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Differential market reaction to selected accounting changes.

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DIFFERENTIAL MARKET REACTION TO SELECTED ACCOUNTING CHANGES

A Dissertation Presented

By

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ABSTRACT

Differential Market Reaction to
Selected Accounting Changes

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A number of previous studies have indicated that not all accounting principle changes convey the same information signal to financial statement users. Financial statement users may perceive different implications for various types of accounting principle changes. Accounting Principle Board Opinion No. 20, however, essentially requires uniform accounting and disclosures for all changes in accounting principle. If financial statement users do perceive different implications for various types of accounting principle changes, it can reasonably be argued that the accounting for all types of changes in accounting principles should not be the same. It may better serve the needs of financial statement users if the information that is most relevant for each type of change is disclosed.

This study develops a typology of accounting principle changes. The typology consists of a dichotomy

based on whether or not the change possesses economic consequences. A change can have economic consequences for four reasons: 1) the change itself has a direct economic impact on the firm, 2) the change has an indirect economic impact on the firm via its effect on managerial behavior, 3) the change is associated with events having an economic impact and 4) the change, independent of the above three considerations, provides financial statement users with a new view of economic reality. It is further suggested that since financial statement users are concerned about economic decisions, only changes possessing economic consequences will be relevant to them (i.e., have information content). Since information content is often used as a rationale to justify costly accounting disclosures, an assessment of the information content of various accounting principle changes should be of interest to accounting policy makers.

The information content of three accounting principle changes was examined in this study: changes to LIFO, changes in response to SFAS No. 13 and changes to the flow-through method of accounting for the investment tax credit. The latter two changes were treated as a composite since neither was thought, on an a priori basis, to possess economic consequences.

Information content was operationalized by examining the stock market reaction associated with the accounting principle changes. A matched-pair control group was used. This study also employed a multivariate approach in assessing market reactions. The dependent variables examined were risk adjusted rates of return, changes in systematic risk and abnormal trading volume activity. Statistical significance was assessed using Hotelling's T^2.

The results of the study indicated that there was an adverse market reaction to the LIFO changes in terms of risk adjusted rates of return. This finding runs counter to the conventional wisdom which posits that the market would react positively to a LIFO change because of the associated improvement in cash flows generated by reduced tax payments. Subsequent analyses indicated that the LIFO changes examined in this study acted as signals regarding increased inventory costs that could not be passed along to consumers. There was no discernable market reaction to the other changes examined.

LIFO changes were found to posses information content and therefore to be worthy of disclosure. Changes induced by SFAS No. 13 and changes to the flow-through method

were found not to have information content. The rationale used to justify disclosures relating to these changes must rest on grounds other than information content. A small sample size for the latter two changes and the possibly time specific nature of the LIFO results are two primary limitations of this study.

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INTRODUCTION

Problem Statement

It is well recognized that a major objective of financial accounting is to provide decision relevant information to the users of financial statements. The Financial Accounting Standards Board (FASB), for example, has taken the position that "Financial reporting should provide information that is useful to present and potential investors and creditors and other users in making rational investment, credit and similar decisions" (1978, paragraph 34). One area where financial accounting and reporting is well articulated relates to changes in accounting principles. Accounting Principles Board (APB) Opinion No. 20, Accounting Changes, details the accounting and disclosure requirements for changes in accounting principles. Broadly stated, the objective of this study is to assess the information content of selected accounting principle changes and their related disclosures.

More specifically, this paper investigates the information content of joint signals related to changes in accounting principles. Several recent studies have

indicated that the market does not react uniformly to the same accounting event. Rather, the market reacts differentially with respect to similar events, where the differential reaction is a function of other related events or variables. Harrison (1977) found different stock return activity associated with discretionary vs. nondiscretionary accounting changes that increased net income. Abdel-khalik and McKeown (1978a) found a significant interaction between a firm's decision to switch to the Last-in, First-out (LIFO) inventory method and the sign of the forecasted change in net income. These studies can be viewed as part of a larger and growing body of research examining the joint information effect of accounting items and other variables (see e.g., Patell (1976) and Griffin (1976)).

The studies cited above indicate that not all accounting changes convey the same information signal to financial statement users. Financial statement users may perceive different implications for various types of accounting principle changes. APB Opinion No. 20, however, essentially requires uniform accounting and disclosure for all changes in accounting principles. If financial statement users do perceive different implications for various types of accounting principle changes, it can reasonably be argued that the accounting

for all types of changes in accounting principles should not be the same. It may better serve the needs of financial statement users if the information that is most relevant for each type of change is disclosed.

For example, due to current tax regulations, switches to LIFO are usually associated with an improved cash flow generated by reduced tax payments. The improvement in cash flow is a major result associated with the LIFO change. It may be desirable for the tax savings associated with a LIFO switch to be disclosed. Other changes in accounting principles are not necessarily associated with a tax savings but may reflect other changes in the economic situation of the firm. A change in depreciation method may reflect a change in management's perception of the pattern of expiration of fixed assets' services. For this type of change, it may be beneficial to disclose the impact of the change on depreciation expense. Still other accounting changes may be associated with no alteration in the economic situation of the firm.

