t = S^t, \forall t$$

$$\sum_j S_j^t = \sum_j D_j^t, \forall t$$

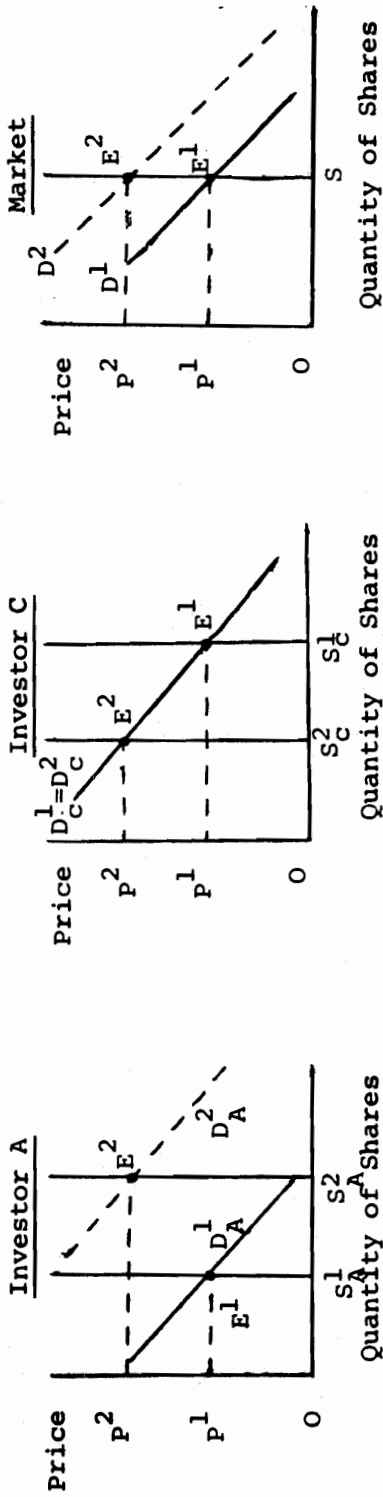
FIGURE 2

SCENARIO 1:

INFORMATION CAUSES INVESTORS TO CHANGE THEIR EXPECTATIONS IN EXACTLY OFFSETTING MANNERS: TRADING OCCURS BUT EQUILIBRIUM PRICE IS UNCHANGED.

above). A's demand for XYZ shares increases from D_A^1 to D_A^2 , while C's demand for XYZ declines from D_C^1 to D_C^2 . C sells XYZ shares to A. If C's decline in demand is exactly offset by A's increased demand, then total market demand does not change, and the equilibrium price also remains unchanged. This scenario is depicted in Figure 2; information release has generated an increase in trading volume, but has not caused prices to change. The accounting report had information content since it induced investors to change their behavior (i.e., trade). A volume study would indicate that the report did have information content because trading volume increased. On the other hand, a price study would indicate that XYZ's annual earnings had no information content because prices did not change. Jennings, Starks, and Fellingham's (1981) theoretical model, as well as Epps and Epps' (1976) empirical evidence suggest that large trading volume increases are sometimes unaccompanied by price changes. For this reason, trading volume should be investigated in order to capture the situation where trading occurs but prices do not change because extra supply equals extra demand.

Volume research also signals the case where the release of information causes both price and volume changes (as shown in Figure 3). Suppose that XYZ's annual earnings are good news to A, so he increases his demand for XYZ shares from D_A^1 to D_A^2 . However, the release merely confirms C's expectations, so his demand for XYZ shares does not change. Ceteris paribus, C sells XYZ shares to A. In this case, total market demand increases, and the equilibrium price of XYZ shares rises. Since equilibrium price has changed and trading has occurred, either price or volume research would indicate that the report had information content.



Where:

D_j^t = individual j 's demand for XYZ shares at time t

S_j^t = individual j 's supply of XYZ shares at time t

p^t = equilibrium price of XYZ shares at time t

E^t = equilibrium point at time t

S = market supply of XYZ shares (fixed supply)

$$\sum_j S_j^t = S^t, \forall t$$

$$\sum_j S_j^t = D_j^t, \forall t$$

FIGURE 3

SCENARIO II:

INFORMATION CAUSES INVESTOR'S EXPECTATIONS TO CHANGE IN A NON-OFFSETTING MANNER: TRADING OCCURS AND EQUILIBRIUM PRICE CHANGES.

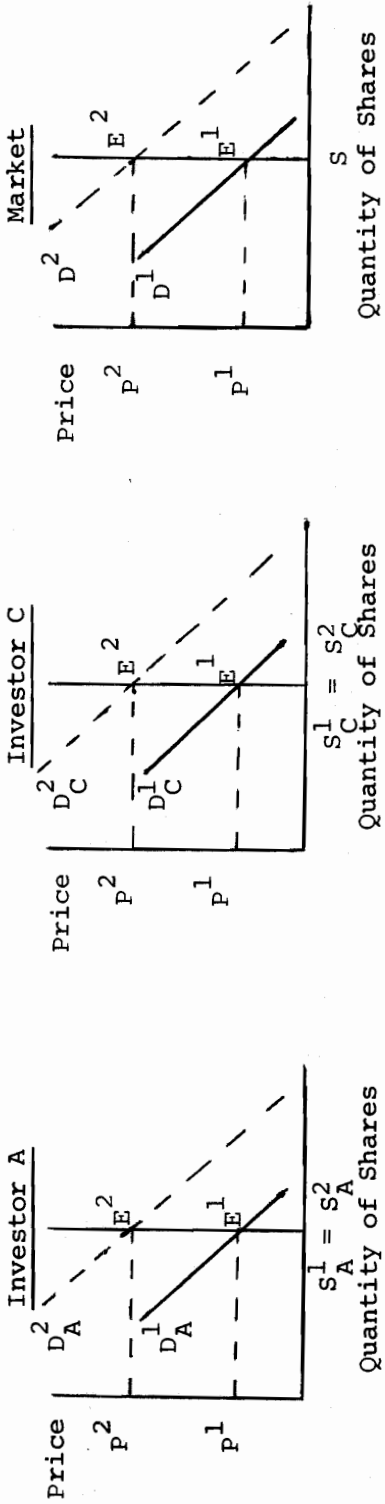
The third scenario recognizes that although it is unlikely to occur in reality, it is theoretically possible that the receipt of information may cause implied price to change even in the absence of trading (Figure 4). This situation would occur if the investors perfectly anticipated the change in equilibrium price. Suppose now that XYZ's earnings release is perceived as equally good news by both investors, and so their demand curves for XYZ shares increase equally (see Figure 4). Total market demand will increase, and equilibrium price will rise. If, however, the investors perfectly agree on the price increase, they will not trade, and the price will simply rise to the new equilibrium. Price research, but not volume research, would signal this type of reaction.

In summary, volume research can detect investor reaction to information when:

- 1) investors' changes in security demand curves are completely offsetting. In this scenario, individuals change their portfolios, but the aggregate market price does not change (the first scenario). Price research could not detect this use of information.
- 2) investors' changes in security demand curves are not completely offsetting, and the resulting price changes are not perfectly anticipated (the second scenario). In this case, either price or volume research would detect the use of information.

Analytical Volume Research

This section begins by reviewing the early theoretical work of Epps. Next, the two major current directions of volume research are discussed--the sequential information arrival models and the social welfare models. Finally, two recent but unrelated developments are presented.



Where:

D_j^t = individual j's demand for XYZ shares at time t

S_j^t = individual j's supply for XYZ shares at time t

p^t = equilibrium price of XYZ shares at time t

E^t = equilibrium point at time t

S = market supply of XYZ shares (fixed supply)

$$\sum_j S_j^t = S^t, \forall t$$

$$\sum_j D_j^t = \sum_j S_j^t, \forall t$$

FIGURE 4

SCENARIO III:

INFORMATION CAUSES INVESTORS TO CHANGE THEIR EXPECTATIONS EQUALLY AND IN THE SAME DIRECTION: EQUILIBRIUM PRICE CHANGES BUT NO TRADING OCCURS.

The Early Works

Epps (1975) constructed a model under the following assumptions:

- 1) all investors are mean-variance, expected utility of terminal wealth maximizers,
- 2) no short-selling or borrowing constraints, and
- 3) perfect and frictionless markets.

Epps classifies the investors in two groups, bulls (optimists) and bears (pessimists), according to the way they interpret new information. He adopts the idea of cognitive dissonance to support his assumption that bulls will discard small bits of bad news (bad news is inconsistent with their optimistic outlook) while bears will discard small bits of good news (which is inconsistent with their pessimism). Large news items, however, will cause a regrouping of bulls and bears in accordance with the item received. Finally, bulls must be at least as risk-averse as bears. Epps constructs an economic model based on these assumptions, and concludes that trading in response to good news should exceed trading in response to bad news. Epps attempts to test this hypothesis by using price increases (decreases) as a surrogate for good (bad) news. He examines the trading volume of twenty NYSE bonds at the time of price increases and declines, and he finds that trading volume is higher for price increases than for price decreases.

Schneller (1978) points out the internal inconsistencies in the Epps' model. The mean-variance investor model requires either normal distribution of security return expectations (Epps' are lognormal) or quadratic utility functions (Epps requires constant risk-aversion). In addition, the requirement that bulls are at least as risk-averse as

bears is counter-intuitive, and this assumption seems to drive the model. Schneller concludes that based on Epps' model, no unequivocal relationship between price change and volume can be specified.

Epps (1976) also develops a model of trading volume in relation to transaction costs. The major assumptions are:

- 1) investors are mean-variance expected utility of terminal wealth maximizers,
- 2) risk aversion is constant and independent of wealth,
- 3) investors can be divided into two groups with identical demand functions, and
- 4) perfect, frictionless markets, except that there are
- 5) constant transaction cost per share of stock.

Epps' (1976) model predicts that trading volume is a decreasing function of transactions costs (composed of transfer taxes, the bid-ask spread, and broker commissions). In an empirical investigation of ten stocks from March to August 1968, Epps finds a negative correlation between transaction cost per share and trading volume. Although his empirical evidence is admittedly weak, Epps' results question the validity of one of the major assumptions inherent in subsequent theoretical work: the assumption of frictionless markets.

Sequential Information Arrival Models

The initial work on sequential information arrival models was Copeland's (1976) theory of asset trading. The theory requires the following assumptions:

- 1) perfect, frictionless markets,
- 2) no short sales, and
- 3) information shocks cause investors' demand curves to shift up or down by equal amounts.

One testable implication of Copeland's model is that trading volume is a logarithmically increasing function of the number of traders and the strength of the new information. Since stronger information should also cause larger price changes, the model predicts that these large price changes should be accompanied by abnormally high trading. The traditional tatonnement model, however, makes the opposite prediction that the highest trading volume will occur when investors disagree.³ Copeland (op. cit.) suggests that investors would be likely to disagree on the implications of weak information, and weak information would be expected to cause only a small price change, if any. The tatonnement model, therefore, predicts that trading volume will be higher at the time of smaller price changes. Crouch (1970) and Epps (1975) provide evidence that changes in price and trading volume are positively correlated, so the empirical evidence is consistent with Copeland's sequential information arrival model. Unfortunately, a positive correlation between price and volume changes could also be explained by the existence of transaction costs: investors may be unwilling to trade unless their expectations change by more than a threshold amount.

Copeland (1977) constructs a second model of trading under sequential information arrival, and the assumptions for this model follow:

- 1) perfect, frictionless markets,
- 2) single indivisible asset bid upon by the investors, and
- 3) no short selling.

In this model, investors sequentially and randomly receive new information, then immediately reevaluate the asset based on the new information. Investors may interpret the same news differently, so they may submit different bids for the asset. The model predicts that more trading is generated by good news than bad news (the same prediction as Epps (1975)). The problem with this model is that it seems to be driven by the exclusion of short sales. Investors who interpret the news as favorable can buy infinite amounts, while investors who interpret the news as unfavorable can sell only what they own. More trading will occur if investors can trade infinitely (good news) than if they can trade only what they own (bad news).

Morse (1980) extends Copeland's sequential information arrival models to derive empirically testable hypotheses. His model predicts that information asymmetry, a result of sequential information arrival, will be characterized by:

- 1) increased trading--informed investors will attempt to trade with less-informed investors
- 2) monotonic price movements--as more investors receive the information, the price will gradually move toward its equilibrium position
- 3) trading before a large price change--by insiders who have early access to information which causes a price change upon release.

Morse investigates 25 NYSE, 5 AMEX, and 20 OTC stocks for the period 1973-1976. Although he is unable to devise a strategy to earn abnormal returns, he does find evidence of positive serial correlation of price residuals during periods of abnormally high trading. This finding suggests that monotonic price movements (the second hypothesis above) do occur for his sample. Morse also finds that periods of high price

residuals are preceded by abnormally high trading. Thus, his sample evidence supports the third hypothesis above. As he points out, the next logical step involves examining specific information releases or signals rather than using ex-post returns as surrogates for signals.

Jennings, Starks, and Fellingham (1981) develop the most realistic economic model of sequential information arrival and its effect on trading volume. They generalize Copeland's (1976) model by allowing short sales (with some restrictions) rather than completely excluding them. Assumptions of their model follow:

- 1) there exists a single risk-free asset and a single risky asset,
- 2) all lending occurs at the risk-free rate which is exogenously determined,
- 3) investors deduce no information from prices,
- 4) investors maximize the expected utility of their terminal wealth,
- 5) when investors receive information, they adjust only their expectations of the security return distribution; they do not adjust their estimation of the distribution's variance, and
- 6) perfect, frictionless markets, except that
 - information arrives sequentially--it is not equally accessible, and
 - transactions costs are levied in the form of a margin requirement on short sales. An investor wishing to take a short position must put up the margin requirement, along with the proceeds of the short sale, until she or he eliminates the short position. The transactions cost arises in the form of an opportunity cost--the broker does not pay the risk-free rate on the sale proceeds or margin requirement.

All investors initially occupy equilibrium positions. Information is released and passed sequentially from one investor to the next, in

random order, until all investors are informed. For example, the first investor to receive the information may decide she or he is no longer satisfied with the portfolio position. The investor will trade until a new equilibrium obtains. This new equilibrium will probably occur at a different price, so other investors may trade as a result of their dissatisfaction with their portfolios. The point is that the information causes the first investor to trade, but the first investor's trade may change the market price. If so, then other investors may trade as a result of the new equilibrium price. The information release thus generates a series of equilibria as each investor receives the information.

The authors investigate the relationship between information shocks, price changes, and volume changes. In the absence of margin requirements on short sales, Jennings, Starks, and Fellingham develop an analytic solution. Imposition of the margin requirement, however, complicates the model so much that simulations were employed to solve the relationships. The number of traders, their initial beliefs, and the percentage of optimists (investors who interpreted the information as good news) were input into the simulation. The maximum price change occurs when all investors agree on the implications of the information. Since the price change depends upon the difference between the expectations of uninformed investors and the average final market expectation, the price change will be greater if investors are all optimists or are all pessimists. In either situation, all investors will change their expectations in the same direction, so there will be no offsetting.

With the margin requirement, the minimum price change occurs when the economy consists of more pessimists than optimists (rather than a 50-50 split). This result occurs because the margin requirement makes a short position more expensive than a long position. For this reason, pessimists will adopt less extreme positions than they would otherwise. Pessimists, therefore, exert a smaller impact on equilibrium price (than optimists), so more pessimists are required to offset the effects of the optimists.

Under fairly general assumptions, however, trading volume is a concave function of the percentage of optimists. That is, the maximum volume occurs at the point of maximum disagreement of the investors--when the optimist-pessimist split is about 50-50.

In summary, the model yields two important conclusions for future volume research:

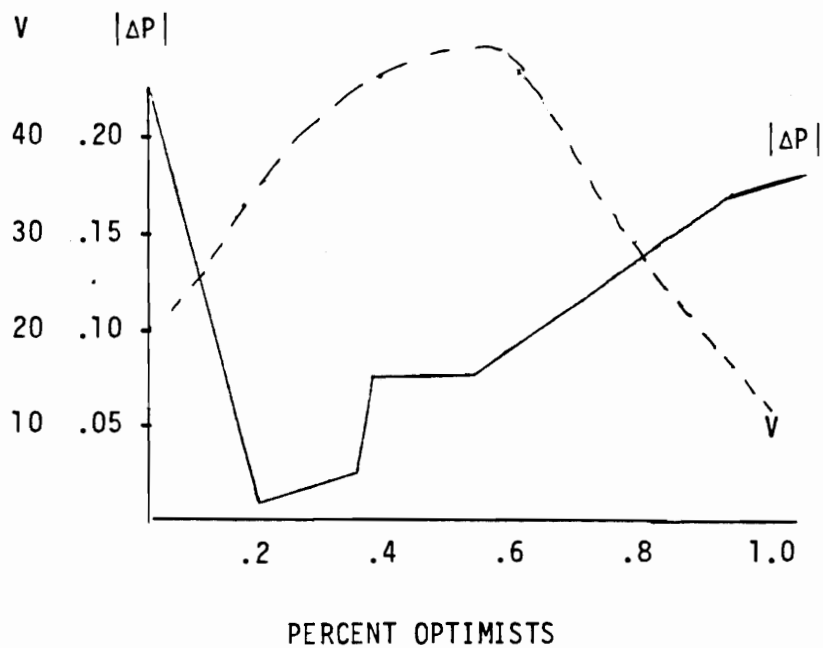
- 1) while price and volume changes may be positively correlated over a certain range (26 to 60 percent optimists), these changes will probably be negatively correlated outside this range (see Figure 5, adapted from Jennings, Starks, and Fellingham, (op. cit.))
- 2) the minimum price change often occurs at the time when trading volume is at a maximum (see Figure 5)

For these reasons, price research alone may be insufficient to fully describe the market adjustment to new information: volume research may provide different insights into old problems.

Social Welfare Models

In a 1977 paper, Hakansson articulated his opinion concerning the direction of accounting research:

...While the capital asset pricing model has been very valuable in generating hypotheses for important empirical



$|\Delta P|$: The absolute value of the price change associated with the information shock.

V : The trading volume associated with the information shock.

FIGURE 5

JENNINGS, STARKS, AND FELLINGHAM'S SIMULATION RESULTS OF THE PRICE CHANGE - VOLUME RELATIONSHIPS

tests, it is essentially useless as a basis for policy recommendations. Such recommendations must be founded on richer models of financial markets, models which would also have to be consistent with current empirical findings. Unfortunately, such models have not been forthcoming in the quantity and 'quality' one might desire... (p. 20).

In an attempt to remedy this problem, Hakansson has created a new direction for analytical accounting research. Hakansson, Kunkel, and Ohlson (forthcoming) develop a model which may eventually provide a basis for policy recommendations. In the article, the authors specify the necessary and sufficient conditions for publicly released information to have value in a pure exchange economy. In this setting, the translation of the abstract concept of social value into a concrete, measurable phenomenon reduces to an investigation of the necessary and sufficient conditions for public information to induce trading (since rational investors will not trade to an allocation which leaves them worse off than their original endowment). In their two-period model, the authors specify the following plausible sufficient conditions for trading to occur (i.e., public information to have social value):

- 1) investors' initial endowments are equilibrium allocations with respect to their prior beliefs,
and one of the following:
 - 2) consumers do not have essentially homogeneous conditional and unconditional message beliefs,
or
 - 3) The financial markets are not fully allocationally efficient,
or
 - 4) investors' utility functions are not time-additive.

Kunkel (forthcoming) extends this work to a production and exchange economy. In a pure exchange economy, information aids investors' portfolio decisions. When the economic model is extended to include production as well as exchange, information fulfills another function-- it may be used to reallocate resources across time and firms by causing changes in production plans. Therefore, one would expect information to be at least potentially more useful in a production and exchange economy than in a pure exchange economy. Indeed, Kunkel's results support this intuition. He finds that the sufficient conditions for trading (i.e., public information to have social value) in a pure exchange economy (as listed above) may be relaxed in a production and exchange economy. That is, even if the above requirements are not met, public information may still induce trading in a production and exchange economy if that information is used to reallocate resources across time or firms.

Kunkel lists the following sufficient conditions for trading to occur in a production and exchange economy:

- 1) endowments of current consumption and firm shares are in equilibrium with respect to consumers' prior beliefs, and
- 2) no consumers are endowed with short positions in any firm, and
- 3) the financial market is complete, and
- 4) firms maximize their net market value.

Ohlson and Buckman (1981), in an overall synthesis of results point out that in contrast to the sequential information arrival models, the social welfare literature generally makes the following assumptions:

- 1) one and two period models,
- 2) homogeneous information (that is, public information is simultaneously available to all investors), and

- 3) specific characteristics of the trading structure (set of rules governing the opening and closing of markets) and the set (number) of available securities.

The authors reiterate the point that in a pure exchange economy, the conditions under which information has social value depend upon two factors: 1) whether the initial endowments are Pareto-efficient given no information, and 2) whether the information yields new and desirable allocational opportunities (i.e., generates trading). That is, if the initial endowments are Pareto-efficient with respect to no information, then the investor can always ignore the information and remain as well off (in an ex ante sense) as he was without information. If the information generates an opportunity for the investor to increase his expected utility, then he may trade to achieve his preferred position. Trading, therefore, is evidence that the information helped investors to achieve preferred positions; it is thus an indicator of social value. It is noteworthy that this result is independent of the trading structure, or set of rules governing the opening and closing of markets. It should also be emphasized that the definition of social value employed in this literature states that information has social value if and only if no investor is worse off and at least one investor is better off, subsequent to receipt of the information. The terms "worse off" and "better off" are, however, defined in terms of ex ante expected utilities, not in terms of the utility of the actual outcome. Thus, the social value of information is an ex ante concept.

Lev and Ohlson (forthcoming) suggest that accounting researchers must not expect too much from social welfare models:

It is important to appreciate that sweeping and general results about the net social benefits of changes in the

economic system are rarely, if ever, available. Any conclusions are bound to be extremely contextual, and models with discernable welfare implications are typically based on rather strong and specific assumptions. In any sufficiently rich and realistic setting it goes almost without saying that some individuals will be better off while others will be worse off...

The authors derive three interesting welfare implications, but as they admit, their results are based upon very restrictive assumptions. Their first result addresses efficient markets research. The authors adopt Beaver's (1981) definition of market efficiency as price-invariance. That is, the market is said to be informationally efficient with respect to a particular piece of information if pre-disclosure prices are the same as post-disclosure prices, whether or not the information is known by all investors. Lev and Ohlson argue that disclosure of information already impounded in market prices may have social value. If the market is efficient (i.e., price-invariant) with respect to a signal, then disclosure of that signal cannot harm any investor because no previously available opportunities are eliminated. Such disclosure might, however, generate abnormal trading: price-invariance does not necessarily imply belief-invariance, although the converse is true. It is therefore possible that poorly informed, price-taking investors might improve their expected utilities (well-being) by trading. Additionally, the released information might facilitate a more efficient risk-sharing arrangement. Either of these situations would be evidenced by additional, or abnormal, trading. If such abnormal trading occurs, the investors involved must be strictly better off. This result occurs because the investors would not engage in additional trading unless they perceived an increase in their

well-being or expected utility. The authors state their conclusion as follows:

volume of trading is a relevant welfare indicator provided that an appropriate hypothesis is imposed on the system of prices. (Emphasis in the original).

In their first result then, the "appropriate hypothesis" is one of price-invariance: disclosure of the signal must not change the market-price structure.

Lev and Ohlson's second result pertains to information content studies. Their general conclusion that trading volume is a valid measure of social welfare now hinges on a different pricing hypothesis. They recognize, of course, that the price-invariance assumption is not appropriate for information content studies. The price-invariance hypothesis is replaced by the weaker assumption that prices in the pre-disclosure economy are unbiased estimates of prices in the post-disclosure economy. In order to avoid negative redistributive effects, this weaker price assumption must be accompanied by two further assumptions:

- 1) investors have homogeneous information and identical beliefs about the realization of specific signals
- 2) investors' endowments are constrained--Pareto-efficient equilibrium positions with respect to their beliefs prior to signal disclosure.

If these assumptions are satisfied, no investors will be worse off (in the ex ante expected utility sense), and some may be better off with disclosure. Again, to the extent that additional trading occurs, those involved will be strictly better off.

These first two results apply to pure exchange economies where information is costless. The authors emphasize that even in a pure

exchange economy, trading should not be viewed as a zero-sum game. Although the size of the market pie is fixed in a narrow sense, trading may improve the risk-sharing arrangements. To the extent that risk is shared more efficiently, social welfare will improve. As long as negative redistributive effects are prevented (through satisfaction of the authors' assumptions), investors engaging in trades must be improving their risk positions (and thus their expected utilities), otherwise they would have no incentive to trade.

Lev and Ohlson's final result yields implications for regulation research. They move from a pure exchange to a production and exchange economy, because they recognize that regulation can affect both security prices and the firms' production plans. The authors demonstrate that an increase in the security price (generated by the change in production plans) is not sufficient to ensure that social welfare increases, because all investors may not see the price increase as an improvement in their well-being. That is, stockholders are not necessarily unanimous about the optimal way to maximize firm value unless:

- 1) the production of new commodity bundles is not contemplated, and
- 2) the price structure of basic commodities is unchanged across production plans.

Research results are currently mixed with respect to the descriptive validity of the second requirement.

If stockholders are in fact unanimous about the optimal means to maximize firm value and if regulation results in higher firm share price, then social welfare has improved. Lev and Ohlson argue that the

absence of trading is consistent with stockholder unanimity. The absence of trading is also an implicit result of Erkern and Wilson's (1974) scenario. If:

- 1) the change in firm plans is small,
- 2) the new plan does not change the commodity-bundle, and
- 3) the pre-change market is in equilibrium,

then there is ex post unanimity about the change in plans. If one investor believes he is better (worse) off, then all investors will believe they are better (worse) off, and further, the value of the firm's plans will increase (decrease).

Lev and Ohlson conclude that if researchers investigating regulations are worried about "stockholder unanimity regarding the alleged productive effects," they should test for absence of abnormal trading. Unfortunately, the production and exchange economy is so much more complex than the pure exchange economy that researchers have not yet discerned a relationship between volume of trading and improved risk-sharing when the pure exchange economic model is extended to include production.

As might be expected, the development of social welfare theory is not without its critics. Verrecchia (forthcoming) points out that the proponents cannot even agree on the meaning of the term "social value." The first candidate definition is that information has social value if it increases the aggregate supply of the economy's goods and services. The second definition requires a Pareto-improvement in the investors' expected utilities (this is the definition used by the research reviewed here). However, even if this second definition is accepted, Verrecchia

points out that researchers are still faced with the problem of which beliefs to use in measuring the change in investors' utilities: the ex ante beliefs prevailing before the information is revealed, or the ex post beliefs which result after the investor receives the information? Although the latter measure would seem to make more sense if the research is investigating the usefulness of the information, it appears (although it is never explicitly stated) that the research reviewed here has employed the former measure--ex ante beliefs.

In conclusion, the social welfare theory reflects a change in emphasis from essentially positive research on the impact of a signal, to a more normative approach appropriate for policy recommendations. These articles have developed models designed to illustrate how welfare economics can be exploited to enrich the interpretation of empirical findings; in particular, they suggest that if certain assumptions are satisfied, market trading volume may provide a valid index of the social value of information releases.

Two Miscellaneous Theories

An article published in the Wall Street Journal unveiled "bubba psychology," the creation of Stanley Schacter and Donald Hood, two Columbia psychologists. Schacter explains that "bubba psychology is the study of what Jewish grandmothers know without benefit of graduate training," (i.e., common sense). Schacter and Hood argue that although investors may behave rationally during stable markets, they react more emotionally during bull and bear markets. For example, they found that strong "buy" or "sell" recommendations in the "Heard on the Street" column are accompanied by larger price changes and more trading during a

bull or a bear market, as opposed to a stable market. The authors also examined 1972-1979 episodes where the industrial average rose for at least five consecutive days. During stable or bear markets, NYSE trading tended to peak on the fourth day at about a 20 percent increase over the first day. In a bull market, however, trading tended to increase until the final day of the run, peaking at a volume nearly 70 percent higher than the first day's. This theory that investors' decisions are affected by the general mood of the market (bull or bear) suggests that market-wide factors may indeed be important explanators of trading volume.

