THE INFORMATION CONTENT OF ACCOUNTING CHANGES

DISSERTATION FOR THE DEGREE OF Ph.D.

MICHIGAN STATE UNIVERSITY

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1976

This is to certify that the

thesis entitled

THE INFORMATION CONTENT
OF ACCOUNTING CHANGES

presented by

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has been accepted towards fulfillment
of the requirements for

Ph.D. degree in Accounting

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Date February 26, 1976

O-7639

ABSTRACT

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By

Walter Thomas Harrison, Jr.

This research is an attempt to answer some questions that previous inquiries into the stock market effects of accounting changes (ACs) have left unanswered, and largely unaddressed. One is the general question of whether the market reacts systematically to ACs. The failure of most earlier studies to report the results of a control group of nonchange firms casts some doubt on the ability of their research designs to answer this question. Also, no previous study has examined the market effect of nondiscretionary ACs. All previous AC studies have been based on the assumption that security return distributions are univariate in nature. This assumption precluded the detection of any dependency between the information effect of ACs and a particular risk class of firms. And finally, no previous study has examined both means and variances of security return distributions in assessing the informational effects of ACs.

In order to correct for the abovementioned omissions, this study compares rates of return on a large

sample of firms that made ACs during the years 1968-1972 to a control group of nonchange firms that were individually matched on fiscal year-end, relative risk, and less stringently, on industry membership. It tests for the effects of ACs that have four possible directional effects on net income: (1) positive; (2) negative; (3) zero or immaterial; or (4) directional effect not disclosed. It also tests for the effects of ACs that were made with three degrees of management discretion: (1) discretionary --made at the discretion of management; (2) nondiscretionary--made as the result of a pronouncement by an exogenous body such as the Accounting Principles Board, or (3) both--the firm made atleast one discretionary AC and at least one nondiscretionary AC during the same year. Then tests are performed on each of the four directional effect categories in combination with each of the three degrees of relative discretion (e.g., discretionary ACs with positive effects on net income, etc.). Return distributions are assumed to be multivariate in nature in order to give effect to the possibility of detecting any existing dependency between AC information and a particular risk class. And finally, tests are conducted on both mean vectors and covariance matrices of returns. The study assumes that capital markets are efficient with respect to publicly available information and that the capital asset pricing model reflects the market's mechanism for establishing equilibrium values for firms. The test period extends from six months before the month when year-end AC disclosures are made to six months after, a thirteen-month period.

The omnibus test for the information effects of ACs (of all types) in general produced results that are consistent with the null hypothesis of no information content. The four tests for the effects of ACs with the four directional effects all produced results leading to the same inference. Also, the three tests for the effects of ACs with the three relative discretion characteristics, in general, implied no information content in ACs. However, all eight of the tests just mentioned were shown to bring together ACs with widely different characteristics such that, in some cases, the effects of ACs with more narrowly defined (and competing) characteristics canceled each other. This was evident from the results of tests on ACs with a specific directional effect on net income <u>and</u> a specific relative discretion characteristic. A number of these tests produced results that led to rejection of the null hypothesis and hence to the inference that ACs are strongly associated with changes in firms' equilibrium values. On this basis, it appears that these ACs have information content.

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Walter Thomas Harrison, Jr.

A DISSERTATION

Submitted to
Michigan State University
in partial fulfillment of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

Department of Accounting and Financial Administration

1976

D E D I C A T I O N

This dissertation is dedicated to my beloved wife Nancy. Her mental, spiritual, and physical encouragement was an indispensable element that contributed mightily to the completion of this study.

ACKNOWLEDGMENTS

I wish to express my gratitude to my dissertation committee of Professor Daniel W. Collins, Professor James B. Greene, and Professor Melvin C. O'Connor (Chairman). At virtually every stage of the development of this project they were available on an almost unlimited basis for suggestions as to topical areas for the research, advice as to writing style, and encouragement toward its completion.

The research was partially financed by grants from the Haskins & Sells Foundation and the Department of Accounting and Financial Administration of the Graduate School of Business Administration at Michigan State University. In addition, valuable data were supplied by the American Institute of Certified Public Accountants and by Merrill Lynch, Pierce, Fenner & Smith. To these organizations I extend my hearty thanks.

Walter Thomas Harrison, Jr.

February 27, 1976

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A B B R E V I A T I O N S

AC	Accounting change
ACF	Accounting change firm
API	Abnormal performance index
APB	Accounting Principles Board
<u>ATT</u>	<u>Accounting Trends and Techniques</u>
CAPM	Capital asset pricing model
CRSP	Center for Research in Security Prices
Eq.	Equation
FASB	Financial Accounting Standards Board
FYE	Fiscal year-end
ITC	Investment tax credit
NC	Nonchange
NCF	Nonchange firm
NYSE	New York Stock Exchange
SIC	Standard Industrial Classification

CHAPTER 1

INTRODUCTION

In recent years several researchers have conducted studies on various aspects of accounting changes (ACs). Some of the studies have focused on the smoothing of income.[1] One reported the degree to which firms have complied with specific AC disclosure requirements.[2] Others have considered the behavioral characteristics of firms which made ACs.[3] Several studies have focused on consistency exceptions arising from ACs.[4] The intent of another group of AC studies has been to determine the effect, if any, of ACs on the rates of return[5] on common stocks.[6]

The present study falls within the last category; the purpose is to assess whether ACs and disclosures of ACs have an effect on stock returns. Justification for additional AC-stock return research lies in (1) the many questions left unanswered (and largely unaddressed) by previous AC research and (2) the substantial refinements in research design applied in the present study as compared to earlier ones. The first chapter is a discussion of these questions and refinements.

The extent to which this study addresses questions ignored by other researchers does not imply a weakness in their studies. Rather, analysis of their designs and results has led this researcher to ask additional questions which were beyond the scope of these earlier studies. Hopefully this research will provide the impetus for additional research into the effect of ACs on stock returns.

Lev states:

> Empirical evidence generally indicates that accounting changes had no significant effect on stock prices, implying that investors are able to recognize economic reality despite the different reporting modes. However, a qualification is warranted here; in a few cases, accounting changes seemed to have some effect on stock prices.[7] Whether such effects are systematic and of importance is yet to be determined.[8]

After reviewing essentially the same AC studies referred to in footnote six, Gonedes and Dopuch state:

> Summing up, the above studies' results are consistent with the statement that the capital market does distinguish between changes that appear to be reporting changes of no economic importance and those that appear to have substantive economic implications. This inference must, however, be qualified because not all aspects of the conditional distribution, $F(e_{it} | \theta_{it})$, have been examined. In addition, the studies suggest that some types of accounting changes (e.g., those examined by Sunder and Ball) are associated with events affecting firms' relative risks. The nature of this association has not been thoroughly investigated.[9]

Lev raises three issues: (1) whether the effects of ACs on stock prices are systematic; (2) whether the effects of ACs on stock prices are of economic importance; and (3) whether the market intelligently prices stocks of accounting-change firms (ACFs). Gonedes and Dopuch raise two additional issues: (1) the need to base inferences on an examination of more aspects of the distribution of stock returns of ACFs than just the first moment, the mean; and (2) the nature of the association between ACs and firms' relative risks. Each of the five issues deserves consideration in order to explain the scope, the limitations, and the inferences to be drawn from this study.

The design tests directly whether there is a contemporaneous association between ACs and unusual stock return movement. It was constructed to substantially reduce the effects of confounding variables and to test the market effects of ACs which had specific directional effects on primary earnings per share (EPS) and/or which were discretionary or nondiscretionary. Such refinements concerning the effects of ACs have been largely overlooked in previous studies.

The economic importance of the effect of ACs on stock prices is complicated by the fact that there are a number of very different potential economic factors which may be associated (either causally or effectually) with an AC. For example, an AC may result from a firm's shift into a high technology field where accelerated depreciation is more appropriate than previously-used straight line. The market's perception of that firm's inability to compete in the new field may cause the firm's stock price to drop contemporaneously with disclosure of the AC. In this hypothetical case the economic effects are very real, both internally--from the possibility that the shift may significantly impair the firm's competitive position, and externally--from the effect this negative possibility may have on the firm's ability to obtain financing. In this case the AC is associated with a market effect and disclosure of the AC may be a significant part of the message to the marketplace that the firm has made a shift in its production-investment decision. Or alternatively, the AC may only be a by-product of the shift and merely be contemporaneously associated with the unusual stock price movement.

Another example of an economic effect of an AC is the change to LIFO inventory costing. In most cases this AC will actually cause a net cash flow increase because of the reduction in taxable income. This internal economic effect could be associated with at least two opposite external effects, a reduction in stock price due to lower reported profits or an increase in stock price due to the higher real profits from reinvestment of the cash saving.[10] Here the AC actually causes the increase in cash flow which may affect stock prices.

A third example of the economic importance of ACs is the firm which changes from the cost to the equity method of accounting for long-term investments in stock. Generally, if the investee earns more profit than it distributes in dividends, the investor increases reported profits as a result of the change. This AC, which appears to have no internal effect on the firm (in terms of cash flows or other economic betterment) and which appears to convey no economic message about the firm (e.g., in terms of its production-investment decisions), may nevertheless have an economic effect insofar as the increase in reported profits makes it possible for the firm to obtain financing. In this example any economic effect associated with the AC is not induced by a shift in production-investment decisions or altered cash flows, and yet there may be a very real effect on financing, which in turn may affect the firm's production-investment decisions and hence the return on the stock.

The preceding discussion should make it apparent that the effect of some ACs on security prices is complicated in some cases by the presence of exogenous non-accounting factors. Accordingly, one must be cautious in interpreting the results of market-based research in this area. Many other examples could also serve to illustrate the difficulty in making general statements about the economic implications of ACs.[11]

Lev's third issue is related to the second because an assessment of market reaction to an AC depends in part, at least, on the perceived economic substance of the AC. Therefore, the inherent difficulty in assessing the economic substance of an AC makes it equally difficult to make normative statements about how (or whether) the market should react in a given situation. Consequently, the present study deals only indirectly with the second and third of Lev's issues. This will become apparent in the interpretation of the empirical findings of the study.

The first issue raised by Gonedes and Dopuch, namely, the need to examine higher moments of the distribution of returns in order to draw inferences about the underlying events or information transmissions related to ACs, is incorporated into the design of this study in that tests are performed on variances as well as on means.

Except for Baskin's study previous AC research examined only distribution means. To ignore the higher moments is to say that they are not important to investors' decision processes, and this is simply not true. To illustrate, consider an investor with \$100. He may choose between two stocks A and B which both have expected returns of \$75. However, the return on A has a standard deviation of \$25 and the return on B has a standard deviation of \$100. The investor's decision is not obvious, but it is obvious that the risk-averse investor should consider the variability of returns because stock B offers the greater probability of losing the entire \$100. Skewness is not a consideration as long as return distributions are symmetrical, and monthly return distributions have generally been found to be symmetrical.[12]

The point made by this simple illustration is supported in the theoretical work of Tobin and Richter, among others. In Tobin's work he gives an example of a risk-averse investor's utility map that is composed of quadratic utility functions that are concave to the expected return axis (in a mean-variance space).[13] And Richter has shown that only the first two moments of the distribution of returns are needed by the risk-averse investor with utility functions of degree three or lower and who views a symmetrical (or two-parameter, e.g., the normal) distribution of returns.[14] Empirical support for the need to consider stock return variability is provided by the fact that investors seek information about Betas (βs).[15]

Gonedes and Dopuch's second issue, the association between ACs and firm riskiness, has at least two facets. One is the possibility that ACs actually affect firm risk. This is one implication of the work of Ball and of Sunder. If ACs do affect firm risk, the other AC studies' results may be suspect because of their failure to control for the effect of this phenomenon in their designs. All the other studies assumed constant risk, which became the independent variable in their assumed return-generating functions. To the extent that the constant risk assumption was untrue, the dependent variables (i.e., abnormal returns) in these studies were misspecified. There appear to be two ways to control for a changing risk. One is some kind of moving average risk estimation procedure, which takes into consideration shifts in firm riskiness, and the other is the use of a control group whose risk is the same as the risk of the groups of ACFs. In this study the latter method is employed.

Another facet of Gonedes and Dopuch's second issue is incorporated into the present design in that tests are performed on firms having different relative risks in order to determine whether there is a unique association between the effects of ACs and specific risk classes. It is possible that the effect of ACs on stock prices is risk-dependent in the sense that there is an AC effect for firms in specific risk classes. For example, the market may react more demonstrably to high-risk firms' ACs which could be construed to be motivated by the desire to manipulate net income than to similar ACs made by low-risk firms due to the inherently greater riskiness associated with high-risk firms. Or, following a different line of reasoning, if ACs are associated with changes in firms' risk levels, this change could affect the earnings rate used to discount firms' future cash flows to a present value of the firm. If this is the case, then, other things being equal, the effect of an AC on the market value of low-risk firms will be greater than on high-risk firms. This possibility will be explored more fully in Chapter 3. Earlier AC research was not designed to detect any such risk dependence. As a result, it is not known whether such an association exists.

Thus, in summary, the present study was conducted in an attempt to overcome some of the limitations in prior AC research and to implement some of the extensions suggested in the more recent literature. Specifically, the association between the information content of ACs and particular risk classes of firms is examined. And tests are conducted on variances as well as on means. Furthermore, the use of a control group, in addition to the above refinements, provides a framework within which to assess whether ACs have a systematic effect on stock returns.

There are many different ways one can approach the determination of whether ACs have a systematic effect on stock returns. One way is to test whether all types of ACs aggregated together have an effect. This is essentially what Ball and Baskin[16] tested. Another way is to test whether ACs involving specific accounts have an effect. Archibald tested depreciation, Kaplan and Roll tested depreciation and the investment tax credit, and Sunder tested inventory. A third way is to test whether ACs with predictable effects on net income systematically affect profits. With the exception of the Ball and Baskin studies, previous AC studies provided tests of whether ACs with specifically predictable effects on net income systematically affected stock returns. But none of them provided tests of the market effects of ACs with every different directional effect on net income. The present study does provide tests of whether ACs having all types of directional effects (positive, negative, zero, and directional effect not disclosed) affect stock returns.

A fourth way to approach the determination of whether ACs systematically affect stock returns is to test whether discretionary ACs and nondiscretionary ACs are viewed differently by the market. Archibald's, Kaplan and Roll's, and Sunder's studies all provide tests of discretionary changes involving specific general ledger accounts. Ball's study aggregates the effects of discretionary ACs and nondiscretionary ACs and thereby fails to test for the systematic effect which may be uniquely associated with each type of AC. The present study tests whether each of three levels of relative discretion in making ACs has a systematic effect. The three levels are discretionary, nondiscretionary, and both (for firms which in the same year made at least one discretionary AC and at least one nondiscretionary AC).

In addition, the four levels of directional effect (positive, negative, zero, and directional effect not disclosed) are multiplied by the three levels of discretion to include more refined tests of direction and discretion interactively, e.g., positive-discretionary,

negative-discretionary, . . ., directional effect not disclosed—both discretionary and nondiscretionary. In this way tests can be performed on ACs having virtually all directional effects on net income and/or virtually all levels of discretion.

There is one refinement in the present study, the use of a control group, that is found in Baskin's and in Kaplan and Roll's studies but not in the other AC studies. In the absence of a control group one wonders whether the observed behavior of the dependent variable is unique to the group being investigated or whether similar (nonunique) results would also have been obtained for a control group.

Another refinement lies in the assumption of a multivariate normal distribution of stock returns. The multivariate nature of the return distribution provides for the inclusion of covariances in overall return variability. This provision in turn allows a researcher to more convincingly infer statistically significant differences (should they result from the data) than is possible when a univariate distribution is assumed. All previous studies made the assumption of a univariate return distribution. The literature review chapter will discuss in more detail the use of a control group and the advantages of the assumption of a multivariate return distribution.

CHAPTER 2

THEORETICAL FOUNDATIONS AND EMPIRICAL SUPPORT FOR MODELS EMPLOYED

Chapter 2 is in five parts. The first four parts briefly summarize information content, efficient capital markets, capital asset pricing, and the distributional properties of security returns. Part five integrates all four notions as they apply to the present research.

Information Content

The purpose of this study is to determine whether ACs and their disclosures have an effect on stock prices. Stated differently, the purpose is to determine whether ACs have information content. The latter (and equivalent) statement of the purpose is useful for providing a framework within which to test the hypotheses addressed here.

An event y has information content if the conditional distribution $f(s|y)$ is not identical to the unconditional distribution $f(s)$ for some s, where s is a random variable. Obviously for an experiment to have substance, there must be some reason for believing that y and s are related in some way. If $f(s|y) = f(s)$, then y does not have information content because its presence is not associated with changes in s; in short, y makes no difference to agents whose activities impact on s.[1] This general notion of the concept of information is incorporated into the present study in the paragraphs that follow.

Inferences can be made about the information content of accounting events y by observing the behavior of some random variable s during a period of time when it is reasonable to believe the accounting event may affect the random variable. There is the need to identify three items: the event, the random variable, and the time period. The event y being considered here is an AC. The random (dependent) variable s to be examined is the stock return movement of ACFs. The association between the event and the random variable results from a prior belief that disclosures of accounting events are functionally related to stock return movements because accounting disclosures represent one element in the information set available to investors. Empirical research supports this belief.[2] Selection of the relevant time period could present difficult problems if there were no evidence to indicate when the effects of accounting events are impounded in stock returns. However, evidence concerning the speed with which the securities market impounds information provides relevant insights in this regard.

Efficient Capital Markets

In order to assess the information content of accounting events (and disclosures of accounting events) one must identify some period of time within which any potential information effects could be expected to be manifest. Evidence supporting market efficiency suggests that security prices adjust instantaneously to new information once it becomes publicly available. Accordingly, one potentially relevant time reference for studying the effects of ACs is the period immediately <u>following</u> the disclosure of the change. Fama provides a summary of the theory and evidence of the efficiency of capital markets.[3]

Based on the findings of previous research (see footnote 2) it appears that the information effects of some economic events that are reflected in accounting numbers (dividend announcements and preliminary reports of income numbers, for example) appear in stock prices several months <u>before</u> public disclosure. The same could also be true of ACs. Some firms announce their intentions to make ACs prior to release of the annual report. Other firms announce after the fact but before release of the annual report that they have made ACs.[4] A third group of ACFs wait until the annual report to disclose their ACs, but the information content of some of these ACs (such as depreciation changes due to a shift in the firm's production-investment activities or changes induced by Accounting Principles Board directives of which market agents were aware) may have been impounded in stock prices prior to release of the annual report. Thus, in addition to the period immediately following disclosure of the AC, the period immediately preceding the disclosure of the AC is also examined.

Sunder[5] points out that certain studies designed to measure the relationship between accounting information and market prices have stated that their evidence supports the efficiency hypothesis.[6] At the same time these studies relied upon capital market efficiency to measure the contemporaneous relationship between stock prices and accounting events. This type of circular reasoning has caused some confusion. Either the research design makes an assumption about the effects of accounting information and tests for market efficiency, or it assumes efficiency and tests for effects. It appears logically invalid to test hypotheses about the efficiency of capital markets (which makes assumptions about the effects of events) and simultaneously to test hypotheses concerning the effects of events (which makes assumptions about the efficiency of capital markets). Accordingly, the present study assumes capital market efficiency and tests for the effects of accounting changes.

Information theory provides the definition of information, in terms of differences between distribution functions. Market efficiency provides the temporal setting for the assessment of the effect of accounting events on stock prices. But one question still remains: How does one assess the effect of accounting events on stock prices?

Capital Asset Pricing

The normative capital asset pricing model (CAPM) of Sharpe[7] provides an equilibrium expectation of the return from holding an asset (such as a stock). It is possible to compare various aspects of the unconditional expected return distribution to the same aspects of the realized return distribution conditioned on some information set.[8] Assuming that the effects of other competing factors have been controlled, differences between the two distributions represent the information content of the event under investigation. This method fits the formal definition of information presented earlier. It was employed in all the AC studies referred to in footnote six of Chapter 1.

The CAPM, plus the empirical evidence on the efficiency of capital markets, provides a well-defined framework for assessing the information content of ACs. The form of the CAPM assumed here is the one proposed by Black[9] and tested by Black, Jensen, and Scholes[10] and by Fama and MacBeth.[11] Jensen[12] provides a summary of the various forms of the CAPM and tests thereof, as well as the model's assumptions, which are adopted here.

The model is given by the expression:

Eq. 1 $$E(\tilde{R}_{it}) = E(\tilde{R}_{zt}) + \beta_{it}(E(\tilde{R}_{mt}) - E(\tilde{R}_{zt}))$$

which states that the expected return on asset i, $E(\tilde{R}_{it})$, is equal to the expected return on portfolio z which has $\beta_{zt} = 0$, denoted $E(\tilde{R}_{zt})$, plus β_{it} times the difference between the expected return on the market portfolio, $E(\tilde{R}_{mt})$, and $E(\tilde{R}_{zt})$, all for period t. β_{it} is equal to $\mathrm{cov}(\tilde{R}_{it}, \tilde{R}_{mt})/\mathrm{Var}(\tilde{R}_{mt})$, the relative risk of asset i in the market portfolio, m. Tilde (~) denotes a random variable.

Empirical tests of this and other models as summarized in Jensen indicate that, at least for New York Stock Exchange firms, the CAPM explains security returns reasonably well. The most important consistencies between the theory and the evidence of the model are (1) there seems to be a positive tradeoff between return and risk, (2) the relationship between return and risk is linear, and (3) only systematic risk represented by β_{it} affects average returns. All of these implications hold where asset i is a portfolio of securities and not necessarily for individual securities.

The Distributional Properties of Security Returns

Fama has shown that security returns are members of the symmetric, stable family of distributions (of which the normal distribution is a member) with finite means but infinite variances (unlike the normal distribution which has a finite variance).[13] This departure from normality has been more recently documented by Gonedes.[14] Fortunately the tails of the return distributions Gonedes observed are not so leptokurtic as to completely invalidate the normality assumption. This is fortunate because it permits utilization of well-developed and reasonably robust statistical tests based on the assumption of normality.

Consequently, as in other studies, this study assumes that security return distributions are normal. In fact, they are assumed to be multivariate normal. The multivariate assumption is invoked to provide for the possibility of detecting different information effects of ACs made by firms in different risk classes. Two risk classes are considered: high-risk and low-risk. Therefore, the return distributions are assumed to be bivariate normal. The rationale underlying tests for the risk-dependency of the information content of ACs is discussed in the final section of Chapter 3.

Integration of Theories and Evidence into the Present Design

This study adopts Demski's[15] definition of information (in terms of differences between probability distributions). It assumes the market for stocks is efficient in the semistrong form, that the CAPM explains security returns, and that return distributions are multivariate normal.

The design used here is an adaptation of one developed by Gonedes.[16] It is similar to the API analysis utilized in the other AC studies, but it differs in that the comparison is made between total realized returns of groups of paired firms which have the same equilibrium expected value. (Recall that the API comparison is between the realized returns and the expected returns of a single group of firms.) The two groups of firms to be compared here differ only in the values of the information variable $\tilde{\theta}_i$. In the remainder of this report let θ_1 represent the presence of an AC and θ_2 represent no AC.[17]

Omitting the subscript t for convenience and using underlined notations to refer to vectors, let $\underline{R}_1 | \underline{\theta}_1$ represent the 2 × 1 vector of returns of ACF conditioned on disclosure, $\underline{\theta}_1$ (also a 2 × 1 vector) of an AC in the annual report. Let $\underline{R}_2 | \underline{\theta}_2$ represent a similar vector of NCF returns conditioned on no AC disclosure, $\underline{\theta}_2$. Vectors of returns are used to give effect to the bivariate nature of return distributions, with groups of high-risk and low-risk firms being the two elements in the return vectors. Then if $F(\underline{R}_1 | \underline{\theta}_1) = G(\underline{R}_2 | \underline{\theta}_2)$, $\underline{\tilde{\theta}}_i$ does not have information content. Conversely, $F(\underline{R}_1 | \underline{\theta}_1) \neq G(\underline{R}_2 | \underline{\theta}_2)$ indicates that $\underline{\tilde{\theta}}_i$ does have information content in the sense that $\underline{\tilde{\theta}}_i$ is associated with changes in $\underline{\tilde{R}}_i$.

Chapter 1 notes that investors are likely to make their investment decisions on the basis of the expected return and the riskiness of the perceived return distributions. The last implication of Fama and MacBeth's test of the CAPM indicates that the relevant risk measure is the systematic risk captured by β_i. The only item in Eq. 1 unique to security i is its relative risk, β_i. Therefore if $\underline{\beta}_1 = \underline{\beta}_2$, then $E(\underline{R}_1) = E(\underline{R}_2)$ because R_m and R_z are constant for all i during a given t. Differential mean returns indicate that $F(\underline{R}_1 | \underline{\theta}_1) \neq G(\underline{R}_2 | \underline{\theta}_2)$. Therefore, $\overline{\underline{R}}_1 | \underline{\theta}_1 \neq \overline{\underline{R}}_2 | \underline{\theta}_2$ is sufficient to show that $F(\underline{R}_1 | \underline{\theta}_1) \neq G(\underline{R}_2 | \underline{\theta}_2)$; but it is not necessary.

Under the assumption of multivariate normality (a symmetric distribution) the only other aspect of the return distributions worth considering is the variance Σ_i, a 2 × 2 variance-covariance matrix. $\Sigma_1 | \underline{\theta}_1 \neq \Sigma_2 | \underline{\theta}_2$ is also sufficient for rejecting the no information hypothesis. Thus if the two distributions differ on either parameter, the implication is that $\underline{\tilde{\theta}}_i$ has information content. It takes equality on both parameters to accept the no information hypothesis.

$\underline{\beta}_1 = \underline{\beta}_2$ insures that $E(\underline{R}_1) = E(\underline{R}_2)$, but it does not insure that $\Sigma_1 = \Sigma_2$. Equality of $\underline{\beta}_1$ and $\underline{\beta}_2$ implies only that the systematic variability of returns on ACFs (denoted by subscript 1) is equal to the systematic variability of returns on NCFs (denoted by subscript 2). It says nothing about the unsystematic portion of total variability.[18] Therefore additional measures must be taken to insure that $\Sigma_1 = \Sigma_2$ prior to testing in order to be able to ascribe differences during the test period between $\Sigma_1 | \underline{\theta}_1$ and $\Sigma_2 | \underline{\theta}_2$ to $\underline{\tilde{\theta}}_i$. This is covered in the section on covariance matrix tests in Chapter 5.

The results of the Kaplan and Roll[19] and the Sunder[20] studies suggest that the period beginning six months before and ending six months after disclosure of the AC can be expected to reflect the most intense market behavior affected by ACs. The evidence on the vehicle that companies use to disclose ACs suggests that the annual report is more important than the preliminary earnings announcement (see footnote 4). Unfortunately, the author is unaware of any evidence on the exact timing of the publication of firms' annual reports, but the exact timing of the release of the earnings announcement is documented in Ball and Brown. They found that fifty percent of the 1965 firms in their sample of December 31 firms had made a preliminary year-end report by February 8 of the following year; seventy-five percent had done so by February 21. These dates compare to February 25 and March 10 for 1957 firms in their sample.[21] A crude extrapolation of Ball and Brown's findings suggests that public disclosure of the preliminary report is moving closer to the year-end date. The latest date for public disclosure of the entire annual report is ninety days after the fiscal year-end, when the 10-K report is due to be received by the Securities and Exchange Commission (SEC).[22] The annual report to stockholders is believed to be issued close to the date when firms file their 10-K reports to the SEC. Therefore, it is reasonable to assume that market agents have disclosures of ACs available to them some time during the first, second or third month after firms' fiscal year-ends.

Month 0 was identified as being uniformly two months after the fiscal year-end of all sample firms. This approximation does not exactly identify the month when all investors are aware of firms' ACs, but it is believed to characterize the average case. It represents a compromise between firms that disclose their ACs in preliminary earnings reports during the first or second month after year-end and firms that disclose their ACs in the annual report during the second or third month after year-end. Month 0 will be referred to as the AC disclosure month.

The time period for testing is the thirteen months centered on the AC disclosure month. Based on the assumption of market efficiency, any information contained in the AC disclosure is assumed to appear some time within six months of the disclosure date. The exact time is unknown, and for that reason the thirteen-month period has been decomposed into three different periods in order to determine when the information, if any, affects the market. Letting t = 0 be the month during which the annual report is made public, one subperiod runs from t = -6 through -1. This period is used to test whether the market anticipates any information associated with ACs that is pertinent to valuing ACFs. This period prior to the release of the annual report is examined because of the possibilities that (1) some firms may disclose their ACs before year-end and (2) some firms may experience some stock market activity during the latter part of a year which actually influences their decision to make an AC. A second subperiod runs from t = +1 through +6 and is used to test for information content in the AC disclosure. The third period tested is the entire thirteen-month period from t = -6 through +6 and is used to test for both anticipation of and reaction to AC information.

C H A P T E R 3

LITERATURE REVIEW

Chapter 1 mentioned some differences between this study and previous AC studies, and in so doing described some of the previous studies very briefly. Chapter 3 describes the AC studies involving stock price effects in greater detail and expands on the differences mentioned earlier in order to explain more fully the rationale for conducting yet another AC study.

Archibald

Archibald examined the changes made by sixty-five firms from accelerated to straight-line depreciation during the period 1955-1966. In his analysis he used ordinary least-squares procedures and the logarithmic version of the market model:

Eq. 2 $$\tilde{R}_{it} = a_i + b_i\tilde{R}_{mt} + \tilde{e}_{it} \qquad t = 1, \ldots, 204.$$

Where $\tilde{R}_{it}$ and $\tilde{R}_{mt}$ are the natural logarithms of the returns for asset i and the market, m, respectively, for period t. The natural logarithm of the residual return peculiar to asset i for period t is $\tilde{e}_{it}$, and a_i and b_i are regression parameters (assumed to be constant for all t) peculiar to asset i. In estimating firms' relative risks, b_i, around the AC date, Archibald omitted the two-year period on either side of the AC. He then used the estimates $\hat{a}_i$ and $\hat{b}_i$ to form expectations and isolate the return component $\hat{e}_{it}$ peculiar to asset i, in the following manner:

Eq. 3 $$\hat{e}_{it} = R_{it} - \hat{a}_i - \hat{b}_i R_{mt}.$$ Hat (^) denotes an estimate.

He averaged the $\hat{e}_{it}$ cross-sectionally and plotted the $\bar{e}_t$ over forty-eight months surrounding the announcement month. This describes the mechanics of the API analysis referred to in Chapter 2.

Archibald's conclusions were that switch-back firms exhibited below normal stock market performance in the two-year period preceding the change and that the switch-back announcement and resultant profit improvement apparently had no impact on stock market performance.

Archibald performed no formal test of the statistical significance of the residuals he computed. A t-test on the average prechange $\bar{e}_t$ he plotted in Figure 1 of his paper indicates that they differ from zero at the .005 level; a similar test on the postchange $\bar{e}_t$ indicates that they do not differ from zero at the .20 level. Thus, while his results indicate some abnormal stock price activity for ACFs, it is not known whether he would have obtained the same results for a control group of NCFs. Furthermore, the univariate assumption that the e_{it} were independent may have been violated, and this may have caused the t-test to have yielded spurious significance of the prechange $\bar{e}_t$.

More recent evidence of β_i nonstationarity observed by Ball and by Sunder casts some doubt on whether Archibald's use of constant parameter estimates in Eq. 3 resulted in an unbiased measure of $\hat{e}_{it}$.[1]

Baskin

Baskin examined 128 ACs of various types which resulted in auditors' consistency exceptions during 1965-1968. He analyzed the absolute value of $\hat{e}_{it}$ (from a log version of the API) to determine whether market response to consistency exceptions was more diffuse (i.e., more variable) than market response to a control group of firms with no consistency exceptions. His design was a two-way analysis of variance over the five weeks including and immediately following publication of the annual report.