To reiterate, if different types of accounting principle changes provide dissimilar information signals, there is no compelling rationale to require uniform accounting and disclosure across all types of changes in

accounting principles. A more useful approach would be to tailor the accounting for each type of change so that the information most relevant for a certain type of change is provided. This study is an attempt to assess whether financial statement users do perceive different changes in accounting principles as emitting different information signals. In particular, this study examines whether certain accounting principle changes possess information content and whether others do not. Operationally, this is accomplished by examining the stock market reaction to various types of changes in accounting principles. This is a first step toward the delineation of specific disclosure requirements for different types of accounting principle changes. The identification of the nature and extent of these disclosure requirements is not the topic of this study.

The finding that certain groups of changes have information content and other groups do not has importance for the formulation of accounting policies. The process by which accounting data are accumulated and communicated to financial statement users is a costly one. The monetary costs involved include salaries, computer usage and auditing fees among others. Costs of a different nature are also borne by financial statement users. They must process and analyze the data that appears in

financial statements; these costs will increase as the amount of data that is provided expands. One rationale that is often invoked to justify costly accounting disclosures is that of information content. Accounting disclosures are said to be warranted if they provide information that is useful to financial statement users in making economic decisions. If it can be shown that certain accounting changes do not possess information content, justification for disclosures relating to these changes must rest on other grounds. If this support does not exist, the FASB should consider the elimination of disclosure requirements for those changes lacking information content.

On the other hand, disclosure of changes that do have information content can be justified on the grounds that they convey decision relevant information. Furthermore, if the information content is related to economic consequences, the FASB should consider tailoring the disclosure requirements for different types of changes so that the information most relevant with respect to each change is disclosed. It should be noted that the FASB has recently expressed interest in the economic consequences of mandated changes in accounting principles (see Zeff (1978)) and presumably is also concerned about the economic consequences of discretionary changes in

accounting principles.

Typology of Accounting Principle Changes

A necessary element of this study is the identification of a useful classification scheme or typology of accounting principle changes. The scheme should be useful in the sense that it possesses the potential for generating accounting policy recommendations. That is, the scheme should be structured so that an a priori argument can be advanced regarding 1) which changes might be relevant to financial statement users and 2) what the appropriate disclosures might be for the various categories of the scheme.

Harrison (1977) investigated joint information signals related to accounting changes by developing a typology based upon the discretionary/nondiscretionary dichotomy and the sign of the effect of the change on net income. It will be argued in Chapter II that Harrison's classification scheme has certain shortcomings. It does not, for example, provide any compelling rationale as to why a differential reaction would be expected and therefore holds little promise in the way of recommendations for accounting policy.

An improved scheme based upon whether the change

possesses economic implications for the firm is presented in Chapter III. Certain types of changes have an economic impact on the firm or are associated with events having an economic impact, whereas other changes, often labeled as "bookkeeping" or "cosmetic", do not. This study will test the hypothesis that financial statement users will not react in a uniform manner to these two broad groups of changes in accounting principles. It is expected that financial statement users will percieve those changes that have direct or indirect economic implications for the firm as having information content and that they will perceive those changes that do not have economic implications for the firm as not having information content.

Three specific accounting principle changes have been selected for study. Changes to LIFO have been selected as being representative of a change having economic implications. Changes to the flow-through method of accounting for the investment tax credit (ITC) and changes induced by Statement of Financial Accounting Standards (SFAS) No. 13, Accounting for Leases, have been selected as being representative of changes that do not have direct economic implications. The typology of accounting principle changes used here and the rationale for selecting the three changes mentioned above are discussed more fully in Chapter III.

Methodological Improvements Over Past Studies

A number of previous studies have examined the stock market reaction to changes in accounting principles. Only a limited amount of confidence can, however, be placed in these studies due to certain weaknesses in their research designs. There are two major problems. One relates to the selection of a control group and the other concerns the absence of statistical tests. Generally, the control groups used in prior studies were selected randomly from those firms that did not make any accounting change. The possibility then arises that the dependent variable is influenced by confounding variables. This can occur because firms cannot be randomly assigned to the change and nonchange groups; they have pre-selected themselves into these groups and this pre-selection may be correlated with events or variables other than the act of changing accounting principles. Any difference between the two groups on the dependent variable may potentially be attributed to factors other than the one of interest (i.e., the accounting change) [1].

The approach taken in this study to deal with the confounding variable problem was to choose control groups on the basis of a careful matching. The matching process, the variables used in this process and the reasons for selecting these variables are described in Chapter IV.

The other problem of the research designs of past studies is that no formal statistical comparisons were made between the experimental and the control groups [2]. Usually cumulative average residuals (CARs) were plotted and visually compared. This study utilizes Hotelling's T^2 which is the multivariate analog of the univariate t test to assess differences between the change and nonchange groups.

Overview of Subsequent Chapters

Chapter II surveys the major empirical studies on accounting changes. This review is done in a critical manner so as to help provide the motivation for the current study. Only those accounting change studies that have a bearing on this research project are included in the literature review.