The final analytical paper reviewed, and perhaps the one most closely related to this study's major hypotheses, is the Griffin and Ng (1978) model. They develop a single-period economy where investors hold uncertain and heterogeneous beliefs. The primary contribution of their approach is the consideration of how investors process information from distinct and competing sources. The major assumptions of the model follow:

- 1) the securities market is perfect (i.e., frictionless and all investors behave as price-takers),
- 2) unrestricted short selling,
- 3) unlimited lending and borrowing at the risk-free rate,
- 4) all investors possess differentiable utility functions of the form

$$V = U[E(\tilde{Y}), \text{Var}(\tilde{Y})]$$

where:

$E(\tilde{Y})$ = mean end-of-period wealth

$\text{Var}(\tilde{Y})$ = variance of end-of-period wealth,

and the partial derivatives behave as follows:

$$U_1 > 0$$

$$U_2 < 0$$

$$U_{11} < 0$$

- 5) investors' risk aversion is independent of the mean and variance of their end-of-period wealth,
- 6) investors may have different beliefs about the mean vector of firm end-of-period market values, but the investors have a common assessment of the covariance matrix of end-of-period market values,
- 7) news shocks have the following characteristics:
 - a) they are used only to revise estimates of the mean (not the variance) of the firms' end-of-period market values,
 - b) they are firm-specific; that is, if some investors use the news to estimate the mean of one firm's end-of-period market values, no investors may use the news to estimate the mean value for any other firm,
 - c) they are public information (known by all investors), and
 - d) they are real numbers,
- 8) investors have linear information processing functions.

Griffin and Ng investigate the effects of signals from competing sources. A pair of signals is defined as reinforcing if they move in the same direction; they are called conflicting signals if they move in opposite directions. Intuitively, it seems plausible that reinforcing signals would generate a higher degree of investor consensus concerning the change in expectations than would conflicting signals. Consequently, the authors first prove that in their model, the absolute value of price changes is higher for reinforcing than for conflicting signals.

In order to compare the volume effects of conflicting and reinforcing signals, the authors make two further assumptions:

- 1) Investors process information in a relatively neutral manner; that is, each investor overreacts (with respect to the average) to half the information sources, and underreacts to releases from the other half of the information sources. In other words, the investor cannot be a consistent over- or under-reactor.
- 2) The investors form two exhaustive and mutually exclusive groups, such that the members of one group overreact (underreact) when the members of the other group underreact (overreact). This assumption does not require an equal over- or under-reaction among individuals in the same group; it does not even require that there are the same number of investors in each group.

When all these assumptions are satisfied, the authors prove that on the average, trading volume of a security is higher if the signals are conflicting than if they are reinforcing. Finally, the authors demonstrate that in their model, trading in response to good news should not be greater than trading in response to bad news.

Griffin and Ng support their analytical results with empirical evidence. They employed the one-year ahead mean Earnings Forecaster analysts' forecasts and actual earnings per share as the two competing sources of information for 158 NYSE firms for the period 1971 to 1973. Using monthly data and the standard one-factor market model, they removed the market-wide return effect, and tested their price hypothesis with the average (standardized) cumulative return residual. A similar methodology was applied to the volume data. The competing signals were defined as the first differences or changes in the actual EPS and forecast EPS. Firms were classified into portfolios based on the magnitude of the earnings signals. For both good news and bad news signals, the price changes were higher for reinforcing signals than for

conflicting signals. Further, the trading volume was higher for conflicting signals than for reinforcing signals, for both good news and bad news portfolios. Finally, there was no evidence that trading volume associated with security price increases exceeded trading volume associated with price decreases.⁴

The primary relevance of Griffin and Ng's work to this study is that their model provides the only formal development of a relationship between trading volume and conflicting versus reinforcing signals. The analytical model predicts that conflicting signals should be associated with abnormally high trading. Signals are defined as changes, or first differences in the underlying information. Conflicting signals occur when these first differences, or changes, lie in opposite directions. In this study, on the other hand, signals are defined as the underlying information itself. Conflicting signals are defined in terms of degree--the greater the difference between the two signals, the greater the conflict between them, and thus, the greater the trading. This study, then, can be viewed as an empirical generalization of Griffin and Ng's model.

Conclusions

From the foregoing literature review, it should be apparent that the theories discussed in this section relate to different levels or aspects of the trading phenomenon. Although on the surface these theories do not seem to conflict, they have not yet been integrated into a single, comprehensive theory of trading in the market place.

The social welfare and sequential information arrival models, for example, both assume that trading occurs only in response to the release

of firm-specific information. In other words, the effects of market-wide trading volume fluctuations should be removed. The volume theories assume that this adjustment has been made, although they are not yet sufficiently developed to give specific guidance on just how to remove the market effect. Price theories, in contrast, do provide some advice on the appropriate method of adjustment for market-wide fluctuations. If all of the assumptions detailed in Appendix A are met, then the market-model (which removes the effects of market-wide security return fluctuations) can be derived from the theoretical model of equilibrium asset pricing, the CAPM. Trading volume research, unfortunately, has no analogue to either the market-model or the CAPM. One advantage of volume research is that it does not require all the assumptions inherent in the price theories, but the price paid for this freedom is the lack of guidance on exactly how to mitigate the effects of market-wide trading fluctuations.

The lack of a more developed theory of trading also makes the specification of the research hypotheses somewhat arbitrary. Unfortunately, there is no comprehensive analytical volume model which neatly yields empirically testable assumptions about the relationship between accounting figures and trading volume (Griffin and Ng (1978) seems to be the only possible exception to this statement). This criticism is not unique to volume research, however. Even price research lacks a formal theory relating security prices to accounting phenomena (except perhaps for those accounting phenomena which change cash-flows, such as switching to accelerated depreciation for income tax purposes).

Although this lack of a formal theory relating accounting numbers to market statistics is certainly not a desirable situation, it should not necessarily preclude empirical work in the area. First, the discovery of empirical regularities may spur the development of a better or more comprehensive theory, and second, knowledge of such empirical regularities may be useful in itself. Accountants might like to know, for example, whether their reports are useful to investors, even if we cannot yet determine a theoretical reason why they should be useful.

Although the volume theories which have been discussed in this section are far from perfect or complete, they nevertheless do yield some insights relevant to this study. First, Jennings, Starks, and Fellingham's (op. cit.) sequential information arrival model suggests that under certain conditions, price and volume changes may be negatively correlated. Further, the minimum price change may occur at a time when trading volume is peaking. This result emphasizes the point that market price and market trading volume measure different phenomena. Hence, both price and volume research will be useful in achieving a more complete understanding of the market. Second, Lev and Ohlson (op. cit.) extend the implications of the social welfare literature to assert that if certain assumptions are met, trading volume may be a valid welfare indicator. To the extent that one accepts these assumptions, trading volume research can provide insight into the social value of information releases. Finally, Griffin and Ng (op. cit.) develop a model which predicts that conflicting information signals will generate more trading volume than will reinforcing signals. Their assertion lies at the heart of this study's hypotheses.

Empirical Trading Volume Research

To the extent that the analytical research included empirical tests of model predictions, the empirics were also discussed above. This section presents a chronological review of the strictly empirical volume research, beginning with Beaver (1968), and concluding with Morse (1981).

The first published accounting study employing volume reactions was Beaver's 1968 paper on the information content of annual earnings announcements. Beaver studied 506 annual earnings announcements from 143 non-12/31 (non-calendar year) firms for the years 1961-65. Non-12/31 firms were chosen to reduce clustering of earnings release dates. Beaver used weekly data to fit a market-model type regression equation to the volume data:

$$V_{it} = a_i + b_i(V_{mt}) + \epsilon_{it}$$

where

$$V_{it} = \frac{\text{number of shares of firm } i \text{ traded during week } t}{\text{number of shares outstanding for firm } i \text{ in week } t} \times \frac{1}{\text{number of trading days in week } t}$$

$$V_{mt} = \frac{\text{number of shares traded for all NYSE firms during week } t}{\text{number of shares outstanding for all NYSE firms in week } t} \times \frac{1}{\text{number of trading days in week } t}$$

Beaver found that the mean volume residual, ϵ_{it} , during the announcement week was 30 percent larger than the mean volume residual during the non-report period. He also found that volume was below normal for the eight weeks prior to the announcement, suggesting that investors may have postponed their trading until the earnings report was released.

This result conflicts with Morse's (1980, 1981) evidence, and also fails to support the Jennings, Starks, Fellingham (op. cit.) sequential information arrival model, which suggests that abnormal trading will occur prior to the public release of information, as insiders trade with less informed outsiders.

Kiger (1972) extended Beaver's (op. cit.) work to interim earnings announcements. His experimental group consisted of 30 NYSE firms announcing second and third quarter earnings in 1968 and 1969. Two test periods were used: three-day and five-day periods surrounding the earnings announcements. Market volumes observed during these test periods significantly exceeded the volumes traded during a five-day control period when no announcements about the firms occurred. Kiger also found that the results of the test of the five-day period were almost as strong as the results of the tests of the three-day test period, suggesting that the reaction of individual investors is not instantaneous.

Foster (1973) investigated the information content of earnings per share (EPS) estimates made by 68 corporate officials from 1968-1970. He used Beaver's V_{it} , the weekly average of the daily percentage of shares traded. V_{it} was computed for a treatment group of 68 12/31 (calendar) year-end firms for the 17-week period surrounding the EPS estimate, and the 17-week period surrounding the preliminary earnings announcement. Parametric and nonparametric tests indicated that the largest volume reaction (increase) occurred around the time of the earliest announcement of EPS. This evidence is consistent with the hypothesis that investors do use the EPS estimate. The volume results were corroborated

by a price test which used the API (Abnormal Performance Index)⁵ to estimate the abnormal returns that could be earned with advance knowledge of the sign of the EPS prediction error.

Nichols, Tsay, and Larkin (1979) also investigated the volume reaction to voluntary corporate forecasts of EPS. Again they used Beaver's V_{it} , the weekly average of the daily percentage of shares traded, and ϵ_{it} , the volume residual after removing a market-wide trading effect. They analyzed data from 74 firms during the period 1971-1973, and found similar results using both the unadjusted V_{it} , and the volume residuals, ϵ_{it} . Nonparametric statistical tests supported their hypotheses:

- 1) there was a significant increase in trading activity during the week when the corporate officials announced the EPS estimates,
- 2) longer term forecasts (9-12 months) were associated with larger volume increases than were shorter term forecasts (3-6 months), and
- 3) trading volume increased more for forecasts of larger EPS changes (over 20 percent) than for forecasts of smaller EPS changes.

Ro (1981) used trading volume to investigate the market's reaction to replacement cost (RC) data. He hypothesized that if the initial release of RC data had information content, trading volume would increase upon release of that information. Ro used an ad hoc regression model (similar to Beaver, op. cit.) to remove a market effect. Treatment firms disclosing RC data on their 10-K reports were matched with control firms not disclosing RC information. The firms were matched on the basis of beta, sign of the previous year's earnings change, and the week the 10-K's were filed with the SEC. The treatment firms were

grouped into portfolios according to beta and an estimate of the effect of RC disclosures. Ro analyzed data from 73 matched pairs of treatment and control firms, and he was unable to reject the null hypothesis of no difference in weekly trading volume residuals between the RC disclosers and non-disclosers.

Freeman (1981) pointed out that the failure to control for the difference in firm size between the treatment and control groups constitutes a serious weakness in Ro's research. Grant (op. cit.) and Atiase (op. cit.) have provided evidence consistent with the hypothesis that accounting reports constitute a larger proportion of the information available about smaller firms. Specifically, they show that the earnings releases of smaller firms are associated with larger price reactions. If an analogous relationship holds for trading volume, Ro's study would be biased against finding a greater increase in volume for the larger RC-disclosing firms. Freeman suggests that preliminary research should investigate 1) whether trading volume responds to the release of 10-K's, and 2) whether the Grant-Atiase size effect holds for volume reactions (the results of this study do support the Grant-Atiase size effect) before attempting to test for a volume impact of RC data disclosed on 10-K's. In essence, Freeman is arguing for the development of a broad data base for volume reactions. The results of this study contribute to this objective.

In the most recently published empirical volume study, Morse (1981) examined the price and volume reactions surrounding the release of annual and interim reports of 20 NYSE, 5 ASE, and 25 OTC stocks for the period 1973-76. He also used a NYSE-based market volume index to adjust

for market-wide volume effects. The volume residuals were standardized by dividing each residual by its own standard deviation. Using daily returns, and the Wall Street Journal earnings announcement date, he found abnormally high volume residuals for several days after the earnings were announced (although the most significant residuals occurred the day before and the day of the announcement). Morse suggests that this slow adjustment process is consistent with the sequential information arrival models. Perhaps investors engage in costly and time-consuming information processing, thereby delaying their reaction to the public information releases.

This study improves upon prior work by examining the trading of a comprehensive sample of 397 firms over a period of three years. Separate analyses were conducted for various subsamples (e.g., by firm size, by firm stock exchange, by firm fiscal year-end date). Analyses were conducted on 1) unadjusted trading volume, and trading volume adjusted for both 2) firm-specific level of trading, and 3) market-wide fluctuations. A major methodological contribution of this study is the development of three distinct exchange indices: one each for NYSE, AMEX, and OTC stocks. Previous research which included non-NYSE firms used a NYSE index for all firms.

Chapter Summary

The first section of this chapter expanded upon the introductory chapter's discussion of market price versus market trading volume statistics. Three scenarios were constructed to illustrate the conditions where price tests and volume tests yield conflicting results.

The section concluded by suggesting that since price tests and volume tests measure different phenomena (i.e., price changes measure changes in the aggregate market's expectations while trading volume provides a summary of changes in individuals' expectations), research on both statistics may be necessary to gain a more complete understanding of market behavior.

The following section reviewing analytical research reinforced this theme. A major implication of the sequential information arrival model is that under certain conditions, price and volume changes may be negatively correlated; indeed, price changes may be near a minimum as volume reaches its peak. This message that market trading volume is of interest in its own right is further supported by the social welfare literature, which suggests that if certain assumptions are met, trading volume can provide an index of changes in social welfare.

The final section reviewed prior empirical volume research. The discussion highlighted weaknesses in that research which have been mitigated in this study.

The purpose of the chapter has been to motivate this study, as well as to provide a basis or perspective from which to analyze and evaluate the study. Previous research suggests that further investigation of trading volume may yield interesting insights into market behavior. The chapter has discussed both empirical evidence and analytical models provided by volume research to date, and has suggested potentially fruitful areas for extension of current research (e.g., the relationship between conflicting signals and trading volume, the size effect), as well as methodological weaknesses which can be corrected in this study

(e.g., more comprehensive sample, more appropriate indices of market-wide trading, replication of results on various subsamples to ensure the generality of results).

FOOTNOTES

¹Hakansson, Kunkel, and Ohlson (forthcoming) specify necessary and sufficient conditions for information to induce trading in an exchange economy. Kunkel (forthcoming) has performed the same task for production and exchange economies. Implications of these results are clarified in Ohlson and Lev (forthcoming). All three papers will be discussed later in this chapter.

²Beaver (1968, p. 69) states that "[increased] volume reflects a lack of consensus regarding price." Verrecchia (1981) shows that this statement is incomplete. Trading may occur because:

- 1) investors disagree on the pricing implications of a piece of information (lack of consensus), or
- 2) although investors may agree on the pricing implications of a piece of information, they may exhibit nonhomogeneous, non-constant, or nonlinear risk preferences.

Of course, accountants would like to know whether accounting reports induce consensus among investors' price expectations. This is the question Verrecchia asks. His question, however, is completely different from this study's research questions which ask: 1) whether the accounting reports are useful to investors and if they are useful, 2) what are the characteristics of the reports that make them useful to decision-makers? A report would certainly be useful to an investor if it causes him to rebalance his portfolio based on revised expectations. At this early stage of volume research, accountants should be interested in whether or not volume tests are sensitive enough to detect investors' use of accounting information in portfolio decisions. Subsequent research can investigate whether investors trade because they have different price expectations, or because they have different, nonlinear, or nonconstant risk preferences.

³The tatonnement model assumes that (1) information reaches all traders simultaneously, and (2) the vector of equilibrium prices is established through recontracting by the market-maker before trading takes place. Therefore, trading occurs only when investors disagree; if they all agree on the meaning of the information (as well as the pre-information equilibrium price) then by the assumptions of the model, they will all agree on the post-information price. If they all agree on the post-information price, no trading will occur.

⁴This result conflicts with Epps (1975) and Copeland (1977). Griffin and Ng speculate that the difference arises because the former studies do not differentiate conflicting from reinforcing signals. If, for example, reinforcing signals are predominantly bad news while

conflicting signals are predominantly good news; there would be more trading for the good news group (resulting mostly from conflicting signals), than for the bad news group (resulting mostly from reinforcing signals).

⁵The API (Abnormal Performance Index) is a measure of abnormal security returns, which is computed as follows:

$$API = \prod_{t=1}^T (1 + u_{i,t})$$

where V_{it} is the residual of the Sharpe (1964) market-model regression discussed in Appendix A:

$$R_{i,t} = a_i + b_i R_{m,t} + u_{i,t}$$

where:

R_{it} = natural log of the return on security i at time t

$$= \ln \frac{P_{i,t} + D_{i,t}}{P_{i,t-1}}$$

$R_{m,t}$ = natural log of the return on the market portfolio

$$= \ln \frac{I_t}{I_{t-1}}$$

where:

$P_{i,t}$ = price of security i at time t

$D_{i,t}$ = dividend of security i at time t

I_t = closing value of a market index (e.g., Standard and Poor's Price Index or Fisher Price Relative) at time t

In words, the API measures the value of \$1 invested at time t and held until time T , abstracting from market effects. For a more complete discussion, see Ball and Brown (1968).

CHAPTER III

DESCRIPTION OF THE RESEARCH METHODOLOGY

The overall objective of this study is to extend the investigation of the information content of annual earnings releases. The specific research objectives, as well as certain methodological advances used in this research were motivated by a study of the literature reviewed in the previous chapter. To reiterate, the specific objectives addressed by this study include:

- 1) To determine whether abnormally high trading occurs around the time annual earnings are released,
- 2) To determine whether the volume of trading is positively associated with the magnitude of the surprise in the earnings release,
- 3) To investigate the size effect; in particular, to ascertain whether firm size is negatively associated with abnormal trading. If trading is found to be proportionately greater for the smaller firms, the study will also investigate whether this result may occur because the earnings releases of the smaller firms are more surprising than the releases of the larger firms,
- 4) To determine whether the results are sensitive to adjustments for market-wide and firm-specific average levels of trading,
- 5) To determine whether the type of news in the earnings release (good news or bad news) affects the research results, and
- 6) To determine whether the results hold across fiscal year-end dates and stock exchanges.

This chapter presents the experimental methodology used to investigate these research objectives. In order to facilitate discussion of the specific methodological details, the chapter is organized into five major sections. Since the overall purpose of the study is to investigate the information content of annual earnings releases, the first section discusses three definitions of the term "information content," and justifies the definition used in this study. The second section describes the selection of the sample firms and data sources. The research variables are defined and discussed in the third section, while the fourth contains a list of the study's four major hypotheses and relates these to previous research. The statistical techniques used in this investigation are then described in the final section.

Information Content: An Operational Definition

The discussion in the previous chapters has assumed that the amount of information in an earnings release could be measured. Information content, however, may be defined in several ways. For example, Beaver (1968) gives three definitions of the term. First, he says that a report has information content if "it leads to a change in investors' assessments of the probability distribution of future returns (or prices), such that there is a change in equilibrium value of the current market price" (p. 68). Investors' probability assessments cannot, of course, be measured directly, so researchers adopting this definition of information content generally use changes in the observable variable,

market prices, as a surrogate for changes in the unobservable investor probability assessments.

Beaver's (1968, p. 69) second definition requires that "not only must there be a change in expectations, but the change must be significantly large to induce a change in the decision-maker's behavior." According to this definition, an earnings report has information content only if that report induces individuals to change their investment portfolios. The rebalancing of portfolios associated with a change in investor expectations or risk assessments, is evidenced by an increase in trading volume.

Although he recognizes that information has been defined as the reduction of uncertainty, Beaver rejects this third definition for capital markets research purposes. A message which increases uncertainty may be attended to and used as a basis for portfolio rebalancing. From an accounting standpoint, such a message might be useful to investors; it could be said to have information content, even though the message is dysfunctional in an entropy sense.¹

In selecting an operational definition of information content, one must consider both the meaning of the term in the institutional environment, and the limitations of the data base. In a financial accounting context, the Financial Accounting Standards Board has placed great emphasis on the usefulness of accounting data. In Statement of Financial Accounting Concepts Number 2 (SFAC No. 2), the Board states: "The purpose of this Statement is to examine the characteristics of accounting information that make that information useful." Consequently, a definition of the term "information content" might require

that data labelled as possessing information content be useful to investors. SFAC No. 2 lists several qualitative characteristics (e.g., relevance, reliability, verifiability, neutrality, comparability) which the Board believes contribute to usefulness of financial accounting data. These characteristics are too subjective to measure in a capital market study, given current methodology. What other indicator of decision usefulness can be measured objectively? Investors' expectations, the basis of Beaver's (1968) first definition of information content, cannot be easily measured. Uncertainty reduction, the basis of the entropy definition, cannot generally be measured for the aggregate market.² But the researcher can measure changes in investors' actions, the basis of Beaver's (1968) second definition of information content.

The Operational Definition

For this study, information content will be defined as follows:

data will be said to possess information content if those data cause investors to change their portfolio holdings, as evidenced by an abnormally high amount of trading.

This definition of information content was chosen for two reasons. First, since the definition is based on the actions of investors, it is consistent with Beaver's (1981) definition of market efficiency as a state where investors act as if they are aware of the information in question. Second, when defined in this way, information content can be measured--the researcher can objectively observe the amount of trading, or portfolio rebalancing.

Limitations of the Operational Definition

Unfortunately, the definition as stated is not without difficulty. The definition specifies that information content can be inferred if a signal generates "an abnormally high amount" of trading. As discussed in the introductory chapter, investors may trade for many reasons:

- 1) to balance income and consumption,
- 2) to obtain tax advantages,
- 3) in response to changes in risk/return preferences, or
- 4) in response to information which causes reassessment of the securities' expected returns or risk levels.

Because trading occurs for all four reasons, and because accounting earnings releases are not the only information flowing into the market, an earnings release is said to have information content if it is associated with abnormally high trading volume. The research must focus on abnormal trading activity, because an appropriate control period does not exist. Ideally, in the control period there would be no:

- 1) trading for diversification, income tax, change in risk-return preference, or consumption-earnings reasons, or
- 2) information of any sort released to the market.

Unfortunately, such a control period does not exist; one must recognize that trading normally occurs for the above reasons. Therefore, a test of the information content of accounting reports must investigate trading beyond the normal level of activity. The results of such a test show the differential, or above normal, information content of the accounting report.

The other major problem is that the definition is too narrow. A one-to-one correspondence between the concept of usefulness and the

operational definition of information content is lacking. For example, data may be used or processed by investors without causing a change in their actions (e.g., data may change investors' expectations or risk assessments, but this change may be too small to motivate a change in action (portfolio holdings) when transaction costs enter the calculation). In this paper's terminology, information content implies usefulness for investor decisions, but the converse does not hold.

This problem may be clarified by reference to Figure 6. In this diagram, the universe consists of data useful to investors making portfolio decisions. Price research, which is based on Beaver's (1968) first definition of information content, will indicate that only data falling within circle A have information content. Volume research based on Beaver's (op. cit.) second definition of information content, will indicate that only data falling within circle B have information content. Finally, entropy measures would indicate that only data falling within circle C have information content.

To date, only circle A has been well investigated, empirically. The extent to which circle A overlaps circles B and C is an unanswered empirical question. Analytically, Jennings, Starks, and Fellingham (1981) show that under certain conditions, abnormal trading and abnormal returns are negatively correlated. Their result suggests that the shaded area of circle B (the portion of circle B that is not overlapped by circle A) may indeed be significant.

In conclusion, although:

- 1) abnormal trading implies usefulness, and
- 2) abnormal returns imply usefulness, and
- 3) uncertainty reduction implies usefulness,

the converse is not necessarily true. Just because data are useful for decision-making, does not mean that they will generate abnormal trading or abnormal returns or reduce uncertainty. Given this situation, and the fact that each definition captures or identifies a different subset of useful information, all three definitions may provide insight into the question of which data are useful. Because the first definition, circle A, has been well researched, while the third definition, circle C, would be very difficult to operationalize (as discussed above), the second definition, or circle B, was chosen for this study.

Sample Selection and Data Sources

For reasons that will be discussed in the following section of this chapter, the research design required analysts' forecasts of firm annual earnings per share. Since the Valueline Investment Survey provides the only available source of quarterly earnings forecasts³ for a large number of firms, the sample firms were randomly chosen from those listed in the 1979 edition of Valueline. Valueline does not provide a list of criteria for the inclusion of firms in their publication. Kirshbaum (1982) states, however, that their philosophy is to "do the most good for the largest number of subscribers." More specifically, Valueline concentrates its efforts on:

USEFUL DATA

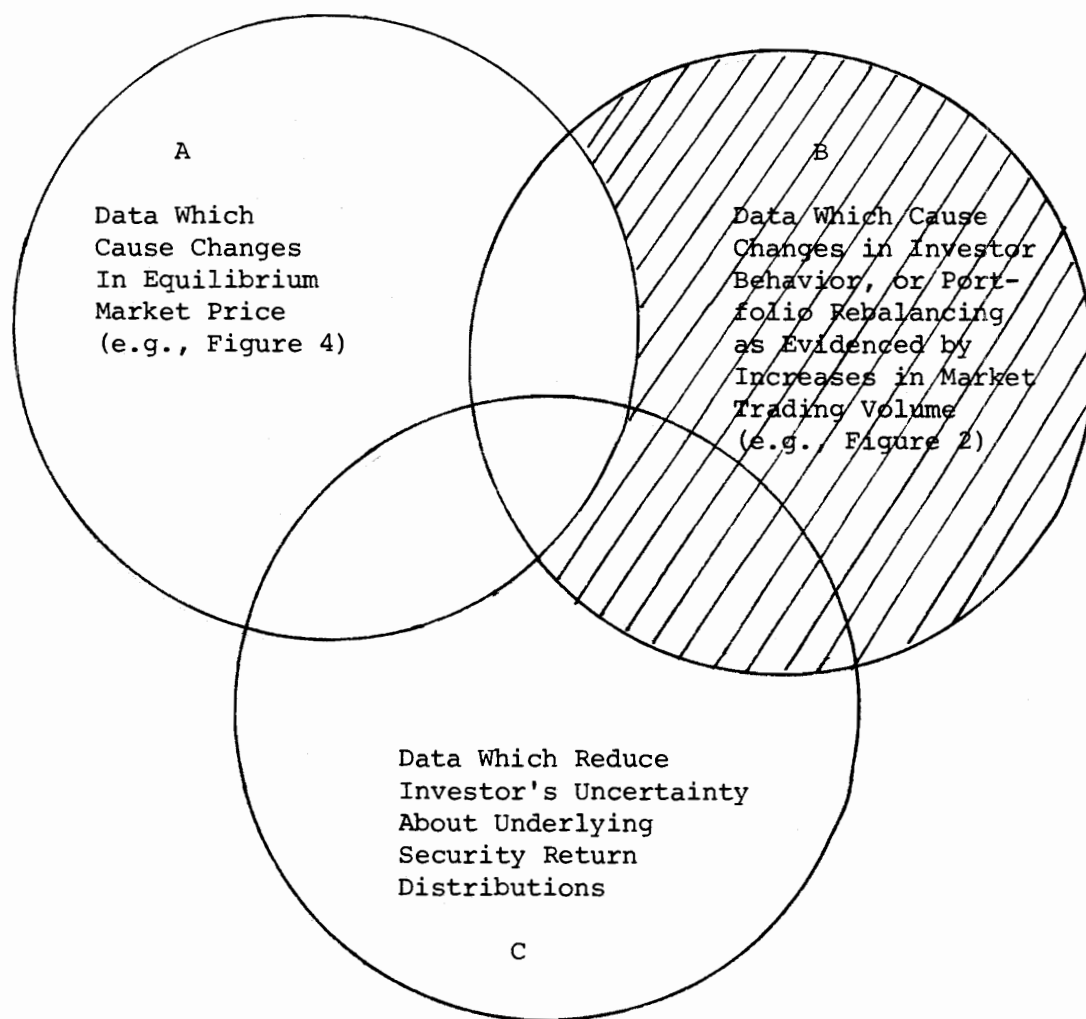


FIGURE 6

THREE DEFINITIONS OF INFORMATION CONTENT

- 1) large firms with a capitalization in excess of \$50,000,000,
- 2) firms which are widely held with at least 2,000,000 shares held by outside investors, and
- 3) firms which are actively traded--over 100,000 shares per month.