In order to measure the effect of consistency exceptions, as distinguished from ACs, he identified three groups of firms. Experimental sample I made an AC and notified shareholders of the change in the year-end earnings announcement in the Wall Street Journal. Experimental sample II also made an AC but made the disclosure in the annual report. The control sample made no AC. To test for the information content of consistency exceptions Baskin first showed that the three groups of firms had similar stock performance (in terms of diffusion) for the five weeks[2] following the Wall Street Journal announcement. He then performed the analysis over the five weeks including and immediately following publication of the annual report.

How Baskin intended to ascribe differences to consistency exceptions is not clear from his article. Table I below gives one reasonable set of decision rules for the possible outcomes of the three pairwise comparisons between the groups of firms in his analysis. It is an effort to reconstruct the logic underlying Baskin's design.

TABLE I

BASKIN'S DECISION RULES FOR THE

ANAYLSIS OF INFORMATION IN THE ANNUAL REPORT

Pair	Significant Difference between	Implies Information Content in
1	Exper.I & Control	Consistency exception
2	Exper.II & Control	Neither AC disclosure nor consistency exception
3	Exper.I & Exper.II	AC disclosure

A difference between the members of pair one would imply information in the consistency exception because the market had already had the opportunity to respond to the AC disclosure of experimental group I in their earnings announcements. Therefore, any differential response to the annual reports must be due to the consistency exception of experimental group I.

A pair two difference could conceivably result from differential market response to either experimental group II's AC disclosure or to its consistency exception. This is because both items appear in its annual report which is being examined as a possible information source. Following Baskin's logic, the market had no opportunity to respond to the AC disclosure of experimental group II until issuance of its annual report, which also contained its auditor's consistency exception.

A pair three difference would not be likely to result from the information content of the consistency exception because both groups received auditors' exceptions. Any such difference could result from information in the AC disclosure of group II's annual report because group I had already made its AC disclosure a few weeks earlier.

The above interpretations of differences between pairs one and three are rather weak because both of them imply that the market effect, if any, to group I's AC disclosure in the preliminary earnings announcement is dissipated before issuance of the annual report, thereby yielding an unbiased measure of the information contained in the annual report.

The outcome of Baskin's test was a difference between the third pair listed above, at the .079 level. On this basis Baskin concluded that consistency exceptions made no differences to market agents, because .079 is greater than the conventional .05 level. It appears that his conclusion is correct, but for a reason he did not acknowledge--namely that the pair for which differences occurred hint at information content in the AC disclosure rather than in the consistency exception.

Baskin also implicity assumed a constant β_i in his analysis. Any resulting bias in his results should be nominal due to the very short period over which he examined market reaction.[3]

Kaplan and Roll

Kaplan and Roll examined 275 changes from deferral accounting to flow-through for the investment tax credit (ITC) and 75 changes from accelerated to straight-line depreciation, all from 1962 through 1968. They compared the ITC change firms to a control group of fifty-seven firms which did not change their ITC accounting method. Their design was similar to Archibald's in that they utilized the API. In addition to plotting cumulative average residuals, they also constructed 80 percent confidence intervals around the weekly average errors ($\bar{e}_t$) of ACFs and of NCFs. The intervals included zero for only 60 percent and 70 percent, respectively, of the sixty weeks of the analysis.

At face value this could imply that there was no differential information effect associated with ACs. But a closer view of the behavior of the APIs they computed suggests that ACs and AC disclosures do contain information. They found a positive API for ITC changers up to around fifteen weeks after the earnings announcement. Then the API became negative. Kaplan and Roll interpreted this to mean that investors reacted positively to the higher reported earnings (from flowing through the ITC) at the preliminary earnings announcement date. Then a few weeks later investors, armed with the annual report disclosing the reason for part of the firms' income, systematically bid change firms' prices down. The API of NCFs stayed positive.

The pattern for depreciation change firms was similar to that of ITC changers in that there was some increase in the API around the earnings announcement date and it became negative soon thereafter. The depreciation change firms' API was even more negative than that of the ITC change firms.

Kaplan and Roll's results suggest that firms' disclosures of their ACs involving the ITC and depreciation are used by investors in pricing stocks, at least when the effect of the change on profits is positive. Their design represented a refinement in that they did not assume that distributions of residuals are normal. Instead they characterized the distributions in their analysis merely as symmetric stable, which is consistent with the evidence provided by Fama (see chapter 2—distributional properties).

There are two weaknesses in their analysis, however. The first is their failure to control for changes in ACFs' β_i during the sixty weeks of their analysis. Thus, their results might have been biased in a manner similar to the results of Archibald and of Baskin. Their relatively short impact period of sixty weeks suggests that the bias in their tests is somewhat less than the bias in Archibald's study, but greater than that in

Baskin's study. (See Ball, Figure 4 for the behavior in β_i around the month when ACs are normally disclosed.)

The second weakness results from the fact that 263 of the 275 ITC changes in their study occurred in 1964. This suggests that R_{m64} may have been nontrivially influenced by the effect of the ITC changes. Hence, removal of $\hat{\beta}_i R_{m64}$ from R_{i64} of the ITC change firms i may have removed much of the effect, if any, which ITC changes had on R_{i64}, leaving a biased measure of $\hat{e}_{i64}$ and hence of $\bar{e}_{64}$.[4]

Ball

Ball examined 267 changes involving a variety of accounts between 1946 and 1958. His design was an API analysis similar to the studies reviewed above. Ball observed that the estimates of relative risk of ACFs increased over a 161-month period around the month when the annual report was issued. He sought to adjust for possible misspecification of β_i (and hence of e_{it} as well) in two ways. His estimates of β_i were based on a moving average of 100 monthly observations. He then used his β_i estimates to cross-sectionally estimate R_{zt} and R_{mt} in Eq. 1. With these estimates he predicted $E(R_{it})$ and compared his predictions to realized R_{it} to compute $\hat{e}_{it}$.

He plotted the cumulative average residuals, $\bar{e}_t$, in a time series in a manner similar to Kaplan and Roll.

Ball's objective was to see whether the market can correctly price the shares of firms making ACs. To do this he assumed that the market should disregard the income effects produced by ACs. His results indicated that the average residuals in the sixty months before and the fifteen months after the AC disclosure were very close to zero, and he concluded on that basis that the market is able to distinguish real from unreal income, i.e., that the market is efficient.

Ball's conclusion of no effect is curious in light of the fact that some of the changes he analyzed may have had substantive economic content (e.g., 71 changes to LIFO) to which an efficient market has been shown to react.[5,6] Also, he averaged across errors related to firms whose incomes were increased and firms whose incomes were decreased by the accounting changes they made. If a market effect were present, it could have been cancelled in the aggregation. Ball did supplement his time series with a contingency table to test for a relationship between the sign of the income effect of the change and the sign of the cumulative average error at month 0. He found no such relationship. Even this decomposition could only show that there was a systematically positive (or negative) relationship between the sign of the cumulative average return residual <u>up to AC disclosure</u> and the sign of the income effect. It would imply nothing about the information content of AC disclosures.

Ball's conclusions would have been strengthened if he had performed a similar analysis on a control group of NCFs. He may have found that the time series of cumulative errors on NCFs was noticeably different from the one of ACFs. In the absence of a control group, one still wonders whether the market does price the shares of ACFs uniquely.

Sunder

Sunder (1973) performed an API analysis of 129 changes to LIFO (for reporting and for tax purposes) and 26 changes to FIFO from 1946 to 1966. His purpose was to study the relationship between alternative inventory costing methods and stock prices. He observed that firms changing to LIFO experienced steady increases in stock price during the year before the change and no pattern during the subsequent year. Firms changing to FIFO

experienced downward price movement, but the sample was so small that this result was not conclusive. A control sample exhibited no noticeable price trend.

Sunder reasoned that firms changing to LIFO would be earning more "real" income than before making the change. He then hypothesized that, because firms comprising the market portfolio were New York Stock Exchange firms and were net debtors also benefiting from inflation, accounting change firms' β_i should rise due to a rising $Cov(R_i, R_m)$. In his view the opposite would be true of firms changing to FIFO, i.e., a lower $Cov(R_i, R_m)$ resulting in a lower β_i.

He followed with a second study (1975) in which he used an adaptive regression model developed by Cooley and Prescott, to adjust for a changing β_i, in the estimation of relative risks. His results for firms adopting LIFO were essentially the same as in his first study, but firms abandoning LIFO exhibited no significant price movement, in contrast to his former results.

Table II summarizes the research performed on ACs.

TABLE II

SUMMARY OF ACCOUNTING CHANGE STUDIES

Author	Purpose	Changes Examined	Control Group	Signifi-cance Test	Control for Effect of Changing β_i	Assumed Distri-bution	Dependent Variable	Conclusions
Archibald	Examine market reaction to ACs	To SL from accelerated depreciation	No	No	No	Uni-variate normal	$\bar{e}_t$	Switch-back firms had below-normal prices in 2-year period before AC and normal prices in subsequent 2-year period.
Baskin	Measure market effect of auditors' consistency exceptions	Various	Yes	Yes	No	Uni-variate normal	$\sum_{t=0}^{4} e_{it}$	Market does not react uniquely to consistency exceptions.
Kaplan and Roll	Measure market effect of ACs	To flow-through from deferral of ITC; To SL from accelerated depreciation.	Yes	Yes	No	Uni-variate symmetric stable	$\sum_{t=-30}^{29} \bar{e}_t$	ITC changers had above-normal prices for 10 weeks around earnings announcement date and below-normal prices for the next 15 weeks. Firms changing to SL depreciation had below-normal prices over an entire 60-week period around earnings announcement date.
Ball	Test market efficiency in pricing shares of ACPs	Various	No	No	Yes	Uni-variate normal	$\sum_{t=-110}^{11} \bar{e}_t$	ACs do not affect stock prices; market is efficient.
Sunder (1975)	Measure association between ACs and price changes	To LIFO from FIFO To FIFO from LIFO	No	No	Yes	Uni-variate normal	$\sum_{t=-11}^{12} \bar{e}_t$	Firms changing to LIFO had abnormally high prices; firms changing to FIFO had no abnormal market response.
Present study	Examine market reaction to ACs and AC disclosures	Various	Yes	Yes	Yes*	Bi-variate normal	$\bar{R}_1 \mid \underline{\theta}_1 - \bar{R}_2 \mid \underline{\theta}_2$ and $\Sigma_1 \mid \underline{\theta}_1 - \Sigma_2 \mid \underline{\theta}_2$	See Chapter 6.

*See next section of this chapter.

Differences between This Study and Previous Studies

To further distinguish the research design in this study from those of previous studies, two items in Table II deserve mention.

One is the control for changing β_i. The control feature used here is different from the moving average estimates of β_i employed by Ball and by Sunder. Control is achieved by the use of a matched group of NCFs and the equalization of the estimated β_i of ACFs and NCFs prior to testing. Recall that $\underline{\beta}_1 = \underline{\beta}_2$ implies that $E(\underline{R}_1) = E(\underline{R}_2)$; any observed difference between $\bar{\underline{R}}_1 | \underline{\theta}_1$ and $\bar{\underline{R}}_2 | \underline{\theta}_2$ can be attributed to $\tilde{\underline{\theta}}_i$ or to testing error. Therefore, it does not matter that $\underline{\beta}_1 \neq \underline{\beta}_2$ during the test period because the two $\hat{\underline{\beta}}_i$ were equalized at the beginning of the experiment. Thus even if $\underline{\beta}_1 \neq \underline{\beta}_2$ during the test period, that difference can be attributed to $\tilde{\underline{\theta}}_i$. But the design used here will not decompose any difference between $\bar{\underline{R}}_1 | \underline{\theta}_1$ and $\bar{\underline{R}}_2 | \underline{\theta}_2$. Difference between mean vectors may be comprised of differences between $\underline{\beta}_1(\underline{R}_{mt} - \underline{R}_{zt})$ and $\underline{\beta}_2(\underline{R}_{mt} - \underline{R}_{zt})$ or between $\bar{\underline{e}}_1 | \underline{\theta}_1$ and $\bar{\underline{e}}_2 | \underline{\theta}_2$. Which component is not of interest here because it is sufficient that $\bar{\underline{R}}_1 | \underline{\theta}_1 \neq \bar{\underline{R}}_2 | \underline{\theta}_2$ to infer that $\tilde{\underline{\theta}}_i$ has information content.

In terms of variances, $\underline{\beta}_1 = \underline{\beta}_2$ and $\text{Var}(\underline{e}_1|\underline{\theta}_1) = \text{Var}(\underline{e}_2|\underline{\theta}_2)$ imply that $\Sigma_1|\underline{\theta}_1 = \Sigma_2|\underline{\theta}_2$ unless $\tilde{\underline{\theta}}_i$ has information content. Certainly if $\underline{\beta}_1$ and $\underline{\beta}_2$ are changing differentially during the test period, such changes will contribute to differences between $\Sigma_1|\underline{\theta}_1$ and $\Sigma_2|\underline{\theta}_2$. But as in the test on means, control is achieved in the research design by making $\hat{\underline{\beta}}_1 = \hat{\underline{\beta}}_1$ prior to testing. And it is also necessary to show that the two groups' unsystematic risks, $\text{Var}(\underline{e}_i|\underline{\theta}_i)$ are equal in order for $\Sigma_1|\underline{\theta}_1 \neq \Sigma_2|\underline{\theta}_2$ to imply that $\tilde{\underline{\theta}}_i$ has information content.

The second major distinction is the assumption made in this study that security returns are multivariate normal. The multivariate assumption has two important benefits. (Recall that the multivariate nature of the assumed return distribution results from the dichotomization of firms into high- and low-risk groups.) The first benefit is the ability of the present design to detect any dependency between the information content of ACs and firms' relative risk. At face value this benefit is intuitively pleasing, but further justification is needed. In fact there must be some reason for believing such a dependency may exist.

Firms' production-investment and financing decisions are major determinants of their relative risks.

Since the effects of new information conveyed by a signal such as an AC depend on the nature of the AC and since the nature of the AC and the relative risks of firms both depend, in general, upon a firm's production-investment and financing decisions, it is reasonable to expect a relationship between the effects of new information and relative risk.[7] Gonedes did observe such a dependency between relative risk and the information content of special income items.[8]

With respect to ACs, the market may infer more strongly negative characteristics about low-risk firms which make discretionary ACs that increase net income (i.e., ACs potentially motivated by the intent to manipulate net income) than about high-risk firms that make the same ACs. To illustrate, suppose that two firms have equal expected cash flows of $1,000. The market views one firm as high-risk and in arriving at a theoretical value of the firm, market agents apply a discount rate of .12 to its expected cash flows prior to the AC. The second firm's cash flows are discounted at .08 prior to the AC inasmuch as this firm is viewed as being less risky. The prechange firm values are $8,333 for the high-risk firm and $12,500 for the low-risk firm.

Suppose further that in both cases the market views the ACs as signals that the firms are more risky than before they made the changes and increases both discount rates by .02, to .14 for the high-risk firm and to .10 for the low-risk firm. If the ACs have no effect on the two firms' cash flows, the firm values will decrease to $7,143 and $10,000, respectively. Thus, the high-risk firm value decreased by $1,190 (around fourteen percent), while the decrease in the value of the low-risk firm is $2,500 (twenty percent). The same example can also serve to illustrate a stronger negative effect on low-risk firms' market values if high- and low-risk firms' ACs have the same effects on expected cash flows and the two discount rates are unaffected by the ACs.

It is also possible that the fact that the market already appraises high-risk firms as such makes it impossible for them to make potentially manipulative ACs without being significantly penalized by the market. Although there is no well-developed theory to support such a belief, the use of leverage provides an analogy to support this position. Theoretically, the use of lower-cost debt in a firm's capital structure is generally wise, but empirically there appears to be an upper limit on the amount of debt a firm can incur and remain viable. In the same way, there may be an upper limit on the risky undertakings (such as potentially manipulative ACs) a firm can become involved in without suffering in the capital market. This counter theory can be used to support the belief that the market effect of some ACs may be stronger for high-risk than for low-risk firms.

It should be recognized that the theories of firm value which utilize the discounted cash flows of an entity are not the same as the CAPM of Eq. 1. Nevertheless, the use of various competing theories to explain empirical phenomena is legitimate because of the diversity of the models which are currently in use and which may impact on the observed phenomena. The important point is that both theories, the CAPM and those utilizing discounted cash flow models, provide frameworks within which ACs that are made by firms in different risk classes can be expected to be associated with different market effects.

Another facet to the issue of the risk dependency of any AC information has to do with an industry effect. It is possible that a large number of firms from the same industry, with similar relative risks, may make the same AC, and the market may react similarly to the industry-wide AC. Sunder's results suggest that, insofar as the steel companies in his sample were of similar relative risk, there might have been different market reactions to the two risk classes of firms (one composed largely of the steel companies and the other composed largely of nonsteel companies).

Gonedes points out that the alternative is to assume that market effects of new information (from whatever source) are constant for firms in different risk classes.[9] But such an assumption may obscure information effects that are unique to particular risk classes of firms. Conducting tests on different risk classes is a means of blocking subjects into homogeneous groups. This technique reduces the within-group variability on the dependent variable and enhances the statistical power of one's test if there is a relationship between the blocking variable and the phenomena underlying the behavior of the dependent variable.[10] This relationship (between the blocking variable, i.e., relative risk, and the phenomena underlying the behavior of the dependent variable, i.e., the market's reaction to ACs) is posited above. Therefore, not only does the use of different risk classes enable one to detect a dependency between relative risk and the information content of ACs, but it also has the possibility of enhancing the statistical power of the resulting tests.

Use of two risk classes, high and low, reflects a need to balance the desire to detect the presence of information effects for particular risk classes against the resulting loss of degrees of freedom in the multivariate significance tests. The number of risk classes selected for testing, R, is subtracted from the sample size, N (number of monthly return observations used to estimate the average return difference between ACFs and NCFs in the tests on mean vectors) in arriving at the denominator degrees of freedom in the F-tests on mean vectors.[11] Consequently, use of a larger number of risk classes results in a correspondingly greater loss of statistical power. Since the tests in this study span only six and thirteen months, it is not considered feasible to increase the number of risk classes beyond two. The point at which the greater power from examination of different risk classes begins to diminish is an empirical issue conditional upon the specificity of the association between information effects and relative risk. At any rate, the examination of information effects for two different risk classes of firms represents a refinement of previous AC research, all of which implicitly assumed that ACs impact similarly on returns of firms in all risk classes. Gonedes performed his tests for the information effect of special items of income on ten, six, and two risk classes, and the results of his multivariate tests on all three numbers of risk classes were essentially the same.[12]

The second benefit is purely statistical in nature. Given that it is desirable to test for information effects on different risk classes of firms, there are two ways to go about testing. One way is to perform separate univariate tests on each risk class. The other way is to perform one multivariate test on all risk classes simultaneously. Use of the multivariate test has the advantage of allowing one to make a single probability statement about the null hypothesis which takes into consideration all risk classes being tested. Where the risk classes are correlated on the dependent variable (security returns), separate univariate tests on the different risk classes are not statistically independent. Therefore, as Bock and Haggard point out:

> No exact probability that at least one [risk class] of them will exceed some critical level on the null hypothesis can be calculated. The multivariate tests, on the other hand, are based on sample statistics which take into account the correlations between variables and have known exact sampling distributions from which the required probabilities can be obtained.[13]

In any security price research that uses the CAPM (Eq. 1) or the market model (Eq. 2), the presence of the return on the market portfolio (R_{mt}) in the return-generating process of each security i induces a positive dependency among returns on the different securities. Conventional univariate analysis ignores this dependency by assuming that the covariance between the residual returns e_{it} and e_{jt} on securities i and j is zero.[14] However, because e_{it} and e_{jt} are residual returns of R_{it} and R_{jt}, after the market effect, R_{mt}, has been removed, the positive dependency between e_{it} and e_{jt} is probably not as strong as the positive dependency between R_{it} and R_{jt}.

Previous studies have all assumed that security returns are univariate. If the estimated residuals (in the API analysis) are positively cross-sectionally correlated, failure to include in total estimated variability the covariances between residuals could have overstated the significance of test statistics where formal tests were conducted. This would appear to have especially affected Kaplan and Roll's results because so many of their ITC changes occurred in one year. Had they corrected for cross-sectional correlation, their results would have likely supported the no information hypothesis even more strongly than they did. The relative absence of temporal bunching of ACs in the other AC studies suggests that the effect of cross-sectional correlation was less severe in them. Nevertheless, the assumption that security returns are distributed in a multivariate fashion will take into consideration any cross-sectional correlation among the sampling units.

Another important difference between this and previous AC studies is that separate statistical tests are performed on the anticipation period (t = -6 through -1) and the reaction period (t = +1 through +6) in order to determine the timing of any unique market effect associated with ACs. This provides the framework within which to formally assess whether AC disclosures after firms' fiscal year-ends are used by market agents. Therefore, to the extent that this study sheds light on that very important topic, the project will directly address an issue of interest to accountants and other business people.

As indicated in Chapter 1, a number of different types of accounting changes previously not separately analyzed are the subject of this study. Examples are nondiscretionary ACs which increase reported profits, nondiscretionary ACs which reduce reported profits, and ACs made by firms with both discretionary and nondiscretionary ACs in the same year.

Also as mentioned in Chapter 1, a third distinction between this study and previous AC research is that other AC studies examined either mean returns or a diffusion index (as a measure of return variability) in order to detect a market response to ACs. The present study examines both means and variances in order to determine whether the distribution of returns on ACFs differs from the distribution of returns on NCFs. Any difference will suggest that ACs make a difference to investors.

Finally, and least important, is the replication aspect of this study. The most recent data included in previous AC studies is 1968. Inasmuch as this study uses data for 1968-1972, it can be viewed as an updated replication of earlier AC research.

CHAPTER 4

DATA AND DATA SELECTION

Chapter 4 is in five parts. Part one identifies the population from which the sample was selected. The second part describes the steps taken to obtain samples of ACFs and matched NCFs. Part three is an example of the sample selection procedures. Part four covers problems in internal validity. And part five includes five tables of data describing the sample, along with a discussion of each table.

External Validity

The population of interest consists of firms which made ACs. Firms were identified as having made ACs from the 1969-1973 issues of Accounting Trends and Techniques (ATT), which provides an annual tabulation of the numbers of firms (from its 600) which had various types of disclosures in their annual reports of the previous calendar year. Most of the 600 firms included in ATT are listed on the New York Stock Exchange (NYSE); a few are American Stock Exchange firms, and fewer still are traded over-the-counter. The firms in the sample used in this study are all NYSE firms. One additional restriction is that monthly return data for all firms be included on the tape developed by the Center for Research in Security Prices (CRSP).[1]

In a strict sense one can generalize the results of this study to the population of firms that are represented on all three data sources: ATT, NYSE, and CRSP. However, to the extent that firms of this group are representative of the entire population of ACFs, the generalization broadens. The reader may decide for himself how far to generalize the results.

The external validity of the period 1968-1972 is quite good because the sample period spans both good market and economic years (1968, 1971, 1972) and bad years (1969, 1970). Therefore, the results are not temporally biased in either direction. Furthermore, the selected time period is long enough to afford good external validity aside from the relative heterogeneity of the individual years in the sample period.

Sample Selection

The beginning point in selection of firms making ACs was with the 766 ACs enumerated in ATT over the sample period. These ACs were identified with 675 ACFs, which number was reduced because some firms were not listed on the NYSE (seventy firms), either the annual report or 10-K report was not available or the AC could not be identified from the report (sixty firms), insufficient months of return data were available to estimate the firm's β_i (six firms),[2] no suitable matched NCF was available (six firms),[3] or the firm was included in ATT because of a pooling of interests (eight firms). The conclusions of this research are based on the remaining 525 ACFs listed in ATT plus thirty-five additional ACFs not listed in ATT. The additional ACFs were located while searching for the control group of NCFs. Firms that accounted for business combinations as poolings were eliminated because such firms were apparently included in ATT with ACFs because they had restated the prior years' financial statements. Such firms were not considered to be ACFs in this study because of the inseparability of the underlying event, the act of acquiring another entity, and the AC-like behavior, restating a prior year's financial statements. Throughout this study an AC was construed to be two different accounting treatments in two successive periods accorded the same set of circumstances or the same event.

After an ACF was initially identified, its annual report or its 10-K report was examined in order to categorize the change (1) on the basis of its directional effect on earnings per share (EPS) and (2) whether it was discretionary or nondiscretionary. If the ACF had two or more ACs with opposite directional effects on EPS, the firm was classified as positive, negative, or zero, depending on the net effect of all ACs made during the year.

In this study a nondiscretionary AC is one which a firm made in response to an opinion of the Accounting Principles Board (APB)[4] or as a result of an Internal Revenue Service tax ruling tied to the firm's financial reporting to shareholders.[5] The relative discretion the firm used was determined from reading the appropriate footnotes in the annual or 10-K report. If the change was not judged to be nondiscretionary, as defined above, it was assumed to have been made solely at the discretion of the firm's management.

In order for the same R_{zt} and R_{mt} to be impacting on the two R_{it} of a matched pair of firms, the adopted impact period must be the same for both members of a pair. This requirement, coupled with the assumption that ACs are disclosed in the second month after fiscal year-end (FYE), means that both members of a pair must have the same FYE. Matching the firms on this dimension allows for the $\hat{\beta}_i$ matching to insure that $E(R_1) = E(R_2)$, and for this reason FYE is the most basic dimension on which ACFs and NCFs were matched.

The CAPM asserts that a security's return is a function of its relative risk, measured by β_i. The design requires that the control group of NCFs should have the same group relative risk, β_g, as the group of ACFs. The subscript g refers to the gth group, where g = 1 for ACF and g = 2 for NCF. One way to obtain a control group is to select a group of NCFs, then compute the group relative risk and rely on the "law of large numbers" to hopefully provide that $\hat{\beta}_1 = \hat{\beta}_2$. Gonedes used this method with non-random selection of the control group in the initial application of this testing technique. Examination of Table 5 of Gonedes' paper (1974a) suggests that in a number of his matched pairs of portfolios, p, the $\hat{\beta}_p$ of one portfolio of the pair differs from the $\hat{\beta}_p$ of the other portfolio by as much as .05. On this basis he concluded that the risk-matching requirement was satisfied.

The present study assumes that the CAPM explains security returns, subject to a minor qualification. Reference to Sunder's AC study provides the reason for the qualification. Of his 118 firms changing to LIFO, twenty-two were steel companies.[6] The time series of cumulative average residuals for the steel firms only faintly resembled that of the other ninety-six (nonsteel) firms.[7] By plotting the steel firms' residuals separately, he acknowledged the presence of an industry effect (in his data) which is nontrivial.

The suggestion is commonplace that there is a need to include an industry factor with the market factor in models of the stochastic process underlying the formation of security returns. One study in the area, by King,[8] suggests that at most, around ten percent of the variability in security returns is explained by an industry factor, where an industry is defined as a two-digit Standard Industrial Classification (SIC) code number. The conclusions of another study, by Meyers,[9] do not deny the ten percent figure. But the principal components analysis of Meyers' study does suggest that an industry factor does not account for a substantial portion of the returns on individual securities' returns. Typically researchers have dismissed the ten percent figure as nominal and opted instead for the simpler market model (Eq. 2) as the stochastic analog to the CAPM.

The approach to control group selection taken here represents a compromise between entire omission of the industry factor and inclusion of it in the mode of estimating relative risks. Gonedes' failure to make

$\hat{\beta}_1 \equiv \hat{\beta}_2$ and Sunder's observation of an industry effect motivated the two extra steps taken here to insure that the control group of NCFs did not systematically differ from the matched ACFs on industry as well as on $\hat{\beta}_i$.

First, to insure that $\hat{\beta}_1 = \hat{\beta}_2$, matching was done at the individual firm level with the idea that if $\hat{\beta}_1 = \hat{\beta}_2$ for individual firms, it would also hold for groups of firms. ACFs' $\hat{\beta}_1$ were taken from monthly issues of Merrill Lynch, Pierce, Fenner & Smith's Security Risk Evaluation,[10] which presents estimates of firms' β_i computed over sixty-month periods. Merrill Lynch uses the market model of Eq. 2 to estimate firms' β_i using Standard and Poor's Index of 500 firms as R_m.[11] The particular β-estimation periods used in this study ended six months prior to disclosure of an AC, conditional on the assumption that ACs are disclosed during the second month after fiscal year-end (FYE). The matched NCF's estimated β_i was taken from the same issue of Security Risk Evaluation to insure that its β_i was estimated over the same time period as the β_i of the matched ACF. Some of the firms' β_i in Security Risk Evaluation are estimated over periods shorter than sixty months. There should be no adverse effect from including such firms in this sample because the number of such firms in the present sample is small (around five percent).

It was not possible to select each NCF with a $\hat{\beta}_i$ identically equal to the $\hat{\beta}_i$ of its matched ACF, except in a very few cases. Around forty percent of the matched pairs of $\hat{\beta}_i$ differ by .05 or less; around eighty percent differ by .10 or less; around ninety percent differ by .20 or less; around ninety-six percent differ by .30 or less; the remaining four percent differ by as much as .40, which was arbitrarily selected as the largest acceptable difference. The average absolute difference between the two $\hat{\beta}_i$ of a matched pair is around .085, and the median absolute difference is around .06. The average standard error of the $\hat{\beta}_i$ is around .33 for firms which, in the context of this study, are classified as high-risk;[12] the average standard error of low-risk firms' $\hat{\beta}_i$ is around .24. The standard errors are large in relation to the differences between the two $\hat{\beta}_i$ of a pair. This problem in estimating $\hat{\beta}_i$ with precision is a source of motivation for grouping firms. The standard errors of the average group estimated beta, $\hat{\beta}_g$, are estimated to fall between .05 and .11 for high-risk groups and between .035 and .08 for low-risk groups in this study.[13] These estimates are in line with the standard errors of portfolio $\hat{\beta}_p$ in Gonedes' study. But they are a little larger, probably because the average $\hat{\beta}_g$ is somewhat larger than the $\hat{\beta}_p$ of his portfolios.[14]

Second, where it was possible, each NCF came from the same three-digit SIC code category as its ACF. This was possible for about half the 560 pairs of firms. If the three-digit SIC code equalization was not possible, equalization at the two-digit level was attempted. The match-up is somewhat better at the group level, at which actual comparisons are made, than for individual pairs of firms. Because of the relative success of the industry matching (see table IV), the results of none of the hypothesis tests in this study are affected by an important difference in industry representation between the ACFs and NCFs being compared. Consequently, the numbers of firms from various industries within the several hypothesis groups are not reported. In short, industry differences do not appear to impair the inference that observed differences between $\underline{R}_1 | \underline{\theta}_1$ and $\underline{R}_2 | \underline{\theta}_2$ result from $\underline{\tilde{\theta}}_i$.