Chapter III presents the typology of accounting principle changes that is used in the study. The typology is described in detail and the reasoning for the selection of the three changes examined in this study is discussed. The hypotheses tested in this study are also presented.

Chapter IV describes the methodology employed in this study. The hypotheses generated in Chapter III are operationalized by explicitly defining all variables and

describing the statistical tests used. The matching process used in the generation of the control groups is described as well.

Chapter V contains the results of the analyses. The sample of change and nonchange firms are identified along with the results of the matching process. The results of the tests of the research hypotheses are also presented.

Chapter VI includes conclusions reached based upon the results described in Chapter V, limitations of the study and suggestions for future research.

Footnotes

1. This issue is discussed in detail in Foster (1980).

2. Harrison (1977), Abdel-khalik and Mckeown (1978a) and Brown (1980) are prominent exceptions.

C H A P T E R II

LITERATURE REVIEW

A number of past research efforts have empirically investigated the stock market reaction to accounting changes. However, only those having direct implications for the research conducted here are reviewed. Studies treating accounting changes as a composite and studies dealing with accounting principle changes other than changes to LIFO, changes in the accounting for the investment tax credit and changes in response to SFAS No. 13 will not be discussed.

LIFO Studies

Ball (1972) examined 71 changes to the LIFO inventory method that occured from 1947 to 1960 [1]. He found that on average, in the year prior to the change, firms that switched to LIFO earned a risk adjusted rate of return of 7%. An inference that can be drawn from this result is that the market perceived there to be tax advantages associated with LIFO and used this information in establishing the securities' equilibrium prices. In other words, the market perceived the change to have information content.

Sunder (1973) studied both changes to LIFO and changes to the First-in, First-out (FIFO) inventory method. With respect to the LIFO group, he found results similar to Ball's. Sunder accumulated residuals for the fiscal year in which the change was implemented. On average, the LIFO group experienced a 5% risk adjusted return and the FIFO group earned a .8% risk adjusted return. Again the inference could be drawn that the market used the information regarding the perceived tax advantages associated with LIFO in establishing equilibrium prices of the firms' securities. Sunder (1975) re-examined his sample using techniques to adjust for a possible change in the firms' systematic risk. Substantially the same results were found.

Both the Ball (1972) and Sunder (1973; 1975) studies suffer from several methodological flaws. Ball does not use an explicit control group. Implicitly, he is using the entire market as a control by employing as his dependent variable risk adjusted rates of return. This does not, however, control for a host of variables that could account for the departure of the residuals from zero. Industry factors are an example. In fact, Sunder (1973) provides some evidence on this point. His sample contained a disproportionately large number of steel firms. While the risk adjusted return of the entire LIFO

group for the year of the change was 5%, the risk adjusted return for the steel firms that switched to LIFO was 18% for the same period. It therefore seems quite plausible that the industry composition of a sample may have a marked impact on the results and accordingly this variable should be explicitly controlled for.

A recent study by Brown (1980) suggests that yet another confounding variable may have had an impact on both Ball's and Sunder's results. Brown randomly selected a group of firms that changed to LIFO and a group of firms that did not. Table 1 summarizes the earnings history of these two groups. Since 73 out of 86 of the change companies switched to LIFO in 1974, the results of that year are of the most interest. It can be seen that in every year the average earnings per share (EPS) of the change companies exceeds that of the nonchange companies. Moreover, in 1973 and 1974 the differences are statistically significant at the .05 and .001 level, respectively. It is also interesting to examine the increases in EPS from year to year. In all years, the change group experienced a greater increase in EPS (in both absolute and percentage terms) than did the nonchange group.

The implication of the above for the interpretation

of the Ball and Sunder studies is as follows. LIFO generally results in tax savings and this is usually considered to be the primary motivator for management to switch to LIFO. As income prospects improve, the tax savings associated with LIFO increase and the motivation to switch to LIFO accordingly increases [2]. It would not be surprising to find that firms that have switched to LIFO have also recently experienced a favorable growth in earnings. This is exactly what Brown has found. His two groups differ not only with respect to the LIFO switch, but with respect to EPS and changes in EPS. Furthermore, there is theoretical support for the contention that earnings are associated with the value of the firm (e.g., Miller and Modigliani (1958) and Hamada (1972)), and ample empirical evidence on the association of stock price changes with unanticipated changes in earnings (e.g., Ball and Brown (1968) and Beaver et al. (1979)). Ball's and Sunder's results can now be reinterpreted. The positive risk adjusted rates of return they found to be associated with LIFO changes may well not have been solely a reaction by the market to the switch to LIFO, but may also have reflected a reaction to another variable that distinguished the two groups: favorable unanticipated earnings changes.