The publication does make exceptions when these criteria result in gaps in coverage. If one or two major firms would otherwise be omitted from a covered industry, Valueline will include those firms. Likewise, if coverage is thin in a particular industry, they will make exceptions and include smaller, less widely held or traded firms

Sample Size

After consulting David's (1938) tables of correlation coefficient distribution, an approximate sample size of 400 was chosen, (see Appendix B for a detailed discussion of this process). Since it was recognized that data problems would reduce the final usable sample size, an initial sample of 428 firms was randomly selected from the population of firms covered in the 1979 Valueline Investment Survey. Data problems discussed in the next section reduced the final usable sample to 397 firms. The number of firms deleted for various reasons appears in Table 1, while Appendix C contains a list of all the sample firms.

Data Sources

Once the sample firms were selected, analysts' forecasts of earnings per share (EPS) were hand-gathered from Valueline. The forecast closest to one month before the firm's fiscal year-end was chosen. Although arbitrary, this criterion assured that the forecasts were reasonably current, without being based on preliminary estimates of

TABLE 1
SUMMARY OF SAMPLE FIRMS

Initial sample size		428
Firms deleted:		
Not on Compuserve <u>Value</u> database	3	
Insufficient trading volume data on <u>Value</u> database	15	
Change in fiscal year-end date	2	
No announcement of annual EPS cited in <u>Wall Street Journal</u>	10	
Confounding news releases in all three sample periods	<u>1</u>	
Total deleted firms		<u>31</u>
Final usable sample firms		<u>397</u>

EPS. Daily trading volume (summed over all markets), stock exchange, fiscal year-end month, and total shares outstanding were all provided by Compuserve's Value database. Firm size, measured by total assets as well as total owners' equity, and actual EPS, both before and after extraordinary items, were obtained from the Compustat tapes when available, otherwise, the data were hand-gathered from Valueline. The annual earnings release dates were retrieved from the Wall Street Journal Index. At the time the data were gathered, 1979 was the most recent available full year of the Wall Street Journal Index, so the sample period included the calendar years 1977-79. Further examination of the Index highlighted any potentially confounding news announcements occurring within a week of the earnings announcement date. Major news items, such as stock split, merger, divestiture, litigation, and strike

announcements were subjectively deemed to be potentially confounding events. In case of such an announcement, that year's observation for the sample firm was discarded. For this reason, there were different numbers of observations in each year, although the number of sample firms was a constant 397.

The Research Variables

Since this study consists primarily of nonparametric correlation analyses, it would be misleading to discuss dependent versus independent variables. A correlation analysis yields inferences about associations between variables, not about causation. The first three segments of this section will therefore discuss the three major research variables: unexpected earnings, trading volume, and firm size. The final segment will explain the control for three potentially confounding variables: stock exchange, fiscal year-end date, and recency of the Valueline forecast.

Unexpected Earnings

This study's major hypotheses question the association between surprises in the annual earnings release (i.e., unexpected earnings) and the amount of trading at the time of the earnings announcement. One of the most difficult problems in the experimental design was developing a measure of unexpected earnings. Obtaining the actual earnings was relatively easy; most were available on the annual or over-the-counter Compustat tapes. Actual EPS for the remaining firms were hand-gathered from Valueline. The thorny problem was to obtain the market's expectation of earnings.

Expectation models. The literature (e.g., Brown and Rozeff, 1978, 1979; Collins and Hopwood, 1980; Fried and Givoly, 1981) identifies two types of expectation models: mathematical time-series models and actual analyst's forecasts. Appendix D reviews the recent literature comparing the two types of models. The results of these studies suggest that in predicting EPS, financial analysts outperform several different mathematical models (e.g., random walk, seasonal, and Box-Jenkins models). This relationship seems to hold over different sample firms, different sources of analyst forecasts, and different time periods. The result is intuitively appealing, since analysts not only have access to the predictions of these time-series models, but they may also have more recent data, as well as information which is not amenable to time-series analysis (e.g., litigation, strikes). In conclusion, analysts' forecasts seem to be more accurate than the predictions of time-series models, and they are also available to the public. For these two reasons, this study employs analysts' forecasts as the proxy for market expectations of EPS.

Although the forecasts had to be hand-collected, Valueline was chosen as the source of analyst forecasts for the following reasons:

- 1) a single source ensures the homogeneity of the models and assumptions underlying the forecasts,
- 2) Valueline forecasts have been employed in many other research efforts (Abdel-khalik and Espejo, 1978; Richards, 1976; Brown and Rozeff, 1978, 1979; Collins and Hopwood, op, cit.),
- 3) Richards (op. cit.) provides evidence that the three publicly available earnings forecasts (presumably Valueline, Earnings Forecaster and Research Institute, see footnote 1 in Richards), do not differ significantly in accuracy, and

- 4) Valueline publishes a single forecast for each firm. This procedure contrasts with Standard and Poor's Earnings Forecaster, which is a compendium of different analysts' forecasts for each firm. If the Earnings Forecaster had been chosen, the several forecasts would have had to be reduced to a point estimate.

Measures of unexpected earnings. Because it is difficult to reduce the concept of unexpected earnings to a single measure, six different metrics were constructed by varying three factors. First, it was unclear whether the Valueline forecasts were for EPS before or after extraordinary items. Second, although the hypothesized relation is between trading and the absolute magnitude of the unexpected earnings, in order to determine whether the results held across good news and bad news classifications, it was necessary to ascertain whether the unexpected earnings were favorable (actual EPS exceeded forecast EPS) or unfavorable (actual EPS were less than forecast EPS). Third, as a basis for comparison with the previous measures (and with prior research) two random-walk measures were tested. The random-walk models were used for three reasons. They were easy to construct; they have been used in previous research (e.g., Crichfield, Dyckman, and Lakonishok, (1978), Barefield and Comiskey, (1975)) but most important, they seem to be appropriate models for the expectations of the truly naive investor who does not make projections, or study analysts' forecasts or other publicly available information.

The formulae for computing the six measures appear in Table 2. All six error metrics are based on the mean (absolute) percentage error statistic. This statistic has been used in Richards, (op. cit.), Brown and Rozeff (1978), Collins and Hopwood (op. cit.), and Fried and Givoly (op. cit.).

Trading Volume

This segment describes the measurement of the second major research variable--trading volume. After a general discussion of the types of metrics used, focus will turn to a detailed justification and specification of the three categories of trading volume metrics used in this study--1) unadjusted volume, 2) trading volume adjusted for firm-specific average level of trading, and 3) trading volume adjusted for the overall market-wide level of trading.

The main research objectives require a measure of trading at the time of the annual earnings release. Obviously, the amount of trading depends upon the number of shares available, or shares outstanding. For this reason, trading volume was divided by total shares outstanding. This common procedure was originated by Beaver (1968) and has been adopted by virtually all subsequent researchers (e.g., Foster, 1973; Nichols, Tsay, and Larkin, 1979; Ro, 1980; Morse, 1981) in the volume area.

Previous evidence suggests that volume measures are skewed (Morse, op. cit.). Accordingly, a Kolmogorov-Smirnov (K-S) test of normality was performed on this study's trading volume data. The results indicate, at a .000 level of significance, that the data are not normally distributed.⁴ Since the amount of trading has a finite lower bound (zero trading), but no upper bound, this result should not be surprising. In an attempt to mitigate potential problems caused by nonnormal data, this study uses nonparametric statistical procedures whenever possible.

TABLE 2
SUMMARY OF UNEXPECTED EARNINGS METRICS

<u>Measure</u>	<u>Computation</u>
1) UE1	$\frac{\text{EPSBEI}_a - \text{EPS}_f}{\text{EPSBEI}_a}$
2) UE2	$\left \frac{\text{EPS}_a - \text{EPS}_f}{\text{EPS}_a} \right $
3) UE3	$\left \frac{\text{EPSBEI}_a - \text{EPS}_f}{\text{EPSBEI}_a} \right $
4) UE4	$\frac{\text{EPS}_a - \text{EPS}_f}{\text{EPS}_a}$
5) UE5	$\frac{\text{EPSBEI}_t - \text{EPSBEI}_{t-1}}{\text{EPSBEI}_t}$
6) UE6	$\left \frac{\text{EPSBEI}_t - \text{EPSBEI}_{t-1}}{\text{EPSBEI}_t} \right $

where:

EPSBEI = earnings per share (before extraordinary items)
 EPS = earnings per share (after extraordinary items)

and subscripts:

a = actual earnings figure
 f = forecast earnings figure
 t = actual earnings figure at time t

One objective in constructing the volume metric, then, was to create a measure consistent with nonparametric philosophy (i.e., based on rank, or ordinal principles, rather than cardinal position). For this reason, an approach similar to Kiger (1972) was employed. Three test periods of varying length were centered on the earnings release date. The test periods consisted of the release date ± 1 day, ± 2 days, and ± 5 days, respectively.⁵ The volume measures discussed below were calculated for each day of the test period. The median (a statistic based on rank) volume measure was calculated for each period. These median measures comprised the test statistics subjected to further analysis.

Unadjusted measures of trading volume. Because of the controversy concerning the propriety of adjusting for firm-specific and market-wide trading effects, this study analyzes an unadjusted measure of trading, as well as measures adjusted for these two potentially confounding sources of trading. The unadjusted measure is based on V_{it} , the unadjusted percentage of shares traded:

$$V_{it} = \frac{\text{number of firm } i\text{'s shares traded on day } t}{\text{number of firm } i\text{'s shares outstanding on day } t}.$$

These percentages, V_{it} , were computed for each sample firm for each trading day from January 1, 1977 to December 31, 1979.

Subsequent statistical analyses required a point estimate of the trading volume at the time earnings were released. Since it is doubtful that any reaction would occur instantaneously, three different test periods were constructed: three day, five day, and eleven day periods, all centered on the earnings release date. In order to obtain a point

estimate of trading volume, the median percentage of shares traded, V_{it} , was calculated for each of the three test periods. This measure, denoted MEDIANVOL, represents the unadjusted volume of trading at the earnings release date.

Adjustment for a firm-specific trading effect. As discussed earlier, the volume theories reviewed in the previous chapter assume that all information signals are specific to the firm, and that trading occurs only in response to this information. Trading occurring for other reasons may confound the analysis. Two potentially confounding sources of trading--1) firm-specific average level of trading, and 2) effects of market-wide fluctuations in trading--are controlled in this study. This segment discusses the adjustment for the first factor, while the following segment described the adjustment for the second factor.

Different firms may exhibit different levels of trading: a closely held firm would probably be less actively traded than a widely held firm. In an effort to control for this firm-specific level of trading (perhaps occurring due to income-consumption balancing, or other reasons discussed in Winsen, 1976 or Foster, 1978), this study constructs additional measures of trading volume. A base or average level of trading is calculated for each firm and this base level is then deducted from the level of trading occurring at the time of the earnings release. The resulting volume statistic measures the trading volume at the date of the earnings release adjusted for the overall average level of trading for the particular firm.

More specifically, the first step involves calculating a measure of the average, or base level of trading for each firm. The median percentage of shares traded, denoted RBASEMEDIAN, was calculated for each firm, for each calendar year. One year was chosen as the averaging period because it was felt that this period was long enough average out random fluctuations, but not too long to obscure any trend over time. The resultant measure of average or median firm trading was deducted from the unadjusted measure of trading at the earnings release date, MEDIANVOL. That is, RMEDIANVOL, the measure of trading volume adjusted for firm-specific levels of trading, is defined as MEDIANVOL minus RBASEMEDIAN. This statistic is calculated for all three (+ 1 day, + 2 days, + 5 days) test periods.

Adjustment for a market-wide trading effect. The third and final category of trading metrics attempts to adjust for the effects of variations in market-wide levels of trading. In price research, one can theoretically justify the removal of a market-wide effect. As explained in Appendix A, if certain assumptions are met, the market-model can be derived from the capital asset pricing model. A similar regression model has been used in volume research in an attempt to abstract from market-wide trading effects (e.g., Beaver, op. cit.; Nichols, Tsay, and Larkin, op. cit.; Morse, op. cit.). This regression methodology has been carried over from price research, it is completely ad hoc, and has no basis in any formal volume theory. There is, however, a qualitative theoretical basis for attempting to remove a market-wide trading effect: all the theories reviewed in the previous chapter (social welfare models, sequential information arrival models, and Griffin and Ng's

(1978) model of competitive information sources) assume that all information signals are specific to the firm, and that trading occurs only in response to this information. Removal of a market-wide trading effect is consistent with these theories, however, the theories give no guidance on how such an effect might be removed.

Aside from consistency with the developing volume theories, adjustment for a market-wide trading effect has the following additional advantages:

- 1) it removes some of the noise in the data. Beaver (op. cit.) found that the market index explained about 7 percent of the variance in the trading volume (using weekly data),
- 2) it reduces the problem of contemporaneous cross-sectional correlation occurring due to macroeconomic factors. For example, if XYZ earnings happen to be released on a day when trading is particularly high (for some reason unrelated to XYZ) the average market effect of this high trading volume will be removed. In other words, the effect of the macroeconomic factors will be reduced, to the extent that these factors affect the market as a whole, and
- 3) it removes trading volume's trend to increase over time. As more shares are sold, and more investors enter the market, trading has increased.

Since there is no clear-cut answer to this controversy (to remove or not to remove?) this study has performed statistical analyses on trading volume data, both with and without adjustment for the level of market-wide trading. Calculation of the unadjusted trading volume metric was straightforward, as discussed earlier. Computation of the measures of trading adjusted for market-wide effects is more complicated. A preliminary statistical manipulation must remove the effect of the market-wide level of trading.⁶ As the current volume theories provide little guidance on how this adjustment should be made,

this study adopted prior methodology (e.g., Beaver, op. cit.; Nichols, Tsay, and Larkin, op. cit.; and Morse, op. cit.) with one major improvement. Prior research has essentially adopted Beaver's (op. cit.) approach of calculating a separate regression (over time) for each firm i :

$$V_{it} = a_i + b_i (V_{mt}) + \varepsilon_{it}$$

where:

V_{it} = percentage of firm i 's outstanding shares traded at
time t

V_{mt} = percentage of the market's outstanding shares traded at
time t

a_i, b_i = regression coefficients specific to firm i .

$t \in T$, the given test period (January 1, 1977 to December 31, 1979)

ε_{it} = regression residual for firm i at time t

In prior work, the V_{mt} trading index was based on the NYSE firms, or some subset of the NYSE firms. Even those studies including non-NYSE firms used an index composed of NYSE firms. Morse (op. cit.) suggested that this practice may account for the lower r^2 's for the OTC as opposed to NYSE or AMEX firms. The OTC firms are smaller, on the average, than the exchange firms (Morse, op. cit.), and there may be other (as yet undiscovered) important differences between NYSE, AMEX, and OTC firms. One of the major contributions of this study lies in the development of three trading indices-- one composed of NYSE firms, one composed of AMEX firms, and one composed of OTC firms.

The three indices developed in this study include all firms in the Compuserve Value data base which meet the following requirements:

- 1) the Value data base identifies the exchange on which the stock is traded, and
- 2) the Value data base has at least two years of trading volume data for the stock during the period 1977-1980, and
- 3) the firm's stock was actively traded (the firm was not in bankruptcy, or subject to trading suspension, etc.), and
- 4) the stock was traded on at least one of two randomly selected dates during the test period.

The first two requirements assure data availability, while the latter two requirements reflect an attempt to match the market indices to the sample population. As discussed earlier, the sample population is biased in favor of widely held and actively traded firms, so the latter two requirements attempt to remove from the pool of index firms those which are rarely traded. These two requirements eliminated more OTC firms than AMEX or NYSE firms. The final OTC index included 2209 firms, the AMEX index consisted of 738 firms, while the NYSE index was comprised of 1475 firms.

As mentioned above, this study essentially adopts Beaver's (op. cit.) regression model for removing the market-wide trading effect, except that instead of a single NYSE-based index, this study matched each firm to the appropriate (NYSE, AMEX, or OTC) index. Accordingly, this study's adjustment for the market-wide trading effect can be modelled as follows:

$$V_{it} = a_i + b_i (V_{x_i,t}) + \varepsilon_{it}$$

where:

V_{it} = percentage of firm i 's stock traded on day t

$$= \frac{\text{shares of security } i \text{ traded on day } t}{\text{shares of security } i \text{ outstanding on day } t}$$

$V_{x_i t}$ = percentage of shares traded on day t on the exchange (NYSE, AMEX, or OTC) to which firm i belongs

$$= \frac{\text{shares traded on exchange on day } t}{\text{shares outstanding on exchange on day } t}$$

a_i, b_i = regression constant and coefficient (specific to firm i) determined by simple linear regression of daily data

ε_{it} = volume residual for firm i on day t

In addition, this study follows Morse's (op. cit.) lead and standardizes the volume residuals, ε_{it} , through division by their own standard deviation in order to help attain distributional comparability across firms. It is these standardized residuals which are used as measures of the trading volume variable.

As will be discussed in the following chapter, subsequent analysis revealed significant differences between the behavior of the three market indices. The behavior of the AMEX, and to an even greater extent, the OTC index, was erratic in comparison to the NYSE index. Because many of the non-NYSE firms' trading appeared to be more closely associated with the NYSE index than with their own AMEX and OTC indices, regressions were replicated using the NYSE index for all firms.

As discussed above, when the effects of market-wide trading fluctuations are removed through regression analysis, it is the standardized regression residuals, e_{it} , which are of interest:

$$\frac{\varepsilon_{it}}{\sigma_{\varepsilon_{it}}} = e_{it}.$$

These standardized residuals, e_{it} , were computed for all sample firms for the entire sample period, 1977-1979.

Subsequent analyses required a point estimate of the trading volume at the time earnings were released. Since it is doubtful that any reaction would occur instantaneously, three different test periods were constructed--three day, five day, and eleven day periods, all centered on the earnings release date. In order to obtain a point estimate of trading volume, the median standardized residual, e_{it} , was calculated for each of the three test periods. This measure, denoted RESMEDIAN, represents trading volume around the earnings release date adjusted for market-wide trading effects.⁷

This measure, RESMEDIAN, was calculated for two sets of standardized residuals. First, it was computed for the residuals of the regression of firm trading volume on the market index corresponding to the exchange where the firm's stock is traded. Second, because of the volatility of the AMEX and particularly the OTC index, further computations regressed all firms' trading on the NYSE index. The measure was also calculated for all three test periods (+ 1 day, + 2 days, + 5 days).

In summary, this study used three types of trading volume measures--1) unadjusted volume, 2) trading volume adjusted for firm-specific levels of trading, and 3) volume adjusted for variations in the market-wide level of trading. The third measure was calculated for two sets of data--adjustments for changes in 1) the own (NYSE, AMEX, or OTC) index, and 2) the NYSE index. To test the sensitivity of the results to the length of the test period (around the earnings release date), the study calculated these volume measures for three test periods of different lengths--+ 1 day, + 2 days, and + 5 days surrounding the

earnings release date. A summary of the trading volume metrics used appears in Table 3.

Firm Size

Of the three major research variables--unexpected earnings, trading volume, and firm size--the third was the easiest to measure. Two measures of size were employed. First, total assets seems to provide a reasonable measure of firm size. This metric is unaffected by the firm's debt/equity ratio. For example, suppose two firms are identical except that one is financed by 50 percent debt, 50 percent equity, while the other is 100 percent equity. For most purposes, it would seem that the two firms are of equal size, and of course, the total asset measure would classify them as equal in size.

However, it is unclear whether the size effect reported in the markets literature (e.g., Atiase, 1979; Grant, 1980; Banz, 1981) relates to the gross size of the firm, or to the amount of equity shares traded in the market. For this reason, an additional measure of size, total owner's equity, was investigated. Most of the statistical analyses were replicated for both size measures. Where possible, both total assets and total owner's equity were retrieved from the annual and over-the-counter industrial Compustat tapes. Data for firms not in the Compustat base were hand-gathered from the Valueline reports.

Potentially Confounding Variables

In order to investigate the validity of generalizing the results across different types of firms, the data were classified into groups according to three potentially confounding variables: stock exchange,

TABLE 3
SUMMARY OF TRADING VOLUME METRICS

	<u>Measure</u>	<u>Meaning</u>	<u>Computation</u>
1)	MEDIANVOL ^a	unadjusted measure of trading volume	median $[V_1, \dots, V_n]$
2)	RMEDIANVOL ^a	measure of trading volume adjusted for firm-specific median level of trading	median $[V_1, \dots, V_n] - h_t$
3)	RESMEDIAN ^b	measure of trading volume adjusted for market-wide level of trading	median $[e_1, \dots, e_n]$

where:

V_t = percentage of shares traded on day t

h_t = median (base) percentage of shares traded, calculated over the calendar year to which day t belongs

e_t = standardized trading volume regression residuals for day t

n = number of days in the analysis period (either 3 days, 5 days, or 11 days)

a = recall that these measures are used for 3 (3 day, 5 day, 11 day test period) manipulations of the data

b = recall that these measures are used for 2 (NY index, own index) x 3 (3 day, 5 day, 11 day test period) = 6 data manipulations

fiscal year-end month, and recency of the Valueline forecast. All statistical analyses are replicated on each of these groups, in order to determine whether any of the three variables affect the results. The reasons for investigating each variable will be discussed in turn.

The first variable, stock exchange, was chosen because of the limited evidence available on non-NYSE firms (Grant, op. cit. and Morse, 1980, 1981 provide the most recent published research which includes AMEX and OTC firms). Compuserve's Value data base lists each firm's primary exchange. All firms not traded on either the NYSE or the AMEX (e.g., Cincinnati or Toronto) were classified as OTC firms. The final sample was classified as follows:

NYSE - 349 firms
AMEX - 16 firms
OTC - 32 firms

Fiscal year-end month was the second variable investigated. Although many of the previous market studies concentrated on 12/31 firms (e.g., Foster, 1973; Abdel-khalik and Espejo, 1978; Beaver, Clarke, and Wright, 1979), this study provides evidence on both 12/31 and non-12/31 year-end firms. Another advantage of this approach is that analysis of non-12/31 firms reduces clustering of earnings announcement dates, since non-12/31 firms' annual earnings releases will be spread throughout the year. The final sample included 281 12/31 firms, and 116 non-12/31 firms.

The third and final potentially confounding variable was the recency of the Valueline forecast. This variable results from Valueline's publishing policies. Valueline publishes one issue each week throughout the year. They divide the year into four 13-week

quarters, and the approximately 1600 firms are grouped by industry. Each week, they publish an analysis of several industries, so that they report on each industry (and firm) within the 13 week quarter. Thus, revised analyses and earnings forecasts appear over varying 13-week intervals. This problem, compounded by the fact that different firms have different lag periods between the fiscal year-end and earnings release date, resulted in different lags between earnings forecast and earnings release dates. To control for this problem, firms were divided into groups according to this announcement lag (defined as announcement date minus forecast date). The median lag was about 56 days, reflecting the methodological decision to gather forecasts closest to one month before the fiscal year-end date. This procedure avoids the possibility that the forecasts reflect preliminary EPS announcements.

Research Hypotheses

Now that the research variables have been discussed, the specific research hypotheses can be detailed. (All hypotheses will be stated in their null form.) As explained previously, the overall research objective is to investigate the information content of annual earnings releases, using market trading volume as the measure of information content. This broad objective breaks down into several specific objectives. The first hypothesis relates to the initial specific research objective: whether abnormally high trading occurs around the time annual earnings are released.

H₁: The volume of trading does not increase significantly around the time annual earnings are released.

This hypothesis should be rejected. Previous price (e.g., Beaver, op. cit.; May, 1971) and volume (e.g., Beaver, op. cit.; Morse, 1980, 1981) research has provided consistent evidence that earnings releases do have information content, as evidenced by market price changes and increases in trading volume. The results of the test of this hypothesis will replicate prior research results, using different measures of trading volume, and a more comprehensive sample. Since previous research has established that annual earnings announcements do have information content, rejection of this hypothesis will suggest that the trading volume metrics employed in this study do provide valid measures of information content.

The remaining hypotheses are based upon the assumption that the first hypothesis is rejected; that is, these hypotheses assume that this study's trading volume metrics are valid measures of information content. The second hypothesis relates to the second research objective, which is an extension of the first objective. While the first objective is merely to document the existence of a phenomenon (i.e., earnings releases have information content) the second objective seeks an explanation for the phenomenon.

H₂: The absolute value of unexpected earnings is not positively correlated with the volume of trading around the date of the annual earnings release.

This hypothesis should also be rejected. It represents an intuitively appealing continuation of prior research (e.g., Beaver, op. cit.; Foster, 1973; Morse, 1980, 1981) by investigating the extension of a previously documented dichotomous relationship (earnings releases are associated with greater than average trading) to an ordinal relationship

(more surprising earnings releases are associated with higher levels of trading than are less surprising earnings announcements). Additional motivation for this hypothesis is provided by Beaver, Clarke, and Wright's (op. cit.) finding of a positive correlation between security price changes and unexpected earnings (the latter measured by deviation of actual EPS from mathematical time series' models' predictions). The results of the tests of this hypothesis will provide further evidence on the convergence of the price and trading volume proxies for information content.

The final two hypotheses relate to the size effect introduced in the third research objective, (which represents another extension of the first objective). Like the second objective, the third research objective also seeks an explanation for the documented observation that earnings releases are associated with abnormally high trading.

H₃: The volume of trading around the date of the annual earnings release is not negatively correlated with firm size.

This hypothesis should be rejected. Previous research (Atiase, 1979; Grant, 1980) has suggested that smaller firms exhibit greater price reactions (than larger firms) upon the release of accounting earnings. In addition, Banz (1981) presents evidence that security returns are higher for smaller firms than larger firms. These results are consistent with the conjecture that few alternative sources of information are available for the smaller firms. If less information is available about the smaller firms, then one might expect the earnings announcements of smaller firms to have more surprises.

H₄: The absolute value of unexpected earnings is not negatively correlated with firm size.

This fourth hypothesis should also be rejected. If it is rejected, it may provide implications for the big GAAP⁸ - little GAAP controversy. That is, rejection of this hypothesis would indicate that the accounting earnings form an important and major part of the information available about smaller firms.

The last three research objectives address the generality of the results of the four hypothesis tests. Although these objectives are not articulated in formal hypotheses, additional statistical analyses are performed on various subsamples in an effort to determine whether the type of surprise (good news versus bad news), the fiscal year-end month, the stock exchange on which the firm is traded, the recency of the Valueline earnings forecast, or adjustments for firm-specific or market-wide trading effects change the results of the four hypothesis tests.

Statistical Analysis

Testing the above hypotheses requires various statistical procedures. After a brief discussion of the advantages of nonparametric statistical procedures, this section proceeds sequentially, from the preliminary regression analysis employed to remove the market-wide trading volume effect to the statistical tests from which inferences are drawn about each of the four hypotheses. The presentation includes a justification for using each test, plus a brief explanation of the test, including the underlying assumptions.

Advantages of Nonparametric Statistical Tests

Since the Kolmogorov-Smirnov test of normality suggests that the volume metrics employed in this study are skewed with medians below zero, the propriety of using statistical tests requiring normally distributed data is questionable. Although assumptions vary from test to test, nonparametric procedures do not require normally distributed data. Nonparametric procedures have the additional advantage of being relatively insensitive to outliers. This property arises from the fact that nonparametric tests generally use the rank, or ordinal properties, (rather than cardinal properties) of the sample observations. The importance of this factor is highlighted in Boatsman (1982), where it is pointed out that capital markets research results appear to be highly sensitive to outlier observations. Finally, Hollander and Wolfe (1973) show that nonparametric procedures are much more powerful than their normal-theory counterparts when data are not normally distributed, and are nearly as powerful when data do follow the normal distribution.

Preliminary Regression Analysis

The first step in the statistical analysis involved computing the unexpected earnings and trading volume metrics summarized in Tables 2 and 3. Calculation of the unexpected earnings measures was straightforward, but computation of the trading volume metric adjusted for market-wide effects required a preliminary step: regression of the percentage of firm shares traded on the appropriate trading index (construction of the three indices was discussed previously). Unfortunately, the statistical package did not contain a program for nonparametric regression-type analysis. Hand calculation of 400

regressions, each based on approximately 1000 data points (from four years of daily trading volume), was infeasible. For this reason, the standard parametric regression package was used.⁹

As discussed earlier, and summarized in Table 3, these trading volume residuals were calculated each trading day for every firm. The median residuals were calculated for each of the three test periods, (3-day, 5-day, and 11-day), centered on each of the three earnings release dates (1977-1979).