An Example

To illustrate the matching procedures, ACFs and NCFs were matched on FYE, $\hat{\beta}_i$, SIC code, in that order of importance. For example, suppose U.S. Steel during 1971 made a discretionary AC which increased reported EPS and involved U.S.'s accounting for pension costs. Suppose that Bethlehem Steel, which also has a December FYE and made no AC during 1971, is matched with U.S. for 1971. U.S. Steel's $\hat{\beta}_i$ is .99, and Bethlehem's is 1.06, both estimated over the sixty months ending with August 31, 1971. Both firms are in SIC group 331, steel. U.S. (ACF) and Bethlehem (NCF) for 1971 are both included in four hypothesis test groups: ACF - NCF; ACF - NCF, where the effect of the AC on EPS is positive; ACF - NCF, where the AC is discretionary; and ACF - NCF, where the effect on EPS is positive and the AC is discretionary. Wherever U.S. is used as an ACF, Bethlehem is used as an NCF. This holds regardless of the fact that U.S.'s $\hat{\beta}_i$ and Bethlehem's $\hat{\beta}_i$ are not identical. This is because U.S.'s $\hat{\beta}_i$ of .99 characterizes U.S. as a low-risk firm in the context of this study; therefore Bethlehem's $\hat{\beta}_i$ of 1.06 is also classified as low-risk. The sorting of pairs into the various hypothesis groups was performed on the $\hat{\beta}_i$ of the ACF of a pair. Incidentally, this is an actual example from the data used here. For an understanding of the research design of this study, it is important to recognize that the unit of analysis is the pair-year, e.g., U.S. Steel (ACF) and Bethlehem (NCF) for 1971. Groups of pair-years are used for estimation and testing to dampen the effects of sampling error.

The previous example covers U.S. and Bethlehem for 1971. Another feature of the design strengthens the inference that an observed difference between $\underline{R}_1|\underline{\theta}_1$ and $\underline{R}_2|\underline{\theta}_2$ measures the effect of $\underline{\tilde{\theta}}_i$. In summary, it is that a given firm may be used as an ACF in one pair-year and as an NCF in another pair-year, both in the same hypothesis test. For example, U.S. Steel did <u>not</u> make an AC in 1970 nor in 1972. Bethlehem did make an AC in each of those two years. U.S. (NCF) was matched with Armco Steel (ACF) in 1972 and with Republic Steel (ACF) in 1970. Bethlehem (ACF in 1972 and 1970) was matched with Inland Steel (NCF) in 1972 and in 1970. As mentioned previously, U.S.'s 1971 AC increased its 1971 net income. Republic's 1970 AC increased its 1970 net income. Thus, U.S. for 1971 is an ACF, and U.S. for 1970 is an NCF, both in the test of market reaction to ACs that increase EPS. This particular example, which does not universally characterize the success in matching firms, closely resembles Gonedes' description of a perfectly controlled experiment when he states:

> If we had a perfectly controlled experiment, we could generate values of $\underline{\tilde{R}}_t$ conditional on, say, $\underline{\theta}'_t$ and $\underline{\theta}''_t$, holding all other things constant--including the specific assets represented in $\underline{\tilde{R}}_t$. This is, of course, not descriptive of actual data. For example, given t, we observe only one realization of $\tilde{\theta}_{it}$ for

> each i. We cannot, therefore, get returns for each asset i conditional on each of two realizations of $\tilde{\theta}_{it}$, for given t.[15]

However, inclusion of the same firm in each of the two groups with opposite i in $\underline{\tilde{\theta}}_{it}$ of a given test would appear to be a step toward the type of control that Gonedes views as desirable, even if the t are not the same.

Problems in Internal Validity

One may argue that market anticipatory or reaction effects may extend beyond the boundaries of the thirteen-month test period. If this is true, then the market effect related to one member of a pair-year may be partially offset by inclusion of that same firm as the opposite member of another pair-year in the same test. This is entirely possible; however the strongest market response to the AC is believed to occur within six months after the AC is disclosed. This is implied by the Kaplan and Roll results. Also, the results of Kaplan and Roll and Sunder suggest that most of the market's anticipation effect shows up during the six months before disclosure is made.[16] Even if market effects do extend beyond the test period, and even if the use of the pair-year group as the unit of analysis does partially offset the effect of interest, the design as constructed will only produce a conservative test. As a result those hypothesis tests which produce statistically significant differences will strongly imply information content in the event being observed.

During the data gathering, it became obvious that a large number of ACFs had made ACs during the β-estimation period of five years preceding the AC year. If ACs are uniquely associated with nonzero residuals in the market model (Eq. 2), the incidence of many ACs during the β-estimation period could violate the least-squares assumption that $E(\tilde{e}_{it}) = 0$ and thereby induce a bias in $\hat{\beta}_i$. One way to deal with this potential problem is to eliminate from the β-estimation period of each firm all periods when ACs occur. This solution has one important drawback, the undesirability of sacrificing recent years (when ACs occur) for less recent and less currently representative years (when ACs do not occur).

A second solution would be to eliminate from the sample all firms which made ACs during the β-estimation period. But this procedure would reduce the overall sample to around 150 pairs of firms, and the results of tests performed on this sample would only extend to ACFs that had not made ACs in the preceding five years.

In order to determine whether the ACFs and NCFs had the same proportion of ACs during the β-estimation periods, the McNemar test for differences in proportions[17] was performed on a subsample of the data analyzed in this study. It was relatively easy to determine whether an ACF or an NCF listed in ATT had made an AC in the β-estimation period. For this reason only the ATT firms in the total sample of 1,120 were analyzed. The numbers of ATT firms were 525 for ACFs and 431 for NCFs. It was not possible to identify the firms which made ACs during 1966, a β-estimation year for ACFs and NCFs of 1971, 1970, 1969, and 1968, by referring to ATT. Therefore 1966 was omitted from the analysis.

The unit of analysis in the test for differences in proportion is the β-estimation year. The number of β-estimation years analyzed for the 525 ACFs is 2,231; the number of β-estimation years for the 431 NCFs is 1,820.[18] During these numbers of years ACFs had ACs in 383 β-estimation years, and NCFs had 229. Stated in terms of percentages, 17.2 percent of the β-estimation years of ACFs had ACs, while the percentage for NCFs was 12.6. This difference (.046) was significant at .001.

The obvious implication of this result is that the ACFs in this sample had more potentially disequilibrating factors in the form of ACs during the years over which their β_i were estimated than did the NCFs. To the extent that the slightly larger number of ACs made during the β-estimation period of the ACFs caused a bias which differs from that included in the estimation of NCFs' β_i, the condition that $\underline{\beta}_1 = \underline{\beta}_2$ may not have been strictly met. However, it must be remembered that ACs are only one of many potential sources of information impacting on the stock returns used in estimating β_i. In view of this fact, the economic impact of the .046 difference in the number of β-estimation periods affected by ACs may not be particularly damaging. Accordingly, this difference was not considered sufficiently large to impair the ability of the β-estimation procedures to arrive at the condition that $\underline{\beta}_1 = \underline{\beta}_2$.

Further Sample Description

Tables III-VII present profiles of the various aspects of the firms in the sample. Table III gives the numbers of ACFs selected from each of the five years examined here. Table IV gives the numbers of ACFs and NCFs from the industries most represented by ACFs. Table V gives the numbers of ACs which affected the various accounts. Table VI gives the sample sizes and average $\hat{\beta}_g$

for the various hypothesis groups on which formal tests were performed in this study. Table VII presents the distribution of the $\hat{\beta}_i$ of ACFs and NCFs in the sample.

Table III shows that over half the sample ACFs come from 1971 and 1972. This bunching is caused primarily by the switch to the equity method of accounting for long-term investments in stock, mandated by APB Opinion No. 18. The problem with temporal bunching of any event affecting a large portion of a sample is that, for the periods when it occurs, $\tilde{R}_{mt}$ is apt to be substantially affected by the event. For such periods the historical relationship between $\tilde{R}_{it}$ and $\tilde{R}_{mt}$ may not be characterized by β_i for the firms i that are affected by the event. As a result, the estimation of descriptive statistics based on the assumption of the stability of historical relationships can be systematically biased for a large portion of the sample. If, to take an extreme case, ACFs number one-half of the population of firms, and all ACFs make the same type of AC during the impact period t, $\tilde{R}_{mt}$ should be affected if the AC has information content. ACFs' β_i may change drastically during the impact period, while the β_i of NCFs are unchanged. Use of the single-sample market model (Eqs. 2 and 3) or the single-sample cross-sectional model will almost certainly obtain a biased measure of $\tilde{e}_{it}$ if

TABLE III

CALENDAR YEARS FROM WHICH ACCOUNTING

CHANGE FIRMS WERE SELECTED

Year	Number of Firms
1968	46
1969	98
1970	109
1971	167
1972	140
Total	560

the AC has information content because $\hat{\beta}_i \neq \beta_i$ for impact-period t. In this case, bunching will have rendered the conclusions of such research suspect at best and meaningless at worst.

The bunching in the present sample is similar to that observed by Kaplan and Roll in 1964, but it is much less severe.[19] Even where some bunching occurs, the research design used here should be able to obtain a less biased measure of the information content of Acs than the single-sample analysis used by other researchers. This is due to (1) the use of a $\hat{\beta}_g$-matched control group and (2) the assumption that security return distributions are multivariate in nature. Direct comparison of ACFs and NCFs with the same $\hat{\beta}_g$ (estimated in the same manner over the same time period) insures that the two groups are similar prior to the impact period (when the bunching occurs). Thus, virtually any impact-period difference should be closely related to the AC.

The positive dependency believed to normally exist between $\tilde{R}_{it}$ for different i and the same t (see the final section of chapter 3) should be even stronger among firms that make the same type of AC during the same t. And this dependency should become stronger yet as the number of firms making the same type of AC increases. In such cases the value of univariate test statistics may be seriously overstated because of the omission from total variability of the potentially positive covariances between returns on individual securities. The assumption of a multivariate return distribution provides for inclusion of covariances of returns in total variability that forms the basis for the denominator in most test statistics (see eqs. 6, 7, and 8 in chapter 5). As a result, the multivariate test statistic is more likely to be correctly estimated, especially when a high proportion of the sampling units are affected by the same event (such as an AC).

Table IV reveals that more of the ATT firms in four industries--oil, automobiles and auto parts and accessories, chemicals, and steel--made ACs during 1968-1972 than firms in other industries. In fact, the firms in these four industries comprise twenty-eight percent of the entire sample of ACFs. This makes it appear that the attempted matching of firms by industry was not a wasted exercise. In this regard Table IV suggests that, for the entire sample of 560 pairs of firms, the industry match was relatively successful. In all likelihood a randomly selected sample of NCFs would have differed from the ACF group in terms of its industry composition. And any differences between return distributions of the two groups

TABLE IV

INDUSTRIES OF SAMPLE FIRMS

SIC Code	Industry	Accounting Change Firms	Non-change Firms
291	Oil	42	40
371	Automobiles, Parts and Accessories	40	39
280	Chemicals	36	34
331	Steel	41	35
357	Office and Business Equipment	14	15
372	Aerospace	15	17
367	Electronics	15	23
333	Aluminum and Primary Smelting and Refining	15	10
220	Textiles	15	9
999	Conglomerates	13	12
200	Packaged Foods	10	9
531	Retail Department Stores	10	10
541	Retail Food Chains	11	8
355	Specialty Machinery	11	10
283	Drugs	10	11
356	Industrial Machinery and Pollution Control	12	16
260	Paper	12	10
300	Tires and Rubber Products	9	3
374	Railroad Equipment	10	8
324	Cement	6	10
230	Textile Apparel Manufacturing	5	15
353	Construction and Oil Well Machinery and Cranes	8	12
		360	356
	All others	200	204
	Total	560	560

of firms may have been due to an effect associated with an industry differential rather than to the information content of ACs.

Table V summarizes the numbers of ACs affecting various accounts. The ACs are also separated into discretionary ACs and nondiscretionary ACs. Within each of these categories, there are four directional effects of ACs on net income: "+," for an AC that increased net income; "-," for an AC that decreased net income; "0," for an AC with no effect or an immaterial effect on net income; and "?," for an AC which the firm disclosed in a footnote but did not disclose the direction of the net income effect. These data present a rather sharp contrast with Ball's sample of ACs.[20] One difference is in the number of nondiscretionary ACs. Ball did not identify the number of nondiscretionary ACs among the 267 ACs made by his sample of 197 firms, but it is unlikely that the proportion is as high as in this sample (twenty-five percent). This is due to the fact that during the period covered in Ball's work (1946-1958) the authority of the pronouncements of the American Institute of Certified Public Accountants rested solely on the general acceptability of the pronouncements. By contrast, during the period covered by this study (1968-1972), authority to specify methods of accounting rested

TABLE V

TYPES OF ACCOUNTING CHANGES BY ACCOUNT AFFECTED, DISCRETION OF MANAGEMENT IN MAKING THE CHANGE, AND DIRECTIONAL EFFECT ON NET INCOME

	Discretionary				Nondiscretionary				Total
	+	-	0	?	+	-	0	?	
Equity Investments									
To Equity from Cost	13	2	11	13	70	9	29	32	179
To Inclusion	16	1	40	29					86
To Exclusion	1		15	4					20
Depreciation									
To Accelerated	1	1	1						3
To Straight-Line	59		12	1					72
Service Lives	14		2		3				19
Other	5	1	1		1				8
Inventory									
From LIFO	19		6	1	1		1		28
To LIFO		2	5						7
Other	1	2	3	1					7
Expense Recognition									
Intangibles		3	1	1		1			6
To Capitalization--To Cash	25	1	7	1	3				37
To Expense--To Accrual	4	10	9	2					25
Other	2	2	1		1				6
Revenue Recognition									
Construction Contracts	4		4	1					9
Installment Sales				1				1	2
Other	1	3	2	1		1	1		9
Income Taxes									
To Tax Allocation	2		1	2	3	1	2	6	17
Investment Tax Credit	14	6	4	1					25
On Undistributed Subsidiary Income	1				2	6	5	3	17
Loss Carryforward	2		1						3
Pensions									
Actuarial Assumptions	51	1	23	16					91
Actuarial Methods	8	2	11	1					22
Amortization Period	6	2	4	3					15
Foreign Currency Translation	5	1	5						11
Discount Receivables/Pay				1		1			2
Other	1	1	1	1					4
Totals	255	41	170	81	84	19	38	42	730

with the Accounting Principles Board of the American Institute.[21] Consequently, firms were more likely to have been required to make nondiscretionary ACs during 1968-1972 than during 1946-1958.

There are other differences between the types of ACs examined in this study and the types Ball examined. For example, his sample included eighty-five ACs that affected inventory, and seventy-one were changes to LIFO. In this sample only forty-two ACs affect inventory, and of these just seven are changes to LIFO. Seventy-five of Ball's ACs (over one-fourth) related to depreciation, and forty-six were changes to an accelerated method. This sample also contains a large number of depreciation changes, 102 (but only around one-seventh of the total sample ACs); here changes to straight-line are far more numerous than changes to an accelerated method. Another difference is the large number of changes affecting pension costs in the present sample. The majority of the pension cost ACs increase reported net income. Ball's sample included no ACs specifically classified as affecting pension cost. In summary, most of the more recent ACs have increased net income, while a higher proportion of the ACs in Ball's sample decreased net income.[22]

As previously mentioned, no AC study has examined the market reaction to ACs having all possible directional effects on profits. No AC study has specifically tested whether the market reacts differently to discretionary ACs and nondiscretionary ACs. Only some of the directional effects have been examined, and only discretionary ACs have been examined separately.

The possibility that a relationship exists between the sign of the income effect of an AC and the market effect of the AC follows from Ball and Brown's evidence on the market effect of accounting income numbers. This evidence prompted Ball to perform the association tests between the sign of the income effect and the sign of the cumulative average residual. And it also provides the motivation for examining this relationship again in this study.

The striking difference between the income effects of the ACs in Ball's sample and this sample suggests the possibility that managers who make ACs are trying to "manage" earnings. This belief is widespread in the financial community and was the underlying motivation for the Kaplan and Roll study. Their study covered discretionary ACs which increased profits by affecting amounts for income tax expense and depreciation on the income statement.

Ball examined many different types of ACs and even went so far as to implicitly assume that ACs reflect unreal changes in wealth. Sunder, on the other hand, examined discretionary inventory changes with both effects on profits and hypothesized a real change in wealth. But none of these studies has examined the market's behavior toward nondiscretionary ACs.

If managers do use discretionary ACs to manipulate profits, then there is reason to believe the market may react differently to nondiscretionary ACs. Supposedly the opportunity for manipulation is eliminated if the AC is not discretionary. In fact, Ball may have observed no relationship between returns and the sign of the income effect of ACs because he did not separate the ACs with particular directional effects on EPS into discretionary and nondiscretionary groups. This possibility, plus the possibility that the abundance of recent ACs which increase profits are indicative of the apparent intent to deceive, provides the motivation for examining discretionary and nondiscretionary ACs separately.

The following set of notations will be used throughout the remainder of this study and is particularly helpful in understanding Table VI which follows. Let ACF(i,j) represent an ACF that may be classified on two

TABLE VI

NUMBERS OF PAIRS OF FIRMS AND AVERAGE $\hat{\beta}_g$ FOR HYPOTHESIS GROUPS

Hypothesis Group[a]	Number of Pairs	Average $\hat{\beta}_g$ of High-Risk		Average $\hat{\beta}_g$ of Low-Risk	
		ACF	NCF	ACF	NCF
Omnibus					
ACF(·,·) - NCF	560	1.608	1.605	.928	.940
Direction of Income Effect					
ACF(+,·) - NCF	275	1.556	1.549	.923	.942
ACF(-,·) - NCF	43	1.827	1.812	.868	.864
ACF(-,·) - ACF(+,·)	30	1.653	1.638	.810	.823
ACF(O,·) - NCF	142	1.604	1.612	.952	.961
ACF(?,·) - NCF	100	1.667	1.667	.944	.950
Relative Discretion					
ACF(·,D) - NCF	377	1.640	1.635	.943	.951
ACF(·,N) - NCF	135	1.593	1.606	.937	.961
ACF(·,B) - NCF	48	1.389	1.370	.819	.821
Direction of Income Effect and Relative Discretion					
ACF(+,D) - NCF	183	1.575	1.564	.924	.940
ACF(-,D) - NCF	27	1.915	1.898	.870	.870
ACF(O,D) - NCF	106	1.627	1.639	.967	.961
ACF(?,D) - NCF	61	1.746	1.742	1.008	1.016
ACF(+,N) - NCF	60	1.560	1.580	.951	.989
ACF(-,N) - NCF	10	1.810	1.800	1.188	1.196
ACF(O,N) - NCF	30	1.555	1.563	.937	.971
ACF(?,N) - NCF	35	1.585	1.597	.879	.876
ACF(+,B) - NCF	32	1.421	1.414	.881	.859
ACF(-,B) - NCF	6	NA	NA	NA	NA
ACF(O,B) - NCF	6	NA	NA	NA	NA
ACF(?,B) - NCF	4	NA	NA	NA	NA

where: NA = Not Applicable because sample size is too small; lower limit for testing is ten pairs.

[a]Note that, in general, each hypothesis group is the difference between the matched ACFs and NCFs, hence the notation ACF-NCF.

dimensions i and j, where i refers to the direction of the net income effect(s) of the AC(s) made by the firm in one year, and j refers to the firm's relative discretion in making its AC(s) that year. The direction variable i may take on one of four possible values: "+," a positive net income effect; "-," a negative net income effect; "0," a net income effect that is zero or immaterial; and "?," a net income effect that is unknown because the ACF did not disclose the direction of net income effect of its AC(s). The discretion variable j can take on three possible values: "D," for discretionary ACs; "N," for nondiscretionary ACs; and "B," for both, which means that the firm made at least one discretionary AC <u>and</u> at least one nondiscretionary AC during the same year.

A dot (·) is used to signify the absence of a specific direction or discretion value, in which case the dot (·) indicates that the variable takes on all possible values assignable to that variable. For example, ACF(·,·) refers to all ACFs in the sample, aggregated across all directional effects on income and all values of the discretion variable. ACF(+,·) refers to all ACFs whose ACs increased EPS irrespective of the value of the discretion variable. Thus, ACF(+,·) includes three separate categories of ACFs with ACs that increased net income. They are ACF(+,D), firms whose discretionary ACs increased net income; ACF(+,N), firms whose nondiscretionary ACs increased net income; and ACF(+,B), firms that, during a given year, made at least one discretionary AC and at least one nondiscretionary AC and the overall effect of the ACs on net income was positive.

Table VI gives the numbers of ACFs categorized on the basis of the twenty-one hypothesis groups examined here. The sample sizes naturally become smaller as the groups become more homogeneous. The positive and negative income effects groups and the discretionary and nondiscretionary groups were discussed above. One may wonder why the zero effect and unknown effect categories are included. They do not appear to share the behavioral characteristics of either the positive effects group or the negative effects group. Accordingly, because there may be potentially different market reactions to these types of ACs, they are tested separately from the other types of ACs. The group for which the effect on net income was not disclosed (i.e., is unknown) is of some interest because the market may view nondisclosure of the income effect of ACs as a lack of good faith on the part of the ACF and systematically bid down the stock prices of firms in this category. This possibility provided motivation for separate tests on the two categories.

One may also wonder why the "both" category was tested separately. The reason is the lack of an a priori belief as to the market's reaction to ACFs with both discretionary and nondiscretionary ACs. Again the motivation is heuristic--to see whether the market's response to the "both" group more closely resembles its reaction to discretionary ACs or to nondiscretionary ACs.

Table VI also presents the average $\hat{\beta}_g$ of the high- and low-risk groups of ACFs and NCFs. The $\hat{\beta}_g$ appear to be matched reasonably well. Across all hypothesis groups the average absolute difference between the two $\hat{\beta}_g$ of a risk-class pair is .0116. The largest difference of .038 occurs between the low-risk pair for the hypothesis test on ACs which increased net income and were not made at management's discretion.

It is possible that a $\hat{\beta}_g$ difference as large as .038 plus its related confidence interval could distort the results of a test. In order to measure the effect of such a difference, the tests involving mean vectors and covariance matrices were re-run for all three time periods for the ACF(+,N) hypothesis group (where this difference occurs for the low-risk pair). To gauge the effect of this $\hat{\beta}_g$ difference, the cross-validation test corrected for the difference. Then the results of this corrected test were compared to the results of tests which did not make the correction. Suffice it to say that the results of the two tests are essentially the same. Details are given in subpart B of part 1 and part 2 of appendix B to Chapter 6.

If the ACFs were representative of all firms, the average $\hat{\beta}_i$ of all the ACFs would be near unity. The average $\hat{\beta}_i$ of the sample ACFs is 1.268, which indicates that the average ACF in this sample is more risky than the average firm. The average $\hat{\beta}_i$ of the sample NCFs is 1.273. This is shown in Table VII, which gives the distribution of $\hat{\beta}_i$ in the sample of firms. As one would expect, there is a clustering around unity. But the distribution is skewed to the right. The difference between the average firm and unity is probably exaggerated somewhat due to the regression effect identified by Black, Jensen, and Scholes.[23]

The positive skewness of $\hat{\beta}_i$, and hence of $\hat{\beta}_g$, does not impair the internal validity of the study because the overestimation in $\hat{\beta}_i$ of ACFs is offset by that of NCFs. Recall that the two $\hat{\beta}_i$ of every ACF-NCF pair were estimated over the same monthly observations and using the same estimation method and $\tilde{R}_{mt}$ values. Likewise the external validity of the study is not impaired because the

TABLE VII

DISTRIBUTION OF $\hat{\beta}_i$ OF SAMPLE FIRMS

$\hat{\beta}_i$ Interval	Frequency ACFs	Frequency NCFs
0- .20	0	1
.21- .40	5	5
.41- .60	22	16
.61- .80	42	43
.81-1.00	103	93
1.01-1.20	99	108
1.21-1.40	104	115
1.41-1.60	80	68
1.61-1.80	38	48
1.81-2.00	30	22
over 2.00	37	41
	560	560
Average $\hat{\beta}_i$	1.268	1.273

average ACF $\hat{\beta}_i$ is not unity. Rather, the generalization of the study is to ACFs, not to firms in general. These data merely indicate that for 1968-1972 the average ACF had a somewhat higher relative risk than the average firm. One reason for the higher relative risk is the omission of utilities from the sample of firms.

At this point ACFs and NCFs are equated on FYE, $\underline{\hat{\beta}}_g$, and industry. Therefore, any differences between $\underline{R}_1 | \underline{\theta}_1$ and $\underline{R}_2 | \underline{\theta}_2$ may be attributed to information content in $\underline{\tilde{\theta}}_i$ or to chance.

CHAPTER 5

HYPOTHESES AND DATA ANALYSIS PROCEDURES

Part one of Chapter 5 identifies the hypotheses tested. Part two describes the procedures for grouping firms for testing and computing returns. Part three covers the statistical tests employed in the analysis and is divided into two subparts; the first covers the test on mean vectors and the second the test on covariance matrices. Part four describes the procedures for determining whether there are dependencies between information effects of ACS and different risk classes.

Hypotheses

Table VI of Chapter 4 lists the eighteen hypothesis groups on which tests are performed in order to determine whether ACs are associated with market effects. The last three types of ACs were each made by less than ten firms during the sample period, and tests were not performed on those groups. Ten was an arbitrarily selected lower limit for the number of pairs of firms on which tests were performed. For each of the eighteen hypothesis groups with ten or more pairs of firms, two tests were performed: (1) on the estimated conditional mean return difference vector $\underline{\bar{d}}|\underline{\theta}$, $(\underline{\bar{R}}_1|\underline{\theta}_1 - \underline{\bar{R}}_2|\underline{\theta}_2)$, used to estimate the true mean difference vector, $\underline{\mu}_d|\underline{\theta}$, of returns between high- and low-risk ACFs and NCFs; and (2) on the estimated conditional covariance matrices, $S_1|\underline{\theta}_1$ and $S_2|\underline{\theta}_2$, which are used to estimate the true variances of returns on high- and low-risk ACFs, $\Sigma_1|\underline{\theta}_1$, and on high- and low-risk NCFs, $\Sigma_2|\underline{\theta}_2$.

The general forms of the null hypotheses for all eighteen groups are given by Eq. 4 and Eq. 5:

Eq. 4 $\quad H_0^a: \quad \underline{w}_r'\underline{\mu}_d|\underline{\theta} = \underline{\mu}_0 = 0 \qquad r = 1, 2$ for high-risk and low-risk, respectively.

Eq. 5 $\quad H_0^b: \quad \Sigma_1|\underline{\theta}_1 = \Sigma_2|\underline{\theta}_2$

The two-sided alternative hypotheses are given by Eq. 4' and Eq. 5'.

Eq. 4' $\quad H_1^a: \quad \underline{w}_r'\underline{\mu}_d|\underline{\theta} \neq \underline{\mu}_0 = 0$

Eq. 5' $\quad H_1^b: \quad \Sigma_1|\underline{\theta}_1 \neq \Sigma_2|\underline{\theta}_2$

The superscripts a and b refer to tests on means and covariances, respectively, and as mentioned previously, underlined notations refer to vectors. The vector $\underline{w}_r$ is a 2×1 column vector of weights, the components of which can be varied to combine the high- and low-risk groups, r, into assets with different overall risk coefficients. Prime (') in the equations denotes a transpose. The conditional difference vector, $\bar{\underline{d}}|\underline{\theta}$ is used because the matching of ACFs and NCFs induces a dependency between the two groups. As a result the appropriate mean vector test is a matched-pairs test on the mean difference vector, $\bar{\underline{d}}|\underline{\theta}$. Other symbols were defined in the last part of Chapter 2. Thus in the case of each of the eighteen hypothesis groups with ten or more pairs of firms, the two hypotheses were tested, for three time periods: t = -6 through -1; t = +1 through +6; and t = -6 through +6.

Procedures for Grouping Firms and Computing Returns

The multivariate hypotheses are stated in terms of mean vectors and covariance matrices to give effect to the joint simultaneous tests on high- and low-risk groups of firms. This part of the chapter describes the procedures for grouping firms into high- and low-risk categories and for computing returns.

Each $\hat{\beta}_i$ was taken from the issue of <u>Security Risk Evaluation</u> with estimates covering the sixty-month period ending four months before the FYE of firm i.[1] For each of the eighteen hypothesis groups, the $\hat{\beta}_i$ of ACFs were ranked in descending order. The high-risk group of ACFs is made up of the ACFs with the $\hat{\beta}_i$ in the top half of the ranked $\hat{\beta}_i$. The low-risk group of ACFs have the $\hat{\beta}_i$ in the bottom half of the ranked $\hat{\beta}_i$. The average $\hat{\beta}_g$ for each risk group is the arithmetic average of all the $\hat{\beta}_i$ in the group. If the total number of pairs is odd, the low-risk group is one larger than the high-risk group. The high- and low-risk groups of NCFs are the firms matched with the ACFs that fall in the respective high- and low-risk groups of ACFs.

Once the high- and low-risk groups of ACFs (and NCFs) are formed, monthly returns are computed on each ACF and each NCF from t = -6 through +6. Returns on the ACFs within each risk class are averaged for each of the thirteen months of the impact period, from t = -6 through +6. Note that within an hypothesis risk group, the ACFs come from different calendar years and are pooled together for estimation of $\bar{\underline{d}}|\underline{\theta}$. This means that t = -6 through +6 for one ACF is a different calendar period from the t = -6 through +6 period for an ACF from another year in the same hypothesis group and the same risk class. One may argue that this procedure aggregates across different return-generating functions, given by Eq. 1. However, this apparent threat to internal validity dissipates in light of the ACF-NCF matching which insures that t = -6 through +6 is the same calendar period for both members of an ACF-NCF pair.

Letting month t = 0 be the second month after each firm's FYE and not an identical calendar month for all ACFs in an hypothesis risk group represents a fundamental difference between the estimation procedure used here and that used by Gonedes. He equated $\hat{\beta}_p$ at March of each year and computed returns from April through March of the next year, thereby ignoring the different FYEs of firms in the hypothesis group.[2] As a result, it is possible that he was capturing in $\underline{\bar{d}}|\underline{\theta}$ some "noise" that was unrelated to the information variable, $\underline{\tilde{\theta}}_i$. The procedure used here, on the other hand, is designed to measure the most pronounced effects of ACs and AC disclosures (assuming that such effects are revealed during the thirteen-month period around the post-year-end date when most ACs are disclosed to the public).

Continuing with the procedure for estimating $\underline{\bar{d}}|\underline{\theta}$, after the returns of all ACFs within each risk class have been averaged for a given month, the same procedure is followed for NCFs, yielding an average return on the two NCF risk classes for the same month in relation to the FYE. The average return difference vector, $\underline{\bar{d}}|\underline{\theta}$, is computed by taking $\underline{\bar{R}}_1|\underline{\theta}_1 - \underline{\bar{R}}_2|\underline{\theta}_2$ for each of the thirteen months. These average return differences are then used in tests on mean vectors for the three test periods specified above.

For the test on covariance matrices the procedures are identical up to the point where the return difference $\underline{d}|\underline{\theta}$ is computed. The covarance matrix test uses the ACF average returns to compute two variances, one on high-risk and another on low-risk ACFs' returns, and the covariance between returns on high-risk ACFs and returns on low-risk ACFs. Variances and covariances of NCF returns are computed in a like fashion.

Statistical Tests

Tests on Mean Vectors

The test on mean vectors employs Hotelling's T^2 statistic, a multivariate analog to the standard t-test.

The single sample test was performed because of the dependency between ACFs and the matched NCFs, so the test is a multivariate matched-pairs t-test.

The T^2 statistic has the form:

Eq. 6
$$T^2 = \max_{\underline{w}_r} t^2(\underline{w}_r) = \frac{\underline{w}_r'(\underline{\bar{d}} - \underline{\mu}_0)(\underline{\bar{d}} - \underline{\mu}_0)'\underline{w}_r N}{\underline{w}_r' S_d \underline{w}_r} .$$

An equivalent form is given by:

Eq. 7
$$T^2 = N(\underline{\bar{d}} - \underline{\mu}_0)' S_d^{-1}(\underline{\bar{d}} - \underline{\mu}_0).$$

All terms in Eq. 6 and Eq. 7 are as previously defined except N, which is the sample size, and S_d, which is the sample covariance matrix of the mean difference vector $\underline{\bar{d}}$. The elements of S_d are defined in Eq. 8, where H denotes high-risk and L denotes low-risk. The information conditioning argument, $\underline{\theta}$, is omitted from Eqs. 6-8 to avoid clutter.