The final study to be reviewed regarding LIFO changes

was conducted by Abdel-khalik and McKeown (1978a). They used analysis of variance to examine the CARs of 107 firms that switched to LIFO and a matched control group [3]. By using a factorial design, they also assessed the impact of the sign of the forecasted change in earnings on the CARs. A significant interaction was found between the decision to switch to LIFO and the sign of the forecasted earnings change. Firms that switched to LIFO and that had a forecasted increase in earnings had substantially larger CARs than did any other group. This implies that the market is selective in its interpretation of accounting information. A firm that experiences an earnings increase will, ceteris paribus, benefit more from the tax advantages of a LIFO switch than a firm that experiences an earnings decrease [4]. The results of this study indicate that the market used information about the forecasted increase in earnings along with the LIFO switch in establishing equilibrium prices. After isolation of the interaction, the main effects were not significant.

ITC Studies

Another group of studies has examined changes from the deferral to the flow-through method of accounting for the ITC. Kaplan and Roll (1972) were the first to examine the market impact of this type of change. Their sample consisted of 275 firms that changed to the flow-through

method of accounting for the ITC and 57 firms that maintained the use of the deferral method. Weekly measures of risk adjusted returns were calculated for the 30 weeks before and after the date of the change (i.e., the earnings announcement date). It was found that the change group experienced consistently positive risk adjusted rates of return during the 10 weeks surrounding the earnings announcement. In the ensuing weeks, however, these gains to stockholders were virtually eliminated. This finding is consistent with an efficient capital market (in the semi-strong sense) and an information market characterized by an imperfection. With the preliminary earnings figure inflated by the accounting change (which may then be unknown to capital market participants), investors responded by bidding up a security's price. When the accounting change subsequently became known to the market with the publication of the annual report, the security price correspondingly declined.

The control group experienced a 9% risk adjusted return over the 60 week test period. Kaplan and Roll speculate that managers of the control group firms did not switch methods of accounting for the ITC because they anticipated reporting subtantial earnings increases in any event. The possibility certainly exists that the abnormal

CARs of the control group are, in part, caused by improved earnings.

Cassidy (1976) provides some evidence as to why the CARs of Kaplan and Roll's experimental and control groups differ. He studied approximately the same sample of firms as did Kaplan and Roll. He found that during the five years prior to the change, firms that switched methods experienced negative risk adjusted rates of return and that nonchange firms earned positive risk adjusted returns over the same period. This indicates that the difference in the CARs between the change and nonchange groups found by Kaplan and Roll may have been caused by a confounding variable: favorable business activity reflected by abnormal wealth increases to shareholders.

There is one problem common to both the Kaplan and Roll study and the Cassidy study. Both conducted binomial tests on the change and nonchange groups separately. The characteristic of concern was the proportion of positive (or negative) residuals. A significant test statistic is taken to be an indication of a market reaction to the change. However, it is important to note that the test period was centered on the date of the earnings announcement. It may be then, that a significant binomial test should be interpreted as a reaction by the market to

an unanticipated earnings change. As with the LIFO studies, the confounding variable of differential unanticipated earnings changes may, in part, explain past empirical results.

SFAS No. 13 Studies

Martin et al. (1979) examined the risk adjusted rates of return of 17 firms that engaged in a substantial amount of leasing. The period of time over which the residuals were examined included 36 months both before and after the announcement month. However, focusing only on the period starting five months before the announcement date and ending five months after the announcement date, the average risk adjusted rate of return amounts to approximately -5%. At face value, this would be an indication of an adverse market reaction to changes brought about by SFAS No. 13. However, only limited statistical tests were undertaken and no control group was employed. These flaws in the research design reduces the confidence that can be placed in this study.

Only one other study has empirically examined the market reaction to accounting principle changes motivated by SFAS No. 13. Finnerty et al. (1980) investigated whether firms that engaged in substantial leasing were characterized by a change in systematic risk. They

employed a control group matched on industry as well as a randomly selected control group. They found no change in systematic risk for the change group or either of the control groups.

In summary, many of the past studies dealing with LIFO changes, changes in the accounting for the ITC and changes in response to SFAS No. 13 have been flawed by problems in their research designs. In ex post research, experimental units self-select themselves into the experimental and control groups. This self-selection may be on the basis of variables other that the one of interest. It may be these other variables to which any difference between groups on the dependent variable should be attributed. Limited evidence, cited above, is available which indicates that confounding variables were present in previous studies. It is essential that future studies take steps to ensure that these confounding variables do not differentially influence the dependent variables across groups.

Harrison's Study

The final study to be reviewed, Harrison (1977), has many similarities to the research which is proposed here. He argues that not all accounting changes provide the market with the same information signal. Discretionary

accounting principle changes provide information regarding a new income construct and also imply something regarding managements' motivation for undertaking the change. Nondiscretionary accounting principle changes provide signals primarily regarding the new income construct since management is not voluntarily undertaking the change. Furthermore, the sign of the effect of the change on net income is thought to interact with the act of making a change. A discretionary accounting change that increases (decreases) net income may imply that management is anticipating unfavorable (favorable) business prospects in the future. On the other hand, nondiscretionary accounting principle changes that increase (decrease) net income may reflect managements' past conservative (liberal) bias in reporting net income.