Nonparametric Procedures

Analysis of the first hypothesis. For the reasons outlined above, the analyses from which statistical inferences concerning the research hypotheses are drawn consist of nonparametric procedures. The first hypothesis, that trading increases around the release of the annual earnings, may be written in the null form as:

$$H_0: V_{i,r} \leq V_{i,b}$$

or in the alternative form as:

$$H_1: E(V_{i,r}) > E(V_{i,b})$$

where:

$V_{i,r}$ = the measure of firm i's trading around the earnings release date r (e.g., RESMEDIAN, MEDIANVOL)

$V_{i,b}$ = the measure of firm i's median (or base) trading in the calendar year in which the earnings are released (e.g., RBASEMEDIAN)

The data form paired replicates. One can view $V_{i,b}$, the measure of normal trading, as the "pretreatment" observation, and $V_{i,r}$, the measure

of trading around the earnings release, as the "post-treatment" observation. The purpose of the statistical analysis is to determine whether a shift in location has occurred due to application of the "treatment," the release of earnings data. Two nonparametric procedures are appropriate for analyzing a shift in location of paired replicates data: the Fisher sign test and the Wilcoxon signed rank test. Both procedures are based on the following model:

$$\begin{aligned} Z_{i,r} &= V_{i,r} - V_{i,b} \\ Z_{i,r} &= \theta + \varepsilon_{i,r} \end{aligned} \quad \begin{array}{l} i = 1, \dots, n \\ r = 1, \dots, m \end{array}$$

where:

$V_{i,r}, V_{i,b}$ are defined above

θ = unknown treatment effect, in this case, the increase in the trading metric due to the earnings announcement

$\varepsilon_{i,r}$ = unobservable random variables

i = firm index ($i = 1, \dots, 397$)

r = release date index ($r = 1, 2, 3$, where $r = 1$ indicates the first release, occurring in 1977, etc.)

Given this model, the first hypothesis may be restated in the null and alternative forms as:

$$H_0: \theta \leq 0$$

$$H_1: \theta > 0.$$

The first procedure, the Fisher sign test, employs the following assumptions:

- 1) the ε 's are mutually independent
- 2) each ε comes from a continuous population (not necessarily the same one) that has median zero so that

$$P(\varepsilon_{i,r} > 0) = P(\varepsilon_{i,r} < 0) = \frac{1}{2} \quad \begin{array}{l} i = 1, \dots, n \\ r = 1, \dots, m \end{array}$$

In the sign test, the number of positive Z's, (or differences between earnings release trading and normal trading) are counted and if this number exceeds a cutoff point, the null hypothesis that $\theta \leq 0$ will be rejected. The advantage of this test lies in its weak assumptions, but reduced power is the price of these less restrictive assumptions. The test only considers whether the Z's are positive or negative, it considers neither the relative magnitude nor ranking of the Z's.

The Wilcoxon signed rank test, the second appropriate procedure, requires the Fisher sign test assumptions, plus the additional stipulation that the ϵ 's populations are symmetric about zero. The Wilcoxon signed rank test ranks the absolute values of the Z's, and then sums the ranks of the positive Z's. If this sum exceeds a cutoff point, then the null hypothesis that $\theta \leq 0$ will be rejected. Because the signed rank test uses information on the relative rankings of the Z's (in addition to the Z's sign), it is more powerful than the sign test. The signed rank test is less sensitive to outliers than is a parametric test, because the nonparametric procedure uses only the relative rankings (i.e., ordinal information) rather than the values (cardinal information) of the Z's.

Analysis of the second, third, and fourth hypotheses. The three remaining hypotheses predict ordinal relationships between variables. The second hypothesis that bigger earnings surprises are associated with greater increases in trading may be written in the null form as:

$$H_0: \rho (|UE_{i,r}|, V_{i,r}) \leq 0$$

or in the alternative form as:

$$H_2: \rho (|UE_{i,r}|, V_{i,r}) > 0$$

where

$V_{i,r}$ is defined above

$|UE_{i,r}|$ = the measure of firm i 's absolute unexpected earnings at earnings release date r

ρ = correlation coefficient

The data form bivariate observations, and again, two appropriate nonparametric procedures--Kendall's tau and Spearman's rank correlation coefficient--test for independence of the two variates involved in a bivariate structure. Both procedures are based on the following model and assumptions:

The data consist of mn bivariate observations

$$(|UE_{i,r}|, V_{i,r})$$

where:

$i = 1, \dots, n$ (firm index)

$r = 1, \dots, m$ (release date index)

- 1) the bivariate observations are mutually independent, and
- 2) each $(|UE_{i,r}|, V_{i,r})$ comes from the same continuous bivariate population.

These two assumptions should be satisfied by the research data. The firms were randomly selected, each observation comes from a different firm-year (fiscal year of data about one firm), and earnings releases occur throughout the year. Thus, the observations are spread over time and firms, so there is no reason to suspect they are not independent. As for the second assumption, there is no a priori reason to believe that the relationship between trading volume and unexpected earnings differs over various levels of either variate. This question is, however, an empirical one. For this reason, subsequent analyses test

the hypothesis over different levels of unexpected earnings, from very bad news to very good news.

Although they are based on the same model and assumptions, the Kendall and Spearman procedures test different properties of the data. The Kendall procedure measures concordance between pairs of bivariate observations. The general idea is to compare all pairs of bivariate observations, and count 1 if both the $|UE_{i,r}|$ and the $V_{i,r}$ variates are larger in the same observation, and count -1 if they are not. Consider the following example, with three observations.

<u>Observation Number</u>	<u>$(UE_{i,r} , V_{i,r})$</u>
1	(15,2)
2	(16,3)
3	(14,4)

Compare observation Number 1 to Number 2. Both $V_{i,r}$ and $|UE_{i,r}|$ are greater in observation Number 2, so count 1. Compare Number 1 to Number 3. Notice that $V_{i,r}$ is higher in Number 3, but $|UE_{i,r}|$ is higher in Number 2, so count -1. Finally, compare Number 2 to Number 3. Again, $V_{i,r}$ is higher in Number 3, but $|UE_{i,r}|$ is higher in Number 2, so count -1. The final count $1-1-1 = -1$ is compared to a cutoff level. If this count is high, it indicates that $V_{i,r}$ and $|UE_{i,r}|$ increase together, or are positively associated. Therefore, the null hypothesis is rejected when this count exceeds a cutoff. The Kendall procedure is similar to a sign statistic for the independence problem, since it assigns a value of 1 or -1, regardless of how near or separated are the ranks of the observations in the individual $V_{i,r}$ and $|UE_{i,r}|$ rankings.

The Spearman test, on the other hand, assigns greater weight to those $(|UE_{i,r}|, V_{i,r})$ differing more in their respective $|UE_{i,r}|$ and $V_{i,r}$ rankings. This test, therefore, is more like a signed rank test for the independence problem. In fact, the Spearman's correlation coefficient, ρ , is the classical sample correlation coefficient applied to the rankings (again, an ordinal rather than cardinal concept) of the $V_{i,r}$ and $|UE_{i,r}|$ within their respective samples. That is, the $V_{i,r}$ are ranked, and the $|UE_{i,r}|$ are ranked, and the Spearman's ρ measures the correlation between these two rankings. Formally:

$$\rho = \frac{12 \sum_{i=1}^n \left[R_{i,r} - \frac{n+1}{2} \right] \left[S_{i,r} - \frac{n+1}{2} \right]}{n(n^2 - 1)}$$

where:

R_i = rank of $V_{i,r}$ in the joint rankings of $V_{1,1}$ to $V_{n,m}$

S_i = rank of $|UE_{i,r}|$ in the joint rankings of $UE_{1,1}$ to $UE_{n,m}$

i = firm index

r = release date index

On several preliminary tests, the results of the Kendall and Spearman procedures were nearly identical. Since Spearman's ρ has a convenient interpretation as the classical sample correlation coefficient applied to the rankings of the variates, and since it seems to be more widely accepted, the Spearman procedure was used throughout the analysis, and reporting will be confined to discussion of the Spearman ρ .

This Spearman procedure is also used in analyzing the data for the third and fourth hypotheses. The third hypothesis can be written in the null and alternative forms as:

$$H_0 : \rho (S_{i,r} , V_{i,r}) \geq 0$$

$$H_3 : \rho (S_{i,r} , V_{i,r}) < 0$$

where:

$V_{i,r}$ is defined above

$S_{i,r}$ is the size of firm i at the earnings release date r .

As discussed above, it is anticipated that the null hypothesis will be rejected, and the data will show an inverse correlation between firm size and trading volume. If the null is rejected, the fourth hypothesis suggests an explanation for the phenomenon. Perhaps relatively greater trading occurs for the smaller firms because the smaller firms' earnings releases are more surprising. In the null and alternative forms, the fourth hypothesis may be stated as:

$$H_0 : \rho (S_{i,r} , |UE_{i,r}|) \geq 0$$

$$H_1 : \rho (S_{i,r} , |UE_{i,r}|) < 0$$

where:

$S_{i,r} , |UE_{i,r}|$ are defined above

The final statistical procedure is employed in case the relationships in the second, third, and fourth hypotheses are not continuous, but exhibit a threshold effect. Observations were ranked by the first of the two variates, unexpected earnings (hypothesis two) or firm size (hypothesis three and four) and then arbitrarily classified into high, medium, and low groups, with about 50 to 100 observations in each group. The extreme observations were analyzed, first, to determine

whether the posited relationship held for them and second, to investigate whether these outliers were driving any results found in the preceding analyses.

The groups, formed according to rankings of the first variate (unexpected earnings or firm size), were treated as separate samples. Since the group formation was based on the first variate, and observations with similar first variates were classified into the same group, the problem was then to test for differences in location of the second variate (between the groups). The Kruskal-Wallis one-way layout, similar to the normal-theory ANOVA model, was the appropriate nonparametric procedure. The basic Kruskal-Wallis model and its assumptions follow:

$$X_{i,j} = \mu + \tau_j + \varepsilon_{ij} \quad \begin{array}{l} i = 1, \dots, n_j \\ j = 1, \dots, k \end{array}$$

where:

$X_{i,j}$ = the observation of the i^{th} firm in the j^{th} sample

μ = unknown overall mean

τ_j = unknown treatment j effect

i = firm index

j = treatment or group index

$$\sum_{j=1}^k \tau_j = 0$$

and:

- 1) the ε 's are mutually independent, and
- 2) each ε comes from the same continuous population.

In the context of the second hypothesis:

$$H_0: (|UE_{i,r}|, v_{i,r}) \leq 0$$

$$H_1: (|UE_{i,r}|, v_{i,r}) > 0$$

the observations were ranked according to $|UE_{i,r}|$. The data were divided into five adjacent groups (the cutoff points were arbitrary and will be discussed in the following chapter, as the statistical results are presented). The μ of the Kruskal-Wallis model could be interpreted as the average or normal level of trading, while τ_j is the additional trading generated by level j amount of unexpected earnings. X_{ij} , of course, represents the value of the trading metric for firm i classified (according to $|UE_{i,r}|$) into group j .

The Kruskal-Wallis procedure involves ranking all the observations (from all the groups). These rankings are then averaged for each group. If the individual group average ranks are significantly different from the overall average rank, then the null hypothesis of no difference in location is rejected. The formal computation follows:

$$H = \frac{12}{N(N+1)} \sum_{j=1}^k n_j (R_{.j} - R_{..})^2$$

where:

N = total number of observations

n_j = number of observations in group j

$R_{.j}$ = average (joint) rank of group j

$R_{..}$ = overall average rank = $\frac{N+1}{2}$

The null hypothesis of no difference in location is rejected if H is sufficiently large (tables are available in Hollander and Wolfe, 1973).

Chapter Summary

The chapter began with a review of the research objectives. Since the overall objective of this study is to extend the investigation of the information content of annual earnings releases, the first section discussed the operational definition of the term "information content" employed in this study, as well as the limitations of the chosen definition. The following section explained the sample selection procedure and the data sources. The third section discussed the concepts this study attempts to measure, and explained in detail how these abstract concepts were reduced to concrete, measurable phenomena. Once all the terms were defined, the specific research hypotheses were presented and related to previous research. The fifth and final section explained the advantages of nonparametric statistics, justified the selection of particular statistical procedures, and explained the underlying assumptions. The statistical analysis of the data obtained for the study, together with the interpretation of the results, is presented in the following chapter.

FOOTNOTES

¹See Itami (1975a, 1975b) for a discussion of the differences between entropy-based and information-economics (action change-based) measures of information.

²If the messages can be ordered according to garbling, sufficiency, or fineness relationships (see Hilton and Kinard, forthcoming) then the entropy measure of information content may be operational in the aggregate market. Ajinkya (1980), for example, argues that line of business disclosures result in a finer message than aggregate asset and earnings data.

³Quarterly (interim) forecasts were gathered for a subsequent research project.

⁴As will be discussed, the trading volume measure adjusted for market-wide trading effects only, was the standardized regression

residual, $e_{it} = \frac{\epsilon_{it}}{\sigma(\epsilon_{it})}$. This measure is theoretically distributed

normal (0,1), and the K-S procedure tests against this alternative. No theoretical distributions were available for the other variables. The sample mean and sample variance were calculated for these other variables, and they were tested against the alternative that they follow

a normal (\bar{X}, S^2) distribution, where \bar{X} is defined as the sample mean

and S^2 is the sample variance. Of course, when the parameters of the test distribution are estimated from the sample, the distribution of the K-S test statistic changes, and the hypothesis that the sample is from the specified distribution will be rejected less often.

⁵The choice of the 3-day, 5-day, and 11-day test periods was necessarily arbitrary. These are, however, the periods selected by Kiger (1972). Because the earnings release dates were obtained from the Wall Street Journal Index, these dates do have a small error component. Occasionally earnings are released to the market before they appear in the WSJ. For this reason, a 1-day period was not selected. The test periods were centered on the release date in order to avoid the necessity to make judgments concerning whether more trading takes place prior to the release (i.e., by insiders) or after the release. Further research is necessary to pinpoint time during which most of the trading reaction occurs.

⁶In other words, these adjusted volume figures form the inputs into subsequent statistical tests from which the inferences are drawn.

⁷The regression model:

$$V_{it} = a_i + b_i (V_{x,t}) + \varepsilon_{it}$$

adjusts for (1) the mean effect of the market-wide level of trading [$b_i (V_{x,t})$], and (2) the mean level of firm i 's trading when there

is no trading in the market, (a_i). Therefore, it would seem as though a better measure of trading adjusted only for the market-wide effects would be:

$$\frac{\varepsilon_{it} + a_i}{\sigma(\varepsilon_{it} + a_i)}$$

This statistic was not used primarily because the simple regression

statistical package outputs only the standardized residuals, $\frac{\varepsilon_{it}}{\sigma(\varepsilon_{it})}$, so

it was not possible to calculate $\frac{\varepsilon_{it} + a_i}{\sigma(\varepsilon_{it} + a_i)}$. Two factors, however,

limit the importance of this shortcoming:

- 1) Theoretically, the intercept a_i should equal zero because if there is no trading in the market (i.e., the market index equals zero) there can be no trading for firm i .
- 2) Empirically, the median value of the intercept term was approximately zero.

⁸GAAP stand for the term, "generally accepted accounting principles."

⁹Strictly speaking, the assumptions of the regression model were not met:

- 1) the residuals were not normally distributed, but instead were skewed with medians below zero, and
- 2) the residuals exhibited positive serial correlation--the median Durbin-Watson statistic was 1.31, indicating significant positive serial correlation at sample sizes exceeding 50 (Durbin and Watson, 1951).

These problems impair the fit of the calculated regression line, and invalidate statistical inferences based on normal-theory assumptions. This study does not attempt normal-theory based statistical inferences, but instead, simply uses the residuals as inputs into nonparametric analyses. Therefore, the power of this study's tests is reduced only to the extent that violations of the underlying assumptions have impaired the fit of the regression line. However, the validity of the statistical analysis is unaffected because:

- 1) no attempt is made to draw normal-theory based statistical inferences from either the regression coefficients or the residuals, and
- 2) although the residuals are subsequently analyzed in nonparametric tests, these tests are not affected by the observed serial correlation because only three (noncontiguous) observations (one around the date of each annual earnings release from 1977-1979) out of the 1000 are used.

CHAPTER IV
STATISTICAL ANALYSIS AND INTERPRETATION
OF THE RESULTS

The purpose of this chapter is to present the results of the investigation described in the previous chapter. The chapter consists of three presentations. The first section details the results of the preliminary regression analysis performed to remove a market-wide trading effect. Specifically, this discussion centers on (1) the behavior of the three trading indices, (2) r^2 statistics concerning the average fit of the regression lines, and (3) subsequent analyses performed on a subset of firms whose behavior seems to be relatively well-explained by the regression model. The second section presents the statistical evidence bearing upon the four formally stated research hypotheses. The results of additional or secondary analyses pertaining to the generality of the results of the statistical tests of these hypotheses is also included in this section. The data are stratified according to the potentially confounding factors listed in the research objectives--good news versus bad news, fiscal year-end date, stock exchange, and recency of the analyst's forecast. These subsamples are then statistically tested in order to ascertain whether the results of the four hypothesis tests are affected by these potentially confounding factors. In addition, attention is directed to the empirical questions

of which measures of trading volume, firm size, and unexpected earnings seem to yield the strongest results. Potential explanations for the observed patterns of trading are sought. The third and final section describes some additional results which may yield insights into factors affecting trading behavior, although they do not specifically relate to any of the four formal research hypotheses.

Stage One: Preliminary Regression Analysis

As discussed in Chapter III, for each sample firm, the daily percentage of shares traded was regressed on a trading index. The purpose of this preliminary statistical manipulation was to mitigate the effect of market-wide or macroeconomic factors on trading volume, since the objective of this study is to analyze the relationship between firm-specific annual earnings releases and trading induced by this information.

Evidence on the Market Indices

The behavior of the three indices is summarized in Table 4. The NYSE and OTC trading indices are negatively correlated, while the AMEX index is positively correlated with both the NYSE and OTC indices. The dispersion of the three indices may provide an insight into this counterintuitive phenomenon. The variance of the OTC index is much higher than the variances of the other two indices, and the variance of the AMEX index is higher than that of the NYSE index. Further, the coefficient of variation, a measure of relative dispersion, was also much higher for the AMEX and OTC indices, than for the NYSE index.

TABLE 4

SUMMARY OF MARKET INDEX STATISTICS

I Spearman Correlation Coefficient Among Market Indices^a

	<u>Market Index</u>		
	<u>NYSE</u>	<u>AMEX</u>	<u>OTC</u>
NYSE	1.0000	.5540	-.2210
AMEX		1.0000	.0928
OTC			1.0000

II Dispersion of the Market Indices

	<u>Number of Firms</u>	<u>Variance</u>	<u>Coefficient of Variation</u>
NYSE	1475	.132	.084
AMEX	738	3.094	2.351
OTC	2209	257.740	14.326

^a Based on 1071 observations.

A potential explanation for these observations follows: the OTC index may be highly sensitive to relatively large trades which may occur randomly rather than in response to market-wide fluctuations. If the OTC firms tend to be more closely held and less widely traded, then large trades of an OTC firm's stock might be more likely to reflect changes in the individual investors' or firms' positions. Large trades of more readily marketable, widely held blue-chip stocks, on the other hand, may be more likely to reflect changes in market conditions as well as changes in investor's and firm's positions. In conclusion, the evidence presented in Table 4 is consistent with the conjecture that the more stable NYSE index better captures market-wide trading factors than do the more volatile AMEX and OTC indices. This conjecture is further supported by an analysis of the individual regression results, summarized in the next segment.

Summary of the Regression Results

Because the NYSE and OTC indices are negatively correlated, regressions of the AMEX and OTC firms' trading volumes were run on both the NYSE and the own trading indices. Of all 397 firms regressed against the NYSE index, only four had negative beta coefficients (which indicate an inverse relationship between firm-specific and market-wide trading). These four firms were all traded OTC, and their r^2 's ranged from .00064 to .03194 (below average, when compared to Table 5). These r^2 's suggest that the firms with negative betas are not well-described by the regression model; in this study, the few inverse relationships are found to be very weak.

A different pattern emerges when the AMEX and OTC firms' trading is regressed on their own (AMEX or OTC, respectively) exchange index. One AMEX firm (out of the 16 sampled) with an r^2 of .00043 and 22 OTC firms (out of the 32 sampled) with r^2 's ranging from .00006 to .02898 have negative beta coefficients. These results support the earlier conjecture that the more stable NYSE index may be better than the more volatile OTC and AMEX indices at capturing the effects of market-wide factors on individual firm trading volume.

This conjecture is further supported by the evidence presented in Table 5. Panel A gives some descriptive statistics on the regression r^2 's. Notice that the mean and median r^2 's are higher when the AMEX and OTC firms are regressed on the NYSE index than when they are regressed on their own respective exchange indices. In addition, the coefficient of variation shows that the relative dispersion of the r^2 's decreases when the non-NYSE firms are regressed on the NYSE index. The results of the Kruskal-Wallis test (of difference in location in the r^2 's) shown in Panel B are also consistent with Panel A's evidence. The mean ranks of the r^2 's are higher when the AMEX and OTC firms' trading volumes are regressed on the NYSE index than when they are regressed on their own indices. In other words, the Kruskal-Wallis test provides additional evidence that the r^2 's are higher when the NYSE index rather than the own index, is used as the independent variable in the regression analysis.

In conclusion, the evidence presented in Tables 4 and 5 suggests that the NYSE index may provide a more appropriate measure of the market-wide factors affecting trading than either the OTC or AMEX

TABLE 5

SUMMARY OF REGRESSION r^2 's

Panel A: Descriptive Statistics						
	<u>Mean</u>	<u>Median</u>	<u>Minimum</u>	<u>Maximum</u>	<u>Standard Deviation</u>	<u>Coefficient of Variation</u>
NYSE Firms Regressed on NYSE Index	.083	.060	.000	.552	.086	1.036
AMEX Firms Regressed on AMEX Index	.005	.001	.000	.024	.007	1.400
OTC Firms Regressed on OTC Index	.005	.002	.000	.029	.007	1.400
AMEX Firms Regressed on NYSE Index	.105	.092	.000	.245	.087	.828
OTC Firms Regressed on NYSE Index	.058	.041	.001	.189	.052	.896

Panel B: Kruskal-Wallis Test ₂ of Difference in Location of r^2 's	
	<u>Mean Ranks of the r^2's</u>
NYSE Firms with NYSE Index	287.09
AMEX Firms with AMEX Index	252.86
OTC Firms with OTC Index	68.68
AMEX Firms with NYSE Index	313.20
OTC Firms with OTC Index	69.50

$\chi^2 = 132.618$
Significant at $\alpha = .000$

indices.¹ The NYSE index, however, is far from perfect. The median r^2 's shown in Table 5 are low: 6.0 percent for NYSE firms, 9.2 percent for AMEX firms, and only 4.1 percent for OTC firms.² The r^2 's for daily data are smaller than the r^2 's for weekly or monthly data, because some of the noise, or trading occurring in response to factors other than the release of earnings, is averaged out in the longer weekly and monthly periods. For example, Beaver (1968) reports an average volume regression r^2 of 7.8 percent using weekly data while Morse's (1981) daily data yield a mean volume regression r^2 of only 3.5 percent.

High r^2 Firms

Although the r^2 's tended to be rather low, several firms had relatively high r^2 's. In other words, these high r^2 firms were more affected by or associated with changes in market-wide trading as measured by the NYSE trading index.³ Because it is not known whether these firms are somehow fundamentally different from the lower r^2 firms (which are less associated with market-wide factors),⁴ a separate analysis was conducted for the high r^2 firms.

Initially, the high r^2 subsample required an r^2 of 20 percent. Only 37 firms, however, met this criterion. The results of the statistical analyses of this sample were mostly insignificant. This could have occurred for at least three reasons. First, the sample size may have been too small to detect the underlying relationships, given the noise in the daily trading volume data. Second, the high r^2 firms may be somehow fundamentally different from the other firms, so that the hypothesized relationships may hold for the low r^2 firms, but not for the high r^2 firms. Finally, the attempt to remove the effects of

market-wide fluctuations was less successful for the low r^2 firms than for the higher r^2 firms. It is possible that some type of market effect (which was not removed from the low r^2 firms by the market-model type regression) may have driven the results in the entire sample (which was heavily affected by the lower r^2 firms).

In an attempt to distinguish between these three competing explanations, the statistical analyses were replicated on a larger sample of high r^2 firms. Since a cutoff level of 10 percent classified the 109 firms listed in Table 6 (about one-quarter of the total sample) as high r^2 firms, the statistical analyses were replicated on this larger subsample. In general, the results of the analyses of the larger subsample were similar to (i.e., in the same direction as) the results of the smaller subsample. For this reason, it seems likely that the first explanation given above--that the initial sample size of 37 firms was too small to detect the underlying relations--may be the most appealing explanation, although the remaining two explanations cannot be ruled out.⁵

Evidence on the Four Research Hypotheses

This section restates and presents statistical evidence bearing on each of the hypotheses. In order to simplify the presentation, discussion will focus on six of the twelve (3 test periods: (3-day, 5-day, and 11-day) X 4 indices: (no index (includes the unadjusted volume measure, MEDIANVOL, and the volume measure adjusted for the firm-specific level of trading, RMEDIANVOL), NYSE index, own index, high r^2

TABLE 6

FIRMS WITH REGRESSION r^2 's GREATER THAN OR EQUAL TO 10 PERCENT

<u>Firm</u>	<u>Exchange</u>	<u>Fiscal Year-End</u>	<u>r^2</u>
Alleghany Corporation	N	12/31	.1098
Amalgamated Sugar Company	N	12/31	.2077
Amax, Incorporated	N	12/31	.1589
Amerada Hess Corporation	N	12/31	.1853
American Airlines	N	12/31	.1174
American Home Products	N	12/31	.25023
American Hospital Supply	N	12/31	.1476
American Maize 'A'	A	12/31	.1456
Anheuser-Bush	N	12/31	.1333
Armco, Incorporated	N	12/31	.1498
Avnet, Incorporated	N	6/30	.1854
Bankamerica Corporation	N	12/31	.2387
Bankers Trust of New York	N	12/31	.1238
Barry Wright Corporation	N	12/31	.1116
Beatrice Foods	N	2/28	.1883
Bristol Myers Company	N	12/31	.2593
Brunswick Corporation	N	12/31	.1020
Capital Holding Corporation	N	12/31	.1074
Ceco Corporation	N	12/31	.1126
Champion Spark Plug	N	12/31	.1034
Citicorp	N	12/31	.2444
Coke Labs	O	12/31	.1613
Colgate-Palmolive	N	12/31	.1703
Cominco Limited	A	12/31	.2452
Coors, Adolpha Company	O	12/31	.1465
Cordura Corporation	N	12/31	.2772
Cubic Corporation	A	9/30	.1948
Deere and Company	N	12/31	.1119
Dekalb Agresearch	O	8/31	.1431
Donaldson, Lufkin, Jenrette	N	12/31	.3511
Dow Chemical	N	12/31	.2826
Dresser Industries	N	10/31	.1663
Eastman-Kodak	N	12/31	.2673
Edwards, A. G. & Sons	N	2/28	.3773
Electric Memories and Magnetics	N	12/31	.1671
Exxon	N	12/31	.4266
Figgie International	N	12/31	.1235
General Motors Corporation	N	12/31	.2446
General Telephone and Electric	N	12/31	.1872
Goodyear Tire and Rubber	N	12/31	.1315
Gould, Incorporated	N	12/31	.1204

TABLE 6, cont.