Eq. 8
$$S_d = \begin{bmatrix} \mathrm{Var}(d_H) & \mathrm{Cov}(d_H, d_L) \\ \mathrm{Cov}(d_H, d_L) & \mathrm{Var}(d_L) \end{bmatrix}.$$

The sample size N in Eq. 6 and Eq. 7 is the number of monthly observations (either six or thirteen) used for estimating $\underline{\bar{d}}|\underline{\theta}$. A condition necessary for virtually all statistical applications is that the sampling units be independent of each other. In this case, the sampling units are monthly return observations, and the weak-form tests of market efficiency provide evidence that monthly price observations (and price changes) are very nearly serially independent. A further discussion of weak-form market efficiency can be found in Fama.[3]

Eq. 6 reflects the fact that the multivariate statistic T^2 forms a linear combination of the high- and low-risk elements in the difference vector $\underline{\bar{d}}$. The weight vector $\underline{w}_r$ can take on any set of values desired, but the T^2 statistic used the value of $\underline{w}_r$ which maximizes the value of T^2. Thus the multivariate T^2 is nothing more than the maximum squared univariate $t(\underline{w}_r)$-statistic. In utilizing the maximizing value of $\underline{w}_r$, the T^2 implicitly considers all values of $\underline{w}_r$.

The statistic $t^2(\underline{w}_r)$ is dimensionless and is unaffected by a change in the scale of $\underline{w}_r$. Therefore, $\underline{w}_r$ can be normalized so that $w_1 + w_2 = 1$ by imposing the constraint, $\underline{w}_r' S_d \underline{w}_r = 1$, on the maximization done in Eq. 6. Then the normalized values of the elements of $\underline{w}_r$ can be determined by multiplying the vector $\underline{x} = S_d^{-1}(\underline{\bar{d}} - \underline{\mu}_0)$ by the scalar $1/\sum_{r=1}^{R} x_r$. The resulting normalized values of w_1 and w_2 can then be viewed as legitimate portfolio weights.

When the null hypothesis (Eq. 4) is true,

$$\text{Eq. 9} \qquad F = \frac{N - R}{R(N - 1)} T^2$$

has the F distribution with degrees of freedom R (= two, for the number of risk classes) and N - R (where N = six or thirteen). Departures of $\underline{\mu}_d | \underline{\theta}$ from $\underline{\mu}_0$ increase the mean of T^2 so the decision rule is to accept the null hypothesis if the observed F-value falls beneath the area of the F distribution corresponding to one's desired level of significance. Otherwise, reject the null hypothesis and infer that the ACs have information content. A thorough description of the T^2 statistic can be found in Morrison[4] or Anderson.[5]

<u>Tests on Covariance Matrices</u>

The test for equality of covariance matrices employs the M statistic proposed by Bartlett:

$$\text{Eq. 10} \qquad M = (n_g)\log_e|S| - \sum_{g=1}^{G} n_g \log_e |S_g|,$$

where G is the number of groups being compared--two in this case, 1 = ACF and 2 = NCF; n_g is the number of monthly observations used to estimate S_g (i.e., sample size N_g) - 1; S is the arithmetic average of the two sample covariance matrices, S_1 and S_2 (in Eq. 11 below); and the parallel vertical lines denote a determinant. The elements of S_g are defined in Eq. 11.

Eq. 11 $$S_g = \begin{bmatrix} Var(R_{gH}) & Cov(R_{gH}, R_{gL}) \\ Cov(R_{gH}, R_{gL}) & Var(R_{gL}) \end{bmatrix}.$$

An important distinction between the sample covariance matrix S_d in the test on mean vectors (in Eqs. 6-8) and the sample covariance matrices S_g in the test on covariance matrices (in Eq. 11) is that S_d is computed on the distribution of return differences, while the S_g are computed on the raw return distributions of groups of ACFs and NCFs.

Reference to Eq. 5 indicates that the multivariate test for equality of covariance matrices is performed without weights, $\underline{w}_r$. In the multivariate context, simultaneous tests are performed on the differences $Var(R_{1H}) - Var(R_{2H})$, $Var(R_{1L}) - Var(R_{2L})$; and $Cov(R_{1H},R_{1L}) - Cov(R_{2H},R_{2L})$. Introduction of the weight vector, $\underline{w}_r$, into the covariance matrix test reduces the null hypothesis of Eq. 5 to a univariate hypothesis because $\underline{w}'_r \Sigma_i | \underline{\theta}_i \underline{w}_r$ is a scalar representing the variability of the return on only one asset. This asset is a linear combination of the returns on high-risk and low-risk firms, and the process of combining the two eliminates the duality necessary in the present study for a multivariate test.

Reference to Eq. 10 indicates that departure of any of the three different elements of S_1 (or S_2) from its corresponding average element in S causes M to have a non-zero value. The M statistic represents Bartlett's application of the generalized likelihood ratio criterion, and in it the determinants of the sample covariance matrices assume the role of generalized variances. Morrison[6] includes a discussion of the M statistic.

For samples of around twenty or more, M can be transformed into a chi-square random variable. For smaller samples, which apply to the principal tests conducted here, Box[7] has proposed a transformation of M into an F random variable with degrees of freedom $f_1 = 1/2(G - 1)R(R + 1)$ and $f_2 = (f_1 + 2)/(A_1^2 - A_2)$.[8] The transformation is given by Eq. 14:

Eq. 14 $$F_{f_1, f_2} = \frac{f_2 M}{f_1 (b - M)},$$

where $b = (f_2)/(1 - A_1 + 2/f_2)$. Box also presents evidence that the F approximation is very good for the case of $G = 2$, $R = 2$ (the dimensions of the present data), and $n = 9$ or more. He presents no evidence on samples of less than ten. Therefore, the goodness of fit of the data to the F-distribution is not known for the tests conducted over six-month periods in this study.

Large (enough) observed F-values lead to rejection of the null hypothesis of no difference in variability, and hence to the inference of information content in ACs. Values of F which fall below the selected fractile of the null hypothesis true distribution of F_{f_1, f_2} lead to acceptance of the no information null hypothesis.

The Chapter 2 section, Integration of Theories and Evidence, discussed the effect that $\underline{\beta}_1 = \underline{\beta}_2$ has on one's ability to infer that differences between $\Sigma_1 | \underline{\theta}_1$ and $\Sigma_2 | \underline{\theta}_2$ are due to $\underline{\tilde{\theta}}_1$. Because the condition $\underline{\beta}_1 \simeq \underline{\beta}_2$ says nothing about the equality of unsystematic variability of Σ_1 and Σ_2, additional steps must be taken in order to be able to attribute conditional variability differences to ACs.

It appears that the most straightforward way to approach the problem is to directly test whether $\Sigma_1 = \Sigma_2$ for the sixty-month period over which β_i were estimated. If $\Sigma_1 = \Sigma_2$ prior to the thirteen-month impact period, then $\Sigma_1 | \underline{\theta}_1 \neq \Sigma_2 | \underline{\theta}_2$ during the impact period indicates that ACs possess information content. On the other hand, $\Sigma_1 \neq \Sigma_2$ indicates that the two groups, unsystematic variabilities were different prior to testing, and it will not be possible to attribute a nonzero $\Sigma_1 | \underline{\theta}_1 - \Sigma_2 | \underline{\theta}_2$ difference to $\underline{\theta}_1$, even though $\underline{\beta}_1 \simeq \underline{\beta}_2$. In this event, the test for equality of conditional covariance matrices will not have much meaning.

The appropriate test of H_0: $\Sigma_1 = \Sigma_2$ also utilizes Bartlett's M-statistic, but the M statistic is transformed into a (large-sample) chi-square random variable with degrees of freedom $1/2(G - 1)R(R + 1)$ because sixty monthly observations are used to estimate the M statistic in this case. The specific transformation is discussed in Morrison.[9]

Risk and Information Effects

In order to determine whether ACs possess information for particular risk groups, the analyses were performed using different values of $\underline{w}_r$. The T^2 test utilizes the $\underline{w}_r$ value which maximizes the value of T^2 and thereby tests all values of $\underline{w}_r$ against the null hypothesis. Therefore any other $\underline{w}_r$ value will produce a T^2 value that is less than the value observed in the test on mean vectors.

By varying $\underline{w}_r$, insight can be gained into the existence of any risk dependency by observing whether different T^2 values result from different values of $\underline{w}_r$. Such tests on conditional mean vectors are carried out in the context of the multivariate test because of the all-inclusive nature of the maximizing T^2 test. The use of different $\underline{w}_r$ values in the test for equality of conditional covariance matrices involves a series of univariate tests, because $\underline{w}_r'\Sigma|\underline{\theta w}_r$ reduces to a scalar (i.e., the variability of one asset).

In addition to the value of $\underline{w}_r$ implicit in the T^2 statistic, the following values of $\underline{w}_r$ were used: $\underline{w}_r' = [1\ 0]$; $\underline{w}_r' = [0\ 1]$; $\underline{w}_r' = [1/2\ 1/2]$; and $\underline{w}_r' = [x\ y]$, where x and y produce an overall group $\hat{\beta}_g$ of one, and $x + y = 1$. The vector $\underline{w}_r' = [1\ 0]$ is analogous to a "high-risk" portfolio with 100 percent of the assets invested in the risk-group of firms with the higher $\hat{\beta}_g$. The vector $\underline{w}_r' = [0\ 1]$ is analogous to a "low-risk" portfolio. The vector $\underline{w}_r' = [1/2\ 1/2]$ is analogous to a portfolio with equal investments in the high- and low-risk groups of firms. The vector $\underline{w}_r' = [x\ y]$ was also used because the ACFs in the sample had an average risk coefficient greater than one. If the average had been one, then $\underline{w}_r' = [1/2\ 1/2]$ could be expected to produce an "average-risk" portfolio with $\hat{\beta}_g \simeq 1$. The vector $\underline{w}_r' = [x\ y]$ is included to accomplish this.

In summary, a difference between returns of ACFs and returns of NCFs on either parameter, $\underline{\mu}_d|\underline{\theta}$ or $\Sigma|\underline{\theta}$, is sufficient for rejecting the no information hypothesis. Therefore it takes equality on both parameters to infer no information content in ACs. The design is multivariate in that tests are performed simultaneously on high- and low-risk groups of firms. In order to know whether information effects, if any, are uniquely related to one of the risk classes, tests are performed using different values of $\underline{w}_r$, which correspond to different overall group risk coefficients. The risk effect tests are multivariate on mean vectors and univariate on variances.

CHAPTER 6

EMPIRICAL RESULTS AND INTERPRETATION

Chapter 6 is divided into three parts. The first part covers the tests on mean return vectors, the second part the tests on covariance matrices of returns, and the last part the association between risk and information effects of ACs. All tests were performed on data for three time periods: t = -6 through -1; t = +1 through +6; and t = -6 through +6, where t = 0 is the post-year-end month when the AC is disclosed. The third, all-inclusive period tests whether ACs and AC disclosures are associated with unique market effects during a thirteen-month period around the month when the AC is disclosed. The two subperiods are of interest because effects may appear during either or both of them. Separate analyses on the two subperiods can indicate the relative timing of the information impact on the market. The first subperiod, prior to the AC disclosure month, reveals whether the market anticipates information associated with ACs prior to firms' disclosures of ACs or, alternatively, whether there was a unique market effect that might have been associated with firms' decisions to make ACs during the latter part of a year. The techniques used in this study will not provide evidence as to which is the case, if indeed unique market effects appear during the first subperiod. The second subperiod, which follows the post-year-end disclosure of ACs, reveals whether the market reacts to AC disclosures in the preliminary earnings report and/or the annual report.

Throughout Chapter 6 the two subperiods will be referred to as "first" and "second." Also, the notational scheme with respect to different categories of ACs and ACFs that was explained immediately prior to Table VI will also be retained in this chapter.[1]

Tests on Mean Vectors

Tables VIII and IX (in appendix A) give the high-risk and low-risk average return differences,[2] the standard errors of the estimates of average return difference, the risk class weights implicit in the T^2 statistic, and the related F-values of the T^2 tests. In Tables VIII and IX each of the eighteen hypothesis groups is identified with a specific line number, and each line contains the results of tests for the respective hypothesis group across all three periods. In addition to the tables, Figures 1-18 show plots of the cumulative average return difference ($\widetilde{CARD}_t$) over the thirteen-month period of the study, as well as the number of pairs of firms used for estimation. Cumulative average return difference for month t is defined as:

$$\text{Eq. 15} \qquad \widetilde{CARD}_t = \sum_{t=-6}^{+6} \bar{\underline{d}}_t | \underline{\theta} \text{ ,}$$

where $\bar{\underline{d}}_t | \underline{\theta}$, $(\bar{\underline{R}}_{1t} | \underline{\theta}_1 - \bar{\underline{R}}_{2t} | \underline{\theta}_2)$, is the 2 × 1 average return difference column vector equal to the difference between the two 2 × 1 average return column vectors of a pair, all conditional on a value of $\tilde{\underline{\theta}}_i$ for month t. The elements in the vectors are groups of high-risk firms and groups of low-risk firms. Tables VIII and IX also give the numbers of pairs of firms pertaining to the test on each hypothesis group.

In interpreting the figure related to a given hypothesis test, it is important to keep two things in mind. One is the fact that the average return differences plotted in the figures are cumulative, and except for the first monthly average return difference plotted in each figure, do not represent the monthly average return differences used in the statistical tests. The second is that the average return difference on high-risk firms is not formally compared in this study to the average return difference on low-risk firms (although such a comparison may be appropriate in assessing the different market effects associated with the ACs firms in different risk classes). Each of the two risk-class plots in each figure represents a difference--between returns on ACFs and returns on NCFs. The related statistical tests make this comparison, between high-risk ACFs and high-risk NCFs and between low-risk ACFs and low-risk NCFs and not between the average return difference on high-risk firms and the average return difference on low-risk firms. Therefore, each risk-class plot should be visually compared to the horizontal zero-return line.

Furthermore, it will become apparent that selection of the two six-month test periods examined here produces test results that differ from the results of similar tests that could have been performed on average return differences over different segments of the thirteen-month period. The rationale for selecting the three time periods examined in this study has been discussed previously.

In most cases where statistically significant differences occur, the risk-class weights in Tables VIII and IX provide a signal as to which risk class contributes more of the overall multivariate return difference to the value of the resulting F-value. Accordingly, discussions of the tests with significant differences are identified with a particular risk class. The related plot of the cumulative average return difference is also helpful in identifying information effects with a particular risk class. Where needed, tables showing a further breakdown of a group of firms' ACs on the basis of accounts affected and percentage effect on EPS are also given.

Omnibus Test of ACs

Results of the omnibus test of the information content of ACs are consistent with the no information hypothesis. See line one of Table VIII and Figure 1 below. This high-risk cumulative average return difference in the figure remains very close to the zero horizontal line. In the case of the low-risk firms, the market effect seems to be more pronounced during the second subperiod; however, the variability in the second subperiod average return difference is large enough to render it statistically insignificant. (Note in particular the increase which occurs in month t = +5 to reverse the previously downward drift in the low-risk cumulative average return difference.) Consequently, the vertical return distance for

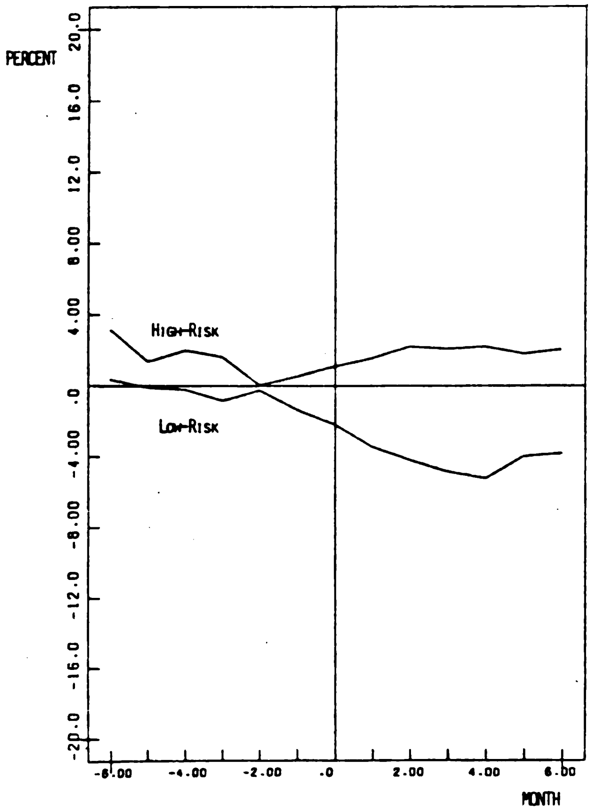

FIGURE 1. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(·,·) - NCF
560 pairs of firms

low-risk firms from t = +1 to +6 is statistically nominal. The observed F-values in Table VIII for all three periods are substantially below the seventy-fifth fractile of the appropriate F distributions. This test is essentially an updated replication of Ball's study, plus a separation of firms into risk classes and the use of a control group of NCFs. The results of tests performed on firms that made more homogeneous types of ACs suggest that the aggregation in the omnibus test cancelled significant effects related to several specific types of ACs. As mentioned previously, the aggregation problem was a source of motivation for this study. Therefore, this result comes as no surprise.

Tests for Effects of ACs with Different Directional Effects on EPS

As mentioned previously, there is evidence to indicate that the sign of changes in net income is an important piece of information to market agents.[3] In fact, many of the stock valuation models of fundamental financial analysis capitalize net income or some derivative thereof to predict equilibrium values of firms.[4] It follows, therefore, that since all ACs have specific directional effects (+,-,0, or ?) on the net incomes of ACFs, ACs with particular income effects may be associated with unique stock market behavior. However, results of tests conducted on mean vectors to ascertain the information content of ACs with the aforementioned directional effects on EPS, when aggregated across discretionary and nondiscretionary ACs, are consistent with the no information hypothesis. (See lines two through six of Table VIII and Figures 2-6 below.) In some cases the average return differences depart from zero by wide margins, but in no case is the multivariate set of differences large enough in relation to its variability to infer that ACs with different directional effects on income are associated with unique market behavior.

The hypothesis test of differential market behavior toward ACs that decreased net income and ACs that increase net income (see line four of table VIII and figure 4) was included along with the tests of ACF-NCF because of the possibility that the effects of ACF (+,·) and ACF (-,·) might be insignificant when compared to NCFs but significant when compared to other ACFs that implemented changes with the opposite signed effect on income. However, the result of this test, like that of the other tests of ACs with different directional effects on income, assigns a high probability to the null hypothesis (Eq. 4).[5]

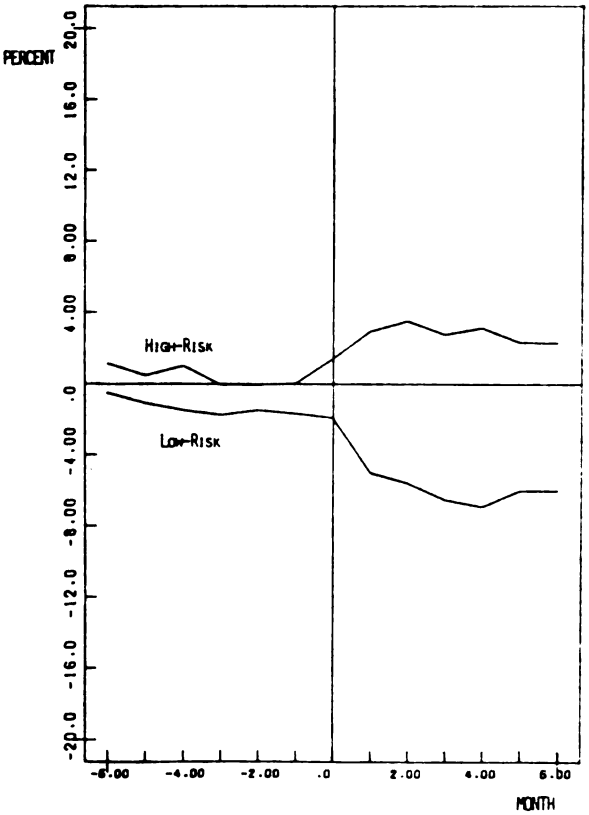

FIGURE 2. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(+,·) - NCF
275 pairs of firms

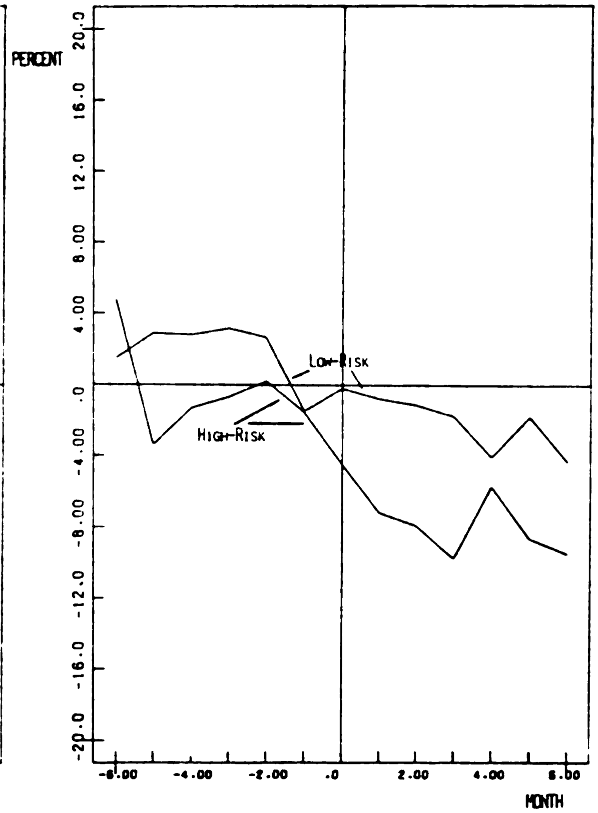

FIGURE 3. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(-,·) - NCF
43 pairs of firms

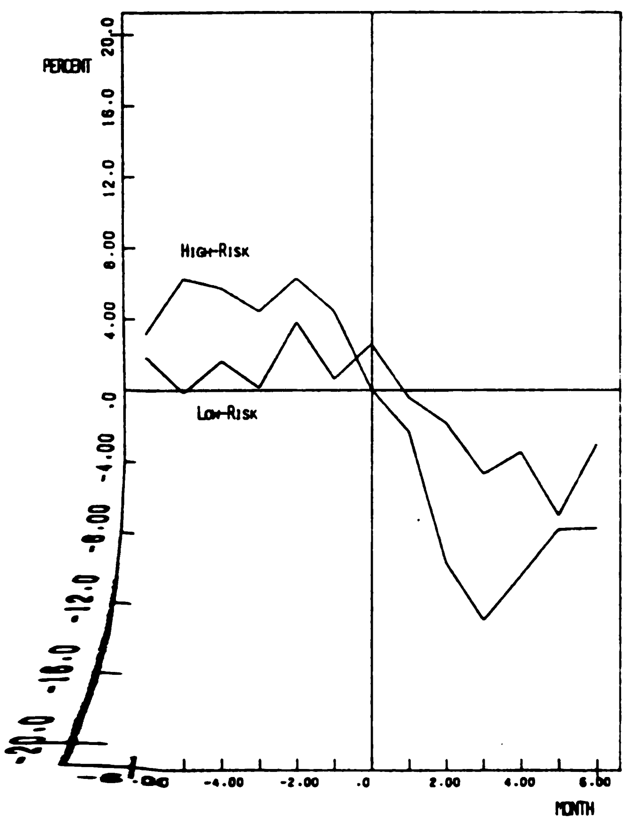

FIGURE 4. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(-,·) - ACF(+,·)
30 pairs of firms

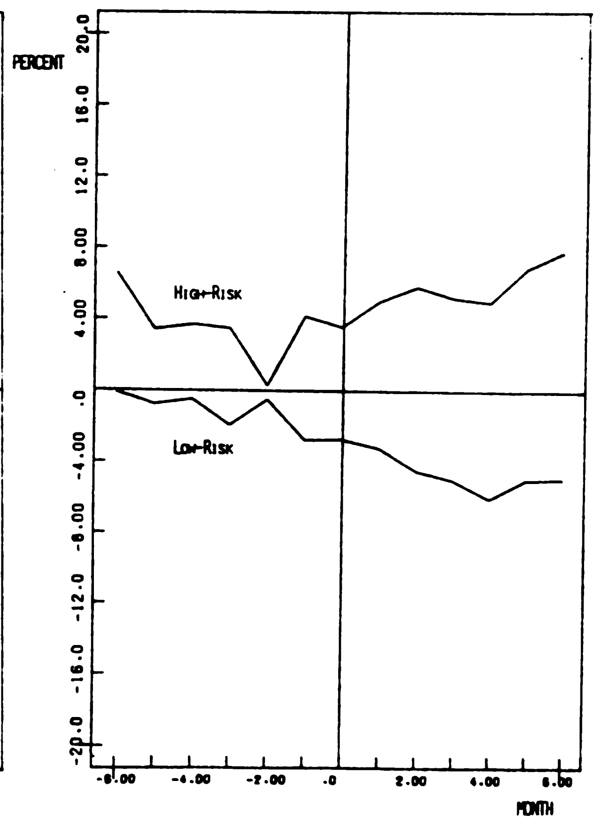

FIGURE 5. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(0,·) - NCF
142 pairs of firms

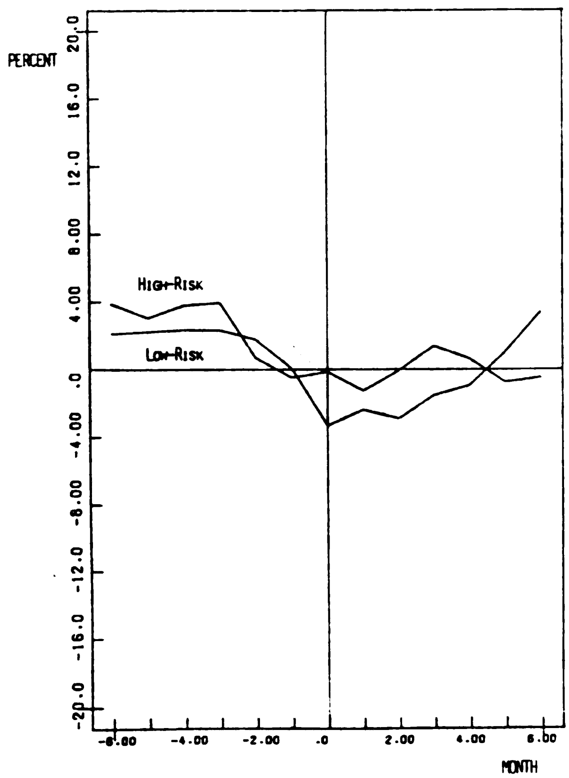

FIGURE 6. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(?,·) - NCF

100 pairs of firms

All the tests of ACs with specific directional effects on EPS were aggregated across all different categories of relative discretion. As a result, these tests for the most part are not directly comparable to previous AC research. For example, the investment tax credit and the depreciation method ACs that Kaplan and Roll examined increased net income, but they were all discretionary ACs. The changes to LIFO and the changes to FIFO examined by Sunder decreased and increased reported net income, respectively, and they too were all discretionary. Archibald's changes to straight-line depreciation were all discretionary. The only aspect of the directional effects tests that is comparable to earlier AC research is the set of tests over the first subperiod. Ball conducted chi-square tests for association between the sign of the income effect and the sign of the cumulative API up to month t = 0.[6] He detected no statistically significant relationship. The present findings tend to corroborate Ball's results. Further refinement of the directional effect analysis on the basis of whether or not ACs were discretionary suggests that the tests discussed above obscure the information effects associated with ACs more specifically identified than just by the directional effect on EPS.

Tests for Effects of ACs with Varying Degrees of Discretion

In Chapter 4 it was hypothesized that the market may react differently to discretionary and to nondiscretionary ACs. It was suggested that the possibility for manipulation of income is likely to be substantially greater for discretionary ACs than for nondiscretionary ACs. For example, the stock of firms that make discretionary ACs which increase EPS in order to conceal otherwise unacceptable operating results may be spurned by the market. On the other hand, the market may view firms that make nondiscretionary ACs with positive income effects differently inasmuch as they have been compelled to change their accounting techniques. Perhaps the imposition of an AC could cause firms to alter their production and investment activities. This shift could, in turn, be considered by market agents to be of substantive economic importance to the firm, and this may give rise to unique market behavior.

The results of the tests of the information content of ACs with varying degrees of discretion generally support the no information hypothesis. The test for the effect of discretionary ACs and the test for the effect of nondiscretionary ACs both indicate that such ACs, when aggregated across different directional effects on net income, make no difference to market agents. (See lines seven and eight of Table VIII, as well as Figures 7 and 8 below.) Thus, it appears that aggregations of discretionary and nondiscretionary ACs, like the aggregations of ACs with specific directional effects on EPS, bring together ACs that are dissimilar enough to obscure the effects associated with a more narrowly defined groups of ACs, if indeed there is unique market behavior toward some types of ACs.

One may wonder why the low-risk average return difference in Figure 7 is not statistically different from zero in view of the fact that the cumulative average return difference reaches a value of around -.07 by month t = +6. Two explanations can be given, both of which were alluded to earlier. First, most of the nonzero behavior occurs from t = -1 through +4. But no test was performed on the average return differences pertaining to this particular six-month period. The average differences over the last six-month period (from t = +1 to +6) were subjected to testing, but (the vertical distance between the <u>cumulative</u> average return differences at t = +1 and at +6 indicates that) the total return difference over this six-month period is only around negative

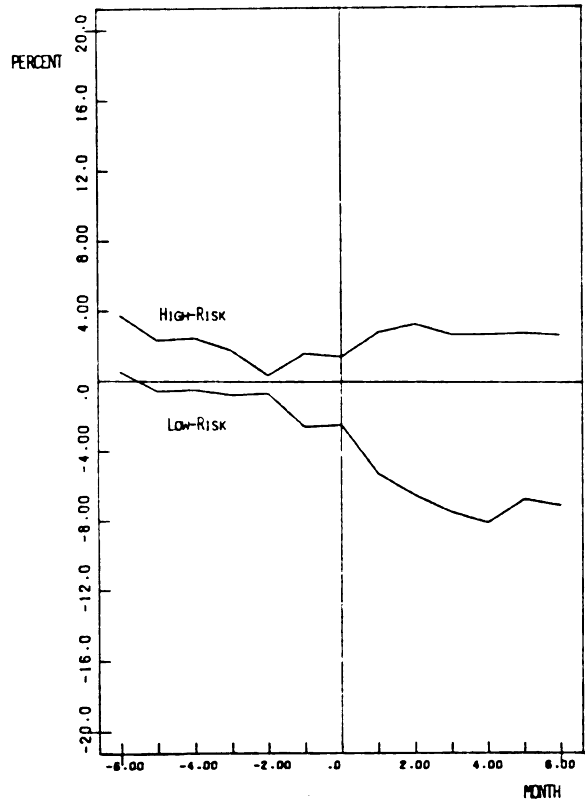

FIGURE 7. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(·,D) - NCF

377 pairs of firms

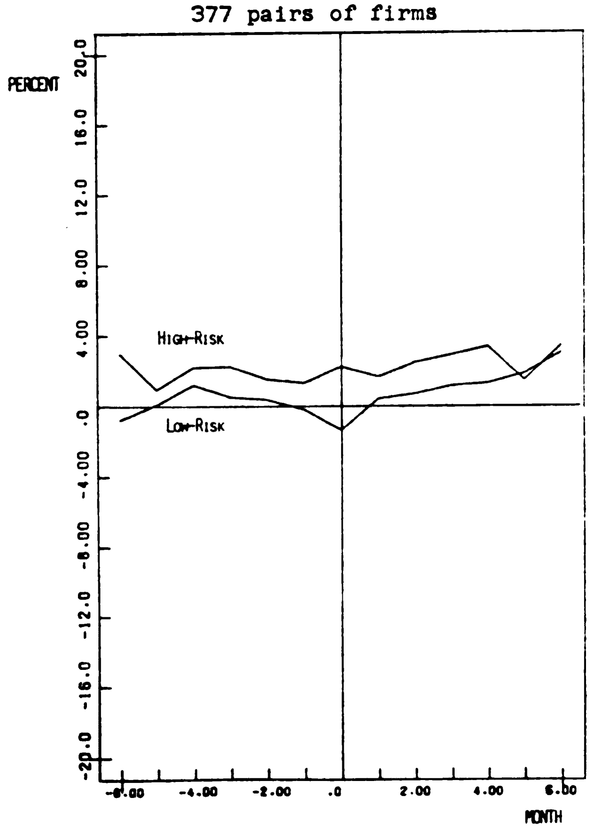

FIGURE 8. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(·,N) - NCF

135 pairs of firms

two percent. When averaged over six months, this return difference is very small indeed.