Harrison (1977) examined 280 accounting changes and classified them into four categories based upon the discretionary/nondiscretionary characteristic and the sign of the impact of the change on net income. Four matched-pair control groups, one for each type of change, were selected. Matching was done on 1) industry, 2) systematic risk and 3) fiscal year end. For each type of change, total returns on iso-beta portfolios of the change and nonchange groups were compared. The returns were accumulated over a thirteen month period centered on the

second month following the fiscal year end. The average return to firms making discretionary accounting changes that increased net income were significantly below that of their control group, while the average return to firms making nondiscretionary accounting changes that increased net income were significantly greater than the returns of their control group.

While the results indicate that the market uses the discretionary/nondiscretionary characteristic of the change, it is difficult to speculate as to why. The evidence indicated that the market penalizes firms making discretionary changes that increase net income. Presumably this is related to managements' motivation. However, as Harrison points out, "the list of potential sources of motivation is almost endless" (1977, p.85). Therefore, although this study indicates that the market does not perceive all accounting changes uniformly, it does little to answer the question of why. Moreover, the results as well as Harrison's a priori reasoning do not provide a firm basis for the generation of specific testable hypotheses. For example, Harrison hypothesizes that firms making a nondiscretionary accounting change that increases net income will experience differentially greater returns than a control group because the change is evidence of managements' former conservative bias in

stating net income. However the efficient market hypothesis would indicate that in past years investors would be aware of this bias and estimate any adjustment to net income that they feel is appropriate. It is only corrections to this adjusted figure, not corrections to the previously reported income construct, that will cause an investor reaction. Therefore, the sign of the difference between the new and old income constructs should not be the basis of an hypothesis regarding investor reaction; the hypothesis should be based on the sign of the diffenence between the new income construct and the old construct as adjusted by the market.

The next chapter describes a classification scheme based upon the economic impact of a change that does permit one to speculate on the market impact of an accounting change.

Footnotes

1. Ball actually examined 267 changes of many types and his primary analysis is on the entire group. However, he also disaggregated his sample based upon the type of the change and reported those results.

2. All other things being equal, firms with increasing earnings may potentially benefit from a LIFO switch more than other firms. Implicit in this proposition is the assumption that as earnings increase, inventory holding gains also increase.

3. Matching was based upon industry and systematic risk.

4. See footnote 2.

C H A P T E R III

TYPOLOGY OF ACCOUNTING PRINCIPLE CHANGES

The hypothesis that various changes in accounting principles emit different information signals to the market is a reasonable one. However, prior to the undertaking of an empirical investigation or the making of an accounting policy recommendation, it is essential that a framework be developed that provides a useful perspective of changes in accounting principles. The framework or typology should fulfill two functions:

1. it should serve as the basis to group various changes in accounting principles based on the correspondence between the characteristics of the change and the categories of the typology, and
2. the categories of the typology should provide guidance in the formulation of specific disclosure and accounting requirements

In addition to Harrison's (1977) scheme, which was reviewed in the previous chapter, several other typologies of accounting changes have been suggested in the accounting literature. They are presented below.

Gonedes and Dopuch

Gonedes and Dopuch (1974) developed a typology based upon the effects of the change. Their typology consists of five categories (p. 84):

1. A change in the techniques used for external reporting (one result of a firm's information-production decisions) that does not affect the information-production costs incurred by the firm or the information-production decisions and/or costs of agents external to the firm

2. A change having the properties of 1. except that it does affect a firm's information-production costs

3. A change having the properties of 1. except that it does affect the information-production decisions and/or costs of agents external to the firm

4. A change in the techniques used for external reporting that is, for whatever reason, associated with a change in some other aspect of a firm's production-investment activities

5. A change in the accounting techniques used for tax reporting

It may well be a misnomer to label the above classification scheme as a typology. The "types" of accounting changes are more in the nature of characteristics than categories, since a given change may belong to more than one category.

This typology has been appropriately critized by Park et al. (1980) on the grounds that it fails to distinguish between 1) changes that have no economic impact and do not provide financial statement users with a new view of economic reality and 2) changes that have no economic impact and do provide financial statement users with a new

view of economic reality. That is, while a change itself may have no impact on the economic situation of the firm, the accounting numbers generated by the new accounting procedure may provide financial statement users with a perspective of the firm that was previously unavailable.

Park

In an attempt to circumvent this shortcoming, Park *et al.* (1980) developed their own typology which consists of three levels:

1. the change has discernable economic effect
2. there is no discernable economic effect and it is possible for financial statement users to translate between the old and new principle
3. there is no discernable economic effect and it is not possible for financial statement users to translate between the old and new principle

According to Park *et al.* (1980), only changes of the second type should be labeled as purely "cosmetic" or "bookkeeping".

The above typology falls short of the mark on several points. The term "discernable economic effect" in the first category is rather broad. Different changes have unlike implications for a firm and should not all be studied as a cohesive group. The first category needs to be more detailed with respect to the types of "economic

effect".