<u>Firm</u>	<u>Exchange</u>	<u>Fiscal Year-End</u>	<u>r²</u>
Gulf Canada Limited	A	12/31	.2268
Heda Mining	N	12/31	.1297
Homestakes Mining	N	12/31	.2448
IC Industries	N	12/31	.1419
International Business Machine	N	12/31	.2512
International Harvester	N	10/31	.1595
International Paper	N	12/31	.2065
Katy Industries	N	12/31	.2502
Lilly, Eli and Company	N	12/31	.1780
Litton Industries	N	7/31	.3302
Louisiana-Pacific Corporation	N	12/31	.1387
McDonnell-Douglas	N	12/31	.1757
Merrill Lynch and Company	N	12/31	.5402
Michigan Sugar	A	9/30	.2074
Mobil Corporation	N	12/31	.3732
Mohawk Data Sciences	N	4/31	.3135
Montana-Dakota Utilities	N	12/31	.1023
NL Industries	N	12/31	.2306
National Distillers	N	12/31	.1907
Natomas Company	N	12/31	.2241
Northern Telecom	N	12/31	.1864
Northwest Airlines	N	12/31	.1168
Northwest BanCorporation	N	12/31	.1232
Northwest Natural Gas	O	12/31	.1471
Outlet Company	N	1/31	.1251
Paine Webber, Incorporated	N	9/30	.4325
Patrick Petroleum Company	N	4/31	.1298
J. C. Penney	N	1/31	.1830
Pennzoil Company	N	12/31	.2319
Perkin, Elmer Corporation	N	7/31	.2140
Petroleum and Research Corporation	N	12/31	.2553
Phelps-Dodge	N	12/31	.2046
Piedmont Natural Gas	N	10/31	.1287
Pittson Company	N	12/31	.1358
Polaroid Corporation	N	12/31	.1582
Prime Computer Corporation	N	12/31	.1225
Procter & Gamble	N	6/30	.2159
Ranger Oil	A	12/31	.1946
Reading and Bates	N	12/31	.1106
Recognition Equipment	N	10/31	.1313
SPS Technologies	N	12/31	.2126

TABLE 6, cont.

<u>Firm</u>	<u>Exchange</u>	<u>Fiscal Year-End</u>	<u>r²</u>
Safeguard Scientifics	N	12/31	.1408
Scientific Atlanta	N	6/30	.1842
Scotty's, Incorporated	N	6/30	.1051
Searle, G. D., and Company	N	12/31	.1823
Sperry Corporation	N	3/31	.3293
Square D Company	N	12/31	.1038
Standard Oil of Indiana	N	12/31	.3334
Standex International	N	6/30	.1091
Sullair Corporation	N	12/31	.1600
Sun Banks of Florida	N	12/31	.1730
Sun, Incorporated	N	12/31	.1639
Sundstrand	N	12/31	.2301
Sunshine Mining	N	12/31	.1336
Texas International	N	12/31	.1095
Textron, Incorporated	N	12/31	.1017
Tiger International	N	12/31	.1337
Transamerica Corporation	N	12/31	.1065
Trans World Corporation	N	12/31	.1653
UAL Incorporated	N	12/31	.1525
Union Carbide	N	12/31	.1388
Union Oil - California	N	12/31	.1879
Wang Labs B	A	6/30	.3255
Warner Communications	N	12/31	.1199
Wickes Companies	N	1/31	.1289
Williams Companies	N	12/31	.3156
Wyle Labs	N	12/31	.2006
Zurn Industries	N	3/31	.2082

subset)) data sets. For each formal hypothesis, statistical results from the following data sets will be presented:

- 1) No index (market-wide trading effect is not removed), 3-day test period,
- 2) No index, 5-day period,
- 3) No index, 11-day period,
- 4) NYSE index, 3-day period,
- 5) Own index, 3-day period, and
- 6) High r^2 firms, 3-day period.

These data sets were chosen because the most consistent and statistically significant results were obtained when (1) no market-wide trading effect was removed, and (2) the shorter 3-day test period was used.⁶ Except that the results tend to be less significant, the evidence from the remaining data sets (NYSE index, own index and high r^2 firms X 5-day, 11-day test periods) is consistent with the results presented.

In order to provide some insight into the generality of the four hypotheses, a secondary analysis classifies the data into groups according to stock exchange, firm size, fiscal year-end month, good news versus bad news, and recency of analysts' earnings forecasts. The statistical analyses were repeated on each of these subsamples in order to determine whether any of these factors affect the hypothesized relationships. In order to avoid needless repetition, the results of these additional analyses are not detailed unless they differ from the results of the primary analysis. When the results of the secondary analyses are presented, however, the results are shown for the six data sets listed above.

Hypothesis One

The first hypothesis can be stated in the null and alternative forms, respectively:

H_0 : The volume of trading does not increase significantly around the time annual earnings are released.

H_1 : The volume of trading increases significantly around the time annual earnings are released.

Table 7 presents evidence indicating that the null hypothesis should be rejected. Panel A shows the results when a market-wide trading effect is not removed. As discussed in Chapter III, MEDIANVOL is defined as the median percentage of firm shares traded during the earnings release test period, and so represents a measure of trading unadjusted for market-wide as well as firm-specific effects. RBASEMEDIAN is defined as the median percentage of firm shares traded during the calendar year of the test period; it provides a measure of the firm-specific average level of trading.

The first section of Panel A shows that the median percentage of shares traded around the date of the earnings release (MEDIANVOL) is greater than the percentage of shares traded throughout the rest of the year (RBASEMEDIAN). As might be expected, Panel A also shows that as the test period lengthens, the median trading volume declines: MEDIANVOL (the unadjusted volume measure) declines as the test period increases. In other words, the increase in trading appears to be concentrated in a short period surrounding the earnings announcement date.

This result is supported by the nonparametric sign and signed rank tests, shown in the second section of Panel A, which analyze the

TABLE 7

STATISTICAL EVIDENCE BEARING ON HYPOTHESIS ONE:
SIGN AND SIGNED RANK TESTS^a

Panel A: Market-Wide
Effect is not Removed

	<u>Mean Values</u>	
	<u>MEDIANVOL</u> (unadjusted measure of trading volume)	<u>RBASEMEDIAN</u> (median level of un- adjusted trading calculated over the entire period 1/1/77 to 12/31/79)
Three-Day Period	1.657	.949
Five-Day Period	1.444	.949
Eleven-Day Period	1.261	.949

	<u>Z Scores</u>	
	<u>SIGN TEST</u>	<u>SIGNED RANK TEST</u>
Three-Day Period	8.334 (.000)	13.165 (.000)
Five-Day Period	6.366 (.000)	10.662 (.000)
Eleven-Day Period	3.259 (.000)	7.016 (.001)

^a Parenthetical numbers indicate the one-tailed significance levels of the z-scores, based on 1081 observations.

TABLE 7, cont.

STATISTICAL EVIDENCE BEARING ON HYPOTHESIS ONE:
SIGN AND SIGNED RANK TESTS

Panel B: Three-Day Test Period Comparison of the Statistical Impact of Removal of Market-Wide Effects		
	<u>Mean Values</u>	
	<u>MEDIANVOL</u> (unadjusted measure of trading volume)	<u>RBASEMEDIAN</u> (median level of un- adjusted trading calculated over the entire period 1/1/77 to 12/31/79)
No Market-Wide Effect Removed	1.657	.949
	<u>RESMEDIAN</u> (trading volume adjusted for market wide effects)	<u>BASEMEDIAN</u> (base level of trading throughout the sample period, abstracting from market-wide effects)
NYSE Index	.043	-.267
Own Index	.042	-.265
High r^2 Firms	.005	-.235
	<u>SIGN TEST</u>	<u>SIGNED RANK TEST</u>
No Market-Wide Effect Removed ^b	8.334 (.000)	13.165 (.000)
NYSE Index ^b	9.852 (.000)	13.461 (.000)
Own Index ^b	9.100 (.000)	12.971 (.000)
High r^2 Firms ^c	11.984 (.000)	7.126 (.000)

^b Parenthetical numbers indicate the one-tailed significance levels of z-scores, based on 1081 observations.

^c Parenthetical numbers indicate the significance levels given 278 observations.

difference between the trading around the earnings release date (MEDIANVOL) and the average level of trading for the firm (RBASEMEDIAN). In large samples, when the null hypothesis of no difference in location is true, the distribution of the sign and signed rank statistics can be approximated by the normal (0,1) distribution. The Z-scores of these test statistics, along with their significance levels, appear in the second section of Panel A. Again, the Z-scores decline as the test period lengthens. All the statistics are, however, highly significant. As might be expected, the Z-scores of the more powerful signed rank test exceed those of the sign test. In summary, the results are striking--when no market-wide trading effect is removed, the percentage of shares traded increases significantly⁷ around the release of annual earnings.⁸

Panel B presents a similar comparison of the 3-day test period (1) no index, (2) NYSE index, (3) own index, and (4) high r^2 firm data sets. As discussed in Chapter III, RESMEDIAN is defined as the median volume regression residual during the test period; it represents a measure of trading volume adjusted for market-wide trading effects, but not for firm-specific effects. BASEMEDIAN is defined as the median volume regression residual during the calendar year to which the test period belongs. Since the regression residuals are skewed with medians below zero, the BASEMEDIAN'S are negative.⁹

Panel B of Table 7 corroborates the earlier results. Regardless of the adjustment for market-wide trading effects, trading around the release of annual earnings (MEDIANVOL, RESMEDIAN) exceeds the average level of trading over the year (RBASEMEDIAN, BASEMEDIAN). The sign test and signed rank test Z-scores are all highly significant, and except for

the high r^2 firms, the signed rank Z's exceed the sign test Z-scores. Notice that the Z-scores of the high r^2 firms are based on only 278 observations, rather than the 1081 observations for the no index, NYSE index, and own index data sets.

The remaining data sets (NYSE index, own index, high r^2 firms X 5-day period, 11-day period) and subsets (based on stock exchange, firm size, fiscal year-end month, good news versus bad news, and recency of analysts' forecasts) exhibit similar patterns, with slightly less significant results.

The magnitude of the reaction to the annual earnings release however, does seem to be affected by two of the potentially confounding factors--stock exchange and fiscal year-end month. Table 8 presents the results of Kruskal-Wallis tests on the extent of the reactions¹⁰ for various types of firms. (Recall that in large samples--this study has over 1000 observations--when the null hypothesis of no difference in location is true, the distribution of the Kruskal-Wallis statistic can be approximated by a chi-square distribution.)

Panel A of Table 8 shows that trading volume around the earnings release date is higher for the non-NYSE firms than for the NYSE firms. This difference is at least marginally significant for all of the volume measures except the measure adjusted for market-wide effects through regression on the own exchange index. And even for this metric the difference lies in the same direction (i.e., more trading for non-NYSE firms),¹¹

Panel B shows that the reactions of the non-12/31 firms significantly exceed those of the 12/31 firms, except for the high r^2

TABLE 8

STATISTICAL EVIDENCE BEARING ON THE GENERALITY
OF HYPOTHESIS ONE OVER SUBSETS OF DATA: KRUSKAL-
WALLIS TESTS OF THE IMPACT OF STOCK EXCHANGE
AND FISCAL YEAR-END MONTH

Panel A: Comparison of the Statistical Impact of Stock Exchange

I	<u>Unadjusted Volume Measure</u>	<u>Mean Ranks</u>		<u>Chi-Square^a</u>
	<u>MEDIANVOL</u>	<u>Non-NYSE</u>	<u>NYSE</u>	
	<u>Length of Test Period</u>			
	Three Days	583.18	543.22	1.842 (.087)
	Five Days	592.80	542.43	2.939 (.043)
	Eleven Days	580.97	544.05	1.579 (.105)
II	<u>Adjusted Volume Measures^b</u>			
	<u>RMEDIANVOL</u> (trading volume adjusted for firm-specific median level of trading)	590.96	534.11	3.817 (.025)
	<u>RESMEDIAN - NYSE Index</u> (trading volume adjusted for changes in the NYSE market index)	601.74	542.88	4.022 (.022)
	<u>RESMEDIAN - Own Index</u> (trading volume adjusted for changes in the own market index)	570.09	547.30	0.591 (.221)
	<u>High r^2 Firms^c</u>	170.52	152.00	1.800 (.090)

^a Parenthetical numbers indicate the one-tailed significance levels, given 1090 observations.

^b Results of 3-day test period, only.

^c Parenthetical numbers indicate the significance levels based on 278 observations.

TABLE 8, cont.

STATISTICAL EVIDENCE BEARING ON THE GENERALITY
OF HYPOTHESIS ONE OVER SUBSETS OF DATA: KRUSKAL-WALLIS
TESTS OF THE IMPACT OF STOCK EXCHANGE AND FISCAL YEAR-END MONTH

Panel B: Comparison of the Statistical Impact of Fiscal Year-End Month

	<u>Mean Ranks</u>		<u>Chi Square</u> ^c
	<u>12/31 Firms</u>	<u>Non- 12/31 Firms</u>	
I <u>Unadjusted Volume Measure</u>			
<u>MEDIANVOL</u>			
<u>Length of Test Period</u>			
Three Days	517.09	615.39	21.840 (.000)
Five Days	532.13	592.75	8.340 (.002)
Eleven Days	513.32	626.50	28.910 (.000)
II <u>Adjusted Volume Measures</u> ^d			
<u>RMEDIANVOL</u>	526.53	567.93	3.924 (.024)
(trading volume adjusted for firm-specific median level of trading)			
<u>RESMEDIAN - NYSE Index</u>	533.93	588.44	6.741 (.005)
(trading volume adjusted for changes in the NYSE market index)			
<u>RESMEDIAN - Own Index</u>	535.19	585.43	5.725 (.008)
(trading volume adjusted for changes in the own market index)			
<u>High r² Firms</u> ^e	154.72	160.51	0.275 (.300)

^c Parenthetical numbers indicate the one-tailed significance levels, given 1090 observations.

^d Results of 3-day test period, only.

^e Parenthetical numbers indicate significance levels given 278 observations.

firm subset. Again, although the difference is not significant, it does lie in the same direction (i.e., a greater reaction for non-12/31 firms than for 12/31 firms). This result is not surprising, since non-NYSE firms are more likely to be non-12/31 firms (at an alpha level of .035).

One potential explanation for these results is that accounting earnings releases may provide a greater proportion of the information available about non-NYSE firms and non-12/31 firms. The non-NYSE firms (in this sample) are smaller (at an alpha level of .001) than the NYSE firms, so there may be fewer alternative sources of information--such as Wall Street Journal articles and Valueline analyses--available for the smaller firms. Non-12/31 firms in this sample are not only smaller than 12/31 firms (at an alpha level of .001), but they may also have one less alternative source of information--intra-industry earnings releases. It seems less likely that applicable intra-industry releases will be available at the time non-12/31 firms release their earnings. The releases of the non-12/31 firms may, therefore, be more surprising than the releases of 12/31 firms since investors in the former probably have less information on industry trends. This explanation is consistent with the results of the secondary analyses of subsequent hypotheses, and will be discussed in more detail in Chapter V.

In conclusion, Table 7 provides consistent evidence that trading increases around the release of annual earnings while Table 8 shows that the magnitude, but not the existence, of the reaction is affected by the stock exchange and fiscal year-end month. This study replicates Beaver's (1968), Kiger's (1973), and Morse's (1981) results with a more comprehensive sample, and extends their work by analyzing the generality

of the results over different subsets of data (based on firm size, etc.).

As discussed earlier, since previous research has shown that annual earnings releases are informative, this study's rejection of this first research hypothesis suggests that the volume metrics employed in this study are valid measures of information content. The interpretation of the remaining results assumes that the volume metrics employed in this study do provide valid measures of information content.

Hypothesis Two

The second hypothesis can be stated in the null and alternative forms, respectively:

H_0 : The absolute value of the unexpected earnings is not positively correlated with the volume of trading around the date of the earnings release.

H_1 : The absolute value of the unexpected earnings is positively correlated with the volume of trading around the date of the earnings release.

Table 9 shows the Spearman's rank order correlation coefficients, between trading volume and unexpected earnings measures. All of the combinations of volume and unexpected earnings are significantly positively correlated (marginally significant for the high r^2 firm subsample) thus indicating that the null hypothesis should be rejected. Results are presented for MEDIANVOL (unadjusted trading volume), RMEDIANVOL (volume adjusted for firm-specific trading levels), and RESMEDIAN (volume adjusted for market effects). As discussed in Chapter III, this study employs three measures of absolute unexpected earnings. UE1 is defined as the percentage difference between the Valueline analyst's forecast and actual EPS before extraordinary items. UE2 is a

TABLE 9

STATISTICAL EVIDENCE BEARING ON HYPOTHESIS TWO:
SPEARMAN'S RANK CORRELATION COEFFICIENT, ρ^a

		<u>Measure of Absolute Unexpected Earnings</u>		
		<u>UE1</u>	<u>UE2</u>	<u>UE5</u>
I	<u>Unadjusted Measure of Trading Volume</u>	(before extra-ordinary items)	(after extra-ordinary items)	(random-walk model)
	<u>MEDIANVOL</u>			
	<u>Length of Test Period</u>			
	Three Days	.0770 (.006)	.0977 (.001)	.2471 (.001)
	Five Days	.0742 (.008)	.0961 (.001)	.2596 (.001)
	Eleven Days	.0709 (.010)	.0888 (.002)	.2542 (.001)
II	<u>Adjusted Volume Measures</u>			
(A)	<u>RMEDIANVOL</u> (trading volume adjusted for firm-specific median level of trading)	.0810 (.004)	.0776 (.006)	.1209 (.001)
(B)	<u>RESMEDIAN</u> (trading volume adjusted for changes in the market index)			
	NYSE Index ^a	.0505 (.050)	.0427 (.082)	.1046 (.002)
	Own Index ^a	.0531 (.042)	.0416 (.080)	.1000 (.004)
	High r^2 Firms ^c	.0851 (.070)	.0641 (.133)	.0877 (.072)

^a Parenthetical numbers indicate the one-tailed significance levels of the correlation coefficient, ρ , given 1081 observations.

^b Results of 3-day test period, only.

^c Parenthetical numbers indicate the significance levels, given 278 observations.

similar measure, except that actual EPS after extraordinary items is used. The final measure, UE5, is a random-walk model which uses last year's EPS (before extraordinary items) as the forecast of this year's EPS (before extraordinary items).

Sections I and II A show the results when no market-wide trading effect is removed. In general, the strength of the relationship declines as the test period lengthens, particularly as it increases from five to eleven days. The results are generally stronger for MEDIANVOL (unadjusted metric) than for RMEDIANVOL (adjusted for firm-specific trading). And surprisingly, the relationship seems to be stronger for the random-walk expectation, UE5, than for the surprise metrics based on the analysts' forecasts, UE1 and UE2.

Section II B of Table 9 presents similar data for the 3-day test period for the (1) NYSE index, (2) own index, and (3) high r^2 firm data sets. All the correlation coefficients are at least marginally significant. Notice that the high r^2 firm significance levels are based on only 278 observations, as opposed to the 1081 observations in the other data sets. In addition, the correlation coefficients are somewhat higher for the no index data set than for the other sets which attempt to remove a market-wide trading affect. Finally, the trading volume metrics seem to be more highly correlated with the random-walk measure of unexpected earnings than with the measures based on the analyst's forecasts. Similar, although less significant results were found for the remaining data sets (NYSE index, own index, high r^2 firms X 5-day, 11-day test periods).

The secondary analysis of potentially confounding factors also yields two items which affect the pattern of correlations between trading volume and unexpected earnings--fiscal year-end month and recency of the analyst's forecast. Table 10, Panel A shows that the 12/31 year-end firms exhibit a similar pattern to that shown in Table 9. The non-12/31 firms, however, do not show a significant correlation between the volume metrics and the measures of unexpected earnings based on analysts' forecasts, UE1 and UE2. Two of the volume metrics, MEDIANVOL (the unadjusted percentage of shares traded) and RMEDIANVOL (the measure adjusted for the firm-specific average level of trading) were, however, significantly correlated with the random-walk measure of unexpected earnings, UE5. Although not significant, the two volume measures adjusted for market-wide effects through regression on the NYSE or own index also exhibit a closer association with the random-walk measure than with the analysts'-based measure of unexpected earnings.

The high r^2 firm subsample did not yield any significant results. The trading of the high r^2 subsample was, however, more closely associated with the analyst forecast measures of UE than with the random-walk measures, and as above, the trading of the 12/31 firms was more closely associated with the measures of unexpected earnings than was the trading of the non-12/31 firms.

As with the 12/31 firms, the significant correlations become somewhat less significant as the test period increases from three to eleven days, and they are also greater for MEDIANVOL (the unadjusted volume measure) than for RMEDIANVOL (the metric adjusted for the

TABLE 10

STATISTICAL EVIDENCE BEARING ON THE GENERALITY OF
 HYPOTHESIS TWO OVER SUBSETS OF DATA: FISCAL YEAR-END MONTH
 AND RECENCY OF ANALYST'S FORECASTS

Panel A: Comparison of the Statistical Impact
of Fiscal Year-End Month

		<u>Measure of Unexpected Earnings</u>		
		<u>UE1</u>	<u>UE2</u>	<u>UE5</u>
I	<u>Unadjusted Measure of Trading Volume</u>	<u>(Before extra- ordinary items)</u>	<u>(After extra- ordinary items)</u>	<u>(random-walk model)</u>
<u>MEDIANVOL</u>				
<u>Length of Test Period</u>				
Three Days				
	12/31 Firms ^a	.1190 (.001)	.1306 (.001)	.2565 (.001)
	Non 12/31 Firms ^b	-.0118 (.424)	.0271 (.330)	.2067 (.003)
Five Days				
	12/31 Firms ^a	.1208 (.001)	.1342 (.001)	.2727 (.001)
	Non 12/31 Firms ^b	-.0241 (.347)	.0148 (.405)	.2099 (.002)
Eleven Days				
	12/31 Firms ^a	.1225 (.001)	.1348 (.001)	.2690 (.001)
	Non 12/31 Firms ^b	-.0499 (.209)	-.0272 (.329)	.1783 (.008)

TABLE 10, Panel A cont.

		<u>Measure of Unexpected Earnings</u>		
		<u>UE1</u>	<u>UE2</u>	<u>UE5</u>
II	<u>Adjusted Volume Measures^c</u>	(Before extra-ordinary items)	(After extra-ordinary items)	(random-walk model)
(A)	<u>RMEDIANVOL</u> (trading adjusted for firm-specific level of trading)			
	12/31 Firms ^a	.1004 (.004)	.0948 (.006)	.1030 (.013)
	Non 12/31 Firms ^b	.0345 (.289)	.0401 (.259)	.1720 (.011)
(B)	<u>RESMEDIAN</u> (trading volume adjusted for market effects)			
	NYSE Index			
	12/31 Firms ^a	.0838 (.013)	.0743 (.023)	.1061 (.010)
	Non-12/31 Firms ^b	-.0213 (.364)	-.0323 (.299)	.0677 (.181)
	Own Index			
	12/31 Firms ^a	.0838 (.012)	.0710 (.028)	.1096 (.008)
	Non-12/31 Firms ^b	-.0299 (.312)	.0409 (.254)	.0602 (.208)
	High r^2 Firms			
	12/31 Firms ^d	.1314 (.113)	.1115 (.152)	.0399 (.383)
	Non-12/31 Firms ^e	.0647 (.172)	.0479 (.242)	.0904 (.141)

^a Parenthetical numbers indicate the significance level given 722 observations.

^b Parenthetical numbers indicate the significance level given 282 observations

^c Results of 3-day test period, only.

^d Parenthetical numbers indicate the significance level given 192 observations.

^e Parenthetical numbers indicate the significance level given 34 observations.

TABLE 10, cont.

STATISTICAL EVIDENCE BEARING ON THE GENERALITY OF
 HYPOTHESIS TWO OVER SUBSETS OF DATA: FISCAL
 YEAR-END MONTH AND RECENCY OF ANALYSTS' FORECASTS

Panel B: Comparison of the Statistical Impact
of Recency of Analysts' Forecasts

		<u>Measure of Unexpected Earnings</u>	
		<u>UE1</u>	<u>UE2</u>
I	<u>Unadjusted Measure of Trading Volume</u>	(Before extra- ordinary items)	(After extra- ordinary items)
<u>MEDIANVOL</u>			
<u>Length of Test</u>			
<u>Period</u>			
Three Days			
	Recent Forecast ^d	.0799 (.166)	.1153 (.080)
	Less Recent Forecast ^e	.1972 (.008)	.1922 (.010)
Five Days			
	Recent Forecast	.1179 (.075)	.1464 (.037)
	Less Recent Forecast	.1940 (.009)	.2091 (.005)
Eleven Days			
	Recent Forecast	.0881 (.142)	.1058 (.099)
	Less Recent Forecast	.2148 (.004)	.2352 (.002)

TABLE 10, PANEL B cont.

II	<u>Adjusted Volume Measures^f</u>	<u>Measure of Unexpected Earnings</u>	
		<u>UE1</u> (Before extra-ordinary items)	<u>UE2</u> (After extra-ordinary items)
(A)	<u>RMEDIANVOL</u> (trading adjusted for firm-specific level of trading)		
	Recent Forecast ^d	.0213 (.398)	.0545 (.255)
	Less Recent Forecast ^e	.1718 (.018)	.1922 (.010)
(B)	<u>RESMEDIAN</u> (trading adjusted for market-effects)		
	NYSE Index		
	Recent Forecast ^d	.0752 (.180)	.0562 (.247)
	Less Recent Forecast ^e	.1010 (.110)	.1051 (.101)
	Own Index		
	Recent Forecast	.0777 (.172)	.0537 (.257)
	Less Recent Forecast	.1376 (.045)	.1170 (.078)

^d The 150 observations with the Valueline analysts' forecast date closest to the earnings release date (up to a 29 day gap between the forecast and actual earnings release). Parenthetical numbers indicate the one-tailed significance levels of the correlation coefficient, ρ , given 150 observations.

^e The 150 observations with the Valueline analysts' forecast date farthest from the earnings release date (at least a 100 day gap between the forecast and actual earnings release). Parenthetical numbers indicate the significance levels given 150 observations.

^f Results of 3-day test period, only.

firm-specific average level of trading), and RESMEDIAN (the measure adjusted for market effects).

Panel B presents more surprising evidence.¹² The relationship between unexpected earnings and trading volume is stronger for distant forecasts than it is for recent forecasts¹³ (although the more recent forecasts are more accurate at an alpha level of .002). That is, the correlation coefficients are higher for the less recent forecasts than for the more recent forecasts. Also, the coefficients do not seem to decline as the length of the test period increases. Similar to the results presented in Table 9, both recent and distant forecast correlation coefficients are stronger for the unadjusted measure of trading volume, MEDIANVOL.

A potential explanation for the results is only outlined here, for it will be discussed in more detail in Chapter 5. The results of the secondary analysis suggest that this study may be capturing the effects of a relatively large number of naive traders. Tables 9 and 10 both show a higher and more significant correlation between trading volume and the random-walk measure of absolute unexpected earnings, UE5, than between trading volume and the more sophisticated measures of unexpected earnings based on the Valueline analysts' forecasts, despite the fact that the analysts' forecasts are more accurate than are the random-walk model predictions (with median errors of 3.8 percent and 19.1 percent, respectively). This result is consistent with Table 10B which shows that the correlations between trading volume and unexpected earnings are higher for less recent, as opposed to more recent earnings forecasts. These results suggest that this study is capturing the trading of a

naive group of investors who base their earnings expectations on naive models such as last years' earnings.

The finding that the non-12/31 firms' trading volume was significantly correlated with the random-walk measure of unexpected earnings, but not with the analyst based measures suggests that investors in non-12/31 firms may base their trading on less information than investors in 12/31 firms. This could occur because there are fewer sources of information available about these non-12/31 firms, as discussed earlier. It could also reflect the fact that collecting and processing information is costly. Investors may find it worthwhile to inform themselves only when they expect to receive earnings reports from the majority of firms they invest in. For this reason, they may be relatively more informed when they receive the releases of the 12/31 firms as opposed to the non-12/31 firms.

The above explanations seem to hold for all the subsets except the high r^2 firms. Perhaps the investors in these larger firms are more sophisticated than investors in the smaller, possibly more closely held, lower r^2 firms. As discussed earlier, it is also possible that some type of market-wide trading effect, which was not adequately corrected in the lower r^2 firms, may be driving the results.

In conclusion, the rejection of hypothesis two is well supported by the evidence in Table 9--earnings surprises are positively correlated with trading volume. Table 10, however, shows that the strength of this correlation is affected by several factors. As discussed above, the measures of trading volume are more closely associated with the less sophisticated earnings expectation measures. Second, the fewer the

adjustments to the volume data, the stronger the correlations. Table 9 shows that the highest correlation coefficients occur for the no index data set, which does not remove a market-wide trading effect. Tables 9 and 10 also indicate that the correlation coefficients for the unadjusted volume measure, MEDIANVOL, RESMEDIAN are higher than the coefficients for trading volume adjusted for firm-specific trading levels, RMEDIANVOL. These results suggest that while the evidence is fairly robust with respect to statistical methods of adjusting the volume measures for trading arising from reasons other than the release of firm-specific information (i.e., market effects and levels of firm-specific trading), these adjustments do weaken the significance of the results.

Hypothesis Three

The third hypothesis can be stated in the null and alternative forms:

- H_0 : The volume of trading (based upon the percentages of shares traded) around the date of the annual earnings release is not negatively correlated with firm size.
- H_1 : The volume of trading (based upon the percentage of shares traded) around the earnings release date is negatively correlated with firm size.