So (1) the fact that the figures plot cumulative average return differences and (2) the selection of the particular six-month periods for testing suggest that care must be exercised in interpreting the data in the figures in relation to the results of the corresponding statistical tests. In this regard, the results of the T^2 test imply that there is enough variability in the low-risk average return difference for the second six-month period that one could easily observe by chance a difference as large as that obtained in the present analysis. Also, the fact that the high-risk and low-risk plots differ markedly from each other is of no consequence to the tests performed in this study. This is because both average return differences in each figure are formally compared to zero and not to each other.

Somewhat paradoxically, the test for the information content of ACs where a firm made both a discretionary AC and a nondiscretionary AC in the same year yields significant differences between return of AC and NC firms. (See line nine of Table VIII.) The F-value of 6.358 for the test over the first subperiod is significant at less than .10, whereas the F-values for the other two periods are not significant at .25 or less. As can be seen by the risk-class weights of 1.361 and -.361, the high-risk group appears to have contributed more to the significance of the statistic than the low-risk group. Figure 9 below shows that the path of the high-risk cumulative average return difference (CARD) is well below zero and quite stable for the first six months, whereas the low-risk CARD stays very close to zero over the entire test period.

The purpose of including the both (B) category was to see whether it more closely resembles the discretionary group or the nondiscretionary group. If the result of this test were similar to the result of either of the other two tests, it could be interpreted in light of that similarity. But the test result on the both group resembles neither of the other two. The market apparently imputes some negative characteristics to the both group which do not apply in general to discretionary ACFs or to nondiscretionary ACFs.

Inspection of the sample sizes in the tests of ACFs that made both discretionary ACs and nondiscretionary ACs, i.e., ACF (·,B), and of the ACFs that made both types of ACs with an overall positive effect on EPS, i.e., ACF (+,B), may provide a clue as to the market's behavior

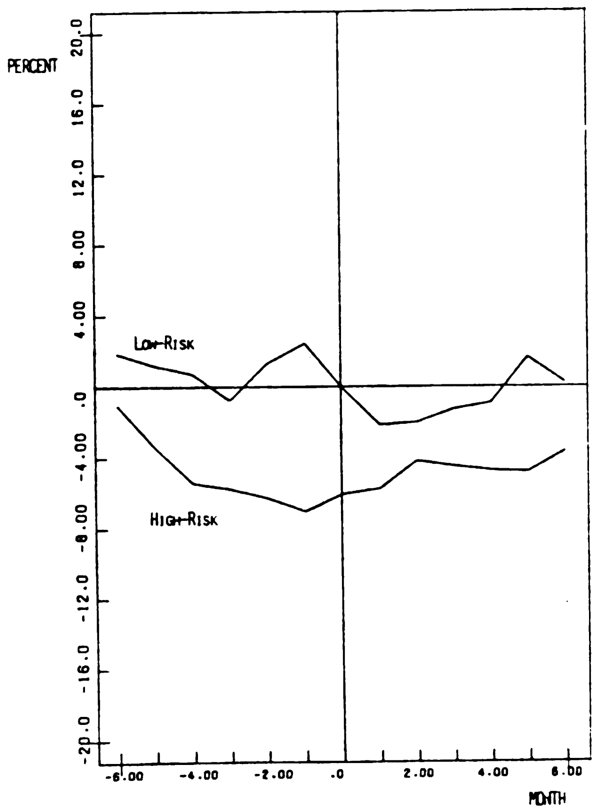

FIGURE 9. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(·,B) - NCF

48 pairs of firms

toward ACFs (·,B). (See line nine of Table VIII and line eighteen of Table IX.) Thirty-two of the forty-eight ACFs (·,B) are also included in the ACF (+,B) category, for which the test results are very similar but more pronounced. (Compare Figures 9 and 18.) Because of this similarity, discussion of possible reasons for the results of ACFs with both types of ACs will not be undertaken here. Rather, the related results of ACF (+,B) will be interpreted in the section on various combinations of directional effects on net income and relative discretion.

Tests for Effects of ACs with Different Directional Effects on EPS and Varying Degrees of Discretion

The tests reviewed above tend, in general, to support one of the theses of this study, namely that tests of the information effect of aggregations of dissimilar types of ACs are likely to result in the inference that ACs are not associated with unique stock market behavior. The results of tests reviewed in this section tend to support the related thesis, that such aggregation hides the unique information effects associated with several types of ACs that are homogeneous with respect to directional effect on EPS and management discretion.

Test of Discretionary ACs That Increase EPS--ACF (+,D). Neither discretionary ACs in general nor ACs that increase EPS in general are associated with unique market behavior. However, ACs with both characteristics do appear to be associated with differential market behavior, as can be seen from line ten of Table IX. The F-value of 3.346 for the full thirteen-month period is significant at less than .10, and the F-value of 6.731 for the second subperiod is significant at close to .05. Thus, it appears that the market perceives some information content associated with discretionary ACs that increase net income. From Figure 10 below it is apparent that the low-risk group of firms contributes more of the overall difference in average returns. This is also evident from the magnitudes of the average return differences on line ten of Table IX. The low-risk differences are clearly larger, both in absolute terms and in relation to their standard errors, than the differences for high-risk firms. Furthermore, the average return differences (of ACF-NCF) are mostly negative. This suggests that the market imputes some negative characteristics to firms that voluntarily make ACs with positive net income effects. Perhaps this result is to be expected in light of recent evidence provided by Bremser. He found that eighty firms

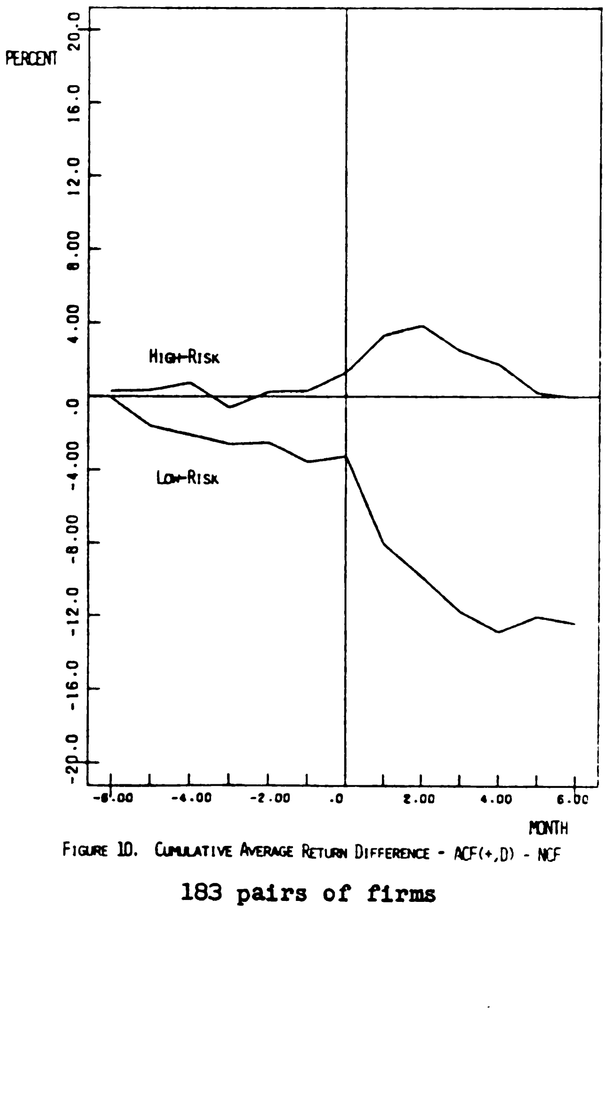

FIGURE 10. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(+,D) - NCF

183 pairs of firms

that made discretionary ACs with a positive effect on EPS exhibited a poorer pattern of EPS and return on stockholders' equity than a randomly selected control group of NCFs.[7] Taken together, these results imply that firms which make ACs (+,D) are less desirable as investments than similar firms that do not make ACs. Thus, it is possible that firms which make discretionary ACs that increase net income do so as a result of poor past performance (in terms of net income and/or stock price activity) or as a prelude to a poor future outlook. Of these two possibilities, the latter is implied here by the fact that the T^2 test for the first subperiod, prior to the year-end AC disclosure, resulted in insignificant differences, while the test for the subperiod after disclosure yielded differences that are significant. (See the two subperiod F-values on line ten of Table IX.) However, the results of Archibald's test on firms that changed to straight-line depreciation suggest that the market's negative behavior toward depreciation switch-back firms began up to nineteen months prior to the change announcement.[8] Also, Kaplan and Roll observed mostly negative market reaction to their sample of ACFs during the thirty weeks prior to release of the year-end earnings summary. Even though the results of these two studies are not strictly comparable to the restuls of the present study, (because of different testing techniques),[9] it appears that the negative relative market behavior toward depreciation method and ITC change firms began prior to the post-year-end disclosure of the ACs.

If firms that make ACs (+,D) do so to conceal otherwise poor performance, it is unlikely that the market is caught completely unaware because of quarterly earnings reports, the multitude of investment letters, and other competing sources of information. One possible explanation for the insignificant results in the first subperiod is that six months prior to the year-end disclosure of ACs may not extend far enough back in time to reveal the negative price activity which might have influenced firms' decisions to make ACs. Or perhaps the market behavior before year-end is simply not strong enough to be statistically significant. Neither of the possibilities with respect to a systematic market effect prior to year-end denies the result of the second subperiod test conducted here, namely that post-year-end disclosures of discretionary ACs with positive income effects appear to convey information that the market uses to price stocks.

The reader may seek an explanation of these results in terms of the accounts affected by the ACs. The

183 ACFs (+,D) made 244 ACs, which are summarized in Table X below.

The discussion that follows examines similarities between this study and previous AC research in an attempt to explain why the market reacts more adversely to the low-risk ACFs than to the high-risk firms in the sample, particularly during the second subperiod (after year-end). The types of ACs on which the two risk-classes differ most are changes to straight-line depreciation (examined by Archibald and by Kaplan and Roll), to flow-through of the ITC (examined by Kaplan and Roll), from LIFO inventory costing (examined by Sunder), and to capitalization of costs. The low-risk group of ACFs in Table X made more depreciation and ITC changes, while the high-risk ACFs made more changes from LIFO. The market reacted negatively after year-end to the ACs examined by Kaplan and Roll.[10] Thus, it could be that the market without knowledge of the depreciation and tax credit ACs was misled into thinking the operating performances of these change firms were better than expected. But when the year-end report disclosed the reason for part of the change firms' net income, the market systematically bid down the share prices of such firms. The market showed no distinct behavioral pattern toward the changes from LIFO examined by Sunder.[11] No AC

TABLE X

NUMBERS OF THE VARIOUS TYPES OF ACs MADE BY ACFs (+,D)[a]

Type of AC	High-Risk ACFs	Low-Risk ACFs
To Straight-line Depreciation	21	32
Pension-related Changes	27	24
To Capitalization of Costs	15	10
To Inclusion of Subsidiary or to Equity from Cost	11	11
From LIFO Inventory Costing	11	5
To Flow-through for Investment Tax Credit	4	10
Increase Depreciable Life of Asset	8	6
All Others (none over 6)	26	23
Total Number of ACs	123	121
Average Increase in EPS per Firm	.120[b]	.107

[a]The sum of ACs of a given type (such as depreciation method, etc.) made by the two risk classes does not necessarily equal the total number of discretionary ACs that increase EPS given in Table V. The difference is made up of the discretionary-positive effects ACs that were made by firms (separately classified as "both") that also made nondiscretionary ACs in the same year.

[b]Excludes three extreme cases in which the ACs increased EPS by 169.6 percent, 198 percent and 472.5 percent. If only the last extreme case (+472.5 percent) is excluded, the average effect per high-risk firm is +15.8 percent.

studies have reported separately the results of tests conducted on ACs involving capitalization of costs that were formerly expensed as incurred. Therefore, one explanation of these results could be the preponderance of changes to straight-line and to flow-through made by the low-risk group, because otherwise the ACs made by the two risk-classes of ACFs (+,D) are quite similar.

It is possible that some variant of the theory underlying the discounted cash flow models could explain why the low-risk firms' values were more strongly affected. That is, the market may have interpreted these particular ACs as indicative of shifts in firm relative riskiness, in which case, other things being equal, there would be a greater impact on the market values of low-risk firms. For an expanded discussion of the reasoning underlying this possibility, see the final section of Chapter 3.

In summary, the results of the tests of the information content of discretionary ACs that increase net income are difficult to compare with the results of previous AC research because only the return diffence (of ACF-NCF) is dealt with in this study. Other researchers have either presented only the API of the change firms or the APIs of change and nonchange firms with no formal comparison of the two. (See in particular footnote nine.)

Also, the fact that several different types of ACs are examined in this study makes comparisons difficult. Notwithstanding the different testing techniques used in this and other studies, there appears to be a negative market reaction (either in relative or in absolute terms) to firms that made discretionary ACs which increase net income. And it also appears that the year-end disclosures of ACs (+,D) convey information that is pertinent to valuing such firms, regardless of the competing sources of information.

Test of Discretionary ACs That Decrease EPS--ACF (-,D). The results of the preceding test suggest that discretionary ACs which increase net income are viewed by the market as attempts to conceal otherwise poor operating performance or as a positive signal regarding NCFs, or both. The fact that the market has been shown in other studies to respond favorably to increases in EPS from non-AC sources suggests that the market views such ACs as attempts to manipulate net income. If this is true, then it appears reasonable that the market may respond favorably to discretionary ACs that reduce reported profits. Such ACs could conceivably be viewed as a show of strength or as a means of smoothing income in unusually successful periods. The negative response to depreciation changes to straight-line and investment tax credit changes to flow-through may be reversed for changes to an accelerated method of depreciation and for changes to deferral of the tax credit, for example. Or, the market may respond favorably to discretionary ACs that reduce taxable income because of a resulting cash savings in tax outlays. This is what Sunder observed for firms that changed to the LIFO method of costing merchandise inventory.[12]

The results of the tests on ACs (-,D) suggest, however, that the market does not assign a higher value to firms which voluntarily make ACs with a negative effect on net income. (See line eleven of Tablx IX.) The F-values for the three periods covered in this study led to rejection of the null hypothesis of no difference only at significant levels of .25 or higher. Moreover, the signs of the average return differences (of ACFs-NCFs) for both risk classes over the full thirteen-month period are negative. This is also evident in Figure 11 below.

The sample of ACs examined here does not provide a rigorous test of the hypothesized positive market reaction to the discretionary ACs which were discussed above. The reason is that very few sample firms made inventory, depreciation, or ITC changes that reduced net

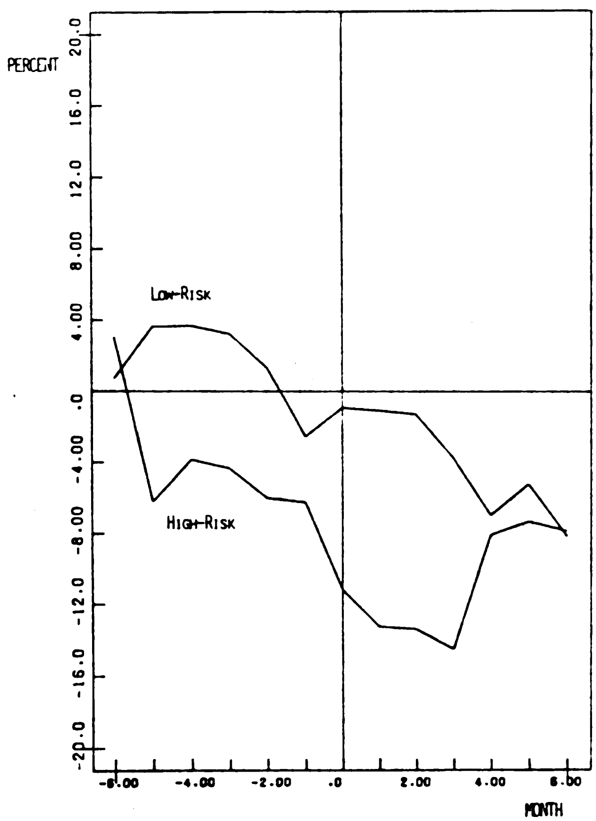

FIGURE 11. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(-,D) - NCF

27 pairs of firms

income. For example, the twenty-seven ACFs (-,D) made only one change to LIFO, four changes to deferral of the ITC, and no changes to an accelerated depreciation method. However, the test does cover the following discretionary ACs that reduced reported profits: eight changes to expense costs as incurred or to accrue expenses previously recognized when paid; four pension changes in method or amortization period; four changes to include subsidiaries or to the equity method from cost; three miscellaneous changes in revenue recognition; and two changes to expense intangibles. The other five types of ACs were each made by only one firm in the sample. On balance, these results suggest that discretionary ACs with a negative effect on net income are not associated with unique stock market effects. But due to the relatively small sample size underlying this test (see figure 11), further evidence should be gathered before making any definitive statements regarding investors' perceptions of such changes.

Test of Discretionary ACs That Do Not Affect EPS--ACF (0,D). The results of tests performed on discretionary ACs that increase net income and on discretionary ACs that reduce net income do not provide a basis for predicting the market's reaction to discretionary ACs which have no effect on net income. The results of the positive-effects test was explained in terms of the income manipulation hypothesis. The results of the negative-effects test did not reveal any unique market behavior, but the small sample size limits the strength of the statements that can reasonably be made about the results of the test. An attempt to apply the income manipulation hypothesis to an explanation of the no-effects test will not be fruitful because such ACs do not appear to have been motivated by the desire to manipulate income. Furthermore, the heterogeneity of the ACs in this category renders such broad generalizations useless. In explaining the results of this test it is necessary to examine the ACs individually in order to better understand why the market reacted uniquely to ACs (O,D).

The results of this test suggest that, for the second subperiod and with the probability of a Type I error less than .10, such ACs are associated with unique stock market behavior (see line twelve of table IX). It appears from the risk-class weights on line twelve of Table IX and from Figure 12 below that the high-risk group of firms contributes more to the overall multivariate average return difference than the low-risk group does. Moreover, the unique market reaction to the

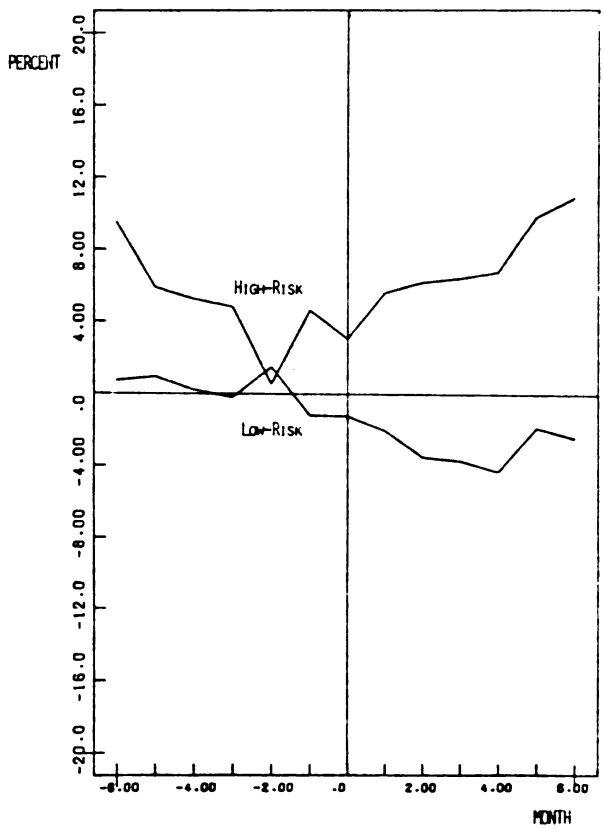

FIGURE 12. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(0,D) - NCF

106 pairs of firms

high-risk firms during the second subperiod is noticeably positive, which implies that the market assigns a systematically higher value to high-risk firms that make discretionary ACs with no income effect, than to a matched group of NCFs. This particular hypothesis group is one for which different test results would probably occur for subperiods other than the two examined in this study. For example, the strong negative movement in the high-risk-cumulative average return difference from t = -6 through -2 would probably be statistically significant by itself, were it not for the interrupting upward movement in month t = -1.

Furthermore, Figure 12 reveals some interesting market behavior which is not formally analyzed in the related statistical tests conducted in this study. It shows roughly offsetting market behavior toward the shares of each of the two risk classes of ACFs (O,D) during months t = -3 through -1. In the case of high-risk firms the market bid down the shares of ACFs in relation to the shares of NCFs during month t = -2 and then returned during the following month to the higher relative level of month t = -3. In the case of low-risk firms the opposite occurs. The market bid up ACF values during t = -2 and then returned to a lower value during t = -1. The market behavior toward high-risk firms is more pronounced during the first subperiod than it is toward low-risk firms, but neither average return difference is statistically different from zero. Nevertheless, Figure 12 reveals some interesting offsetting market behavior during the six months before the year-end AC disclosures.

Table XI below focuses on the ACs made by the high-risk ACFs (O,D) in an attempt to explain why the market values such firms more highly than their control group. It presents the numbers of the various types of ACs made by the fifty-three high-risk firms, along with a subjective judgment as to how the particular ACs in question related to each firm's wealth position.[13]

The messages of AC types T1, T7, and T10 are all neutral because there is nothing economically better about a firm's using consolidation, equity, or cost accounting for equity investments when the income effect is nil. The message of a type T2 AC can be viewed as either favorable or neutral. A favorable impression of a T2 AC could be due to the likelihood that a higher earnings rate on pension assets means the firm needs to fund less of its pension obligation, resulting in a net cash saving. But this interpretation is superficial because the cause of the higher fund earnings during 1968-1972 is almost

TABLE XI

TYPES OF ACs MADE BY HIGH-RISK ACFs(θ,D)

Type Number	Type of AC	Number of ACs	Message
T1	To Inclusion of Subsidiary from Cost or Equity	10	N
T2	Increase Assumed Earnings Rate on Pension Assets[a,b]	8	G or N
T3	To Inclusion of Foreign Subsidiary or to Equity from Cost for Foreign Investee	7	5 G 2 N
T4	To LIFO Inventory Costing	5	G
T5	To LIFO Inventory Costing	5	B
T6	To Straight-Line Deprecation[b,c]	4	B
T7	To Equity from Inclusion of Subsidiary	4	N
T8	To Percent Completion from Completed Contract[d]	2	B
T9	To Percent Completion of Work Performed from Percent Completion of Shipments	1	N
T10	To Equity from Cost	2	N
T11	To GAAP for Insurance Subsidiary	1	N
T12	To Amortization of Past Service Cost[d]	1	G
T13	Shorten Amortization Period of Past Service Cost from 40 to 30 Years[d]	1	G
T14	Reclassify Losses on Foreign Currency as Other Deduction	1	B
T15	Increase Pension Funding Period[a]	1	G
T16	Reclassify Deferred Tax Credit as Reserve	1	N
T17	Change Pension Method	1	N
T18	To Amortization of Goodwill[c]	1	G or B
T19	To Reporting all Assets and Liabilities on Film Contract Rentals	1	N
T20	To Capitalization of Research Development Costs	1	G or B
	Total Number of ACs	58	

G--considered to be "good" or favorable information with respect to a firm's wealth position.
B--considered to be "bad" or unfavorable information with respect to a firm's wealth position.
N--considered to be neither "good" nor "bad" information about a firm, therefore "neutral."

[a]One firm made two pension related ACs.

[b]One firm made a change in the assumed earnings rate on pension assets and also a change to straight-line depreciation.

[c]One firm made a change to straight-line depreciation and also a change to begin amortizing goodwill.

[d]One firm made a change to percentage-of-completion for recognizing revenue on long-term contracts, a change to start amortizing past service cost, and a third change to shorten the amortization period for past service costs.

certainly attributable to a rise in interest rates which is probably related to a rise in the inflation rate. Employees who are covered by pension plans are not unaware of the effect of inflation on their real earnings, and in inflationary times they can be expected to demand greater wages and/or pension benefits which should offset the cash saving from a higher pension fund earnings rate. The message of this AC could therefore be neutral because the overall effect on the firm is nil. Type T12 is believed to convey a favorable impression of the firm that has willfully decided to record an expense. The same reasoning could also be applied to a type T18 AC. On the other hand, the amortization of goodwill could be viewed as the firm's admission that an asset has ceased to exist, an unfavorable piece of information. Type T13 is viewed as good news because the firm is voluntarily hastening the recognition of an expense that is, by definition, related to prior periods. Type T15 conveys good news because of the cash saving from a lower periodic funding requirement. Type T3 reflects favorably on five of the seven firms because the stated underlying reason for the change is improved politico-economic conditions in the domicile of the subsidiary or affiliate. The message for the other two type T3 ACFs is neutral for the same reason given for T1 and T10. Type T4 reflects favorably on a firm because of the cash savings that accompanies a change to LIFO when factor prices are increasing. Type T5 conveys the opposite message about a firm whose inventory prices are increasing. This inference about the T4 changes to LIFO is supported by Sunder's work.[14] Type T6 reflects unfavorably on the firm if the intent is to manipulate earnings. This is implied by the results of the test on discretionary ACs that increase net income, where thirty-two of the 121 low-risk firms, to which the market reacted negatively, changed to straight-line depreciation (see table X). Type T8 suggests the firm is hastening the recognition of revenue, a negative signal. Type T14 is considered to reflect negatively on the ACF because it implies an attempt to hide losses in the "other" section of the income statement. Type T20 is bad news if it is an attempt to arbitrarily defer expense recognition. It is good news if it is motivated by successful research activities which are expected to benefit future periods. The remaining types of ACs are either reclassifications (T11, T16, T19) or are not susceptible to a normative evaluation (T9, T17) of the type accorded the other types. They are rated as neutral.

Based on this rather subjective analysis, it appears from the data in Table XI that the overall reflection of all the ACs on the firms that made them is mildly favorable. On this basis it appears reasonable that an efficient market would have generally responded positively to this group of ACFs. This conclusion is, of course, conditional upon the messages which the ACs convey to the author. Another analyst may disagree with the normative signals inferred from the AC disclosures that the firms used to convey their AC decisions.

Another explanation for these results could be sampling error. The opening comments in this section state that there is little a priori reason for predicting any unique market behavior toward firms that willfully make ACs which do not affect income. Furthermore, the number of "good news" ACs in Table XI is not overwhelmingly greater than the number of "bad news" ACs, especially in relation to the large number which are considered to convey a neutral signal about the firm's wealth position. Out of fifty-four hypothesis tests,[15] around five can be expected to yield spurious results with a significance level of .10. This test may fall into that category. For this reason, the test needs to be replicated in order to determine whether the results are sample-specific or whether such changes do, in fact, reflect favorably on the firms.

Test of Nondiscretionary ACs That Increase EPS--ACF (+,N). The dearth of evidence on the market's reaction to nondiscretionary ACs was a source of motivation for this study. The earlier test for the effect of nondiscretionary ACs in general, provided the expected result--that that level of aggregation may well have hidden any information effects that nondiscretionary ACs with particular directional effects on net income may have.

The test for the effect of nondiscretionary ACs that increase EPS resulted in an F-value of 4.961 which, with a confidence level of greater than .95, suggests that the market assigns a systematically higher value to such firms than to a control group of NCFs. (For details see line fourteen of Table IX.) Figure 14 below reveals that most of market effect occurs during the second subperiod, after the preliminary and/or annual reports have been released.

The timing of the market behavior toward nondiscretionary ACs seems at first somewhat surprising in light of the fact that ninety percent of the ACs in this sample were induced by APB Opinions. Consequently, market agents

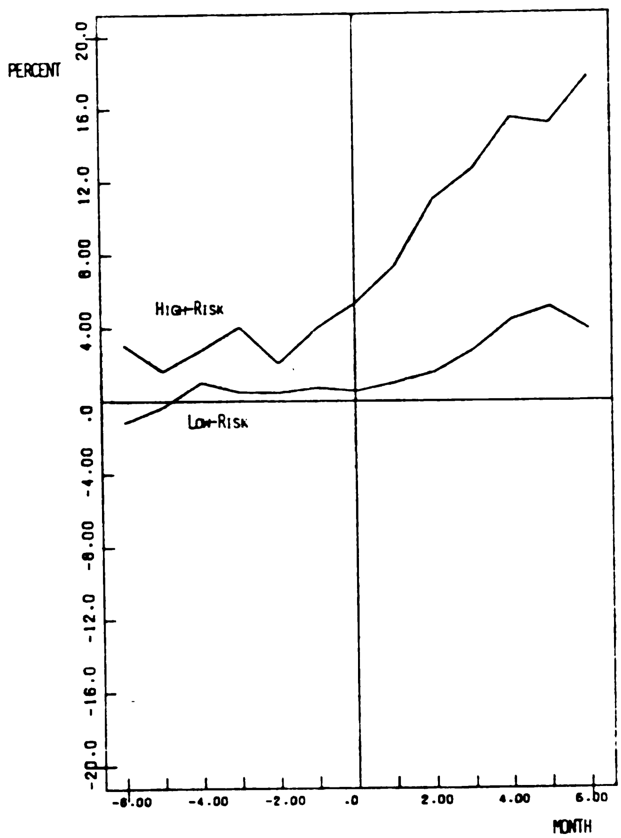

FIGURE 14. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(+,N) - NCF

60 pairs of firms

should have known from exposure drafts and other publicity given the Accounting Principle Board's agenda that these firms would be making ACs. Perhaps they did not act on this knowledge long before year-end because they were unable to predict what effect the ACs would have on net income. The insignificant differences in the first subperiod test, plus the significant results in the post-year-end period, lend support to this proposition. However, a closer inspection of Figure 14 indicates that the unique behavior toward high-risk firms actually began prior to the disclosure of the full year's results. This suggests that analysts and other investors may have used quarterly reports or other media in establishing the higher values for the ACFs in this group. However, inasmuch as Figure 14 plots the <u>cumulative</u> average return difference, the upward trend that continues <u>after year-end</u> suggests that year-end AC disclosures are also associated with unique stock market behavior as well.

One could also hypothesize that the market reaction may have taken place well in advance of the test period examined here. There was generally a time lag of several months between the release of an exposure draft and the effective date of APB opinions. And in many cases there was an additional time lag between the effective date of the opinion and the FYE of affected companies. So it is reasonable to believe that the market may have behaved abnormally toward affected firms prior to the first subperiod examined here. However, even if there was such an anticipation on the part of the market, that does not deny the post-year-end return behavior observed in this test--that year-end disclosures of ACs contained information that was useful for establishing relative prices of firms that made the changes.