A second criticism relates to the third category. In the year of a change, APB Opinion No. 20 requires the reporting of net income under both the old and the new principle. Many companies also voluntarily disclose additional information. For example, a company that changes from the straight line method of depreciation to the sum-of-the-years-digits method is required to report the effect of the change on net income. Additionally, many companies also report the effect of the change on depreciation expense (these two amounts may differ because of tax effects). If this is the case, then financial statement users can translate from one method to the other in the year of the change. Therefore, category three is not especially meaningful.

Typology Based on Economic Consequences

In order to eliminate some of the weaknesses of the above classification schemes, a set of characteristics that can be attributed to an accounting principle change has been developed. It is based on the economic consequences of the change and consists of two broad classes. The scheme is as follows:

1. The change has economic consequences

    1. the change has a direct economic impact on the firm

    2. the change has an indirect economic impact on the firm via its effect on managerial behavior

    3. the change is associated with events having an economic impact on the firm

    4. the change provides financial statement users with a new view of economic reality independent of its economic impact

2. The change is purely cosmetic, meaning that it possesses none of the above characteristics

Categories 1 and 2 are mutually exclusive. An accounting principle change either does or does not have economic consequences for the firm. The sub-categories in category 1 are not mutually exclusive and, in fact, reflect characteristics which could be possessed to a greater or lesser degree by any change having economic consequences.

Illustrations of the categories in the scheme may serve to make the distinctions between the types of changes more clear. Changes to LIFO would generally be considered to be of type 1.1 because of the cash flow effects arising from the tax implications of this change. An accounting change involving substantial bookkeeping costs would also be of type 1.1.

A change induced by FASB Statement No. 8, Accounting

for the Translation of Foreign Currency Transactions and Foreign Currency Statements, is a good illustration of type 1.2. While FASB Statement No. 8 merely relates to the accounting for transactions involving foreign currencies, Evans et al. (1978) have found that the statement has motivated many firms to alter their foreign exchange risk management practices.

A change to the percentage of completion method for long-term construction contracts is a change of type 1.3. A change to this method would generally indicate that a firm and its auditors feel that the degree to which project costs can be estimated has improved and/or the collectability of billings to customers is more assured.

A change in depreciation method may be of type 1.4. This change may provide financial statement users with accounting numbers (a view of reality) which were previously unavailable. This assumes, of course, that there is not a concurrent change for tax purposes nor a change in the pattern by which the assets' services expire.

It should be emphasized again that types 1.1 thru 1.4 are not mutually exclusive; they are more in the nature of characteristics. More than one can apply to a given

change. For example, changes associated with FASB No. 8 possess characteristics 1.2 and 1.4. Accounting changes having economic consequences cannot, generally, be unambiguously assigned to any particular sub-category; judgement must be exercised in the assignment process by identifying the most important characteristic of each change.

A priori, it is suggested that changes of category 1 do possess information content and that those of category 2 do not. Implicit in much of the accounting literature is the presumption that an objective of financial statement users is to make economic or business decisions (see e.g., FASB (1978)). It therefore is reasonable to expect financial statement users to find disclosures about category 1 changes informative but not disclosures about category 2 changes.

Hypotheses

This study is an attempt to ascertain the positioning of certain accounting principle changes within the framework of the typology developed above. Changes to LIFO have been selected as being representative of category 1, changes that have an economic impact. LIFO was selected for study because the tax implications of a switch to LIFO have an obvious economic impact and because

the mid-1970's provides a rich data base of LIFO changes. Two accounting principle changes have been selected as being representative of category 2, purely cosmetic changes: changes in response to SFAS No. 13 and changes to the flow-through method of accounting for the ITC. Neither of these changes has a direct economic impact on the firm; nor is it easy to see them as being associated with events that do have economic significance (e.g., as in the way a change in depreciation method may be associated with a change in the pattern of the expiration of fixed assets' services). They are also the only two recently occurring changes that are not of type 1.4. That is, they are the only two changes which do not provide financial statement users with an income construct that was not previously available. All disclosures mandated by SFAS No. 13 were previously available in 10-K reports due to Accounting Series Release 147. Additionally, the information necessary to convert from the deferral to the flow-through method is also available in 10-K reports (but the information needed to convert from the flow-through method to the deferral method is not publicly available).

There is a possibility that each of these changes may affect managerial behavior. A change in the accounting for the ITC may affect the capital budget because of its impact on reported net income. Arnold (1975) provides

evidence, however, that ITC changes do not alter capital investment decisions. Lease capitalization in response to SFAS No. 13 will generally influence various financial ratios which may result in certain firms violating debt covenants. Management may then be motivated to modify the firm's capital structure in order to avoid such violation. There is as yet no published evidence regarding the managerial impact of lease accounting changes induced by SFAS No. 13. Accordingly, in addition to treating ITC changes and changes in response to SFAS No. 13 as a composite, separate tests were conducted on SFAS No. 13 changes only. An insufficient sample size did not permit separate analysis of ITC changes.

Two null hypotheses can be stated as follows:

H_o^1 : Changes to LIFO do not possess information content.