Table 11 presents the Spearman's rank order correlation coefficients, ρ , between trading volume and firm size. As discussed in Chapter III, this study employs two measures of firm size--total assets and total owners' equity. Except for the high r^2 data set, all of the volume-size combinations are significantly negatively correlated. Although not significant, the high r^2 firm data set coefficients are

TABLE 11

STATISTICAL EVIDENCE BEARING ON HYPOTHESIS
THREE: SPEARMAN'S RANK CORRELATION COEFFICIENT, ρ ^a

		<u>Measure of Firm Size</u>	
		<u>Total Assets</u>	<u>Total Owners' Equity</u>
I	<u>Unadjusted Measure of Trading Volume</u>		
	<u>MEDIANVOL</u>		
	<u>Length of Test Period</u>		
	Three Days	-.1904 (.001)	-.1799 (.001)
	Five Days	-.1845 (.001)	-.1755 (.001)
	Eleven Days	-.1859 (.001)	-.1695 (.001)
II	<u>Adjusted Volume Measures</u> ^b		
(A)	<u>RMEDIANVOL</u> (trading volume adjusted for firm-specific trading)		
	Three Days	-.0597 (.026)	-.0695 (.011)
(B)	<u>RESMEDIAN</u> (trading volume adjusted for changes in the market index)		
	NYSE Index ^c	-.0594 (.025)	-.0403 (.091)
	Own Index ^c	-.0501 (.049)	-.0339 (.131)
	High r^2 Firms ^d	.0388 (.249)	-.0292 (.304)

^a Parenthetical numbers indicate the one-tailed significance levels of the correlation coefficient, ρ , given 1081 observations.

^b Results of 3-day test period, only.

^c Parenthetical numbers indicate the one-tailed significance levels of the correlation coefficient, ρ , given 1081 observations.

^d Parenthetical numbers indicate the one-tailed significance levels given 278 observations.

also negative. This evidence indicates that the null hypothesis should be rejected.

Sections I and II A show the results when no market-wide trading effect is removed. In general, the strength of the relationship declines as the test period lengthens, particularly as it increases from five to eleven days. As before, the results are stronger for the unadjusted volume metric, MEDIANVOL, than for the metric adjusted for firm-specific level of trading, RMEDIANVOL.

Section II B of Table 11 presents similar data for the 3-day test period for the (1) NYSE index, (2) own index, and (3) high r^2 firm data sets. Except for the high r^2 firm set, all the correlation coefficients are significantly negative, and the results of the high r^2 subsample do lie in the hypothesized direction. Again, the results are stronger for the unadjusted metric, MEDIANVOL, than for either (1) the volume measures adjusted for firm-specific average level of trading, RMEDIANVOL, or (2) the measures adjusted for market-wide trading effects, RESMEDIAN. Finally, the significant correlations appear to be slightly stronger for the total asset measure of firm size than for the total owners' equity metric.

The evidence in Table 11 is further supported by Table 12, which presents the results of a Wilcoxon signed rank analysis of two subsamples--the 150 smallest firm size-year observations, and the 150 largest firm size-year observations. Since there were three observations per firm (three years of data), if firm size (as measured by total assets) did not change over the three years, these two subsamples would contain the 50 smallest and 50 largest firms, respectively. The

STATISTICAL EVIDENCE BEARING ON HYPOTHESIS THREE:
WILCOXON SIGNED RANK TEST

	<u>Mean Ranks of Small Firms^a</u>	<u>Mean Ranks of Large Firms^b</u>	<u>Z-Scores^c</u>	
I	<u>Unadjusted Measure of Trading Volume</u>			
	<u>MEDIANVOL</u>			
	<u>Length of Test Period</u>			
	Three Days	158.43	116.81	4.338 (.000)
	Five Days	160.61	116.60	4.572 (.000)
	Eleven Days	161.43	116.93	4.615 (.000)
II	<u>Adjusted Volume Measures^d</u>			
(A)	<u>RMEDIANVOL</u> (trading volume adjusted for median firm-specific trading)			
	Three Days	142.61	132.22	.983 (.163)
(B)	<u>RESMEDIAN</u> (trading volume adjusted for changes in the market index)			
	NYSE Index ^e	138.88	132.88	.630 (.264)
	Own Index ^e	137.87	133.97	.410 (.341)
	High r ² Firms ^f	58.29	45.08	1.920 (.022)

^a Small firms were defined as the 150 smallest firm size-year observations. Since there were 3 observations per firm, if firm size did not change, this group would include all three years of data for the 50 smallest sample firms. These firms had less than \$7,625,000 of total assets.

^b Large firms were defined as the 150 largest firm size-year observations, with total assets in excess of \$288,275,000.

^c Parenthetical numbers indicate the one-tailed significance levels of the Z-scores, based on 300 observations.

^d Results of 3-day test period, only.

^e Parenthetical numbers indicate the one-tailed significance level given 150 observations.

^f Parenthetical numbers indicate the one-tailed significance level given 95 observations. The same dollar value cutoffs, \$7,625,000 for the small firms and \$288,275,000 for the large firms was used to enhance comparability across the volume measures.

Wilcoxon procedure tests for a difference in the location of the trading volume metrics between the small and large firm subsamples. Table 12 indicates that the mean ranking of the small firms' trading metrics exceeds the mean ranking of the large firms' trading metrics. In other words, the volume measures are higher for the smaller firms--the percentage of shares traded around the date of the annual earnings announcement is greater for smaller firms than for larger firms.

Sections I and II A show the results when no market-wide effect is removed. Remember that for large samples, the Wilcoxon signed rank statistic has an approximate normal (0,1) distribution. The Z-scores for the unadjusted volume metric, MEDIANVOL, are highly significant, while those for the firm-specific level of trading adjusted RMEDIANVOL are not significant, although the difference is in the hypothesized direction. The significance of the difference seems to increase slightly, rather than decline as the test period lengthens.

Section II B of Table 12 presents similar data for the 3-day test period for the (1) NYSE index, (2) own index, and (3) high r^2 firm data sets. Except for the high r^2 subsample, none of the Z-scores are significant, although the differences do lie in the hypothesized direction.

The results of Table 12 support those of Table 11. The negative correlation between firm size and volume of trading is most significant for the unadjusted measure of trading volume, and less significant for the measures adjusted for either market-wide effects or firm-specific average trading. This result has at least two potential explanations.

First, the adjustment methods may be inappropriate--they may either add more noise to the data than they remove, or they may remove part of the phenomenon of interest. Second, as discussed above, the opposite scenario is possible. Perhaps the adjustment techniques are valid, and the inability to adequately remove market-wide and firm-specific effects has driven the results for the unadjusted volume measure. Taken together, however, the evidence presented in Tables 11 and 12 suggests that the first explanation may be more appropriate. In Table 11, the inverse correlation is at least marginally significant for all data sets except the higher r^2 subsample, and the results of the high r^2 data set do lie in the hypothesized direction. In Table 12, the inverse correlation is significant for all the volume measures except the two metrics adjusted for market-wide effects through regression on either the NYSE or the own index. Again, the results for these two measures also lie in the hypothesized direction, even though they are not statistically significant. This evidence seems sufficient to warrant the rejection of the third hypothesis, thus suggesting that around the earnings release date, there is relatively more trading for the smaller firms than for the larger firms.

The secondary analysis yields three factors which affect the relationship between trading volume and firm size--stock exchange, fiscal year-end month, and type of news in the earnings release. Table 13, Panels A, B, and C respectively, show the effects of each factor.

Panel A provides evidence that the negative correlation between firm size and trading volume is more pronounced for non-NYSE firms, particularly the OTC firms.¹⁴ (Remember that the AMEX and OTC

TABLE 13

STATISTICAL EVIDENCE BEARING ON THE GENERALITY OF
HYPOTHESIS THREE OVER SUBSETS OF DATA: STOCK EXCHANGE,
FISCAL YEAR-END MONTH, AND GOOD NEWS VERSUS BAD NEWS

Panel A: Comparison of the Statistical Impact of Stock Exchange

		<u>Measure of Firm Size</u>	
		<u>Total Assets</u>	<u>Total Owners' Equity</u>
I	<u>Unadjusted Trading Volume Measure</u>		
	<u>MEDIANVOL</u>		
	<u>Length of Test Period</u>		
	Three Days		
	NYSE Firms ^a	-.1617 (.001)	-.1469 (.001)
	AMEX Firms ^b	-.2335 (.068)	-.2099 (.091)
	OTC Firms ^c	-.3708 (.001)	-.3622 (.001)
	Five Days		
	NYSE Firms ^a	-.1589 (.001)	-.1418 (.001)
	AMEX Firms ^b	-.1873 (.115)	-.1831 (.120)
	OTC Firms ^c	-.3640 (.001)	-.3712 (.001)
	Eleven Days		
	NYSE Firms ^a	-.1614 (.001)	-.1410 (.001)
	AMEX Firms ^b	-.2478 (.055)	-.2138 (.084)
	OTC Firms ^c	-.3495 (.001)	-.3095 (.001)

TABLE 13, PANEL A cont.

	<u>Measure of Firm Size</u>	
	<u>Total Assets</u>	<u>Total Owners' Equity</u>
II <u>Adjusted Volume Measures</u> ^d		
(A) <u>RMEDIANVOL</u> (trading adjusted for firm-specific level of trading)		
NYSE Firms ^a	-.0351 (.142)	-.0463 (.077)
AMEX Firms ^b	-.0631 (.346)	-.0281 (.430)
OTC Firms ^c	-.1787 (.054)	-.1822 (.049)
(B) <u>RESMEDIAN</u> (trading adjusted for market effects)		
NYSE Index		
NYSE Firms ^a	-.0430 (.092)	-.0301 (.175)
AMEX Firms ^b	.0129 (.466)	.0924 (.271)
OTC Firms ^c	-.1090 (.160)	-.1370 (.113)
Own Index		
NYSE Firms ^a	-.0430 (.092)	-.0301 (.175)
AMEX Firms ^b	.0980 (.261)	.1333 (.191)
OTC Firms ^c	-.0972 (.188)	-.0469 (.333)

^a Parenthetical numbers indicate the one-tailed significance levels of the correlation coefficient, ρ , given 948 observations.

^b Parenthetical numbers indicate the significance level given 42 observations.

^c Parenthetical numbers indicate the significance level given 72 observations.

^d Results of 3-day test period, only.

TABLE 13, cont.

STATISTICAL EVIDENCE BEARING ON THE GENERALITY OF
HYPOTHESIS THREE OVER SUBSETS OF DATA: STOCK EXCHANGE,
FISCAL YEAR-END MONTH, AND GOOD NEWS VERSUS BAD NEWS

Panel B: Comparison of the Statistical
Impact of Fiscal Year-End Month

		<u>Measure of Firm Size</u>	
		<u>Total Assets</u>	<u>Total Owners' Equity</u>
I	<u>Unadjusted Trading Volume</u>		
	<u>MEDIANVOL</u>		
	<u>Length of Test Period</u>		
	Three Days		
	12/31 Firms ^e	-.0886 (.009)	-.0631 (.045)
	Non 12/31 Firms ^f	-.3116 (.001)	-.2887 (.001)
	Five Days		
	12/31 Firms ^e	-.0701 (.031)	-.0393 (.146)
	Non 12/31 Firms ^f	-.3141 (.001)	-.2937 (.001)
	Eleven Days		
	12/31 Firms ^e	-.0669 (.037)	-.0356 (.170)
	Non 12/31 Firms ^f	-.3257 (.001)	-.2826 (.001)

TABLE 13, PANEL B cont.

	<u>Measure of Firm Size</u>	
	<u>Total Assets</u>	<u>Total Owners' Equity</u>
II <u>Adjusted Trading Volume</u> ^g		
(A) <u>RMEDIANVOL</u> (trading adjusted for firm-specific average level of trading)		
12/31 Firms ^e	.0094 (.402)	.0071 (.426)
Non 12/31 Firms ^f	-.1524 (.006)	-.1628 (.003)
(B) <u>RESMEDIAN</u> (trading adjusted for market-effect)		
NYSE Index		
12/31 Firms ^e	-.0284 (.224)	-.0004 (.495)
Non-12/31 Firms ^f	-.0583 (.159)	-.0670 (.129)
Own Index		
12/31 Firms ^e	-.0274 (.232)	.0007 (.492)
Non-12/31 Firms ^f	-.0394 (.253)	-.0542 (.180)
High r^2 Firms		
12/31 Firms ^e	.0281 (.342)	.0549 (.211)
Non-12/31 Firms ^f	-.1714 (.048)	-.1480 (.075)

^e Parenthetical numbers indicate the significance level given 722 observations.

^f Parenthetical numbers indicate the significance level given 282 observations.

^g Results for 3-day test period, only.

^h Parenthetical numbers indicate the significance level based on 192 observations.

ⁱ Parenthetical numbers indicate the significance level based on 78 observations.

TABLE 13, cont.

STATISTICAL EVIDENCE BEARING ON THE GENERALITY OF
 HYPOTHESIS THREE OVER SUBSETS OF DATA: STOCK EXCHANGE,
 FISCAL YEAR-END MONTH, AND GOOD NEWS VERSUS BAD NEWS

Panel C: Comparison of the Statistical Impact
 of Good News Versus Bad News^h

		<u>Measure of Firm Size</u>	
		<u>Total Assets</u>	<u>Total Owners' Equity</u>
I	<u>Unadjusted Trading Volume</u>		
	<u>MEDIANVOL</u>		
	<u>Length of Test Period</u>		
	Three Days		
	Good News ⁱ	-.0963 (.004)	.0021 (.484)
	Bad News ^j	-.2744 (.001)	-.1892 (.001)
	Five Days		
	Good News ⁱ	-.1188 (.001)	-.0438 (.076)
	Bad News ^j	-.2596 (.001)	-.2371 (.001)
	Eleven Days		
	Good News ⁱ	-.1386 (.001)	-.1365 (.001)
	Bad News ^j	-.2503 (.001)	-.2083 (.001)

TABLE 13, PANEL C cont.

	<u>Measure of Firm Size</u>	
	<u>Total Assets</u>	<u>Total Owners' Equity</u>
II <u>Adjusted Volume Measures</u>		
(A) <u>RMEDIANVOL</u> ^k		
Good News ⁱ	-.1043 (.001)	-.0147 (.386)
Bad News ^j	-.1657 (.001)	-.1576 (.001)
(B) <u>RESMEDIAN</u> (trading adjusted for market- effects)		
NYSE Index		
Good News ⁱ	-.0458 (.120)	-.0281 (.235)
Bad News ^j	-.0850 (.040)	-.0626 (.097)
Own Index		
Good News ⁱ	-.0263 (.250)	-.0131 (.368)
Bad News ^j	-.0918 (.029)	-.0698 (.074)
High r^2 Firms		
Good News ^l	-.0437 (.271)	-.0133 (.426)
Bad News ^m	-.0513 (.295)	-.0704 (.230)

^h Good news is defined as positive unexpected earnings (actual earnings forecasted earnings) while bad news is defined as negative unexpected earnings.

ⁱ Parenthetical numbers indicate the significance level given 708 observations.

^j Parenthetical numbers indicate the significance level given 294 observations.

^k Results for 3-day test period, only.

^l Parenthetical numbers indicate the significance level given 171 observations.

^m Parenthetical numbers indicate the significance level given 104 observations.

significance levels in Table 13 are based on 42 and 72 observations, respectively, while the NYSE levels are based on 948 observations.) Again, the relationship seems to be much more significant for the unadjusted measure of trading volume, MEDIANVOL, than for either (1) RMEDIANVOL, which is adjusted for firm-specific trading levels, or (2) RESMEDIAN, which is adjusted for market-wide trading effects. The significance does not seem to decline as the test period lengthens, and the results for the total asset measure of firm size appear to be slightly more significant than those for the total owners' equity measure.

Panel B provides evidence that the relationship is stronger for non-12/31 firms than for 12/31 firms. This result is not surprising, since, as discussed above, the non-NYSE firms are more likely to have non-12/31 fiscal year ends than are the NYSE firms. As before, the correlations are stronger for the unadjusted volume metric, MEDIANVOL, and for the total asset measure of firm size. Except for the high r^2 subsample, the results are not significant for the volume measures adjusted for market-wide effects, although the results do lie in the same direction.

Panel C shows that there is a stronger relationship between trading volume and firm size when the earnings release is bad news (actual earnings forecast earnings) than when the release is good news. Except for the high r^2 subset, the inverse correlation between trading volume and firm size is at least marginally significant for all of the bad news subsets. Although not statistically significant,¹⁵ the results of the high r^2 subsample follow the same pattern (all negative correlations

which are stronger for bad news than good news). Finally, for the good news subsets the correlations between trading volume and firm size also lie in the hypothesized direction even though they are only significant for the unadjusted measure of trading volume, and the measure adjusted for firm-specific average level of trading (the latter was significant only for the total asset measure of firm size).

In summary, the associations are stronger for the unadjusted measures of trading volume than for the measures adjusted for either market-wide effects or firm-specific average level of trading. As discussed above, this situation could arise because 1) the adjustment methods are inappropriate, or 2) the adjustments are appropriate, but the inability to sufficiently adjust the majority of firms for market-wide effects and firm-specific average level of trading causes the overall results to be driven by these effects.

All of the significant results indicate an inverse relationship between firm size and trading volume, and most of the results which are not statistically significant also lie in the hypothesized direction. The question posed by Table 13 concerns the generality of the size effect. It seems to be stronger for non-NYSE firms, non-12/31 firms, and bad news firms. This pattern is consistent with that observed in relation to the first two hypotheses.

One potential explanation is outlined here. As discussed before, it is likely that annual earnings reports form a larger proportion of the amount of information available about non-NYSE and non-12/31 firms. These reports may also form a larger proportion of the amount of information available about firms releasing unfavorable earnings

reports. Evidence suggests that voluntary disclosures are normally favorable, and that the release of unfavorable information may be delayed as long as possible. If annual reports do form a larger proportion of the information about non-NYSE, non-12/31, and bad news firms, then one would expect a stronger effect on trading volume when the earnings of these firms are released. Recall that this evidence and explanation is consistent with that offered in relation to the first two hypotheses.

Hypothesis Four

The third hypothesis was rejected because the evidence indicated that around the date of the earnings release, trading was proportionately higher for the smaller firms than for the larger firms. As discussed earlier, one potential explanation for this result is that there may be more surprises in the smaller firms' earnings announcements. This question is the essence of the fourth hypothesis which may be stated in the null form and alternative forms, respectively::

H_0 : The absolute value of unexpected earnings is not negatively correlated with firm size.

H_1 : The absolute value of unexpected earnings is negatively correlated with firm size.

Table 14 presents the Spearman's rank order correlation coefficients, ρ , between firm size and the absolute value of unexpected earnings. Since no measure of trading volume is required for the analysis of hypothesis four, Table 14 is refreshingly short. Of course, firm size and unexpected earnings are not affected by the removal of market-wide or firm-specific trading effects, nor are they affected by calculations of median trading volume over different test periods. In

TABLE 14

STATISTICAL EVIDENCE BEARING ON
HYPOTHESIS FOUR: SPEARMAN'S RANK
CORRELATION COEFFICIENT, ρ^a

<u>Measure of Unexpected Earnings</u>	<u>Measure of Firm Size</u>	
	<u>Total Assets</u>	<u>Total Owners' Equity</u>
<u>UE1</u> (before extraordinary items)		
All Firms (n = 1081)	-.1005 (.001)	-.1188 (.001)
High r^2 Firms (n = 278)	-.1366 (.009)	-.1229 (.017)
12/31 Firms (n = 716)	-.1205 (.001)	-.1198 (.001)
Non 12/31 Firms (n = 270)	-.1913 (.001)	-.2662 (.001)
Good News Firms ^b (n = 630)	-.1019 (.005)	-.1145 (.002)
Bad News Firms ^c (n = 430)	-.1105 (.011)	-.1348 (.002)
<u>UE2</u> (after extraordinary items)		
All Firms (n = 1081)	-.1118 (.001)	-.1472 (.001)
High r^2 Firms (n = 278)	-.1791 (.001)	-.1826 (.001)
12/31 Firms (n = 716)	-.1095 (.001)	-.1204 (.001)
Non 12/31 Firms (n = 270)	-.1667 (.002)	-.2590 (.001)
Good News Firms (n = 630)	-.1114 (.003)	-.1500 (.001)
Bad News Firms (n = 430)	-.1202 (.006)	-.1514 (.001)
<u>UE5</u> (random-walk model)		
All Firms (n = 1081)	-.1733 (.001)	-.2082 (.001)
High r^2 Firms (n = 278)	-.0865 (.113)	-.1420 (.022)
12/31 Firms (n = 716)	-.1131 (.005)	-.1586 (.001)
Non 12/31 Firms (n = 270)	-.2896 (.001)	-.3126 (.001)
Good News Firms (n = 630)	-.0890 (.030)	-.1050 (.013)
Bad News Firms (n = 430)	-.2971 (.001)	-.3573 (.001)

^a The parenthetical numbers indicate the significance levels given n observations.

^b Good news firms are those where forecast EPS < actual EPS.

^c Bad news firms are those where forecast EPS > actual EPS.

other words, the unexpected earnings and firm size measures do not change over the nine (no index, NYSE index, own index X 3-day, 5-day, 11-day test periods) data sets.

Table 14 shows highly significant inverse correlations between the measures of unexpected earnings and the measures of firm size. This result holds across all subsamples, so the evidence indicates that hypothesis four should be rejected. In other words, the evidence supports the conjecture that the smaller firms' earnings releases contain more surprises than the releases of the larger firms. This result provides a potential explanation for the rejection of hypothesis three where it was shown that around the annual earnings release dates, trading is proportionately higher for the smaller firms than it is for the larger firms.

The secondary analysis yields three factors affecting the relationship between unexpected earnings and firm size--regression r^2 , fiscal year-end month, and type of news in the earnings release. The correlation coefficient between earnings surprises and firm size is more significant for the high r^2 firm subsample than for the sample of all firms. Second, like the results for hypothesis three, the relationship is stronger 1) for the non-12/31 firms than for the 12/31 firms, and 2) for bad news firms than for good news firms. A similar explanation (i.e., earnings releases constitute a larger proportion of the information available about the non-12/31 and bad news firms) is posited.

Additional Analysis Focusing
on Signed Measures of Unexpected Earnings

Supplementary analyses were performed to further investigate the relationship between trading volume and the direction of the unexpected earnings.

The data were classified into five groups. This classification was based upon the magnitude of UE3, the signed measure of unexpected earnings calculated as the percentage difference between the Valueline analyst's forecast and actual EPS before extraordinary items. The cutoff points were arbitrarily set as follows:

- 1) Very bad news--93 observations had actual earnings at least 20 percent less than forecast earnings.
- 2) Moderately bad news--296 observations occurred where actual earnings were 1-19 percent less than forecast earnings.
- 3) No news--196 observations had less than a 1 percent forecast error.
- 4) Moderately good news--407 observations occurred where actual earnings were 1-19 percent greater than forecast earnings.
- 5) Very good news--89 observations had actual earnings at least 20 percent greater than forecast earnings.

The criteria were set so there would be about 100 observations at each extreme. A 20 percent error rate accomplished this objective. The no news group was chosen to be those observations with less than a 1 percent error; roughly 200 observations fell into this classification. The remaining observations were classified as moderately good news or moderately bad news, depending upon whether the actual or forecast earnings were greater.

Table 15 presents the results of the Wilcoxon signed rank test for difference in location of the trading volume measures, given the classification of the data by the signed unexpected earnings measure, UE3.¹⁶ Sections I and II A present the results when no market-wide trading effect is removed. The data indicate that there is no significant difference in the amount of trading between the very good news and very bad news groups. Table 15 shows significantly more trading for the moderately good news group than for the moderately bad news group. This association does not seem to diminish as the test period lengthens, but it is stronger for the unadjusted volume measure, MEDIANVOL, than for the measure adjusted for firm-specific trading volume, RMEDIANVOL.

Section II B of Table 15 presents the results of the Wilcoxon tests for the 3-day period (1) NYSE index, (2) own index, and (3) high r^2 data sets. These results support Sections I and II A (except for the high r^2 subsample): there is significantly more trading for the moderately good news than for the moderately bad news groups, but there is no significant difference in trading for the very good news and very bad news groups. Again, the results are more significant for the no index data set than for the data adjusted for market-wide trading effects. The results were also stronger for the volume measure unadjusted for average firm trading volume, MEDIANVOL. Similar, although less significant, results were found for the remaining data sets (NYSE index, own index X 5-day, 11-day test periods).

In conclusion, Table 15 shows that the release of moderately good news in the earnings report is associated with a higher level of trading than is the release of moderately bad news. This result is consistent with

TABLE 15

WILCOXON SIGNED RANK TESTS FOR DIFFERENCE IN LOCATION
OF TRADING VOLUME MEASURES, GIVEN CATEGORIES
OF SIGNED UNEXPECTED EARNINGS

I	<u>Unadjusted Trading Volume</u>	<u>Mean Ranks</u>	<u>Z-Scores^a</u>
	<u>MEDIANVOL</u>		
	<u>Length of Test Period</u>		
	Three Days		
	Very Bad News	90.52	
	Very Good News	86.23	.577 (.288)
	Moderately Bad News	308.78	
	Moderately Good News	367.30	3.799 (.000)
	Five Days		
	Very Bad News	92.55	
	Very Good News	84.98	.982 (.163)
	Moderately Bad News	311.17	
	Moderately Good News	365.79	3.548 (.000)
	Eleven Days		
	Very Bad News	89.52	
	Very Good News	88.41	.144 (.444)
	Moderately Bad News	308.53	
	Moderately Good News	367.62	3.839 (.000)

TABLE 15, cont.

II <u>Adjusted Trading Volume</u>	<u>Mean Ranks</u>	<u>Z-Scores</u> ^a
(A) <u>RMEDIANVOL</u> (volume adjusted for firm-specific trading)		
Very Bad News	87.51	
Very Good News	88.56	.138 (.445)
Moderately Bad News	315.59	
Moderately Good News	353.40	2.475 (.007)
(B) <u>RESMEDIAN</u> (volume adjusted for market effects)		
NYSE Index		
Very Bad News	89.08	
Very Good News	89.89	.445 (.325)
Moderately Bad News	329.49	
Moderately Good News	358.34	1.868 (.031)
Own Index		
Very Bad News	87.64	
Very Good News	86.22	.186 (.425)
Moderately Bad News	330.29	
Moderately Good News	357.78	1.779 (.037)
High r^2 Firms ^c		
Very Bad News	61.82	
Very Good News	58.38	.873 (.189)
Moderately Bad News	94.76	
Moderately Good News	99.68	.583 (.279)

^a Except for the high r^2 firms, the Z-scores are based on the following number of observations in each group: very bad news (93), moderately bad news (296), no news (196), moderately good news (407), very good news (89).

^b Results for 3-day test period, only.

^c For the high r^2 firms, the significance levels are based on the following number of observations in each group: very bad news (26), moderately bad news (68), no news (60), moderately good news (124), and very good news (22).

Epps (1975) and Copeland's (1977) work, but conflicts with Griffin and Ng's (1978) model. The spirit of the result is also consistent with Schacter and Hood's (1982) "bubba theory" which suggests that investors trade more heavily when bullish, or in a state of euphoria.

Chapter Summary

This chapter presents the analysis of data obtained from the research investigation described in Chapter III. The data enabled the testing of four hypotheses which were directly related to the first three research objectives. All four hypotheses were rejected; the evidence presented in this study suggests that:

- 1) The percentage of a firm's shares traded increases around the date the annual earnings are released.
- 2) The percentage of firm shares traded is positively correlated with the absolute value of the unexpected earnings or surprises in the annual earnings release.
- 3) Firm size and percentage of shares traded around the release of annual earnings are negatively correlated; i.e., upon the release of annual earnings, there is a greater increase in trading for the smaller firms.
- 4) Firm size is negatively correlated with the absolute value of unexpected earnings; that is, there are more surprises in the earnings releases of the smaller firms.

A secondary analysis provided insight into the generality of the results. The data suggest that the magnitude, but not the direction, of the results may be affected by factors such as fiscal year-end date, stock exchange, firm size, type of news (favorable or unfavorable), and recency of analyst's forecast.

The next chapter includes a summary of the results, and a discussion of the conclusions and implications which can be derived from the results.

FOOTNOTES

¹Since almost all firms' trading was more associated with the NYSE index than with their own exchange indices, it seems unlikely that a combined index would improve upon the NYSE index. Further, the calculation of a combined index of market trading would present practical problems, in particular, it is unclear what sort of weighting scheme would be appropriate.