The discussion above concerns only the timing of the market reaction; it does not address the reason for the market's favorable response to ACs (+,N). If such ACs are inherently related to increases in the real wealth of firms, then the result is easy to understand. But fifty of the sixty-two ACs made by the sixty firms in this test were changes to the equity method of accounting for investments in common stock, and there is no apparent effect of this change on a firm's cash flows.

Of these fifty ACs, twenty-five were made by the high-risk firms, to which the market reaction was stronger. The thirty high-risk firms also made five additional ACs: three increases in the service lives of assets, one AC to capitalize costs formerly expensed as incurred, and one miscellaneous depreciation change. The market's positive reaction to these ACs could be due to investors' expectations of higher dividends resulting from the higher reported profits that resulted from the ACs. Perhaps the market infers company strength from the fact that ACFs in this cateotry had formerly chosen a more conservative method of accounting and had to be forced to adopt a technique which produces a larger income number. Either market perception could lead to higher relative stock prices for ACFs in this category. In addition to the possible price increase, firms may have actually raised their dividends due to their higher reported net income. Either or both of these factors could account for the higher relative returns on this group of ACFs. Thus, in summary it appears that the market views the EPS improvement from nondiscretionary ACs in a very different light than it does increases from discretionary ACs.

Test of Nondiscretionary ACs That Decrease EPS--ACF (-,N). The test for the information effect of nondiscretionary ACs which decrease EPS resulted in an F-value for the second subperiod that is significant at less than .10 (see line fifteen of table IX). The negative high-risk average return difference for the second subperiod contributes more to the value of the statistic, as can be seen from Figure 15 below.

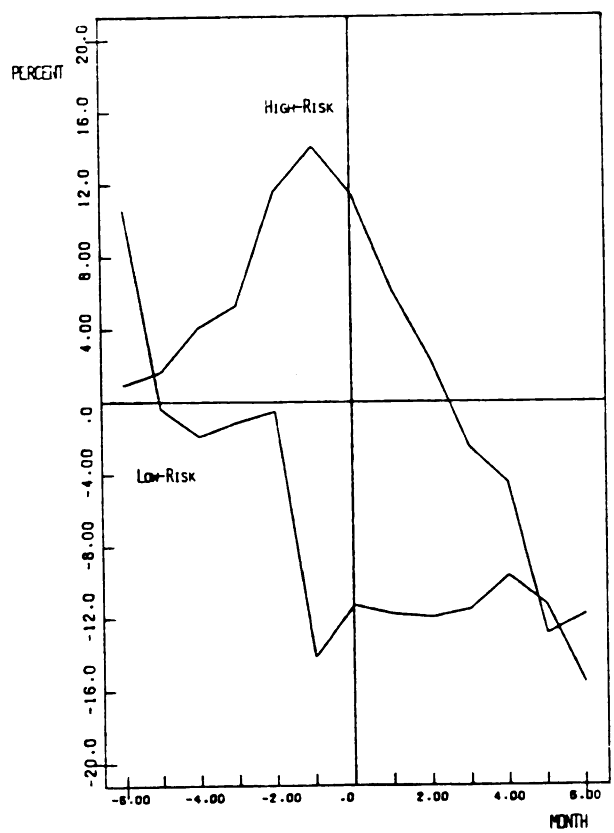

FIGURE 15. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(-,N) - NCF

10 pairs of firms

It is interesting that the market reacted favorably to nondiscretionary ACs with positive income effects (see line fourteen of table IX and figure 14) and unfavorably to <u>these</u> particular nondiscretionary ACs with negative income effects. One possible explanation for the results could be that, since four of the ten ACs in this group were changes to the equity method, the AC disclosures could be investors' initial notification that their firms held stock in other companies which were not earning a profit. This could lead investors to expect lower dividends. Four of the other six ACs were changes to accure income taxes on subsidiaries' undistributed incomes. Another possible explanation of these results could be that the market views with disfavor the fact that these firms had willfully selected an accounting method that produces a higher income number than the more conservative method they were compelled to adopt.

Figure 15 indicates that, for both risk classes of firms, pronounced market movements were reversed in month t = -1. For high-risk firms the subsequent differential (ACF-NCF) return movement was uniformly negative until month t = -5. Apparently investors' favorable expectations up to month t = -1 were not met, and they reacted in a strongly negative manner to the net income decreases that resulted from the ACs (or possible from some other sources). For low-risk firms the first subperiod movement was strongly negative, even considering the positive return differences during months t = -3 and -2. During month t = -1, however, the market's relative reaction toward low-risk ACFs was positive; thereafter there was very little difference between low-risk ACFs and NCFs as can be seen from the nearly horizontal low-risk CARD from t = 0 through +6.

In summary, it appears that month t = -1 was a turning point for both risk classes of firms that made nondiscretionary ACs with negative effects on net income. The fact that this month precedes the assumed disclosure month of t = 0 is consistent with the timing of the unique behavior toward firms whose nondiscretionary ACs increased net income (see figure 14). And this similarity is reasonable because market agents could have anticipated these ACs in advance of the year-end disclosures of them. But here the market reaction to the two risk classes of ACFs is very different, whereas the market reaction to the two risk classes of ACFs charted in Figure 14 is quite similar. Further research should be conducted to ascertain the causes for this observed difference in return performance.

Table IX reveals that the test of ACF (-,N) is based on observations of only ten matched pairs of firms, five in each risk class. Therefore, sampling error is likely to have affected the results of this test to a considerable degree. As a result, the test needs to be replicated on a larger sample before more definitive conclusions are reached concerning the market behavior toward firms whose nondiscretionary ACs decrease EPS.

Test of ACFs Whose Discretionary ACs and Nondiscretionary ACs Increase EPS--ACF (+,B). The test for the market behavior toward firms that made at least one discretionary AC and at least one nondiscretionary AC in the same year (referred to as "Both") resulted in a significant F-value for the first subperiod covered in this study (see line nine of table VIII). At first this result appears to be an aberration because the tests of discretionary ACs in general and of nondiscretionary ACs in general produced insignificant F-values (see lines seven and eight of table VIII). As mentioned in the discussion of the results of tests on the both category, however, the results of this test appear to be dominated by the market behavior toward firms with both types of ACs, where the effect on net income is positive. Compare line nine of Table VIII with line eighteen of Table IX and Figure 9 with Figure 18 below.

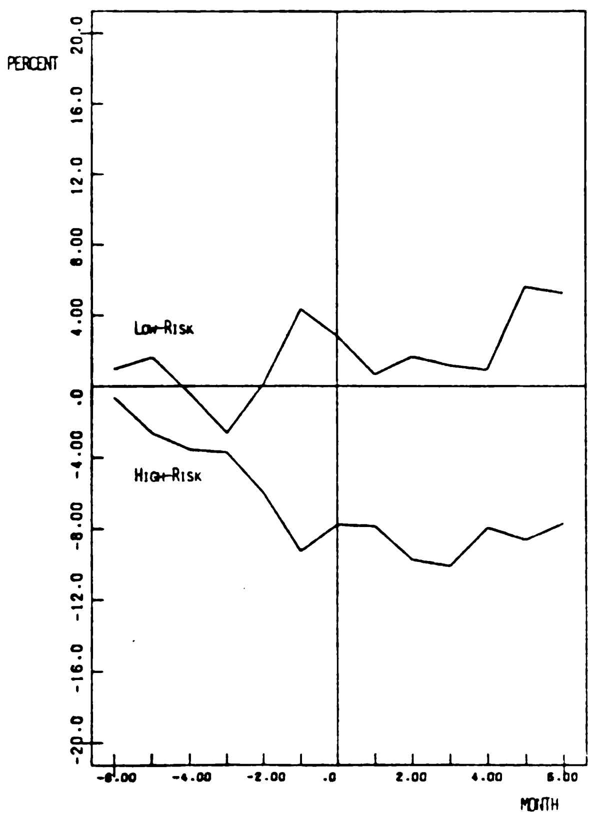

FIGURE 18. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(+,B) - NCF

32 pairs of firms

At this point it becomes necessary to explain the results of the test of both types of ACs with a positive overall effect on EPS. Line eighteen of Table IX indicates that the F-value of 11.553 for the first subperiod is significant with a confidence level of greater than .975. The risk-class weights, along with Figure 18, indicate that the negative average return difference (of ACF-NCF) on the high-risk firms contributes more to the overall multivariate difference than the low-risk difference does. Thus, the discussion which follows chiefly concerns the high-risk firms that made both types of ACs, where the ACs increased net income. To that end Table XII shows a breakdown of the ACs made by firms in this category.

Table XII indicates that the sixteen high-risk firms made seventeen nondiscretionary ACs and twenty-one discretionary ACs. These numbers are comparable to the numbers of discretionary and nondiscretionary ACs made by the low-risk firms, to which the market reaction was slightly favorable (see figure 18). However, the high-risk firms made eighteen discretionary ACs with positive income effects compared to fourteen for the low-risk firms. Earlier it was shown that the market reacts negatively to ACs in this category (see line ten of table IX and figure 10). Moreover, the discretionary ACs made by the high-risk

TABLE XII

DECOMPOSITION OF ACs MADE BY ACFs(+,B)

	High-Risk ACF(+,B)	Low-Risk ACF(+,B)
Number of Firms	16	16
Number of Nondiscretionary ACs	17	16
Effects on EPS		
Positive	11	11
Negative	3	1
Zero	1	1
Not Disclosed	2	3
Average Effect on EPS Per Firm	+.132	+.044
Adjusted Average Effect on EPS Per Firm[a]	+.055	+.044
Number of Discretionary ACs	21	22
Effects on EPS		
Positive	18	14
Negative	1	3
Zero	1	3
Not Disclosed	1	2
Average Effect on EPS Per Firm	+.079	+.048
Average Effect of Both Types of ACs on EPS Per Firm	+.211	+.092
Adjusted Average Effect of Both Types of ACs on EPS Per Firm[a]	+.134	+.092

[a]One high risk firm switched to the equity method of accounting for investments in stock (a nondiscretionary AC) and increased its EPS by 128.6 percent. Deletion of this extreme case reduces the average EPS effects to those referred to as "adjusted average effect . . . per firm."

firms increased EPS by an average of 7.9 percent, compared to 4.8 percent for the low-risk firms. Another relevant piece of information not given in Table XII is that the thirty-two ACFs in this test made only four nondiscretionary ACs that decreased EPS. In every one of the four cases, the firms also made a discretionary AC which more than offset the negative EPS effect of the nondiscretionary AC. Three of these cases occurred in the high-risk group. Thus, on balance, the results are consistent with the notion that market behavior associated with ACF (+,B) is influenced by the perceived manipulative intent of the discretionary ACs which increase net income. If this is true, it then becomes necessary to explain why the market reactions to high-risk (ACFs (+,B) and to low-risk ACFs (+,D) were similar. The similarity probably results from the fact that the ACs made by these two groups of firms have more potentially manipulative characteristics in common than do the two groups of high-risk ACFs or the two groups of low-risk ACFs.

The above interpretation does not address the question as to why the market's negative behavior occurs during the six months prior to the year-end disclosure of ACs. Perhaps, as Bremser observed for firms that made discretionary ACs with positive income effects, the firms that made both types of ACs were also less successful in terms of profitability than their control group. In this case, the negative stock price activity during the latter half of the fiscal years of high-risk firms may have induced them to increase their EPS by making discretionary ACs.

The fact that most of their nondiscretionary ACs also increased net income could account for the tapering off of the market's negative behavior in the six months after the year-end disclosure month (see figure 18).[16] Thus, it is possible that, for both risk classes, the potentially offsetting market effects of the discretionary and nondiscretionary ACs could explain the essentially horizontal movement in the cumulative average return differences during the second subperiod as revealed in Figure 18.

<u>Other Mean Vector Tests Not Discussed Previously</u>. The results of three tests have not been discussed thus far. They are the tests of (1) discretionary ACs for which the income effect was not disclosed (see line thirteen of table IX and figure 13 below), (2) nondiscretionary ACs with no effect on net income (see line sixteen of table IX and figure 16 below), and (3) nondiscretionary ACs for

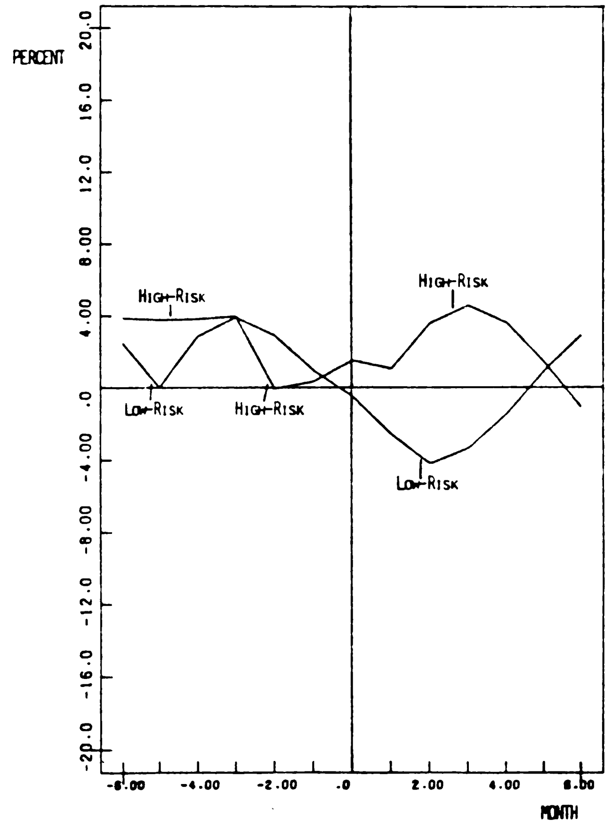

FIGURE 13. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(?,D) - NCF

61 pairs of firms

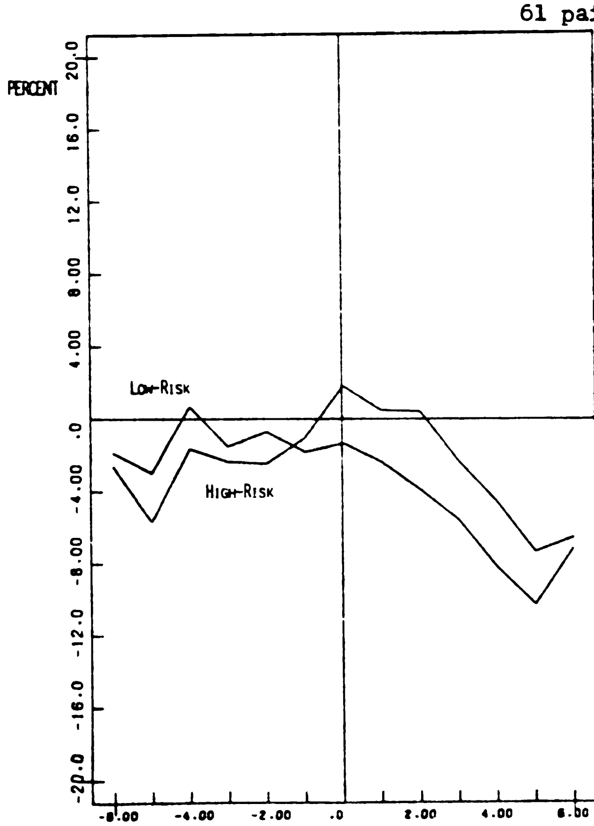

FIGURE 16. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(O,N) - NCF

30 pairs of firms

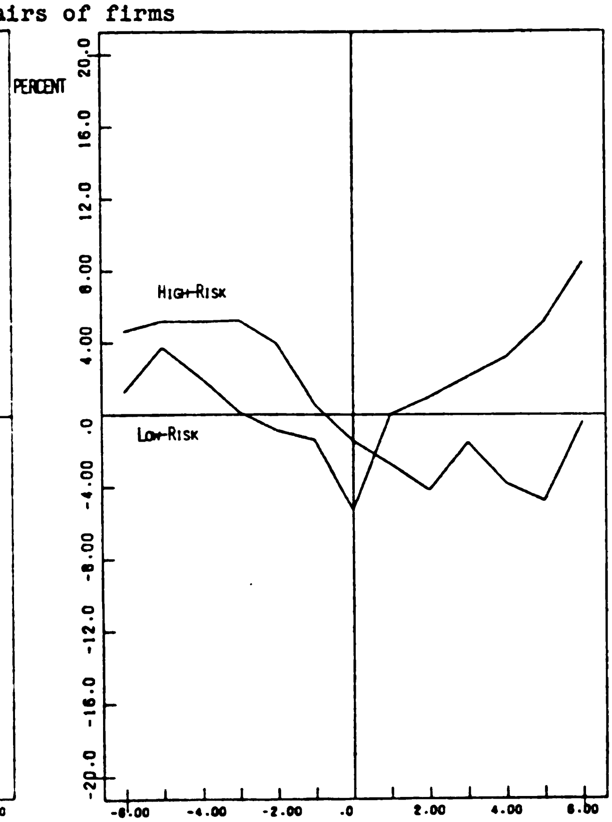

FIGURE 17. CUMULATIVE AVERAGE RETURN DIFFERENCE - ACF(?,N) - NCF

35 pairs of firms

which the effect on net income was not disclosed (see line seventeen of table IX and figure 17 below). The F-values for all of these tests suggest that the market reactions to these types of ACs are, in all likelihood, governed by a random process.

The only one of the above tests which lends itself to a ready a priori belief as to the outcome is the first one. It was reasoned earlier that the market may view a firm's nondisclosure of the income effect of its discretionary AC as a negative signal about that firm because nondisclosure may be viewed as an attempt to hide unfavorable news about the firm. A competing belief is that firms choose not to disclose the income effect because it is immaterial. The test results tend to support the latter belief. Except for case (1) above, there are no compelling reasons for predicting a market response to the above three categories of ACs, and accordingly the three tests are not discussed further.

Tests on Covariance Matrices

The first part of this chapter covers the empirical results and interpretations of the mean vector tests called for by the research design utilized in this study. This part of the chapter discusses the results of tests conducted on covariance matrices of returns, which define the variances of the mean vectors of returns on ACFs and NCFs. (See the Chapter 5 section entitled Tests on Covariance Matrices for a discussion of the nature of the covariance matrices being examined here.) Inequality between the two groups (in general ACFs and NCFs) on either parameter, the mean vector or the covariance matrix, is sufficient for rejecting the null hypothesis (Eq. 4 or Eq. 5) of no information content associated with ACs.

The need to examine variances as well as means in order to assess the information effects of accounting events was discussed initially in Chapter 1, and also in the final section of Chapter 2. Chapter 5 covered the statistical procedures to be followed in performing the tests for equality of two covariance matrices. That section also discussed the need to establish that the unconditional covariance matrices, Σ_1 and Σ_2 (computed over the sixty-month β-estimation period), were equal in order to be able to attribute impact-period differences between the conditional covariance matrices, $\Sigma_1|\underline{\theta}_1$ and $\Sigma_2|\underline{\theta}_2$, to the information variable $\underline{\tilde{\theta}}_i$. Thus, this section of Chapter 6 is divided into two subsections, the first covering tests on unconditional covariance matrices of returns and the second covering tests on covariance matrices of returns conditional on the information effects of ACs.

Tests for Equality of Unconditional Covariance Matrices during the β-Estimation Period

In Chapter 5 it was asserted that satisfaction of the condition $\Sigma_1 = \Sigma_2$ provides a basis for inferring that $\tilde{\underline{\theta}}_i$ has information content if $\Sigma_1|\underline{\theta}_1 \neq \Sigma_2|\underline{\theta}_2$. On the other hand, the condition $\Sigma_1 \neq \Sigma_2$ fails to provide the basis for ascribing differences between $\Sigma_1|\underline{\theta}_1$ and $\Sigma_2|\underline{\theta}_2$ to $\tilde{\underline{\theta}}_i$ because there were differences between Σ_1 and Σ_2 before the information variable $\tilde{\underline{\theta}}_i$ was introduced. As a result, $\Sigma_1|\underline{\theta}_1 \neq \Sigma_2|\underline{\theta}_2$ may be due to either $\tilde{\underline{\theta}}_i$ or to other exogenous factors which cause the variability in $\Sigma_1|\underline{\theta}_1$ to differ from that in $\Sigma_2|\underline{\theta}_2$. In the event that $\Sigma_2 \neq \Sigma_2$, there is no unambiguous conclusion from the test of H_o^b: $\Sigma_1|\underline{\theta}_1 = \Sigma_2|\underline{\theta}_2$.

The results of the test for equality of covariance matrices during the β-estimation period are given in Table XIII in the column, $t = -66$ through -7, which indicates that four of the chi-square statistics[17] are significant at .01 or less. These tests are identified on lines three, four, eleven, and eighteen of Table XIII.

TABLE XIII

SUMMARY STATISTICS FOR TESTS ON COVARIANCE MATRICES

Line Number	Hypothesis Group	t = 66 through -7 Chi-square Value[a]	t = -6 through -1 F-value[b]	t = +1 through +6 F-value[b]	t = -6 through +6 F-value[c]
Omnibus					
(1)	ACF(·,·) - NCF	1.015	.046	.397	.068
Direction of Income Effect					
(2)	ACF(+,·) - NCF	1.024	.120	.006	.105
(3)	ACF(-,·) - NCF	15.993	NA	NA	NA
(4)	ACF(-,·) - ACF(+,·)	12.052	NA	NA	NA
(5)	ACF(0,·) - NCF	4.469	.078	.191	.226
(6)	ACF(?,·) - NCF	6.489	.071	.223	.272
Relative Discretion					
(7)	ACF(·,D) - NCF	3.434	.492	.762	.182
(8)	ACF(·,N) - NCF	1.425	1.112	.817	.424
(9)	ACF(·,B) - NCF	7.168	NA	NA	NA
Direction of Income Effect and Relative Discretion					
(10)	ACF(+,D) - NCF	1.670	.388	.130	.099
(11)	ACF(-,D) - NCF	20.423	NA	NA	NA
(12)	ACF(0,D) - NCF	6.311	NA	NA	NA
(13)	ACF(?,D) - NCF	6.456	NA	NA	NA
(14)	ACF(+,N) - NCF	..844	.847	.537	.932
(15)	ACF(-,N) - NCF	3.931	1.026	.151	.067
(16)	ACF(0,N) - NCF	7.214	NA	NA	NA
(17)	ACF(?,N) - NCF	4.353	.354	.170	.101
(18)	ACF(+,B) - NCF	13.890	NA	NA	NA

[a]Selected fractiles of Chi-square with 3 degrees of freedom are:

Fractile	Value of χ^2_3
.900	6.251
.950	7.815
.975	9.837
.990	11.341
.999	16.268

[b,c]Selected fractiles of $F_{3;\ 13,000}$[b] and $F_{3;\ 104,680}$[c] are both:

Fractile	Value of $F_{3,\infty}$
.900	2.86
.950	3.98
.975	5.26
.990	7.21

NA--Not applicable because the antecedent conditions necessary for this test were not met.

When the significance level is raised to .10, five more of the chi-square values are significant. They are for the tests identified on lines six, nine, twelve, thirteen, and sixteen of Table XIII. These results indicate that because $\Sigma_1 \neq \Sigma_2$ over the β-estimation period, the two unconditional return distributions differ in unsystematic variability. A complete analysis of the reasons for the differences between Σ_1 and Σ_2 is not warranted because the test was conducted in order to determine whether the antecedent conditions of the tests of $\Sigma_1|\underline{\theta}_1 = \Sigma_2|\underline{\theta}_2$ were met. If the .10 level is retained, then the unconditional covariance matrices Σ_1 and Σ_2 are equal for only nine of the eighteen hypothesis groups. This means, of course, that only these nine tests of $\Sigma_1|\underline{\theta}_2 = \Sigma_1|\underline{\theta}_2$ can be performed in the manner called for by the research design developed earlier.

Tests for Equality of Conditional Covariance Matrices during the Thirteen-Month Impact Period

Because of the statistically significant differences found to exist between the unconditional covariance matrices, Σ_1 and Σ_2, for half of the eighteen hypothesis groups examined in this study, only a partial inquiry was made into the equality of the conditional covariance matrices, $\Sigma_1|\underline{\theta}_1$ and $\Sigma_2|\underline{\theta}_2$. For the nine hypothesis groups where the two $\tilde{\Sigma}_i$ differ, it is not appropriate to infer that realized differences between $\tilde{\Sigma}_i|\tilde{\underline{\theta}}_i$ are associated with ACs.

Table XIII indicates that none of the remaining twenty-seven F-values[18] for the tests of $\Sigma_1|\underline{\theta}_1 = \Sigma_2|\underline{\theta}_2$ is significant at .25 or less. At face value the inference is that for all those hypothesis groups $\Sigma_1|\underline{\theta}_1 = \Sigma_2|\underline{\theta}_2$ with a confidence level of .75 or greater.[19] But this inference may not be warranted. The tests over the β-estimation period and the tests over the impact period both use the M-statistic, which is affected by sample size (see eq. 10). The far larger sample size (i.e., sixty monthly observations) in the test on <u>unconditional</u> covariance matrices versus the six and thirteen monthly observations in the test on <u>conditional</u> covariance matrices gives the former test more statistical power than the latter tests. Therefore, the tests of $\Sigma_1|\underline{\theta}_1 = \Sigma_2|\underline{\theta}_2$ may not be powerful enough to detect true differences between the two conditional covariance matrices.

In summary, the results of tests for the equality of conditional covariance matrices are indeterminate. However, the loss is not as great as it may appear at first.

In Table XIII the tests identified on lines nine, twelve, and eighteen yielded results in the mean vector tests which suggest that the return distributions of firms which made those types of ACs differ significantly from the return distributions of their matched NCFs. Therefore, the results of tests on conditional covariance matrices for these three types of ACs, for the time periods when significant differences were observed, could provide only limited additional insights into the information effects of ACs. For the other types of ACs, however, a design different from the one used in this study will be needed to provide more convincing evidence on the effects that ACs have on the variances of security returns.[20]

Risk and Information Effects

One of the principal general hypotheses of this study is that the information effects of ACs may be risk-dependent. This possibility served as the reason for dividing firms into two risk classes and conducting multivariate tests instead of the univariate tests other researchers have used. The final section of Chapter 3 provides the rationale for believing such a relationship may exist. In general, that discussion hypothesizes that similarities in relative risk are pervasive in the sense that firms with homogeneous risk characteristics may be prone to make similar types of ACs that have similar stock market consequences.

In the first section of this chapter ACs of several types (of directional effect on net income and relative management discretion) have been shown to have information content. The discussions of those ACs hint strongly that the information effects are stronger for one risk class than for the other. Moreover, the graphical results bear this out quite vividly. In some cases, even though the multivariate tests on mean vectors indicate no significance, there appears to be rather marked differences in the way the market reacted to similar ACs made by high-risk and low-risk firms. See in particular Figures 2, 5, and 7. Table XIV below presents a summary of the hypothesis tests for which information effects were inferred, along with the market reaction to the risk class that was more affected.

The market's negative reaction to low-risk firms that made discretionary ACs with positive income effects (Figure 10) was explained by the fact that they made a greater number of changes to the straight-line method of depreciation and to flow-through for the ITC than high-risk firms did (see table X). These results generally

TABLE XIV

MARKET REACTION TO THE RISK CLASS MORE AFFECTED BY ACs WITH

SIGNIFICANT F-VALUES IN THE TESTS ON MEAN VECTORS

Line Number	Hypothesis Group	Risk Class More Affected	Market Reaction
(1)	ACF(+,D) - NCF	Low	Negative
(2)	ACF(0,D) - NCF	High	Positive
(3)	ACF(+,N) - NCF	High	Positive
(4)	ACF(-,N) - NCF	High	Negative
(5)	ACF(+,B) - NCF	High	Negative
(6)	ACF(·,B) - NCF	High	Negative

seem to support the findings of studies conducted by Kaplan and Roll and by Archibald, who also observed a negative reaction to these particular types of discretionary ACs.

The results of the test on firms that made both discretionary and nondiscretionary ACs in one year (Figure 9), as well as the test on the both-positive category of firms (Figure 18) was explained in terms of the large number of discretionary ACs with positive income effects made by the high-risk group of firms. Also, the high-risk firms in both of these tests made discretionary ACs with somewhat larger percentage increases in net income than did the low-risk firms (see table XII). Thus, in the three tests noted on lines one, five, and six of Table XIV, it appears that the risk-dependency can be explained in terms of the income-manipulation hypothesis.

The market reacted favorably to nondiscretionary ACs with positive income effects (Figure 14), but demonstrably so only for the high-risk group. The explanation given for the positive market effect was that such firms had formerly adopted accounting techniques which produced a lower net income number than the method they were compelled to accept. Market agents may have (currently) viewed the firms' former election to "understate" profits as a show of strength, and this knowledge in turn could have led them to expect higher dividends or perhaps to view the firms as less risky. Other things being equal, either expectation would seem to reflect favorably on those firms as investments. Around eighty percent of the ACs made by both risk classes were changes to the equity method, prompted by APB Opinion No. 18. The only apparent difference between the two risk classes of firms is in the percentage effects their ACs had on net income. The average effect on the net incomes of high-risk firms was +8.3 percent,[21] as compared to +4.1 percent for the low-risk firms. Thus, it appears that the more strongly positive market effect observed for the high-risk firms is related to this difference. Even though the AC, per se, does not enhance the wealth position of the firm, disclosure of the AC could have been investors' initial notification that their firms were earning, on the average, 8.3 percent more net income than they had thought. The risk dependency of the information effect of these ACs seems to stem from the fact that the high-risk firms were less willing than low-risk firms to use the latitude in generally accepted accounting principles to report a higher profit. This argument could be characterized as a negative version of the income manipulation hypothesis.

It is interesting that in this test of nondiscretionary ACs that increase net income, the high-risk firms appear to be less prone to use ACs to manipulate EPS (see line three of table XIV), while in the tests of ACs (both) and ACs (both-positive) (lines five and six), the high-risk class was shown to be the one which did use ACs to improve reported profits to the greater extent. These opposite test results point toward conflicting conclusions with respect to the AC behavior of firms with high risk coefficients. Additional work will have to be done in this area before the nature of the relationship is more fully understood.

The results of the other two tests where information content was inferred do not lend themselves to such plausible explanations of why the market reacted to only one of the two risk classes (see lines two and four of table XIV). For example, the discussion of the results of the discretionary-zero effects ACs (Figure 12) ignored the low-risk group of firms and only mentioned the high-risk firms, to which the stronger market reaction was positive. The overall impression conveyed by the high-risk firms' AC disclosures was favorable with respect to those firms' wealth positions (see table XI). An analysis similar to the one underlying Table XI was also performed on the ACs made by the low-risk firms. Conditional on the appropriateness of the author's interpretation of the relative messages of those ACs, the results suggest that the market should have reacted even more favorably to low-risk firms' ACs than to those of high-risk firms. This set of results remains a puzzle.

The results in the test of nondiscretionary ACs with negative income effects also provide an ambiguous signal as to the reason for the risk-dependency of this AC information. In that test the market was shown to react negatively after year-end to the high-risk group of firms (Figure 15). Their ACs hardly differed from the low-risk firms' ACs either as to accounts affected or as to the percentage effect on EPS, but the ambiguity of these results are not considered too surprising in view of the extremely small number of firms involved in this test.