H_o^2 : Changes induced by SFAS No. 13 and changes to the flow-through method of accounting for the ITC do not possess information content.

It is expected that H_o^1 will be rejected and that H_o^2 will not be rejected.

The following chapter operationalizes the above hypotheses and describes the methodology used in this

study.

C H A P T E R IV

HYPOTHESES AND METHODOLOGY

Sample

Experimental Groups. Two sources were employed to identify firms making any of the three types of accounting principle changes examined in this study : 1) The Disclosure Journal, Index of Corporate Events and 2) Accounting Trends and Techniques. The Disclosure Journal was used to identify LIFO changes and ITC changes made during the period 1973-1975. The use of the Disclosure Journal was limited to this time period because it was not published before 1973 nor after 1976. All LIFO changes occurring after October 1975 were excluded from the sample since it was considered desirable to have four and one-half years of post-change data to estimate the market model parameters. Firms making changes to LIFO or to the flow-through method of accounting for the ITC between 1970 and 1972 were identified by referring to Accounting Trends and Techniques. Firms making ITC changes during 1976-1978 and firms making changes in response to SFAS No. 13 in 1977 or 1978 were identified by Accounting Trends and Techniques. Changes in response to SFAS No. 13 and ITC changes occurring after January 1978 were excluded from the study since two and one-half years of post-change data

were necessary to estimate the market model paramaters.

Two additional constraints were placed on firms included in the sample. First, firms making two or more of the three changes examined in this study were excluded from the analysis. Secondly, sufficient price, dividend, trading volume and accounting data, for purposes of this study, had to be available on the COMPUSTAT PDE and Industrial Tapes. The COMPUSTAT Tapes were the primary data source used. For certain companies, a small amount of price, dividend and volume data was collected from Standard and Poor's Stock Guides.

Control Groups. A primary objective of this study is the elimination of the effects of a number of potentially confounding variables. The approach used in achieving this objective was to undertake a careful matching in the selection of control groups. For each change firm in the sample, a matched-pair was selected for inclusion in a control group. Matching was done on five variables: 1) industry, 2) unanticipated earnings changes, 3) price-earnings (P-E) ratio, 4) leverage and 5) systematic risk. Below each variable is defined and a justification is presented for its use. The matching procedure is also described.

Industry was defined in terms of SIC codes. Two-digit codes were used. Because of the matching procedure to be employed in this study (which is described below), use of more than two digits would result in industry dominating the other matching variables. The reason for matching on this variable is discussed in Chapter II.

The need to control for the effects of unanticipated earnings changes was discussed in detail in Chapter II. The definitional problem, however, is substantial and must be addressed. The primary difficulty in operationally defining unanticipated earnings changes is the selection of an earnings expectation model to serve as a proxy for the market's anticipation of what a firm's earnings will be. A body of research has developed in the past decade which is of use here. That research has investigated the time series properties of accounting numbers (primarily earnings). Most of the earlier studies (e.g., Ball and Watts (1972)) found that earnings follow a random walk or a random walk with drift model. These studies were followed by another group (Watts and Leftwich (1977) and Albrecht et al. (1977)) that compared random walks and random walks with drift to Box-Jenkins models fitted on a firm by firm basis. With respect to model identification, neither the random walk nor the random walk with drift

models characterized the earnings stream of a broad base of the sample companies. It is important to note, however, that Watts and Leftwich (1977) found substantial evidence of model misspecification in fitting the Box-Jenkins models. Therefore a more appealing approach to comparing the two types of models is predictability tests. On this criteria, the Watts and Leftwich (1977) and Albrecht et al. (1977) studies differ. The former found the random walk model to compare very favorably with the random walk with drift model and the individually fitted Box-Jenkins models, while the latter study found that the random walk model performed substantially worse than the random walk with drift model and the individually fitted Box-Jenkins models.

A major problem with both of the above studies in that the sample sizes were small. Watts and Leftwich examined only 25 firms and Albrecht et al. studied 49 firms. It is not, therefore, surprising that the results of these two studies differed with respect to the relative forecasting ability of the random walk and random walk with drift models. Ruland (1980) has compared the predictability of these two models (and four others) over a sample of approximately 4,000 forecasts. He concludes that "the simple martingale dominates the other models tested" (1980, p.36). Since the random walk model was

found to predict better than the random walk with drift model (and four others), it will serve as the earnings expectation model used in this study.

Instead of relying on a mechanical model based upon past earnings to serve as the proxy for the market's expectations, several other approaches could be employed. Published forecasts by management or security analysts could be used. This approach is not used because of the lack of empirical evidence attesting to the superiority of these forecasts in comparison with the random walk model.

One other earnings expectation model that was considered for use in this study was recently proposed by Beaver *et al.* (1980). They suggest that security prices may contain information that is useful in generating earnings forecasts. This may be so for at least two reasons:

1. aggregation of earnings into yearly figures may result in a loss of information and this information may be imbedded in security prices; and
2. security prices may reflect events that will have an impact on future earnings but which have not had an impact on current earnings (e.g., discovery of oil).