²This result is consistent with the previous conjecture that OTC firms are more closely held, less widely traded, and therefore less affected by (or associated with) the macroeconomic factors influencing the market-wide level of trading.

³It is interesting to note that all these high r^2 observations occurred when the individual firms' trading was regressed on the NYSE index, rather than on the own exchange index.

⁴Investigation of the differences between the high and low r^2 firms lies beyond the scope of this dissertation. It was found, however, that the high r^2 firms are larger ($\chi^2 = 69.9$, significant at $\alpha = .000$) and more heavily traded ($\chi^2 = 16.423$, significant at $\alpha = .000$) than the lower r^2 firms.

⁵The remaining two explanations cannot be ruled out because to increase the high r^2 sample size, firms less well-described by the market-model type regression had to be included. It is possible that these added firms may 1) be different from the initial 37 firms, or 2) overwhelm the effects of the original 37 firms.

⁶The choice of the three test periods was described in footnote 5 of Chapter III on page 105.

⁷For purposes of exposition, in this study the term "significant" indicates that the alpha level, or probability of incorrectly rejecting the null hypothesis, is less than or equal to 5 percent. The term "marginally significant" signifies an alpha level of 5-10 percent. This terminology was selected because researchers cannot seem to agree on a single significance level, and both the 5 percent and 10 percent levels are commonly used. In any case, all of the tables present the alpha levels obtained from the statistical analyses enabling the reader to choose his own significance level.

⁸The measure of trading volume adjusted for the firm-specific average level of trading, RMEDIANVOL, is defined as the extent of the reaction to the annual earnings release (MEDIANVOL-RBASEMEDIAN). A test of the significance of RMEDIANVOL would therefore reduce to the test of difference in location of MEDIANVOL and RBASEMEDIAN reported in Table 7.

⁹See footnote 8 on page 106 of Chapter III.

¹⁰The extent of the reaction is defined as the trading measure at the date of the earnings release (MEDIANVOL or RESMEDIAN) minus the median level of trading calculated over the year (RBASEMEDIAN or BASEMEDIAN).

¹¹Additional analyses were performed to determine whether there was a difference between the reactions of AMEX and OTC firms. No significant difference was found.

¹²The high r^2 subset is not included in Panel B of Table 10 because there were only 18 firms with at least a 100 day gap between the forecast and actual earnings release date. It was felt that this sample size was too small to permit valid generalizations; in any event, the results were not significant.

¹³A Spearman test was run to determine whether firm size was positively correlated with the recency of the forecast, based on the idea that larger firms might publish their earnings more quickly than smaller firms. The analysis yielded a ρ of $-.0122$, significant at an alpha level of $.346$. The forecast gap, therefore, does not seem to be closely related to firm size. This result does not necessarily suggest that large firms are as slow as small firms in releasing their earnings, because the earnings release lag is confounded by the fact that Valueline publishes its analyses on a 13-week rotating basis as explained in Chapter III.

¹⁴The high r^2 subsample is not included in Panel A of Table 13 because there were only 10 AMEX and 6 OTC firms in this subsample. It was felt that this sample size was too small to warrant presentation; in any event, the results were not significant.

¹⁵Recall that the high r^2 bad news statistical analysis is based on 104 observations, as opposed to the 294 observations for the remaining data sets.

¹⁶The Kruskal-Wallis tests were performed on two subsets of data--the extreme observations (very good news versus very bad news) and the less extreme observations (moderately good news versus moderately bad news). That is, the extreme observations were jointly ranked, and Table 15 shows that the mean ranks of the very bad news and very good news categories are not significantly different. The procedure was repeated on the moderately good news and moderately bad news categories.

CHAPTER V

SUMMARY AND CONCLUSIONS

This chapter summarizes the research undertaken, provides a more detailed discussion of the interpretation of the conclusions and implications of the results, discusses the limitations of the study, and makes suggestions for future research.

The chapter consists of five sections. The first section summarizes the research described in the first four chapters. This summary provides the basis for drawing, in the second section, conclusions from the results. Implications with respect to information content studies, research methodology, and accounting practice are discussed in the third section. The fourth section enumerates the limitations of the research, and discusses the possible implications of these limitations on the validity of the study's results. Finally, in section five, suggestions for future research are provided.

Summary

Accountants are interested in the usefulness of the information they produce. Authoritative bodies have voiced concern that financial statements be useful to members of the investment community. Researchers have historically measured the impact of accounting information on investors via three approaches: (1) examination of market

prices, (2) tests of market trading volume, and (3) experimental investigations of individual investors' behavior. This study adopted the second approach, for reasons discussed in detail throughout the first three chapters. The results of the research suggest that trading volume does provide a valid measure of information content. Further, the analysis suggests that two factors affect the extent of information content of the earnings releases--the amount of surprise in the earnings release, and firm size. Finally, the magnitude (although not the direction) of certain relationships seems to be affected by factors such as stock exchange and fiscal year-end date.

Chapter II reviewed the literature relating to volume research. After distinguishing the implications of price research from those of volume research, it was suggested that research on both price and volume statistics may be necessary to gain a more complete understanding of market behavior. For example, a major implication of the sequential information arrival models is that price and volume may be negatively correlated; indeed, price changes may be near a minimum as volume reaches its peak. The results of social welfare research further justify the interest in volume research; if certain assumptions are met, trading volume can provide an index of changes in social welfare.

The overall purpose of the study was to extend the investigation of the information content of annual earnings releases. In particular, the study investigated (1) the validity of using trading volume as a measure of information content, (2) factors contributing to the information content of annual earnings releases, and (3) potentially confounding factors which might limit the ability to generalize the results of (1)

and (2). Chapter III presented the research methodology used in this investigation, related the specific hypotheses to the research objectives, and described and justified the statistical tests employed throughout the study.

Chapter IV presented the analysis and interpretation of the data obtained in the study. The results of the hypothesis tests are summarized in Table 16. Additional analyses included investigation of:

- 1) the three indices of exchange trading,
- 2) a subsample of firms whose behavior was relatively well-described by the regression of individual firm trading on an index of exchange trading,
- 3) potentially confounding factors, (e.g., fiscal year-end date, stock exchange, and recency of the analyst's forecast) which might affect the results of the hypothesis tests, and
- 4) the effect of the direction of the earnings surprise.

The results of this analysis are not summarized here. Rather, they are combined with the results of the hypothesis tests, and the next two sections present an interpretation of the conclusions and their implications.

Conclusions

In a strict sense, the following conclusions apply only to the time period (1977-1979) and the firm population sampled: firms listed in Valueline, which (1) were also available in Compuserve's Value data base, and (2) had their earnings announced in the Wall Street Journal. Only eleven and eighteen firms were eliminated due to the second and third criteria, respectively. The first criterion, inclusion in Value line, was by far the most stringent.¹ As discussed in Chapter III, this

TABLE 16

SUMMARY OF HYPOTHESIS TESTS

Null Hypothesis	Result	Model and Significance Level
H ₁ : The volume of trading does not increase significantly around the time annual earnings are released.	Rejected Increase found	Fisher sign test, .000 ^a Wilcoxon signed rank test, (.000) ^a
H ₂ : The absolute value of the unexpected earnings is not positively correlated with the volume of trading around the date of the annual earnings release.	Rejected Positive association found	Spearman's rank correlation coefficient $\rho = .0770$, (.006) ^{a,b}
H ₃ : The volume of trading around the date of the annual earnings release is not negatively correlated with firm size.	Rejected Negative association found	Spearman's rank correlation coefficient $\rho = -.1904$, (.001) ^a Wilcoxon signed rank test (.000) ^a
H ₄ : The absolute value of unexpected earnings is not negatively correlated with firm size.	Rejected Negative association found	Spearman's rank correlation coefficient $\rho = -.1005$ (.001) ^b

^a Significance levels are based on the no index, 3-day test period data set.

^b ρ and significance level are based on the correlation between total assets and UE1, the percentage difference between analysts' forecasts and actual EPS before extraordinary items.

sample is biased toward large, widely held, and widely traded firms, so no generalizations can be made to smaller, more closely held firms.

As with most capital markets research, it is not possible to provide definitive explanations for the results. Markets research cannot be used to assess causality--in most cases the best one can do is to develop a set of internally consistent explanations. For this reason, no claim is made that the following discussion provides the only explanation for the study's results. However, the explanations provided are internally consistent, and are also consistent with the results of previous research.

The conclusions suggested by this research are organized according to the four research objectives which motivated the study.

The First Research Objective

The first research objective was to determine whether abnormally high trading occurs around the time annual earnings are released. The rejection of hypothesis one provides strong evidence that trading volume does increase significantly around the earnings release date. As expected, the significance of the volume reaction to the earnings release declined as the test period, or length of time around the earnings release date, lengthened. These results held across all of the data sets, as well as across all potentially confounding factors (e.g., fiscal year-end date, stock exchange, and type of news in the earnings release).

Although the existence of the reaction was not affected, the secondary analysis showed that the magnitude of the reaction was affected by the stock exchange and fiscal year-end date. Reactions were

stronger for 1) AMEX and OTC firms than for NYSE firms, and for 2) non-calendar year firms rather than 12/31 firms. One potential explanation for this result is that there may be fewer alternative sources of information available about the non-NYSE and non-12/31 firms. Non-NYSE stocks are more likely to be smaller, less widely held and less widely traded than NYSE stocks. For this reason, there are probably fewer Wall Street Journal articles, Valueline analyses, etc., about these non-NYSE stocks. Except for accounting reports, there may be few sources of information available; therefore, the earnings release may contain a larger proportion of the information about the non-NYSE firms.

Stock exchange and fiscal year-end are not independent of each other; non-NYSE firms are more likely to have non-12/31 fiscal year-end dates. Hence, even in the absence of any other factors, reactions to earnings releases are likely to be greater for non-12/31 firms than for 12/31 firms. Foster (forthcoming), however, has found evidence suggesting that earnings releases of other firms in the same industry provide information supplemental to that released in a particular firm's earnings report. It seems less likely that applicable intra-industry releases will be available at the time non-12/31 firms release their earnings. The releases of the non-12/31 firms may, therefore, be more surprising than the releases of 12/31 firms since investors in the former probably have less information on industry trends. Results discussed in conjunction with the second, third, and fourth research objectives are consistent with this explanation.

These findings related to the first hypothesis tend to confirm Beaver's (1968), Kiger's (1973), and Morse's (1981) results with a more

comprehensive sample, and extend their work by analyzing the effects on the research results of the potentially confounding factors discussed above. These results are also consistent with the sequential information arrival model which suggests that information releases are accompanied by abnormally high trading. Finally, if the assumptions of the social welfare models are accepted, these results suggests that earnings releases increase social welfare by increasing at least one investor's ex ante expected utility, without decreasing any other investor's ex ante expected utility.

Since previous research has shown that annual earnings releases are informative (e.g., Beaver, 1968; Morse, 1981), the rejection of the first hypothesis suggests that the trading volume statistics calculated in this study do provide valid measures of information content. The validity of the volume statistics as measures of information content is assumed in the subsequent interpretation of the research results.

The Second Research Objective

The second research objective was to determine whether the extent of trading is positively associated with the amount of the surprises in the earnings releases. The rejection of hypothesis two suggests that trading volume increases as earnings surprises increase.

Several factors affected the magnitude and significance, but not the direction, of the correlation coefficients. The first factor is the measure of unexpected earnings. The correlations were much higher for the naive, random-walk models of unexpected earnings than they were for the more sophisticated models based on Valueline analysts' forecasts, despite the fact that analysts' forecasts were much more accurate than

the random-walk forecasts. This result suggests that a large group of investors may be trading based on naive expectation models. The conjecture is further supported by the finding that trading volume is more closely associated with less recent, rather than more recent analysts' forecasts. In other words, trading increases as actual earnings deviate from outdated analysts' forecasts. These results suggest that a large number of investors may not be well-informed (with respect to publicly available information) when they trade.

The association between trading volume and unexpected earnings is also affected by the calendar month of the firms' fiscal year-end. For non-12/31 fiscal year-end firms, trading volume was positively associated with earnings surprises only when the earnings surprises were measured by the naive random-walk model; the correlations were not significant when the unexpected earnings measures were based on the analysts' forecasts. This result is consistent with the conjecture that investors may be better informed in January, February, and March, when the 12/31 firms' earnings are released, than they are during the remainder of the year. Such a situation could result from at least two factors. First, if there is a large fixed cost to becoming better informed, some investors may choose to incur this cost only around the time they expect to receive information pertaining to several of their investments. Second, investors may have access to an additional source of information about 12/31 firms. Foster (op. cit.) finds evidence suggesting that investors discern information applicable to a particular firm from the earnings releases of other firms in the same industry. It

is less likely that the necessary intra-industry earnings releases will be available around the time the non-12/31 firms release their earnings.

In summary, the results of the data analysis relating to the second research objective show that trading volume increases as earnings surprises increase. In other words, the trading volume measures of information content employed in this study suggest that the more surprising earnings releases are also more informative to investors. This result is consistent with Beaver, Clarke, and Wright's (1979) conclusion that more surprising earnings releases are more informative, as measured by changes in market price. It is also consistent with Griffin and Ng's (1978) model which states that conflicting signals (e.g., surprising earnings releases) should lead to more trading than reinforcing signals (i.e., confirming releases.)

Third Research Objective

The third research objective was to determine whether the extent of trading around the release of annual earnings is affected by the size of the firm. If so, the next problem was to investigate whether this relation might occur because the releases of the smaller firms contain more surprises. The rejection of hypothesis three provides evidence that around annual earnings releases, trading is relatively greater for the smaller firms than for the larger firms. This result is consistent with both Grant's (1980) and Atiase's (1979) price research which finds greater price reactions (to earnings announcements) for smaller firms than for larger firms. One explanation for this phenomenon is suggested by Banz (1980): since there may be fewer alternative sources of information available about the smaller firms,² their earnings

announcements may be more surprising. The rejection of hypothesis four supports this explanation.

This inverse relationship between firm size and the extent of trading holds across all of the data sets for all of the volume measures, and across all the potentially confounding factors (for at least the unadjusted volume measure). Three of these factors--stock exchange, fiscal year-end month, and type of news (good or bad)--affect the magnitude and significance, but not the direction of the results.

The relationship was stronger for non-NYSE firms, non-12/31 firms, and firms whose earnings reports hold bad news. At least one explanation is consistent with these results. Presumably there are fewer alternative (to earnings reports) sources of information available about the smaller non-NYSE firms than about the larger NYSE firms. Since the non-12/31 firms are more likely to be non-NYSE firms ($\chi^2_{(1)} = 4.72$, which is significant at the $\alpha = .035$ level), and since intra-industry earnings releases will often not be available, there may be fewer alternative sources of information available about the non-12/31 firms than the calendar year firms. Finally, there may be fewer alternative sources of information available about bad news releases than about good news releases. Pastena and Ronen (1979) and Penman (1980) present evidence suggesting that discretionary information disclosures (e.g., management earnings forecasts) are biased toward reporting good news, while bad news is hidden as long as possible. If there are fewer alternative sources of information available about the non-NYSE, non-12/31, and bad news firms, then the earnings releases constitute a larger proportion of the information available about these

firms. If this conjecture is true, then there will be less noise, or trading for reasons unrelated to the earnings release, in their volume data, so a clearer analysis (i.e., more significant correlations) should result.

Fourth Research Objective

The fourth objective was to determine whether the removal of 1) a market-wide, or 2) a firm-specific trading effect would change the results of the hypothesis tests. The two major volume theories--the sequential information arrival models and the social welfare models--both assume that trading occurs only in response to firm-specific information releases. In other words, trading is not affected by market-wide fluctuations or firm-specific average levels of trading. Unfortunately, these theories are not sufficiently developed to provide technical guidance on the proper method of adjusting for these market-wide and firm-specific trading effects. For this reason, the study employed several measures of trading volume, based on different adjustments for these two factors.

First, the effect of the overall market-wide level of trading on the trading of each firm was estimated by regressing individual firm volume on an index of exchange trading. Chapter III describes the construction of the three (OTC, AMEX, and NYSE) indices. The results of this adjustment were somewhat surprising, since both the AMEX and OTC firms' trading was more closely associated with the NYSE index than with the index constructed for their own (AMEX or OTC) exchange. One explanation for this is that the NYSE index may better capture the macroeconomic factors affecting the volume of trading. This conjecture

is supported by the finding that the NYSE index was much less volatile than the AMEX, and especially the OTC indices. The volatility of the non-NYSE indices may reflect the fact that the non-NYSE firms are more closely held, and thus perhaps relatively less subject to trading due to accounting releases, and relatively more subject to trading due to noise factors (e.g., change in investors' risk-return preferences on income-consumption tradeoffs) than are the NYSE firms. In any event, the sample firms' trading was more closely associated with the NYSE index, than with the own index, and the results of the hypothesis tests were slightly more significant for the NYSE-index data than for the own-index data.

In all cases, however, the results were stronger still for the measure of trading which was not adjusted for either 1) the firm-specific average level of trading, or 2) the market-wide trading effects. As this result conflicts with the volume theories reviewed in Chapter II, it is somewhat unsettling. At least two competing rationales can explain this finding. First, the adjustment techniques may be too coarse to accomplish their goals. Perhaps they inject more noise into the data than they remove, or they may remove part of the effect of interest. The second, and more disturbing explanation, is that the adjustments may accomplish their objective, and thus the adjusted metrics may provide better measures of trading volume occurring in response to the earnings releases. If this second explanation is true, then the relationships documented in this study may not be as strong as suggested by the unadjusted volume measure.

In summary, the adjustments in many cases weakened the significance of the research results, but in no case did they reverse the direction of the results.

Implications

The implications of the above conclusions are now discussed under three headings--information content research, methodological concerns, and accounting practice.

Information Content Research

The study's implications with respect to existing information content research should be considered since this line of research motivated the study. First, the results show that for the hypotheses tested in this study, the evidence provided by the volume proxies for information content is consistent with other researchers' evidence derived from price surrogates for information content. This study's first hypothesis was rejected since trading increased around the annual earnings release date. This result is consistent with the price research which shows that price changes occur at the time annual earnings are released (e.g., Beaver, 1968; Morse, 1981). Rejection of the second hypothesis shows that the extent of trading increases as earnings surprises increase. In other words, more surprising earnings releases have more information content. Beaver, Clarke, and Wright (op. cit.) report an analogous relationship between market price changes and the magnitude of unexpected earnings. Finally, this study found that around the release of annual earnings, relatively more trading occurred for the smaller firms than for the larger firms, suggesting that the

releases of the smaller firms have more information content than the releases of the larger firms. This result is consistent with Grant's (1980) and Atiase's (1979) work which suggests that larger price reactions occur for the smaller firms, and it is also consistent with Banz's (1981) suggestion that fewer alternative sources of information are available about the smaller firms.

On the other hand, this study has provided two new insights into the market's response to annual earnings releases. First, the fact that trading volume is more closely associated with naive and outdated earnings forecasts than with current and more sophisticated forecasts, suggests that a large number of investors trade based on relatively naive expectations. As long as these less informed traders do not change the equilibrium market prices, this implication does not conflict with the concept of market efficiency, defined by Beaver (1981) as a state of price-invariance.³

Second, this study suggests that investors in certain types of firms appear to be more informed than investors in other types. It appears that the trading of calendar year firms, NYSE firms, and firms releasing favorable earnings numbers may be based on more informed expectations than the trading of non-NYSE firms, non-12/31 firms and firms releasing bad news. One possible explanation for this situation is the conjecture that, for various reasons, there are fewer alternative sources of information available about the latter categories of firms. If this explanation is valid, then annual earnings releases of these firms may have relatively greater information content than the releases of 12/31, NYSE, and good news firms.

Remember that these results have arisen from a trading volume measure of information content, and not a price measure. As discussed in detail in the first three chapters, the volume statistic sums all investors' trades; i.e., the trades of a naive price-taking investor have the same weight as the trades of a sophisticated market-maker. Whether these latter two results unique to this study could be replicated by price research is an empirical question.

Methodological Concerns

The first implication of this study for the methodology used in subsequent research is that researchers should recognize that factors such as firm size, fiscal year-end date, stock exchange, and type of news in the earnings release all affected the magnitude (although not the direction) of the results. For this reason, if sampling is confined to only some of the categories listed above, the ability to generalize the research results to the population of all firms may be compromised. Additionally, in experimental designs where firms are categorized according to some criterion of interest (e.g., issuance of replacement cost figures), the researcher should ascertain that the resultant categories do not differ significantly in terms of firm size, fiscal year-end date, etc. Ro's (1981) study is a good example of the problems these confounding variables can cause. Since only large firms are required to release replacement cost (RC) information, firm size was confounded with the criterion of interest, the release of RC data. Ro hypothesized that if RC data have information content, there would be more trading for the RC-disclosing firms than for the non-disclosing firms. He found no significant difference in trading between the two

groups. This finding could occur because the RC data were not useful. However, it could also have occurred even if the data were informative: this study provides evidence that trading around earnings releases is relatively greater for the smaller firms than for the larger firms. Therefore, in order for the RC-disclosers' trading to exceed the non-disclosers' trading, the RC data would have to generate enough trading to compensate for the size effect. Ro's results are therefore inconclusive.

The second implication relates to the statistical tests employed. Admittedly, the volume data in this study were skewed. Therefore, all tests of significance reported here were based on nonparametric procedures. For comparison purposes, Pearson's r , the parametric analogue of the Spearman rank correlation coefficient, was calculated. Although the significant results were in the direction hypothesized, the results of the parametric tests were in most cases insignificant. This study provides a good example that when the data are not normally distributed, nonparametric procedures may provide more powerful tests than their parametric counterparts.

Third, the results of this study suggest that NYSE firms may provide a better index of market-wide trading resulting from macroeconomic factors (than non-NYSE firms). The more volatile AMEX and OTC indices appear to be more affected by random trading. Unless better measures of AMEX and OTC indices can be constructed, calculation of these indices may not be worthwhile.

Further, this study's results were strongest for the measure of trading which was not adjusted for either 1) the firm-specific average

level of trading, or 2) the market-wide trading effects. Either the adjustment techniques used in this study were too coarse to accomplish their goals, or the research results are not as strong as suggested by the unadjusted volume metric. Future researchers should be aware of this problem, and in the absence of more specific theoretical guidelines concerning just how these adjustments should be made, a variety of adjustment techniques should be employed in order to determine the sensitivity of the research results to the volume metrics used.

Accounting Practice

This study's first implication for accounting practice lies in the so-called "big GAAP⁴-little GAAP" controversy. The gist of this argument is that it is unfair to force small firms to comply with all the complex accounting pronouncements presently in force. A relaxed, or less complicated, set of rules may be developed for small firms which may not be able to afford to hire the degree of expertise necessary to comply with the detailed accounting rules. Presumably, relaxing the standards applying to the small firms will not greatly affect the investment community, since these firms tend to be more closely held and less widely traded than the large firms. The results of this study, however, suggest that the smaller firms' annual earnings numbers are relatively more important--not less important--than the earnings releases of the larger firms, perhaps because there are fewer alternative sources of information available about the smaller firms. Before relaxing the standards for smaller firms, accounting policy-makers should recognize that accounting reports may provide a

disproportionately large amount of the information available about the smaller firms.

The second message for accounting practice is that a large number of investors may be relatively uninformed. The fact that trading volume is more closely associated with outdated and random-walk forecasts than with the more recent and sophisticated analysts' forecasts suggests that many investors may simply use last year's EPS as their expectation for this year's earnings. This closer association with the more naive expectation models is even more pronounced for non-NYSE firms, non-calendar year firms, and firms releasing unfavorable earnings reports. These results have two implications. First, most practicing accountants are skeptical of the concept of market efficiency; they find it difficult to believe that market prices reflect all publicly available information, perhaps because their clients do not seem to make such well-informed decisions at all times. This study suggests that a sizable number of investors may not trade as well-informed investors; they seem to base their trades on a simple comparison of this year's earnings to last year's. As discussed in the previous section, this situation does not conflict with market-efficiency as long as these less informed investors do not change the equilibrium market prices. In other words, this research supports practitioners' beliefs that many investors behave or trade on the basis of relatively uninformed expectations.

The final implication follows from this discussion concerning relatively uninformed investors. If investors base their expectations on last year's EPS, then accounting reports may be even more important

than previously acknowledged. Although alternative sources of information may be available, some investors may not find it cost-efficient to collect and process information from these sources. Additionally, for reasons discussed above, accounting reports may provide a disproportionately large amount of the information available about smaller, non-12/31; non-NYSE, and bad news firms. Hence, accounting reports may have more value for certain groups of investors, such as less-informed investors, investors in non-12/31 firms, non NYSE firms, or firms releasing unfavorable earnings reports--than has been recognized.

Limitations

This section discusses the limitations of the study which may confound or invalidate the conclusions and their implications.

The most important limitation lies in the definition of information content as data which "cause investors to change their portfolio holdings, as evidenced by an abnormally high amount of trading." Obviously, if the release of data induces trading, the data are useful, or have information content. Unfortunately, the converse is not true. A piece of data may be useful without inducing the recipient to trade. Data may be reinforcing in nature (i.e., may reduce the variance of the investor's expectation distribution), or they may cause the investor to revise his expectations, but not enough to offset the costs of trading.

The absence of an if-and-only-if relationship between information content and trading volume is a serious limitation of the trading volume definition. However, the two other definitions of information content

discussed in Chapter III--uncertainty reduction and change in security returns--are plagued by the same problem. If the release of data causes market prices to change, or reduces the uncertainty about future returns, the data are certainly useful. Again, the converse is not true. Data can be useful in making investment decisions without inducing a price change or reducing uncertainty. Since none of the three definitions are perfect, and each measures a different phenomenon (see Figure 6 in Chapter III), research based on each of these definitions may provide insight into the market's response to information releases. The trading volume approach was chosen for this study since the price-based definition has already been well-researched, while the uncertainty reduction definition is extremely difficult to measure.

The second major limitation of the study lies in confining the sample to firms analyzed in Valueline. As discussed previously, this study's sample is biased in favor of larger, more widely held and more widely traded stocks. Because Valueline provides the compendium of analysts' forecasts most suitable for use in this study, this advantage outweighed the limitation. In any event, this study's sample is more comprehensive than those of most previous volume studies, since this sample includes non-12/31 firms and non-NYSE firms, as well as firms not listed on the Compustat or CRSP tapes.

The third limitation arises from the difficulty in measuring the concept of "abnormally high trading." Since trading occurs for reasons other than the release of accounting earnings, an attempt must be made to mitigate the effects of this noise on the volume data. As described

in excruciating detail in Chapter III, this study adopted the standard regression of individual firm volume on an index of market-wide trading. Presumably a carryover from price research, this technique has been employed in virtually all of the empirical volume studies. The method is essentially arbitrary, however, because the sequential information arrival models and the social welfare models give no guidance on how to accomplish this adjustment, even though they assume that the adjustment is made.

The adjustment for the firm-specific average level of trading was unique to this study. Again, the volume theories presume that the average level of trading in the absence of the earnings release is zero, but they give no guidance on how to make this adjustment.

The success of these two adjustments is questionable: the research results were stronger for the unadjusted measure of trading volume. This finding suggests that either:

- 1) the adjustment techniques are too coarse to accomplish the objective of abstracting from trading occurring for reasons other than earnings releases (perhaps they a) inject more noise into the data than they remove, or b) remove part of the phenomenon of interest).
- 2) the adjustment techniques are valid and the results are not as strong as suggested by the unadjusted volume measure.

The answer to this question requires further research. Researchers can create new adjustments and test the sensitivity of their results to the adjustment technique used.

Since the adjustments used in this study weakened the significance of the findings, but in no case changed the direction of the results, presumably the basic conclusions of the research are valid. The

magnitude of the effects documented here, however, may be less than suggested by the unadjusted volume metric.

The fourth limitation arises from the difficulty in measuring the concept of unexpected earnings. Actual earnings were easily obtained, but investors' expectations of earnings are not available. This study employed two forms of surrogates for earnings expectations--Valueline analysts' forecasts and a naive random-walk model where last year's EPS is the expectation of this year's EPS. The analysts' forecasts were used because they represent the expectations of expert humans, rather than the expectations of mathematical models. As discussed earlier, analysts can use the predictions of time-series models in conjunction with other, more recent information. The research reviewed in Appendix C suggests that analysts are more accurate than mathematical models. The analysts' forecasts are also publicly available. For these reasons, analysts' forecasts appear to be a reasonable surrogate for market expectations. On the other hand, the random-walk model was used because it might reflect the expectations of naive investors who do not consult sources of information outside the annual report. Possibly more appropriate surrogates for market expectations may be found. Again, however, the validity of these surrogates (as opposed to other measures) is a question for future research.