In the tests in which there is no clear reason for the risk-dependency, as is indeed the case for all the other tests conducted in this study, the presence or absence of an auditor's consistency exception could have affected the market's reaction to the firms that made ACs. In Chapter 3 Baskin's study was reviewed, and it was asserted that his research design did not distinguish between the AC or the consistency exception as the independent variable. As a result, it is not clear which event

Baskin was measuring. To some degree this study is subject to the same criticism because a clear distinction between ACs and consistency exceptions was not made. It seems safe to assume, however, that this study measures the effects of ACs because all 560 firms did make ACs, while only 289 of the firms received consistency exceptions. This means that a little under half of the ACFs did not receive consistency exceptions from their auditors. Additional work is under way to determine whether auditors' consistency exceptions are also associated with unique stock market effects.

The foregoing discussion summarizes the tests for which information content in ACs was inferred and gives possible reasons for the observed risk dependency of the information. The second phase of the inquiry into risk and information effects is summarized in Table XV below. It gives, for the full thirteen-month period only, the F-values of mean vector tests using five different values of the weight vector, $\underline{w}_r$, in Eq. 6. The column labeled "Max t^2" pertains to the weight vector which is implicit in the T^2 test (see the chapter section entitled <u>tests on mean vectors</u>). The F-values in this column are the same as those in Tables VIII and IX for the full thirteen-month period. The column labeled "High-Risk"

TABLE XV

RESULTS OF TESTS ON MEAN VECTORS FOR DIFFERENT OVERALL GROUP RISKS

Hypothesis Group	$F_{2,11}$[a] for t = -6 to +6				
	Max t^2	High-Risk	Low-Risk	Equal Weight	Overall $\hat{\beta}_g$ Equal One
Omnibus					
ACF(·,·) - NCF	1.064	.099	1.011	.067	.898
Direction of Income Effect					
ACF(+,·) - NCF	1.614	.228	1.584	.688	1.612
ACF(-,·) - NCF	.618	.286	.208	.569	.393
ACF(-,·) - ACF(+,·)	.215	.202	.045	.189	.104
ACF(0,·) - NCF	.989	.304	.881	.037	.744
ACF(?,·) - NCF	.173	.004	.144	.035	.133
Relative Discretion					
ACF(·,D) - NCF	1.619	.122	1.547	.267	1.465
ACF(·,N) - NCF	.811	.209	.420	.676	.527
ACF(·,B) - NCF	.388	.365	.0003	.147	.036
Direction of Income Effect and Relative Discretion					
ACF(+,D) - NCF	3.346	.0003	2.697	3.007	2.904
ACF(-,D) - NCF	1.311	.157	.536	.863	.857
ACF(0,D) - NCF	.428	.327	.143	.176	.085
ACF(?,D) - NCF	.079	.010	.072	.014	.072
ACF(+,N) - NCF	4.961	3.792	.622	4.960	.980
ACF(-,N) - NCF	.644	.309	.236	.591	.147
ACF(0,N) - NCF	.461	.295	.457	.420	.460
ACF(?,N) - NCF	.472	.002	.407	.132	.336
ACF(+,B) - NCF	.894	.874	.179	.060	.065

[a]See Table VIII in appendix A for the fractiles of $F_{2,11}$ when the null hypothesis is true.

pertains to the weight vector, $\underline{w}_r' = [1\ 0]$, which is analogous to the placement of one's entire investment in the high-risk group of securities. The column labeled "Low-Risk" pertains to the weight vector, $\underline{w}_r' = [0\ 1]$, which is analogous to placing one's entire investment in the low-risk securities in the sample. The column entitled "Equal Weight" pertains to the weight vector, $\underline{w}_r' = [1/2\ 1/2]$, which corresponds to equal investments in the high- and low-risk groups of firms. The final column of Table XV, entitled "Overall $\hat{\beta}_g$ Equal One" pertains to the weight vector, $\underline{w}_r' = [x\ y]$, where x and y sum to unity and they also combine the high- and low-risk groups' $\hat{\beta}_g$ (in the risk vector $\hat{\underline{\beta}}_g$) for each hypothesis group to produce an average $\hat{\beta}_g$ of one for the two risk classes in each hypothesis group.

The purpose of examining the different F-values which result from mean vector tests using different values of $\underline{w}_r$ is to investigate whether the widely different values of $\underline{w}_r$ give rise to widely different F-values. If they do, this would imply that AC information is risk-dependent. On the other hand, if they do not, then there is reason to believe that AC information is constant across the two risk classes being examined. Consistent with the discussion above, Table XV indicates that different values of the weight vector are associated with different F-values. This is evident for virtually all eighteen hypothesis groups in Table XV. On the basis of these summary statistics, it appears that there are some risk dependencies. However, as mentioned in the earlier part of this discussion, these results are only a first step and do not provide a cohesive description of the nature of the relationship between risk and the information effects of ACs. That will have to await additional investigation which is beyond the scope of this study.

Appendix A

Appendix A

Summary Statistics for Tests on Mean Return Vectors

Table VIII

Summary Statistics for Tests on Mean Return Vectors

Line Number	Hypothesis Group	Number of Pairs	t = -6 through -1: Average Return Difference High-β	Average Return Difference Low-β	Weights in T^2 High-β	Weights in T^2 Low-β	F-value[a]	t = +1 through +6: Average Return Difference High-β	Average Return Difference Low-β	Weights in T^2 High-β	Weights in T^2 Low-β	F-value[a]	t = -6 through +6: Average Return Difference High-β	Average Return Difference Low-β	Weights in T^2 High-β	Weights in T^2 Low-β	F-value[b]
	[illegible]																
(1)	ACF(·,·) - NCF	560	.00084 (.00734)[c]	-.00234 (.00259)[c]	-.089	1.089	.344	.00155 (.00156)[c]	-.00274 (.00342)[c]	1.032	-.032	.395	.00154 (.00331)[c]	-.00297 (.00200)[c]	-.166	1.166	1.064
	Direction of Income Effect																
(2)	ACF(+,·) - NCF	275	-.00006 (.00328)	-.00291 (.00127)	.043	.957	2.145	.00139 (.00364)	-.00684 (.00545)	.488	.512	1.016	.00168 (.00239)	-.00469 (.00252)	.137	.863	1.614
(3)	ACF(-,·) - NCF	43	-.00267 (.01796)	-.00254 (.00844)	.175	.825	.043	-.00848 (.01050)	-.00695 (.00708)	.393	.607	1.813	-.00737 (.00934)	-.00340 (.00505)	.376	.624	.618
(4)	ACF(-,·) - ACF(+,·)	30	.00736 (.00913)	.00100 (.01105)	1.378	-.378	.292	-.01289 (.01555)	-.00938 (.01214)	.467	.533	.416	-.00596 (.00898)	-.00136 (.00756)	.753	.247	.215
(5)	ACF(0,·) - NCF	142	.00687 (.01592)	-.00473 (.00534)	-.043	1.043	.317	.00699 (.00397)	-.00376 (.00345)	-48.097	49.097[d]	4.048	.00592 (.00727)	-.00330 (.00381)	-.180	1.180	.989
(6)	ACF(?,·) - NCF	100	-.00094 (.00968)	-.00001 (.00507)	-1.889	2.889	.011	-.00067 (.00497)	.01112 (.00425)	.168	.832	2.892	-.00047 (.00478)	.00251 (.00448)	-.633	1.633	.173
	Relative Discretion																
(7)	ACF(·,D) - NCF	377	.00258 (.00813)	-.00445 (.00370)	-.256	1.256	.713	.00200 (.00279)	-.00770 (.00552)	.236	.764	.793	.00195 (.00379)	-.00551 (.00300)	-.207	1.207	1.619
(8)	ACF(·,N) - NCF	135	.00218 (.00707)	-.00034 (.00342)	.987	.013	.038	.00205 (.00545)	.00741 (.00259)	.070	.930	3.351	.00267 (.00396)	.00236 (.00247)	.335	.665	.811
(9)	ACF(·,B) - NCF	48	-.01177 (.00335)	.00404 (.00605)	1.361	-.361	6.358	.00397 (.00323)	.00044 (.00678)	.820	.180	.764	-.00283 (.00317)	.00012 (.00454)	.857	.143	.388

[a,b] Selected fractiles of $F_{2,4}$ and $F_{2,11}$ are:

Fractile	Value of $F_{2,4}$	Value of $F_{2,11}$
.900	4.32	2.86
.950	6.94	3.98
.975	10.60	5.26
.990	18.00	7.21

[c] Standard error of average return difference estimate immediately above.

[d] This unusual set of weights was recomputed and found to be correct.

Table II

Summary Statistics for Tests on Mean Return Vectors - Continued

Line Number	Hypothesis Group Direction of Income Effect and Relative Discretion	Number of Pairs	t = -6 through -1 Average Return Difference High-β	 Low-β	 Weights in T^2 High-β	 Low-β	 F-value[a]	t = +1 through +6 Average Return Difference High-β	 Low-β	 Weights in T^2 High-β	 Low-β	 F-value[a]	t = -6 through +6 Average Return Difference High-β	 Low-β	 Weights in T^2 High-β	 Low-β	 F-value[b]
(10)	ACF(+,D) - HCF	183	.00049 (.00310)[e]	-.00601 (.00249)[e]	-.347	1.347	2.596	-.00232 (.00550)[e]	-.01516 (.00772)[e]	.554	.446	6.731	-.00007 (.00292)[c]	-.00952 (.00392)[c]	.372	.628	3.346
(11)	ACF(-,D) - HCF	27	-.01052 (.01790)	-.00437 (.00943)	.366	.634	.402	.00548 (.01237)	-.01203 (.00804)	.048	.952	.900	-.00610 (.01043)	-.00632 (.00585)	.315	.685	1.311
(12)	ACF(0,D) - HCF	106	.00759 (.02129)	-.00219 (.00612)	-.399	1.399	.080	.01307 (.00493)	-.00213 (.00558)	2.761	-1.761	5.596	.00830 (.00793)	-.00703 (.00364)	-1.633	2.633	.478
(13)	ACF(?,D) - HCF	61	.00055 (.01023)	.00155 (.00934)	-.181	1.181	.011	-.00434 (.00783)	.00564 (.00796)	-.290	1.290	.205	-.00084 (.00579)	.00221 (.00559)	-.467	1.467	.079
(14)	ACF(+,N) - HCF	60	.00658 (.00408)	.00110 (.00377)	.407	.593	.466	.02062 (.00561)	.00560 (.00404)	.611	.389	6.655	.01361 (.00473)	.00295 (.00253)	.508	.492	4.961
(15)	ACF(-,N) - HCF	10	.02345 (.00849)	-.02363 (.03600)	1.068	-.068	3.271	-.03872 (.01301)	-.00717 (.00854)	.552	.448	4.840	-.00910 (.01109)	-.01200 (.01674)	.625	.375	.644
(16)	ACF(0,N) - HCF	30	-.00182 (.01086)	-.00313 (.00708)	-.647	1.647	.058	-.01358 (.00607)	-.00973 (.00844)	1.490	-.490	2.547	-.00510 (.00636)	-.00557 (.00558)	.114	.886	.461
(17)	ACF(?,N) - HCF	35	.00078 (.01078)	-.00252 (.00712)	-.662	1.662	.078	.00167 (.01095)	.02284 (.00709)	-.036	1.036	4.164	-.00042 (.00694)	.00639 (.00678)	-.571	1.571	.472
(18)	ACF(+,B) - HCF	32	-.01549 (.00490)	.00722 (.01049)	.731	.269	11.553	.00010 (.00377)	.00405 (.00953)	.392	.608	.085	-.00594 (.00430)	.00402 (.00644)	.901	.099	.894

[a,b] Selected fractiles of $F_{2,4}$ and $F_{2,11}$ are:

Fractile	[a]Value of $F_{2,4}$	[b]Value of $F_{2,11}$
.900	4.32	2.86
.950	6.94	3.98
.975	10.65	5.26
.990	18.00	7.21

[e] Standard error of average return difference estimate immediately above.

Appendix B

RESULTS OF TESTS WHICH CORRECT FOR DIFFERENCES IN THE RELATIVE RISKS OF MATCHED GROUPS OF ACFs AND NCFs

Because of the importance of the $\underline{\beta}_g$ matching requirement of the research design utilized in this study, this appendix considers two inherent problems in achieving a perfect matching of $\underline{\beta}_g$. The first problem, covered in Part 1, is the difficulty of matching the point estimates of $\underline{\beta}_g$. This problem is discussed in two subparts: A, which involves a sample revision which was undertaken to more nearly equalize matched $\hat{\underline{\beta}}_g$; and B, which involves supplemental tests that were conducted in order to determine whether the remaining $\hat{\underline{\beta}}_g$ differences could be expected to cause spurious results in the principal tests of this study.

The second problem, which is discussed in Part 2, results from the necessity of using statistical estimates of $\underline{\beta}_g$. Part 2 gives the results of additional supplemental tests which take into account the confidence intervals around the $\hat{\underline{\beta}}_g$ which were used for matching purposes. In both parts of the appendix, the supplemental tests were performed on the hypothesis group which was most likely to have been affected by the problem discussed therein.

Part 1 - Problems in Matching Point Estimates of β_g

Subpart A - Sample Revision

The initial selection of a control group produced some differences in excess of .05 between the estimated group relative risks ($\hat{\beta}_g$) of ACFs and NCFs. These differences were considered to be too large in view of the fact that the major requirement of the matching procedures called for by the research design is that the two β_g of a matched pair (of groups of firms) be equal. A case in point is the low-risk pair for which the AC was discretionary and increased EPS, i.e., ACF (+,D). Initially the NCF average $\hat{\beta}_g$ was .973 for low-risk firms in the sample, or .049 greater than the low-risk ACFs' $\hat{\beta}_g$ of .924. The mean vector test for the period of $t = -6$ through -1 using these firms produced a negative average return difference (of ACF - NCF) which was significant at less than .05. However, when eleven of the NCFs with the risk coefficients that had the largest paired $\hat{\beta}_i$ differences were replaced by eleven NCFs whose $\hat{\beta}_i$ combined with the NCFs retained to reduce the average $\hat{\beta}_g$ difference to .016 (see table VI), the average return difference for the revised sample was not statistically different from zero.

In addition, several NCFs which were initially matched with ACFs in other hypothesis test groups were also replaced in order to reduce the differences between ACFs' and NCFs' matched $\hat{\beta}_g$. But in no case other than the one mentioned above did the replacement of firms in the sample significantly alter the results of the test.

Subpart B - Tests Which Correct for Differences between the Point Estimates of Paired β_g

Even after several of the initial 560 NCFs in the sample were replaced in order to make the two risk vectors of a pair more nearly equal (as described above), some differences in $\hat{\beta}_g$ naturally remain. The largest such difference of .038 occurs between the low-risk pair for the hypothesis test of nondiscretionary ACs with positive effects on EPS, i.e., ACF(+,N) (see table VI). Because the research design utilized in this study requires that $\beta_1 = \beta_2$ in order to be able to attribute differences in realized return distributions to the information content of ACs, it was feared that a difference as large as .038 could distort the results of the mean vector and covariance matrix tests on ACF (+,N). Table VI indicates that the two NCF $\hat{\beta}_g$ exceed the two ACF $\hat{\beta}_g$ by .02 for high-risk firms and by .038 for low-risk firms. This difference implies that the vector of expected returns on these particular NCFs exceeds the vector of expected returns on ACFs (see eq. 1)

In order to measure the effect that such a difference could have on the tests conducted in this study, the mean vector test and the covariance matrix test of ACF (+,N) were performed a second time for all three time periods. In these cross-validation tests, corrections were made for the difference between the $\underline{\hat{\beta}}_g$ of ACFs and NCFs.

In the test on mean vectors the correction involved incrementing the realized returns on the two risk-class groups of ACFs by the respective risk-class $\hat{\beta}_g$ discrepancies multiplied by the average realized risk premium (realized $R_{mt} - R_{zt}$ in Eq. 1) over the period covered in the test. The correction thus raised the average return for each risk-class group of ACFs to the theoretical equilibrium (in relation to the average return for the corresponding risk-class group of NCFs) of Eq. 1 (because the $\underline{\hat{\beta}}_g$ of NCFs exceeds the $\underline{\hat{\beta}}_g$ of ACFs). The resulting increment to the average monthly return on high-risk ACFs was .02 multiplied by .00908, or .00018, and the increment to the average return on low-risk ACFs was .038 multiplied by .00908, or .00035. The average monthly risk premium of

.00908 was the weighted average risk premium (the average monthly return on a portfolio consisting of the Standard and Poor's 500 stocks minus the average monthly return on 90-day U.S. Treasury bills) over the 1968-1972 period of this study. In the averaging process the weights were the relative proportions of firms in this particular test that came from each of the five years, 1968-1972. The weighted average risk premium thus created was the actual risk premium that investors in these particular firms faced during the test period. It was unusually high (.00908) because 48.3 percent of the firms came from 1971, when the average monthly risk premium was .00858, and 41.7 percent of the firms came from 1972, when it was .01283.[1] The conditional covariance matrix (Eq. 8) of the corrected conditional return difference vector (defined in the Hypotheses section of Chapter 5) was assumed to remain unchanged as a result of the $\hat{\underline{\beta}}_g$ correction. Although it may have, in fact, changed, there is no reason to believe the change would be substantial with such small changes in the mean difference vector. But because this is an empirical issue, there is no theoretical model to suggest what the change may be.

The results of these cross-validation tests were essentially the same as the results on the major mean vector tests conducted in this study, in which the $\hat{\beta}_g$ differences were allowed to remain. Specifically, in the test over the period t = -6 through -1, the inference from the result of the test was that returns on ACFs and NCFs were the same, with a confidence level of .75 or greater. In the tests over the periods t = +1 through +6 and t = -6 through +6, the inferences from the results were that returns on ACFs and NCFs were different, with confidence levels of .949 and .975, respectively. As can be seen from the results of these tests on line fourteen of Table IX, the differences in results are nominal.

In the test for equality of covariance matrices, the correction for the difference between ACFs' and NCFs' $\hat{\beta}_g$ uses the definition that the variance of the return on any portfolio p is equal to the systematic variability $\beta^2 Var(R_m)$, plus the unsystematic variability, $Var(e_p)$, from holding the portfolio. Thus, the effect of the $\hat{\beta}_g$ discrepancy on the results of the test may be corrected by incrementing the conditional covariance matrix (Eq. 11) of ACFs by $\hat{\beta}^2 Var(R_m) = (.02)^2$ multiplied by .034 for the variance of the return on high-risk ACFs and by $(.038)^2$ multiplied by .034 for the variance of the return on low-risk ACFs.[2] The covariance between the returns on high- and low-risk ACFs, as well as the covariance between the returns on high- and low-risk NCFs, was assumed to remain constant. As with the tests on mean vectors, the results of the tests for equality of covariance matrices over the three periods were essentially the same as the results obtained in tests in which the $\hat{\beta}_g$ difference was allowed to remain.

Part 2 - Problems in Using Estimates of β_g

In an entirely separate set of cross-validation tests on mean vectors of ACFs that made nondiscretionary ACs with positive income effects, i.e., ACF(+,N), the two (ACF and NCF) β_g were assumed to differ by as much as would be allowed by a ninety percent confidence interval around the two $\hat{\beta}_g$. That is, for the hypothesis test being considered, ACF +,N), where the $\hat{\beta}_g$ of high-risk NCFs exceeds the $\hat{\beta}_g$ of high-risk ACFs by .02 and the $\hat{\beta}_g$ of low-risk NCFs exceeds that of low-risk ACFs by .038, it is possible, because the $\hat{\beta}_g$ are only estimates of the true β_g, that the differences are greater than .02 and .038, respectively.

Therefore, in order to determine whether this possibility is likely to have influenced the inferences drawn from the test of ACF (+,N), the high- and low-risk $\hat{\beta}_g$ differences were allowed to vary up to .316 and .236, respectively. These estimates were derived by hypothesizing that the high-risk β_g of ACFs may be as low as 1.412, which is equal to its point estimate of 1.56 minus the product of 1.645 (the number of standard deviations defining the ninety percent confidence boundary of a normally distributed random variable) multiplied by its estimated standard error of .09.[3] The high-risk NCFs' $\hat{\beta}_g$ was allowed to reach its ninety percent confidence maximum of 1.728 (= 1.58 + (1.645 x .09)). The low-risk firms' $\hat{\beta}_g$ difference was allowed to reach its estimated maximum in like manner, but using .06 as the estimated standard error of the two low-risk $\hat{\beta}_g$. Then the mean return vector of ACFs was incremented in the manner discussed above, and the T^2 tests were conducted again. The results were essentially unchanged from the other (corrected and uncorrected) test results.

In the cross-validation test on covariance matrices in which the two $\hat{\underline{\beta}}_g$ were allowed to reach their ninety percent confidence maximum difference, a different result emerges. The greater resulting differences between the variances of high-risk ACFs and NCFs and the variances of low-risk ACFs and NCFs were large enough to assign probabilities of greater than .975 to the inference that ACFs' covariance matrices of returns differ from NCFs' covariance matrices of returns for tests over all three time periods. This suggests that the covariance matrix test is more sensitive to large differences in the matched $\hat{\beta}_g$ than the test on mean vectors is. This result means, of course, that the covariance matrix test is able to detect large differences. It also implies that the covariance matrix test conducted on samples of as few as six monthly observations may not be as lacking in statistical power as was implied in the Chapter 6 section entitled Tests on Covariance Matrices. But another implication is equally clear--that the estimation of β_g with error can cause spurious results in the covariance matrix test. On balance then, it appears that the conclusion of indeterminacy which is reached in the Chapter 6 discussion is warranted. Furthermore, and unfortunately, there appear to be no alternative means of estimating β_g with substantially less error than the method used here, given this research design.

On the basis of the results of the cross-validation tests described in Parts 1 and 2 above, it appears that the largest sample difference between the two $\hat{\beta}_g$ of a matched pair does not distort the results of the mean vector test which is affected by that difference. By implication then, it is reasonable to assume that none of the other hypothesis tests is adversely affected by the (smaller) differences between the $\hat{\underline{\beta}}_g$ of ACFs and NCFs. However, it is possible that the same differences in $\hat{\underline{\beta}}_g$ can seriously affect the results of tests on covariance matrices.

CHAPTER 7

SUMMARY, CONCLUSIONS, CONTRIBUTIONS OF THE STUDY, AND SUGGESTIONS FOR ADDITIONAL RESEARCH

Summary

Previous research on the stock market behavior associated with accounting changes (ACs) has focused primarily on the question of whether the market was deceived by ACs. That is, researchers generally made assumptions about the information content of ACs and tested to see if the market behaved in the hypothesized manner. They also assumed that the market's behavior is constant across all securities, and only two of the studies have reported the results of tests conducted on a control group of firms that did not make ACs. Finally, previous researchers have all examined only the first moment, the mean, of security return distributions in making their assessments of the effects of ACs.

This study utilized a recently devised research design in order to assess whether and how the stock market reacts to ACs. The market was assumed to be efficient with respect to publicly available data (such as AC disclosures), and the capital asset pricing model was assumed to reflect the market's mechanism for establishing equilibrium values for firms. Firms that made accounting changes during 1968-1972 were taken from the appropriate editions of Accounting Trends and Techniques. The sample consists of 560 change firms. Also, 560 nonchange firms were selected as a control group from the population of firms that did not make ACs. The two groups were matched at the firm level on the bases of fiscal year-end, relative risk, and less stringently on industry membership. The matching procedures were designed to eliminate systematic differences between the two groups in order to maximize the probability that observed return differences reflect the market's reaction to ACs.

The firms were divided into two groups on the basis of their relative risks with the top half thus labeled as high-risk and the bottom half labeled as low-risk. Then multivariate statistical tests were conducted on mean vectors of returns and covariance matrices of returns in order to determine whether the two return distributions (of ACFs and NCFs) differ statistically. The multivariate research design gives effect to the performance of simultaneous tests on the information effects of ACs for the two risk classes. The separation of firms into different risk classes provides the framework for testing whether AC information is unique to a particular risk class (as defined above).

The test period consisted of the thirteen months centered at the second month after each firm's fiscal year-end. This month is believed to include the date when <u>most</u> investors become aware that firms have made ACs.[1] Where month t = 0 is the assumed post-year-end disclosure month, the entire period ran from t = -6 through t = +6. Tests were conducted on data pertaining to the full thirteen-month period, as well as to the two six-month periods, t = -6 through -1 and t = +1 through +6. The separation into the first and second subperiods allows for testing the timing of any unusual market behavior.

Conclusions

Tests were conducted on various subgroups of the 560 matched pairs of firms. The largest such group was the entire sample, and the results of this omnibus test corroborated the findings of Ball, who concluded that ACs are not associated with a unique market effect. This test analyzed many different types of ACs, which could be expected to have widely different market effects. Thus, the blanket inference of no effect could be erroneous in the sense that it does not pertain to subgroups of ACs with more homogeneous characteristics.

In order to determine whether the aggregation hypothesis has validity, tests were also conducted on ACs that had four different directional effects on net income: (1) positive; (2) negative; (3) zero or immaterial; and (4) directional effect not disclosed. The results of all of these tests were also consistent with the null hypothesis of no information content. Because each of these tests also aggregated across ACs with widely different characteristics, the results are not surprising.

One such characteristic hypothesized to be of interest to stock market agents is whether the ACs are made at the discretion of management or whether the firm is compelled by some exogenous body such as the Accounting Principles Board (APB) or the Financial Accounting Standards Board (FASB) to make the change. Accordingly, tests were also conducted on ACs that had three different discretion characteristics: (1) discretionary; (2) nondiscretionary; or (3) both, i.e., the firm made at least one discretionary AC and at least one nondiscretionary AC during the same year. All previous AC research in which tests were conducted on specific types of ACs concerned discretionary ACs only. This study provides the only evidence of which the author is aware on the market effect of nondiscretionary ACs.

The results of the tests on discretionary ACs, and the results of the tests on nondiscretionary ACs both led to the inference that ACs do not have an informational effect on security returns. The results of the test on the both category, however, led to rejection of the null hypothesis and hence to the inference of information content. This test was shown later to have been significantly influenced by the presence of a large number of firms for which the overall effect on net income was positive. In general, however, the level of aggregation in these tests was still high enough to obscure the unique effect associated with some more narrowly defined ACs.

In addition, tests were conducted on nine of the twelve subgroups of ACs defined by the various combinations of directional effect and relative discretion (e.g., discretionary ACs with positive net income effects, etc.). Three groups were dropped since the sample sizes were considered too small to yield meaningful conclusions. Several of these tests yielded statistically significant differences between the mean return vectors of AC and nonchange firms. For example, the market reacted negatively to firms that made discretionary ACs that increased net income, in relation to NCFs. These results are generally consistent with the findings of Kaplan and Roll, but they are more general because of the wider variety of accounts affected by the ACs examined in this study.

The market reacted negatively to firms that made both a discretionary AC and a nondiscretionary AC with an overall positive effect on net income. A negative market reaction was also observed for firms that made nondiscretionary changes that reduced net income (but the empirical results of this test are likely to have been significantly affected by sampling error because of the small number of firms involved). The two significantly positive market reactions were to nondiscretionary ACs that increased net income and to discretionary ACs that had no effect on net income. All other tests resulted in insignificant differences between the returns of AC and nonchange firms.

Thus, it appears that the types of ACs referred to in Table XIV are associated with unique stock market effects. Furthermore, the second six-month period (after the year-end disclosures of ACs) yielded the most pronounced market adjustments. The tests conducted in this study can only firmly establish association, not cause-and-effect relationships. Nevertheless, one possible implication of these findings is that the year-end disclosures of ACs (chiefly in footnotes to financial statements) are used by market agents in establishing equilibrium values of firms. This particular conclusion is not too startling in light of the fact that there are not many competing sources of AC information. However, to the extent that the information is an indicator of firm profitability, dividend policy, or other widely followed characteristics of firms, this finding places some new importance on AC disclosures as a source of information.

The two tests for which the significant differences occurred during the six months prior to the year-end disclosure involved firms that made both a discretionary AC and a nondiscretionary AC. This result appears to have been influenced by the presence of a large number of firms that made discretionary ACs with relatively large increases in net income. An explanation of this result, which appears to be consistent with evidence provided by Bremser,[2] is that firms which make discretionary ACs with positive effects are significantly less successful than nonchange firms in terms of profitability and return on stockholders' equity. In short, such firms may be using ACs to increase net income in order to conceal otherwise unsatisfactory operating performance. This possibility raises a question as to why the market did not react negatively during the first six-month subperiod to the discretionary ACs made by the discretionary-positive effects group of firms. If one were to accept the results of that particular test, subject to the probability of a type I error equal to around .20, then he could conclude that all discretionary ACs with positive income effects examined in this study are associated with a negative market reaction in the six-month period prior to the year-end disclosure month. However, the mild inconsistency between the two test results still remains somewhat of a mystery.

In each test that led to rejection of the no information hypothesis, the return distributions of the two risk classes of firms exhibited substantially different behavioral patterns. This result suggests that the separation of firms into risk classes was a worthwhile exercise in that it allowed for the risk dependencies of the AC information to be revealed. However, in several cases the nature of the risk dependency is far from clear. This study merely indicates that there appears to be a risk dependency associated with AC information. It offers relatively few clues as to the reasons for the risk dependencies observed. More evidence is needed before any firm conclusions are reached as to why the market reacts uniquely to the AC information of a particular risk class of firms.

The results of tests for the equality of covariance matrices of returns on change and nonchange firms were indeterminate because of a combination of factors related to the research design, the failure to satisfy certain antecedent conditions of the tests, and the statistical power of the tests conducted. As a result, the tests on covariance matrices yielded ambiguous conclusions.

Contributions of This Study

The contributions of this research are two-fold. One aspect is technical, and the other concerns the content of the study and the related conclusions. The technical contribution stems from the application of a new research design (or technique) for testing the market effects associated with accounting events such as ACs. The major part of the technical contribution is merely a derivative of Gonedes' work because he formulated the basic design used here. However, several extensions of his work that were introduced in this study are believed to be of value. One is the set of procedures that were followed in order to obtain the control group of nonchange firms. The matching of firms on estimated relative risk <u>and</u> industry membership largely eliminates those variables as competing explanations for the observed results. As a result, the return differences between the two groups are more likely to be related to ACs than in previous AC research.

Inasmuch as the risk coefficients used for matching change and nonchange firms are only estimates that are subject to error, appendix B to Chapter 6 contains the results of some cross-validation tests. These tests were based on the assumption that change firms' true relative risks differ from those of nonchange firms by the widest margin that a ninety percent confidence interval would allow. The results of the cross-validation tests on mean vectors are very little different from the results of tests in which the point estimates of risk are treated as though they were true values. Of course, this added step lends more support to the foundation of internal validity underlying the conclusions of the study. Results of the cross-validation tests on covariance matrices support the opposite conclusion--that possible differences between the true risk coefficients could lead to spurious results in the covariance matrix tests. However, because those results do not, in general, lend themselves to meaningful interpretation, this finding was not considered much of a loss to the import of the study.

The contributions involving content chiefly center around the size of the sample--560 AC firms over the five years 1968-1972. The large sample, as well as the specific years included, allowed for tests to be performed on a wide variety of ACs, some of which had not been subjected to testing previously. The principal example is nondiscretionary ACs. This evidence complements the evidence that was already available on the market reaction to discretionary ACs. In addition, this study provides evidence on a wider variety of discretionary ACs than any previous study has done. And finally, the results obtained here suggest that the information content of ACs is somewhat risk-dependent.