The fifth limitation arises from the decision to use nonparametric statistical tests. Since the volume data were not normally distributed, and since Boatsman (1982) suggests that the results of capital markets research may often be driven by outlier observations, it was decided that these advantages outweighed the limitations of the nonparametric

approach. The major statistical limitation affecting this study was the lack of a nonparametric procedure which would simultaneously test for first and second-order effects. First-order effects were tested by the correlation procedures and tests for difference in location (e.g., the Kruskal-Wallis procedure suggested that trading around the earnings release date was relatively greater for the non-NYSE than for the NYSE firms, while the Spearman procedure indicated that earnings releases generated proportionately greater trading for the smaller firms than for the larger firms).

Testing for second-order effects was less straightforward. The firms were categorized according to potentially confounding factors such as fiscal year-end date and recency of the analyst's forecasts. For each of the hypothesis tests, the statistical procedures were repeated on each of the subsamples, and the results of the tests on each of the subsamples were compared in order to determine whether these factors affected the significance or direction of the research results. Unfortunately, a single test was not available to assess the extent of these interactions. Further, this method was unable to distinguish between the first and second-order effects of these potentially confounding factors. For example, the inverse relationship between firm size and trading volume was stronger for non-12/31 firms than for calendar year firms, but the method used in this study could not distinguish between 1) the trading associated with the fiscal year-end date, and 2) the trading associated with the fiscal year-end-firm size interaction.

These statistical problems may limit the inferences that can be drawn from the secondary analysis. The approach used in this study is justified on two grounds. First, the skewness of the data necessitated the use of nonparametric procedures for testing the hypotheses. Second, the effect of these confounding factors (e.g., stock exchange, fiscal year-end date) has not been examined in previously published research. Although this study cannot assess the magnitude of these effects, it does provide evidence that these factors may affect capital markets research results.

In conclusion, the first limitation, the definition of information content, does not affect the results of the statistical tests, but may affect the conclusions and implications of the results. For example, since data may have information content without inducing trading, this study may understate the information content in annual earnings releases. The fact that the sample was randomly chosen from the population of firms listed in Valueline limits the generality of the results. The third and fourth limitations, the inability to perfectly operationalize the concepts of "unexpected earnings" and "abnormally high trading", reduce the amount of noise which can be removed from the data, and hence reduce the significance and magnitude of the results.

The lack of an appropriate nonparametric statistical procedure for simultaneously measuring the first and second-order effects of the potentially confounding factors limits the ability to assess the magnitude of these effects. This study does, however, provide an initial step by presenting evidence that these factors do affect the relations between trading volume, firm size, and unexpected earnings.

The limitations discussed in this section generally arose from the difficulty of reducing abstract concepts to concrete and measurable phenomena. It is believed that the study has captured the essence of the market reaction, and that even taken together, the limitations do not threaten the main conclusions of the study.

Suggestions for Future Research

Since Jennings, Starks, and Fellingham's (1981) sequential information arrival model suggests that the results of price and volume research will sometimes conflict, one of the first priorities for future volume research should be to investigate phenomena where it is likely that the results of price and volume research will differ. In this study, the primary analyses of the major hypotheses showed that the results of the trading volume metric were similar to those of the results of price research. The secondary analysis, however, suggests that a sizable number of investors may trade on the basis of outdated and naive expectations. Further, this naive-investor phenomenon seems to be more pronounced for certain types of firms--non NYSE firms, non-calendar year firms, and firms releasing unfavorable earnings reports. Because the trading activity of these less-informed investors cannot be offset by the trading of more sophisticated investors (while naive security return expectations can be offset by more sophisticated expectations) it is possible that the trading volume approach may be more sensitive (than the price approach) to the behavior of these less informed investors. Therefore, a replication of the secondary analysis, employing price statistics instead of volume statistics might answer the

empirical question of whether these insights are a unique contribution of volume research.

Length of reaction time poses another area where the results of price and volume research are likely to conflict. Morse (1981) provides some evidence that while the price adjustment (to information releases) is fairly rapid, abnormally high trading seems to continue for a longer period of time. He suggests that this finding reflects the time needed for the less-informed, price-taking investors to obtain and process the information. This conjecture is certainly consistent with this study's suggestion that a sizable number of these less informed investors exist. The conclusions drawn from this study also suggest that the volume reaction might be more prolonged for the categories of firms which seem to have a disproportionately high number of less-informed investors. A comparative investigation of the length of price and volume reaction times across categories of firms, possibly using Defeo's (1981) sophisticated statistical techniques for isolating the reaction period, might yield useful insights into the behavior of the market, as well as potential differences between price research and volume research.

Another project would also involve studying both price and volume behavior around earnings release dates. An investigation could be designed to answer the empirical question posed in Chapter II's three scenarios: how often does the release of information cause (1) a volume reaction, but no price reaction, (2) a price reaction, but no volume reaction, and (3) both price and volume reactions? Such a study could also empirically test the correlations between price and volume reactions. If the number of optimists in the economy could be

approximated by partitioning the sample according to the favorability of the news, such a study could also provide empirical evidence on the validity of Jennings, Starks and Fellingham's (1981) curve depicted in Figure 5 of Chapter II. (This curve suggests that the direction of the correlation between price and volume reactions is an empirical question, which depends upon the number of optimists in the economy.)

This study could also be extended to include interim or quarterly reports. In addition to replicating the analysis performed in this study, an investigation of interim releases could also test whether the trading volume reaction is greater for annual releases than for interim releases. Verrecchia and Holthausen (1982) suggest that annual releases may be more precise than interim releases, because the latter are unaudited and contain more estimates. They also demonstrate analytically that precision and information content are positively associated. If annual releases are more precise than interim numbers, and if precision is positively correlated with the amount of information content, then there should be more trading around the release of annual earnings than interim earnings.

On the methodological front, research is urgently needed to assess the validity of the adjustments for market-wide and firm-specific average levels of trading. The results of this study question the techniques currently in use.

Finally, empirical volume research could be significantly enhanced by the integration and further development of the sequential information arrival and social welfare models into a single, well-accepted theory of trading in the financial markets. Development of such a theory would

not only enhance the status of empirical volume research, but it would also provide additional guidance as to the kinds of questions the empiricists should investigate.

FOOTNOTES

¹Since the sample was randomly chosen from the firms listed in Valueline, there is no evidence on the number of potential sample firms eliminated by this criterion.

²Smaller, more closely held, less widely traded firms are less likely candidates for news releases, Wall Street Journal articles, Valueline analyses, etc. For this reason, there may be few sources of information other than earnings reports available about these firms.

³That is, the market is efficient with respect to a piece of information if the market prices would not change if all investors knew the information.

⁴GAAP stands for the term, "generally accepted accounting principles."

APPENDIX A

A Brief Review of the Sharpe-Lintner-Mossin
Capital Asset Pricing Model and
Sharpe Market-Model

The main idea behind the CAPM is that the rate of return on a security is composed of the riskless rate of return plus a risk premium. The amount of the risk premium depends upon the riskiness of the security. If the following assumptions are met (Beaver, 1972):

- 1) Individuals adhere to Markowitz portfolio theory; that is, they are risk-averse, mean-variance investors interested in maximizing expected utility of their terminal wealth.
- 2) Investors can borrow and lend unlimited amounts at the risk-free rate of interest.
- 3) The capital markets are perfect; there are no taxes, transactions or information costs, and all investors are pricetakers.
- 4) All investors have homogenous time horizons and also hold homogenous beliefs about the expected returns, variance of returns, and covariance among security returns.

Then the equilibrium market prices exhibit the following relationship:

$$E(R_{jt}) = R_F + \frac{\text{COV}(R_{jt}, R_{mt})}{\sigma(R_{mt})} \left[\frac{E(R_{jt}) - R_F}{\sigma(R_{mt})} \right]$$

riskless rate risk premium

where

R_F = the riskless rate of interest

R_{jt} = the return on security j during period t

R_{mt} = the return on the market portfolio during period t

$\frac{\text{cov}(R_{jt}, R_{mt})}{\sigma(R_{mt})}$ = contribution of security j to portfolio risk

$\frac{E(R_{jt} - R_F)}{\sigma(R_{mt})}$ = market price of risk

Unfortunately, the CAPM per se is not empirically testable, since ex ante expected returns are not observable. If the following assumptions are made, Sharpe's (1964) empirically testable ex post analogue, the market-model can be derived from the CAPM (Beaver, 1972):

- 1) $\sigma^2(R'_{mt})$, the variance of the market portfolio proxy is essentially equal to $\sigma^2(R_{mt})$, the variance of the return on the market portfolio.
- 2) Every security is a small fraction of the market portfolio.
- 3) The variance of the error term $\sigma^2(\epsilon_{jt})$ is not much larger than the variance of the market portfolio $\sigma^2(R_{mt})$.

The essence of the market-model follows:

$$R_{jt} = \alpha_j + B_j R'_{mt} + \epsilon_{jt}$$

where R_{jt} = return of security j during period t

R'_{mt} = return of the market portfolio proxy (index) during period t

α_j, B_j = regression coefficients

ϵ_{jt} = error term for firm j during period t

and the following assumptions are met:

$$E(\epsilon_{jt}) = 0$$

$$\sigma(\epsilon_{jt}, \epsilon_{jk}) = 0$$

$$\sigma(\epsilon_{jt}, \epsilon_{kt}) = 0$$

$$\sigma(R_{mt}, \epsilon_{jt}) = 0$$

$$\sigma^2(\epsilon_{jt}) = K$$

α_j, b_j are stable coefficients

Of course, the empirical validity of the market-model does not depend upon the ultimate fate of the CAPM. Researchers may choose to view the market model as a simple regression problem, and evaluate its usefulness according to empirical criteria (i.e., how much of the dependent variables' variation does it explain?).

APPENDIX B

Sample Size Selection

The approximate sample size was chosen after consulting David's (1938) tables of correlation coefficient distribution. These tables specify the distribution of the coefficient of correlation, r (see Neter and Wasserman, 1974, p. 90-92 for a discussion of the properties of r), under different assumptions concerning the alpha level, sample size, and underlying population correlation coefficient.

Unfortunately, analogous tables for Spearman's rank correlation coefficient and Kendall's tau (Hollander and Wolfe, 1973), the nonparametric counterparts to the normal-theory coefficient of correlation, are unavailable. Since the latter nonparametric statistical tests were employed in this study, it is recognized that the use of tables for the normal-theory counterpart may be somewhat suspect. The use of this procedure, however, may be defended on two grounds. First, the tables provide an ex ante indication of the sample size needed (at varying confidence levels) to reject the null hypothesis of no correlation when in fact the actual population correlation is at a given level. In evaluating research results, on the other hand, it is the ex post confidence level, a result of the actual sample size, sample results, and statistical tests employed, which is the relevant measure. These tables, therefore, were used only to suggest an appropriate sample size; they bear no other relation to the evaluation of the sample observations. The second justification for using these tables as a guide is that currently, the only alternative is to randomly select a sample size, and hope that it is large enough to detect the underlying population relationships.

An excerpt from the Tables appears in Table 17. The first line of the table is interpreted as follows: if the true underlying population correlation coefficient, ρ , equals .1, and the sample size is 400, and the researcher is willing to accept an alpha level of .11559, then the probability of incorrectly accepting the null hypothesis (that the population correlation coefficient is zero) is .21037. On the other hand, the probability of correctly rejecting the null hypothesis is $1-.21037$ or .78963. Now observe the second line of the table. At an alpha risk of .05507 one has only a $1-.34290$ or .65710 of correctly rejecting the null hypothesis if the true underlying population coefficient equals .1. In order to reduce the alpha risk (risk of incorrectly rejecting the null hypothesis) the researcher must pay the price of reduced power: the probability of detecting the population relationship has dropped from .78963 to .65710. Of course, as the true population correlation coefficient increases, the power of the test increases. If, for example, increases from .1 to .2, and the sample size and alpha risks are held to their previous levels, the probability of correctly rejecting the null hypothesis increases from .65710 to .99280 ($1-.0072$).

Reducing the sample size will reduce the power of the tests. If the sample size is reduced to 200 observations, while the alpha level is maintained at .04527, then there is only a .38937 ($1-.61063$) chance of correctly rejecting the null hypothesis if ρ equals .1. By halving the sample size, the probability of correctly rejecting the null hypothesis has declined from .65710 to .38937.

TABLE 17

PROBABILITY OF ACCEPTING THE NULL HYPOTHESIS THAT THE POPULATION COEFFICIENT OF CORRELATION EQUALS ZERO, GIVEN THE TRUE COEFFICIENT OF CORRELATION, ρ , THE ALPHA LEVEL, AND THE SAMPLE SIZE

Sample Size	Alpha Risk	True Underlying Population Coefficients			
		$\rho = .10$ $\rho^2 = .01$	$\rho = .20$ $\rho^2 = .04$	$\rho = .30$ $\rho^2 = .09$	$\rho = .40$ $\rho^2 = .16$
400	.11559	.21037	.00222	.00000	.00000
400	.05507	.34290	.00720	.00000	.00000
200	.07944	.49858	.07424	.00164	.00000
200	.04527	.61063	.12282	.00393	.00001

SOURCE: F.N. David, Tables of the Ordinate and Probability Integral of the Distribution of the Correlation Coefficient in Small Samples (Biometrika, London, 1938) p. 52-54.

An approximate sample size of 400 was chosen, because at lower sample sizes, with an alpha level of about 5 percent, the risk of not being able to identify small population correlation coefficients was unacceptably high; i.e., the power of the test was too low.

APPENDIX C

List of Sample Firms

FIRMS IN FINAL SAMPLE

Alleghany Corporation
Allen Group
Amalgamated Sugar Company
Amax, Inc.
Amerada Hess Corporation
American Airlines
American Bakeries
American Can
American Century Trust
American District Telegraph
American General Corporation
American Home Products
American Hospital Supply
American Maize "A"
American Medical International
American Standard Incorporated
American Water Works
Ampco Pittsburgh Corporation
Amp Incorporated
Amrep Corporation
Anchor Hocking Corporation
Anglo American South Africa
Anheuser Busch
Arden Groups, Inc.
Armada Corp.
Armco Inc.
Arvin Industries Inc.
Avery International Corp.
Avnet Inc.
Bally Manufacturing
Bangor Punta Corp.
Bankamerica Corp.
Bankers Trust NY Corp.

Bank of New York
Barnes Group
Barry Wright Corp.
Bausch & Lomb
Beatrice Foods
Belding Hemingway
Bethlehem Steel
BIC Pen Corp.
Boston Edison Co.
Bristol Myers Co.
Brown & Sharpe Manufacturing
Brown-Forman "B"
Brunswick Corp.
Bucyrus Erie
Burndy Corp.
Butler International
CBI Industries
CNA Financial
Capital Cities Communication
Capital Holding Corp.
Carolina Freight Carriers
Carter Hawley Hale
Ceco Corp.
Champion Home Builders
Champion Spark Plug
Chicago Milwaukee Corp.
Chromalloy American Corp.
Cincinnati Gas & Electric
Citicorp
Citizens & Southern Georgia
Cleveland Cliffs Iron Co.
Coachmen Industries Inc.
Cobe Labs Inc.
Coleman, Inc.
Colgate Palmolive

Collins & Aikman
Collins Foods International
Cominco Ltd.
Commonwealth Oil Refining
Consolidated Natural Gas
Consolidated Papers
Consumers Power
Continental Telephone
Control Data Corp.
Conwood Corp.
Coors, Adolph Co.
Cordura Corp.
Cox Broadcasting Corp.
Cray Research
Culbro Corp.
Cummins Engine
Curtiss Wright
Datapoint Corp.
Data Products Corp.
De Beers Consolidated Mines
Deere & Co.
DeKalb Agresearch Inc.
Deluxe Check Printers
Dentsply International Inc.
Dial Corp.
Diversified Industries
Doctor Pepper
Donaldson, Lufkin, Jennrette
Dow Chemical
Dresser Industries
Dreyfus Corp.
Dun & Bradstreet
EG & G Corp.
Eastman - Kodak
Edwards, A.G. & Sons

Electronic Data Systems Corp.
Electric Memories Magnetics
Emhart Corp.
Equimark Corp.
Esmark, Inc.
Essex Chemical Corp.
Exxon
Federated Department Stores
Ferro Corp.
Fidelcor Inc.
Figgie International
Filmways, Inc.
Firestone Tire & Rubber
First Chicago Corp.
First Wisconsin Corp.
Flight Safety International
Florida East Coast
Florida Power & Light
Florida Steel Corp.
Foote, Cone, Belding Communications
Foster Wheeler Corp.
Foxboro Co.
Fruehauf Corp.
Fuji Photo Film
Gearhart Industries
General American Investment
General Foods Corp.
General Motors Corp.
General Telephone & Electric
Genuine Parts Co.
Giant Portland & Masonry
Gifford Hill & Co.
Gleason Works
Goodrich, B.F.
Goodyear Tire & Rubber

Gould, Inc.
Grainger Worldwide
Graniteville Co.
Gulf Canada Limited
Hackensack Water Co.
Harland, John H.
Hecla Mining
Heller, Walter, International
Hercules, Inc.
Hershey Foods Corp.
Hesston Corp.
Homestakes Mining
Honda Motor Ltd.
Hoover Universal Inc.
Horizon Corp.
Hospital Corporation of America
Houghton Mifflin
Houston Industries
Houston Natural Gas
Huffy Corp.
Hunt, Phillip Chemical
IC Industries
ICN Pharmaceuticals
IU International Corp.
Illinois Power Co.
Indiana Gas
Indianapolis Power & Light
Inexco Oil
International Banknote
International Business Machines
International Flavors & Fragrances
International Harvester
International Paper
Iowa-Illinois Gas & Electric
Irving Bank Corp.

JWT Group, Inc.
Jamesway Corp.
Jerrico, Inc.
Jonathan Logan
Justin Industries
KLM Royal Dutch Airlines
Kaiser Cement
Kansas City Power & Light
Kansas City Southern
Katy Industries
Kellwood Co.
Kennametal Inc.
Kentucky Utilities
Kerr Glass Manufacturing
Kollmorgen
Koppers Inc.
Kubota Ltd.
Kysor Industrial Corp.
LLC Corp.
Lancaster Colony
La Quinta Motor Inns
Libbey, Owens, Ford
Lilly, Eli & Co.
Litton Industries
Lomas & Nettleton Financial
Lomas & Nettleton Mortgage
Louisiana Pacific Corp.
Lowenstein, M. Corp.
Lowe's Co.
Lucky Stores
Macy R.H.
Matsushita Electric
McCormick & Co.
McDermott
McDonnell Douglas

McLouth Steel Corp.
Mead Corp.
Medtronic Inc.
Mercantile Texas
Merrill Lynch & Co.
Metromedia Inc.
Michigan Sugar
Mid Continent Telephone
Miller-Wohl
Milton Ray
Mobil Corp.
Mohasco Corp.
Mohawk Data Sciences
Monarch Capital
Monogram Industries
Montana Dakota Utilities
Montgomery St. Inc.
Moore Corp. Ltd.
Moore McCormack
Morrison Knudsen Inc.
Morse Shoe Inc.
Murphy, G.C.
Murphy Oil Corp.
Murray Ohio Manufacturing
NCH Corp.
NL Industries
Nalco Chemical Co.
National Distillers
National Fuel Gas
National Presto Industries
National Utilities & Industries
Natomas Co.
New England Electric System
Nordstrom Inc.
Northeast Utilities

Northern Indiana Public Service
Northwest Airlines
Northwest Bancorporation
Northwest Mutual Life
Northwest Natural Gas
Nucor
Oakite Products
Offshore Logistics
Ogden Corp.
Ogilvy & Mather International
Oklahoma Gas & Electric
Orange Co.
Otter Tail Power
Outboard Marine Corp.
Outlet Co.
Owens-Corning Fiberglass
Ozark Air Lines
PHH Group
PPG Industries
Paine Webber, Inc.
Parker Pen Co.
Patrick Petroleum Co.
Payless Cashways
Penn Central
JC Penney
Pennwalt Corp.
Pennzoil
Perkin, Elmer Corp.
Phelps-Dodge
Philadelphia Electric
Piedmont Natural Gas
Pillsbury Co.
Pittston Co.
Polaroid Corp.
Portland General Electric

Prime Computer Corp.
Procter & Gamble
Publicker Industries, Ind.
Public Service Co.-Colorado
Public Service Co.-New Mexico
Pueblo, Inc.
Puget Sound Power & Light
Questor Corp.
RTE Corp.
Ranco Inc.
Reading & Bates
Recognition Equipment
Reece Corp.
Reichhold Chemicals
Republic Corp.
Research Cottrell Inc.
Rexham Corp.
Reynolds Metals
Robertson, H.H.
Rochester Gas & Electric
Ronson Corp.
Rouse Co.
Royal Crown
SFN Companies
SPS Technologies
Safeguard Scientifics
St. Regis Paper
San Diego Gas & Electric
Savin Corp.
Scherer, R.P. Corp.
Scientific Atlanta
Scot Lad Foods
Scotty's Inc.
Searle, G.D. & Co.
Shapell Industries

Shared Medical Systems
Shoney's
Sierra Pacific Power
Simplicity Patterns
Smith, A.O.
South Carolina Electric & Gas
Southdown, Inc.
Southern Indiana Gas & Electric
Southern New England Telephone
Southern Railway
Southland Corp.
Sparton Corp.
Spectra Physics
Sperry Corp.
Square D Co.
Standard Oil of Indiana
Standex International
Stauffer Chemical
Sterchi Brothers Stores
Stop & Shop
Sun Banks Florida
Sun Chemical
Sun Inc.
Sundstrand
Sunshine Mining
Supermarkets General
Superscope
Sybron Corp.
Syntex Corp.
Texas Air
Texas Commerce Bankshares
Texas Eastern Corp.
Texas Gas International
Texas International
Texas Oil & Gas

Texfi Industries
Textron Inc.
Thrifty Corp.
Tiger International
Times Mirror
Tokheim Corp.
Transamerica Corp.
Transohio Financial
Trans World Corp.
Travellers Corp.
Trinity Industries
Tubos de Acero Mexico
Tyler Corp.
UAL Inc.
UGI Corp.
Union Carbide
Union Oil - California
United Industrial Corp.
United Inns
United Jersey Banks
United States Gypsum
US Home Corp.
US Industries
United Telecommunications
United Virginia Bankshares
Unitrode Corp.
Universal Leaf Tobacco
Vernitron Corp.
Victoria Station
Virginia National Bankshares
Wackenhut Corp.
Wang Labs B
Warner Communications
Washington Water Power
Wayne-Gossard Corp.

Webb, Del E.
Western Air Lines
Westmoreland Coal
Wetterau
Wickes Companies
Willamette Industries
Williams Companies
Woods Petroleum
Wrigley
Wyle Labs
Wyman Gordon
Zurn Industries

FIRMS DELETED FROM THE FINAL SAMPLE

Not on Compuserve's Value Database:

Idaho Power
Cross A.T. "A"
National Homes

Insufficient trading volume data on Compuserve's Value data base:

Atlas Van Lines
Bairnco Corp.
Cigna Corp.
CSX Corp.
Cannon Inc.
Geosource
Heizer Corp.
Lanier Business Products
Liberty National Insurance Holding
Metro-Goldwyn-Mayer
Mitel
Richmond Tank Car
Safeguard Business Systems
Toys R Us

Changed fiscal year-end date

Marriott Corp.
Scudders-Duo-Vest

No earnings release dates in Wall Street Journal Index:

American General Bond Fund
Arkla Gas
Atlantic Met Corp.
Criton Corp.

General American Investment
Kidde, Inc.
Petroleum and Research Corp.
Royal Dutch Petroleum
Western Deep Level
WICOR

Confounding events reported in all three sample periods:

US Steel

APPENDIX D

Research Investigating Analysts' Forecasts
Versus Mathematical Time-Series Model
Forecasts of Earnings Per Share

This Appendix provides a brief review of the recent literature investigating the relative accuracy of analyst forecasts of EPS versus mathematical time-series model predictions of EPS. The major thrust of the research results is intuitively appealing: analysts outperform mathematical models, as one would expect, since analysts have access not only to the models' outputs or predictions, but also to more recent data and information otherwise not amenable to time-series models (e.g., pending strikes or litigation).

In one of the initial efforts in this area, Brown and Rozeff (1978) studied 50 NYSE firms included in Moody's Handbook of Common Stocks and Valueline Investment Survey. They found that the Valueline forecasts were superior to seasonal martingale, seasonal submartingale, and Box-Jenkins time series earnings forecasts. In order to test the generalizability of their results, the authors also analyzed the Standard and Poor's (S & P) forecasts in the Earnings Forecaster (published by S & P); they found that the S & P forecasters outperformed the time-series models by a similar margin. In the statistical analysis, the authors chose the nonparametric Friedman rank sum and Wilcoxon signed rank tests to analyze their primary error metric: mean absolute percentage error:

$$\left| \frac{\text{EPS}_{\text{Forecast}} - \text{EPS}_{\text{Actual}}}{\text{EPS}_{\text{Actual}}} \right|.$$

To test the sensitivity of the results to the choice of error metric, the authors also analyzed mean square error, root mean square error, and relative error squared. The nonparametric tests proved insensitive to

the choice of error metric, but the parametric t tests were very sensitive (in both sign and magnitude) to the error metric employed. This result supports Boatsman's (1982) criticism that outlier observations have driven many market research results.

Crichfield, Dyckman, and Lakonishok (1978) used nine and one-half years of forecasts for 46 firms listed in the Earnings Forecaster (EF). The authors selected the mean forecast in the EF as the point estimate of the market's expectations, and they chose the mean square prediction error as their error metric. They report that the mean forecast in the EF outperformed five different naive time-series models, including variations of random walk, moving average, and seasonal models. A further investigation yielded no evidence of a positive, or overly optimistic prediction error.

Brown and Rozeff (1979) analyzed five years of data for 50 Valueline firms. The authors found that while the Valueline analysts' forecasts were consistent with adaptive expectations models (derived from the Box-Jenkins (1970) methodology) the explanatory power of the time-series models was generally less than 50 percent. That is, more than 50 percent of the variation in the analysts forecasts was not explained by the earnings time-series model. This result suggests that in revising their estimates, analysts use outside information which is not reflected in the earnings series.

Collins and Hopwood (1980) used 20 years of analysts' forecasts from Valueline to investigate the relative accuracy of analysts' versus time-series EPS forecasts for 50 firms. They found that the Valueline analysts outperformed four different mathematical models derived from

the Box-Jenkins (1970) approach to developing time-series models , the analysts generated the lowest mean absolute percentage errors, the lowest standard deviations, and the smallest outliers.

Fried and Givoly (1981) found that financial analysts outperformed two mathematical models: a naive martingale model and a "market-model" type regression model. They analyzed forecasts for 424 firms appearing in the Earnings Forecaster from 1969 to 1979. The authors found a positive partial correlation between actual earnings and analysts' forecasts, given the mathematical models' predictions. This result corroborates Brown and Rozeff (op.cit.) and suggests that analysts use some information not contained in the time-series earnings models. In an attempt to investigate the effects of timing of the information, Fried and Givoly suggested that because of the data gathering time and the publication lag, the Standard and Poor's forecasts were about two to three weeks more current than the other forecasts published in the EF (S & P publishes the EF). They found a stronger correlation between the S & P forecasts and the cumulative abnormal returns (CAR), than between the average financial analysts' forecasts in the EF and CAR. In summary, the authors concluded that analysts' forecasts "are a better surrogate for market expectation of earnings than time-series models customarily used in the literature," (p. 27). Use of these time-series models as proxies for the market expectations may reduce the power of the tests and understate the earnings number's value to investors.

Fried and Givoly also found a tendency of all models, both mathematical forecasts and financial analysts' forecasts, to overestimate the next year's earnings. For financial analysts, a

significant bias occurred in only three of the eleven years. The authors used the decomposition and linear correction suggested by Mincer and Farnowitz (see Mincer (1969) and Theil (1966)). The resulting corrections proved minimal, and analysis of the corrected forecasts yielded results similar to those obtained from the unadjusted forecasts.

In conclusion, the results of these studies suggest that in predicting EPS, financial analysts outperform several different mathematical models. This relationship seems to hold over different sample firms, different sources of analysts forecasts, and different time periods. Therefore, analyst forecasts were used as the measure of market expectations in this study.

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