Beaver suggests that the Financial Accounting Standards Board should consider evidence on the market consequences associated with various modes of reporting financial data. In this regard he states that,

> . . . although evidence cannot indicate what choice to make, it can provide information on the potential consequences of the various choices. Without a knowledge of the consequences (e.g., as reflected in security prices), it is inconceivable that a policymaking body such as the FASB will be able to select optimal financial accounting standards.[3]

Beaver is referring to choices such as straight-line versus accelerated depreciation, equity versus cost for investments, etc. This study presents evidence on the consequences of changes from one method to another. Following Beaver's line of reasoning, one concludes that bodies such as the FASB should be interested in the results obtained here. One reason is obvious: these results provide evidence on how investors react to various types of ACs and by implication to the different reporting modes involved. The results also provide an indication of the consequences of actions taken by the APB (and its predecessor organization, the Committee on Accounting Procedure) to require AC disclosures.[4] Finally, the fact that the market appeared to react systematically to nondiscretionary ACs also suggests that APB opinions and other authoritative pronouncements make a difference to market agents. In summary, this study presents evidence which should be useful in understanding investors' decision models.

How the FASB views these results depends, of course, on the subjective judgments they place on the results. They may conclude that certain investor reactions were irrational and resulted in a serious misallocation of resources. On the other hand, they may accept the position taken here that the market is efficient and interpret the results in terms of the assumed rationality of investors. In either case, this study provides them with new and expanded evidence on the market consequences associated with ACs.

Another implication of the results of this study pertains to business managers. Financial theory states that they are motivated by the desire to maximize the values of the firms entrusted to them by shareholders. If they were pursuing this objective, why then did a large number of the firms make discretionary ACs that increased net income? Did they not know in advance that the market reaction to such an AC would be demonstrably negative? However, because it is easy after the fact to find an explanation for observed phenomena, and then to label the explanation a prediction, *these* particular managers are not to be condemned too harshly. However, future generations of managers would do well to heed these results, and to predict future market reaction toward their ACs in light of this evidence.

Suggestions for Additional Research

This study is subject to one principal line of criticism--that it did not adequately distinguish between the AC or the resulting auditor's consistency exception as the independent variable being measured. However, because only around half of the sample AC firms received consistency exceptions, it seems safe to conclude that the AC was the event more closely associated with market effects.

Nevertheless, a detailed inquiry into the effect that consistency exceptions have on security returns is already under way.

Also, it is possible that ACs with different magnitudes of effect on net income can affect returns differently. The question of materiality has received much attention in the professional and academic literatures, but has been subjected to very little empirical testing outside the laboratory environment. A project in this area is planned for the future inasmuch as the data needed for such an inquiry have already been gathered.

Another refinement of the tests conducted in this study could be separate tests on ACs affecting specific accounts. Depreciation-related and inventory-related ACs have already been examined, as well as changes involving the investment tax credit. The sample in the present study includes a large number of pension-related ACs, most of which increased net income. The fact that some firms increased the assumed earnings rate on their pension fund assets, while others did not, suggests that the former group of firms may have timed their ACs in order to increase net income during periods when their operating profits were not meeting expectations. It would be interesting to know the market's reaction to pension-related

ACs in particular. Similar reasons can be advanced for examining other types of ACs as well. A series of studies on the effects of ACs involving specific accounts would seem to be a natural way to complement the more general results obtained in this study.

There is an extensive literature on the relative stability of risk coefficients, but relatively little is known about the effect ACs have on firms' relative risks. Ball and Sunder have examined this phenomenon, but their results by no means provide a complete description of the effect, if any, that ACs have on risk levels. This question may be related to the fact that each of the unique AC effects observed in this study pertained to only one of the two risk classes of firms. It is possible that the AC information actually affected the firms' risk coefficients, and this effect may have been directly responsible for the differential behavior in security returns. This chain reaction of hypothetical effects is consistent with the major implication of the capital asset pricing model. But it would take additional evidence in order to determine whether this is in fact what occurred to give rise to the differences observed here.

Finally, it would be interesting to look into the behavioral implications of ACs and their market effects.

As mentioned above, managers of firms are supposed to be motivated by the desire to maximize the values of their firms. Yet a large number of the change firms in this study made the (ex post) irrational decision to make an AC which would increase the firm's net income. Given this evidence, it would be enlightening to query managers who made those decisions in order to gain some insight into, for example, the source of their motivation for making the change. It would also be interesting to learn of their perceptions, both prior to and after the AC, of the market's reaction, as well as whether, in retrospect, they would make the same decisions again.

FOOTNOTES

Footnotes - Chapter 1

[1]Two examples are: Ronald M. Copeland, "Income Smoothing," Empirical Research in Accounting: Selected Studies, 1968, Supplement to Journal of Accounting Research 6 (1968): 101-121; and Barry E. Cushing, "An Empirical Study of Changes in Accounting Policy," Journal of Accounting Research 7 (Autumn 1969): 196-203.

[2]Barry E. Cushing, "Accounting Changes: The Impact of APB Opinion 20," Journal of Accountancy 138 (November 1974): 54-62.

[3]Three examples are: T. Ross Archibald, "The Return to Straight-line Depreciation: An Analysis of a Change in Accounting Method," Empirical Research in Accounting: Selected Studies, 1967, Supplement to Journal of Accounting Research 5: 164-180; Martin L. Gosman, "Characteristics of Firms Making Accounting Changes," The Accounting Review 48 (January 1973): 1-11; and Wayne G. Bremser, "The Earnings Characteristics of Firms Reporting Discretionary Accounting Changes," The Accounting Review 50 (July 1975): 563-573.

[4]Two examples are: Fred Neumann, "The Auditing Standard of Consistency," Empirical Research in Accounting: Selected Studies, 1968, Supplement to Journal of Accounting Research 6 (1968): 1-17; and Elba F. Baskin, "The Communicative Effectiveness of Consistency Exceptions," The Accounting Review 47 (January 1972): 38-51.

[5]Return (R) = $\frac{P_1 - P_0 + D_1}{P_0}$ where P = price, D = dividends, and the subscripts refer to arbitrarily-defined points in time.

[6]Examples are: Eugene E. Comiskey, "Market Response to Changes in Depreciation Accounting," The Accounting Review 46 (April 1971): 279-285; T. Ross Archibald, "Stock Market Reaction to the Depreciation Switch-Back," The Accounting Review 47 (January 1972): 22-30; Robert S. Kaplan and Richard Roll, "Investor Evaluation of Accounting Information: Some Empirical Evidence," Journal of Business 45 (April 1972): 225-57; Baskin, pp. 38-51; Raymond J. Ball, "Changes in Accounting Techniques and Stock Prices," Empirical Research in Accounting: Selected Studies, 1972, Supplement to Journal of Accounting Research

10 (1972): 1-38; and Shyam Sunder, "Stock Price and Risk Related to Accounting Changes in Inventory Valuation," The Accounting Review 50 (April 1975): 305-315.

[7]Over the long run stock returns and stock prices are synonymous because the only nonprice element in returns, i.e., dividends, is implicitly included in prices which adjust for dividend declarations and distributions. Therefore returns and prices will be used interchangeably.

[8]Baruch Lev, Financial Statement Analysis: A New Approach (Englewood Cliffs, N.J.: Prentice-Hall, 1974), p. 241.

[9]Nicholas J. Gonedes and Nicholas Dopuch, "Capital Market Equilibrium, Information Production, and Selecting Accounting Techniques: Theoretical Framework and Review of Empirical Work," Studies on Financial Accounting Objectives: 1974, Supplement to Journal of Accounting Research 12 (1974): 48-129. In their quotation Gonedes and Dopuch use e_{it} to refer to the return residual of the ith entity for the tth time period, which is conditioned on the information variable θ with the same subscripts.

[10]Sunder observed the latter result. See Sunder (1975), pp. 305-315.

[11]An expanded discussion of ACs with different economic implications is given in Gonedes and Dopuch, pp. 84-91.

[12]See, for example, Eugene Fama, "The Behavior of Stock Prices," Journal of Business 28 (January 1965): 34-105. Not enough is known about kurtosis to be certain of what it measures.

[13]James Tobin, "Liquidity Preference as Behavior towards Risk," Review of Economic Studies 25 (February 1958): 65-86.

[14]M. K. Richter, "Cardinal Utility, Portfolio Selection, and Taxation," Review of Economic Studies 27 (June 1960): 152-166.

[15]Value Line; Merrill Lynch, Pierce, Fenner & Smith and other firms periodically publish companies' Beta coefficients for investors' use. Intuitively a firm's

Beta is the measure of the sensitivity of that firm's stock return to the return on some market index. Or stated differently, Beta measures the systematic portion of a firm's stock variability. A more rigorous definition will be given later when firms' Betas will be considered in some detail.

[16]Baskin's study ostensibly tested the effect of consistency exceptions. Yet, his design does not distinguish whether the event of interest was the consistency exception or the AC; accordingly his study is considered an AC study.

Footnotes - Chapter 2

[1]This definition of information content is covered in Joel S. Demski, Information Analysis (Reading, Mass.: Addison-Wesley, 1972), p. 14.

[2]See, for example: Raymond J. Ball and Phillip Brown, "An Empirical Evaluation of Accounting Income Numbers," Journal of Accounting Research 6 (Autumn 1968): 159-177; Eugene F. Fama, L. Fisher, M. Jensen, and R. Roll, "The Adjustment of Stock Prices to New Information," International Economic Review 10 (February 1969): 1-21; and William H. Beaver, "The Information Content of Annual Earnings Announcements," Empirical Research in Accounting: Selected Studies 1968, Supplement to Journal of Accounting Research 6 (1968): 67-100.

[3]Eugene F. Fama, "Efficient Capital Markets: A Review of Theory and Empirical Work," Journal of Finance 25 (May 1970): 383-417.

[4]These two groups of firms are believed to be relatively small in relation of the population of ACFs because examination of a full year's Wall Street Journal Index entries for each of twenty firms included in the present sample yielded disclosure prior to the annual report for only one firm. That one disclosure, in a quarterly earnings summary, merely stated that the firm had made a type x change. In almost all cases that a firm discloses its AC prior to year-end, there is reason to believe that the market may react to the AC around year-end because it would be difficult to predict in advance exactly what effect the AC may have on net income. For example, the income effect of inventory changes depends on the value of the ending inventory balance. A change to the equity method of accounting for investments depends on the net income of the investee. Changes in depreciation methods depend on the ages of depreciable assets (which are not normally disclosed) and current-year acquisitions.

[5]Shyam Sunder, "Relationships between Accounting Changes and Stock Prices: Problems of Measurement and Some Empirical Evidence," Empirical Research in Accounting: Selected Studies 1973, Supplement to Journal of Accounting Research 11 (1973): 1-45.

[6]See William H. Beaver and Roland Dukes, "Interperiod Tax Allocation, Earnings Expectations, and the Behavior of Security Prices," The Accounting Review 47 (April 1972): 331, for one example.

[7]William F. Sharpe, "Capital Asset Prices: A Theory of Market Equilibrium under Conditions of Risk," Journal of Finance 19 (September 1964): 425-442. Sharpe's work was an extension of the portfolio theory work done by Harry Markowitz, Portfolio Selection: Efficient Diversification of Investments (New York: Wiley, 1959).

[8]One aspect of return distributions which has been examined is the individualistic portion, e_{it}, of the conditional realized return distribution where $e_{it}|\theta_i - E(\tilde{e}_{it}) = R_{it}|\theta_i - E(\tilde{R}_{it})$. Here $\tilde{\theta}_i$ represents the accounting event, where i can take on two values, 1 for the presence of the event and 2 for the absence of the event. Known as the Abnormal Performance Index (API) method, it draws inferences about the information effects of $\tilde{\theta}_i$ depending on whether $e_{it}|\theta_i$ is equal to its equilibrium expectation of zero. A nonzero $e_{it}|\theta_i$ means that $\tilde{\theta}_i$ has information content. Conversely, a zero $e_{it}|\theta_i$ signals no information. Tilde (~) denotes a random variable.

[9]Fischer Black, "Capital Market Equilibrium with Restricted Borrowing," Journal of Business 45 (July 1972): 444-455.

[10]Fischer Black, Michael Jensen, and Myron Scholes, "The Capital Asset Pricing Model: Some Empirical Tests," in Studies in the Theory of Capital Markets, ed. Michael C. Jensen (New York: Praeger, 1972).

[11]Eugene F. Fama and James MacBeth, "Risk, Return, and Equilibrium: Empirical Tests," Journal of Political Economy 81 (May/June 1973): 607-636.

[12]Michael C. Jensen, "Capital Markets: Theory and Evidence," Bell Journal of Economics and Management Science 3 (Autumn 1972): 357-398.

[13]Fama (1965), pp. 34-105.

[14]Nicholas J. Gonedes, "Risk, Information, and the Effects of Special Accounting Items on Capital Market Equilibrium," Report No. 7429, Center for Mathematical Studies in Business and Economics, University of Chicago, June 1974, Table 10.

[15]Demski, p. 14.

[16]Gonedes (1974a), pp. 2.1-2.13.

[17]Actually the design used here is the comparison of APIs for two groups of firms with different $\tilde{\theta}_i$ and equal $\hat{\beta}_i$. Letting θ_1 represent an AC and θ_2 represent no AC, the group of ACFs' realized return is $R_1|\theta_1 = E(R_1) + e_1|\theta_1$. Where $\beta_1 = \beta_2$, $E(R_1) = E(R_2)$, and the analysis reduces to a comparison of $e_1|\theta_1$ and $e_2|\theta_2$. This assumes, of course, that $\beta_1 = \beta_2 = \beta_{constant}$, which may not remain true throughout the experiment. If $\beta_1 \neq \beta_2$ during the test period, then $R_1|\theta_1 \neq R_2|\theta_2$ may be due to a differential shift in the β_1 of the two groups. For an expanded discussion of the effect of a changing β_i, see the final section of Chapter 3.

[18]For a discussion of variance partitioning see Jack C. Francis and Stephen Archer, Portfolio Analysis (Englewood Cliffs, N.J.: Prentice-Hall, 1971), p. 179.

[19]See Kaplan and Roll, pp. 237-239.

[20]See Sunder (1973), p. 313.

[21]Ball and Brown (1968), p. 167.

[22]Louis H. Rappaport, SEC Accounting Practice and Procedure, 3rd edition (New York: Ronald Press Company, 1972), p. 14.5.

Footnotes - Chapter 3

[1]If, for example, β_i were increasing over the 204-month period of his study, Archibald's constant risk assumption could have caused his estimates of $\bar{e}_t$ to be understated in the prechange period and overstated in the postchange period. The reverse would be true if β_i were decreasing over the period of the study. If the rate of change in β_i were relatively constant, as Ball observed (see Ball, figure 4), the effect on the full four-year period would be small, but the effects on individual pre- and postchange periods may be substantial, depending on the rate of change. It is impossible to know what the effect was without knowing the behavior of β_i during the test period. But if the β_i behavior in Archibald's design was similar to that observed by Ball, this could explain why the average e_t for the prechange months were significantly less than zero.

[2]The exact time period of this test is not clear. Because the later and more important test was conducted over five weeks, it appears reasonable to assume that this test also covered five weeks.

[3]To see why this is true, refer again to Ball, Figure 4, which indicates that the average estimated β_i of the 267 ACFs of Ball's study increased from approximately .91 in the 110th month before the AC disclosure to approximately 1.02 fifty months after the disclosure month. The rate of change during the one month after the disclosure was high, but in absolute terms the change is estimated to be .002, hardly enough to cause spurious results.

[4]Ball makes this observation on page 31 of his study.

[5]See Sunder (1975), pp. 305-315.

[6]For difficulties with normative assumptions about the market's response to particular types of ACs, see Chapter 1, page 3 and Gonedes and Dopuch, pp. 84-91.

[7]See Gonedes (1974a), Sections 2.2 and 2.3.

[8]Ibid., Section 5.5 and Table 7.

[9]Ibid., Section 2.2.

[10]Gene V. Glass and Julian Stanley, Statistical Methods in Education and Psychology (Englewood Cliffs, N.J.: Prentice-Hall, 1970), pp. 491-494.

[11]See the Chapter 5 discussion following Eq. 9.

[12]Gonedes (1974a), p. 4.8.

[13]R. Darrel Bock and Ernest A. Haggard, "The Use of Multivariate Analysis of Variance in Behavioral Research," in Handbook of Measurement and Assessment in Behavioral Sciences, ed. Dean K. Whitla (Reading, Mass.: Addison-Wesley, 1968), p. 102. Bock and Haggard are not writing about security price research in which returns on different risk classes of firms are correlated. Nevertheless, their statement, which concerns different teachers and textbooks, is perfectly analogous to a statement concerning different risk classes and security returns, respectively.

[14]See William H. Beaver, "The Behavior of Security Prices and Its Implications for Accounting Research (Methods)," Supplement to The Accounting Review 47 (1972): 407-436, for a discussion of the CAPM and the market model, as well as the assumptions of the two models.

Footnotes - Chapter 4

[1]The CRSP tape, developed at the University of Chicago, includes various pieces of financial data for all NYSE-listed companies from December 1925 through dates that are constantly being updated. The CRSP data pertinent to this study are the appropriate total monthly rates of return (see footnote five in chapter 1) on the common stock of ACFs and NCFs in this sample.

[2]A firm was deemed to have an insufficient number of monthly returns available for estimating its β_i if its $\hat{\beta}_i$ was not included in Merrill Lynch, Pierce, Fenner and Smith, Inc.'s Security Risk Evaluation, from which firms' $\hat{\beta}_i$ were taken. In general, Merrill, Lynch estimates each firm's β_i over sixty monthly return observations. However, where fewer monthly returns are available for a given firm, the number of available monthly returns is used, down to a minimum of twelve. An expanded discussion of the β- estimation procedures is given below.

[3]What constitutes a "suitably matched NCF" is the subject of the remainder of this chapter.

[4]During the test period the APB issued opinions 13-24. Among this group of twelve, the opinions which gave rise to ACs were those requiring a different accounting method from the method then in use for assets, liabilities, or capital items already entered in the accounts from previously consummated transactions. The opinion contributing the largest number (105) of nondiscretionary ACs to the present sample was Opinion No. 18, "The Equity Method of Accounting for Investments in Common Stock." The number of nondiscretionary ACs resulting from all other opinions issued during the sample period was only eighteen. Of these, twelve result from Opinion No. 23, which mandated the accrual of income taxes on the undistributed earnings of subsidiaries.

[5]This group of nondiscretionary ACs was quite small, only ten. Principal accounts affected were inventories and fixed assets.

[6]The fact that twenty-two steel firms made the same AC, plus the fact that steel firms' $\hat{\beta}_i$ do not differ markedly from one another, provides evidence which supports

the need to examine whether there is an association between informational effects of AC disclosures and particular risk classes.

[7]Sunder (1975), p. 313.

[8]Benjamin J. King, "Market and Industry Factors in Stock Price Behavior," Journal of Business 39 (January 1966): 139-190.

[9]Stephen L. Meyers, "A Re-Examination of Market and Industry Factors in Stock Price Behavior," Journal of Finance 28 (June 1973): 695-705.

[10]The $\hat{\beta}_i$ of 1968 ACFs and NCFs were not taken from Security Risk Evaluation because the publication was not begun until 1969. Instead, 1968 firms' β_i were estimated in the same manner that Merrill, Lynch estimates β_i, all over sixty months' data.

[11]Security Risk Evaluation contains two types of estimates of firms' β_i. One type is the unadjusted $\hat{\beta}_i$, and the other is Merrill Lynch's attempt to adjust for the regression tendency of $\hat{\beta}_i$. The unadjusted $\hat{\beta}_i$ are used in this study.

[12]For the procedure that defines each firm as high- or low-risk, see the Chapter 5 section, Procedures for Grouping Firms and Computing Returns.

[13]These estimates of the standard errors of groups' $\hat{\beta}_g$ were not computed in the normal manner, i.e., from a time-series of $\hat{\beta}_g$ which would be taken from a regression of $\widetilde{R}_{gt}$ on $\widetilde{R}_{mt}$ over t time periods. This method is not feasible here because firms that made ACs in different time periods t are aggregated into each group g. Consequently, there is more than one R_{mt} pertaining to each R_{gt}. Fortunately, however, Fama and MacBeth have developed a way of approximating the standard error of a group (or in their case, a portfolio) $\hat{\beta}_g$ ($\hat{\beta}_p$). They averaged the standard errors of firms' $\hat{\beta}_i$ in their respective portfolios. Then they computed the time-series estimates of the standard errors of the $\hat{\beta}_p$ of their (legitimate) portfolios and found that the ratio of the simple average standard error of the $\hat{\beta}_i$ in a portfolio to the standard error of the portfolio $\hat{\beta}_p$ was between three and seven. This means that the $\hat{\beta}_p$ of portfolios (of around size 40

in their sample) can be estimated with between three and seven times the precision of the average of the standard errors of the $\hat{\beta}_i$ in a portfolio. See Fama and MacBeth, pp. 615-621.

[14]See Table VI of this study and Table 5 of Gonedes (1974a) for a comparison of group $\hat{\beta}_g$ and portfolio $\hat{\beta}_p$, respectively. For the standard errors of Gonedes' $\hat{\beta}_p$, see Gonedes (1974a), p. 5.6.

[15]Gonedes (1974a), p. 219. $\underline{\theta}_t'$ and $\underline{\theta}_t''$ in Gonedes can be thought of as $\underline{\theta}_1$ and $\underline{\theta}_2$, respectively, in this study, omitting the time subscript.

[16]See Kaplan and Roll, pp. 237-239 and Sunder (1975), p. 313. In each study the API's nonzero behavior is contained within a period of around six to eight months of the release of the preliminary earnings report.

[17]For a discussion of the McNemar test, see W. J. Conover, Practical Nonparametric Statistics (New York: Wiley, 1971), pp. 141-143.

[18]The 525 ACFs and 431 NCFs that are listed in ATT each had five β-estimation years, or totals of 2,625 and 2,155, respectively (ignoring the fact that around five percent of the firms' β_i were estimated over fewer than five years' data). The unavailability of the list of 1966 ATT firms that made ACs accounts for the fact that these numbers of β-estimation years are not 2,625 and 2,155. Specifically, the number of β-estimation years analyzed for the 525 ACFs is the sum of five years (1967-1971) multiplied by the 131 ACFs for 1972 that were listed in ATT plus four years multiplied by the 394 ACFs for 1968-1971 that were listed in ATT, for a total of 2,231. The number of β-estimation years analyzed for the 431 NCFs is the sum of five multiplied by ninety-six plus four multiplied by 335, for a total of 1,820.

[19]Of the 600 firms listed in the 1965 edition of ATT, 302 changed to the flow-through method of accounting for the ITC. See Kaplan and Roll, p. 229. During 1971 and 1972 the numbers of the ATT 600 that changed to the equity method were 86 and 66, respectively.

[20]See Ball, Table 2 on p. 7.

[21]The contrast in the authority of American Institute pronouncements is clearly seen in American Institute of Certified Public Accountants, Accounting Research Bulletin No. 51 (August 1959): Notes, and American Institute of Certified Public Accountants, Accounting Principles Board Opinion No. 6 (October 1965): paragraph 1 and Notes.

[22]Of Ball's 267 ACs, seventy-five decreased net income. Of the 730 ACs analyzed here, only sixty decreased net income. See Ball, Table 7.

[23]Briefly stated, the regression effect is a statistical artifact which dictates that values of x estimated for subjects selected from the tails of the distribution of x are exaggerated. Black, Jensen, and Scholes observed in their empirical tests of the CAPM that the β_i of high-risk firms tend to be overestimated while the β_i of low-risk firms are typically underestimated. Thus, while the positive skewness in the present sample of ACFs' $\hat{\beta}_i$ is very real, it is likely to be somewhat less pronounced than Table VII makes it appear.

Footnotes - Chapter 5

[1]Four months before the FYE is equivalent to six months before two months after FYE, when the AC was assumed to have been disclosed. The latter is a more precise statement of the β-estimation period actually employed.

[2]He did observe that around seventy-six percent of his sample firms had December FYEs. See p. 4.8 of Gonedes (1974a).

[3]Fama (1970), pp. 393-399.

[4]Donald F. Morrison, Multivariate Statistical Methods (New York: McGraw-Hill, 1967), pp. 117-124.

[5]T. W. Anderson, An Introduction to Multivariate Statistical Analysis (New York: Wiley, 1958), pp. 101-108.

[6]Morrison, pp. 152-153.

[7]G. E. P. Box, "A General Distribution Theory for a Class of Likelihood Criteria," Biometrika 36 (1949): 317-346.

[8]The small sample correction factors are:

Eq. 12 $$A_1 = \frac{2R^2 + 3R-1}{6(G-1)(R+1)} \left(\sum_{g=1}^{G} \frac{1}{n_g} - \frac{1}{N}\right) \text{ and}$$

Eq. 13 $$A_2 = \frac{(R-1)(R+2)}{6(G-1)} \left(\sum_{g=1}^{G} \frac{1}{n_g^2} - \frac{1}{N^2}\right), \text{ where } N = \sum_{g=1}^{G} n_g.$$

For further details, see Box, p. 334 and p. 338.

[9]Morrison, pp. 152-153.

Footnotes - Chapter 6

[1]Let ACF (i,j) represent an accounting change firm, where i = directional effect of the accounting change on net income and i can take on one of four values: +, -, 0, or ?; j = relative discretion of management in making the change and j can take on one of three values: D for discretionary, N for nondiscretionary, or B for both a discretionary AC and a nondiscretionary AC in the same year.

[2]In Tables VIII and IX and throughout the remainder of the chapter "mean" average return differences are referred to simply as average return differences for purposes of clarity. The double averaging procedures that give rise to the "mean" average return differences result from (1) the cross-sectional averaging across firm returns to arrive at group returns for each month and (2) the averaging across the monthly group returns to obtain the intertemporal average group returns. Between the averaging steps (1) and (2) above, the difference between ACF group returns and NCF group returns is taken to obtain the return difference.

[3]Ball and Brown, p. 176; Beaver (1968), p. 84.

[4]See, for example, James H. Lorie and Mary T. Hamilton, The Stock Market: Theories and Evidence (Homewood, Ill.: Irwin, 1973), Chapters 6-9.

[5]The reason for there being only thirty pairs of firms in this test, whereas there were forty-three pairs in the test of ACF (-,·) - NCF, is due to the difficulty in matching ACF (+,·) and ACF (-,·) on FYE, $\hat{\beta}_i$, and industry. Of course, the principal constraint is the small number of firms that made ACs with negative effects on net income.

[6]Ball, p. 28.

[7]Bremser, p. 572.

[8]Archibald (1972), p. 28.

[9]See Archibald (1972), p. 28 and Kaplan and Roll, pp. 237-239. Archibald does not present the results on a control group of NCFs. Kaplan and Roll's Figure 1-Panel A

gives the cumulative average API of ITC change firms, and Figure 1-Panel B gives the same data for the ITC control group, both for essentially the same period of time (in relation to FYE) covered in this study. In order for Kaplan and Roll's results to be comparable to the results of this study, the cumulative API of their NCFs must be subtracted from the cumulative API of their ACFs. When this is done, the relative market reaction toward (discretionary) ACs to flow-through for the ITC is mildly negative prior to the publication of the annual report and distinctly negative thereafter. The cumulative average return difference (on ACFs-NCFs) is around -.015 in the twenty-fourth week before the preliminary earnings report is announced, as compared to the average on both risk classes of close to .00 during month t = -6 in this study. Their cumulative average return difference is around -.015 in the announcement week, compared to -.01 during month t = 0 here. Twenty-four weeks later, their cumulative average return difference is around -.075, whereas the overall CARD in this study is -.06 six months later. (See Figure 10.) Kaplan and Roll's report of the results of firms that changed to straight-line depreciation was not accompanied by a report on a control group. However, the cumulative average API on depreciation change firms was negative through most of the sixty-week range around Kaplan and Roll's week 0.

[10]See Kaplan and Roll, pp. 237-239.

[11]Sunder (1975), p. 313.

[12]Ibid.

[13]The author is conscious of his own caveats with respect to ventures into the area of predicting better-or-worse wealth positions as related to ACs. However, the market appears to perceive ACFs (O, D) as "better" than the matched control group, and there is some reason for this phenomenon. This exercise is an attempt to identify the reason.

[14]Sunder (1975), p. 313.

[15]Each of eighteen different hypothesis groups is tested over three time periods for a total of fifty-four different hypothesis tests.

[16]Recall that the market reaction to nondiscretionary ACs with positive income effects was significantly positive in the six months after the year-end disclosure month. See line fourteen of Table IX and Figure 14.

[17]See the Chapter 5 discussion of Bartlett's M-statistic (Eq. 10), and particularly the chi-square transformation used to test the equality of covariance matrices with large samples. In all the statistical tests conducted in this study, the samples are monthly observations of security returns. In this case, the sample size is large because the test spans the sixty months of data over which the β_i of firms were estimated.

[18]See the Chapter 5 discussion of the transformation of Bartlett's M-statistic (Eq. 10) into an F random variable (Eq. 14).

[19]Only eight of the twenty-seven F-values exceed one. So for almost all hypothesis tests the confidence level exceeds .75 by a wide margin.

[20]It appears that the only way to conduct the test so that meaningful conclusions can be reached is to increase the monthly observations of the impact period to a number approaching sixty. This would give the test of $\Sigma_1|\underline{\theta_1} = \Sigma_2|\underline{\theta_2}$ statistical power comparable to that in the test of $\Sigma_1 = \Sigma_2$. Then the results on the two sets of tests could be interpreted in terms of the information content of $\underline{\tilde{\theta}_i}$. Unfortunately, the focus on the thirteen months around the AC disclosure date makes this alteration impossible so long as returns on firms that made ACs in different years are aggregated in arriving at $\underline{\bar{R}_{it}}|\underline{\theta_i}$ where $t = 0$ is the AC disclosure month. The alternative is a design similar to that used by Gonedes. He observed that his estimated portfolio risk coefficients were relatively constant across t. Because of this stability he treated the stocks in the portfolio for each year as though they were the same stocks in the portfolios for the other four years of his five-year impact period. This allowed him to aggregate monthly returns per year end-on-end for five years to provide sixty monthly observations. The reason this aggregation method was not used in the present study is the widely varying number of ACFs during the years 1968-1972 (see table III in chapter 4). This wide variation would make the assets used for estimation in one year (for example, 1968, when there were forty-six ACFs) different from the assets in another year (for example, 1971,

when there were 167 ACFs) in terms of their variabilities. This difference would render the end-on-end aggregation invalid.

[21]This average does not include the effect of one equity-method AC, which increased the firm's EPS by 542.9 percent. Inclusion of this extreme case increases the average effect on EPS to +26.1 percent for the high-risk firms.

Footnotes - Appendix B

[1]Roger C. Ibbotson and Rex Sinquefield, "Stocks, Bonds, Bills, and Inflation: Year-by-Year Historical Returns (1926-1974)." To appear in Journal of Business 49 (January 1976).

[2]The variance of the return on the market portfolio, Var(R_m), was computed on the monthly price changes of the Standard and Poor's 500 average for January 1968 through December 1972.

[3]See footnote thirteen of Chapter 4 and the text material pertaining thereto for the manner in which the estimated standard errors of $\hat{\beta}_g$ were derived.

Footnotes - Chapter 7

[1]A related examination of _Wall Street Journal Index_ entries for a small group of the firms in the sample indicates that relatively few firms disclosed their ACs prior to year-end and that most of the year-end disclosures were made in the annual report.

[2]Bremser, p. 572.

[3]William H. Beaver, "What Should Be the FASB's Objectives?" _Journal of Accountancy_ 136 (August 1973): 56.

[4]Were it not for the disclosure of ACs (by whatever means and in whatever form the disclosures take), investors would not even know that the changes had occurred. The very existence of AC disclosures probably arises from the auditing standard of consistency, and even that can be traced to the same institutional arrangements that created the Committee on Accounting Procedure, the APB, and the FASB. Thus, the inseparability of AC disclosures and accounting rule-making bodies is undeniable.

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