By assuming 1) that reported earnings are the result

of a compound process comprised of factors that do or do not affect security prices and 2) that the earnings component that does affect security prices follows a random walk, Beaver et al. (1980) draw a relationship between security price changes and earnings changes. Empirical evidence is provided that indicates that security prices do not move as they would if investors believed earnings followed a random walk. Additionally, a forecasting model based on both earnings and security prices was compared to a random walk with drift model. The former model proved to be marginally superior.

While the idea that security prices may reflect events upon which the market conditions its earnings expectations has appeal, the forecasting model suggested by Beaver et al. (1980) is not suitable for use in this study for several reasons. Their assumption that "ungarbled" earnings follow a random walk is crucial to the derivation of their model and has not been empirically verified. Moreover, their forecasting results are limited to one time horizon and one model of comparison (random walk with drift). Accordingly, the random walk model was used here as a proxy for the market's earnings expectation. Unexpected changes in earnings is defined as earnings available for common stockholders in the year of the change (after adjusting for the effects of the change)

less earnings available for common stockholders in the year prior to the change. The effect of the change on net income was obtained from each firm's 10-K report.

Matching was also based on the price-earnings (P-E) ratio. Basu (1978) has found that the association between security prices and annual income numbers is not independent of the P-E ratio. His results are consistent with the price-ratio hypothesis which claims that investors are unduly optimistic (pessimistic) about firms with high (low) P-E ratios. This optimism (pessimism) results generally in negative (positive) unanticipated changes in earnings. Therefore the market reaction to an earnings announcement is conditional upon the P-E ratio. Ball (1978), on the other hand, interpreted Basu's results as an indication of a misspecification in the Capital Asset Pricing Model. Irrespective of which interpretation is adopted, this variable must be controlled for in order to eliminate it as a potential cause of differences between the experimental and control groups.

Earnings yield (the inverse of the P-E ratio) is defined simply as the earnings available for common stockholders in the year prior to the change divided by the market value of the common stock outstanding at the beginning of the year of the change [1]. Negative ratios

were not constrained in any manner.

Another variable that was used in the matching process was leverage. It has long been hypothesized that wealth gains (losses) will accrue to debtors (creditors) during periods of unanticipated inflation [2]. The converse is hypothesized to occur during periods of unanticipated deflation. Recently, Bloom et al. (1980) have investigated the behavior of risk adjusted rates of return on two portfolios during the period 1959-1975. One portfolio consisted of firms that were net monetary debtors and the other consisted of firms that were net monetary creditors. The symmetric pattern of the CARs of these two groups was quite evident. Creditors experienced gains (losses) when debtors experienced losses (gains). This is persuasive evidence that stock price activity is , in part, conditioned by the joint effect of unanticipated inflation and the firm's leverage. It is, therefore, important to control for this effect.

The definition of debtor position used here is the debt to equity ratio where equity is defined as the market value of a firm's common stockholders' equity and debt is defined as the book value of debt [3]. This particular measure was chosen due to its wide usage and general acceptance as a measure of net debtor position.

Matching was also undertaken on a variable that is closely related to leverage, systematic risk (beta coefficient). This variable was selected for two reasons. One reason is because of its close relationship to leverage which is well documented (see e.g., Bowman (1979)). Additionally, Black, Jensen and Scholes (1972) have shown that firms characterized by different market risk levels do not exhibit the same magnitude of risk adjusted rates of return. Firms of higher risk experience lower levels of risk adjusted rates of return than firms of lower risk. By matching on beta, this potential cause of differences between the control and experimental groups with respect to CARs will be eliminated.

A number of decisions had to be made regarding the estimation of systematic risk. For reasons cited in the next section, the familiar market model was used to generate beta estimates. A time period and estimation interval also had to be selected. Recent evidence by Alexander and Chervany (1980) indicates that an estimation interval of four to six years is optimal when using monthly observations (as are used here). Accordingly, for matching purposes, systematic risk was estimated based upon the 48 monthly observations preceeding the test period [4]. A time period prior to the change was selected since 1) it is possible for the change to affect

systemtic risk and 2) the objective of the matching process is to make the matched-pairs as similar as possible prior to the change.

The changes examined in this study took place in 26 different chronological months. Since systematic risk is not always stable over time, it was desirable to match an experimental group member's beta estimate with the beta estimates of the potential members of the control group that were generated over the same time period. Accordingly, 26 beta estimates (assuming data availability) for each potential member of the control groups were generated. A firm was considered to be a candidate for inclusion in a control group if it 1) did not make any of the three accounting principle changes examined here during the time period covered by this study and 2) appeared on the COMPUSTAT Tapes.

It is well documented that estimates of systematic risk obtained from OLS regressions are considerably biased (see e.g., Blume (1971; 1975)). A common procedure used to adjust OLS betas is Vasicek's (1973) bayesian adjustment. Recent evidence by Eubank and Zumwalt (1979) indicates that this adjustment technique performs as well as other techniques for the estimation periods used in this study. Accordingly, all OLS betas were adjusted

using Vasicek's procedure. The adjustment is:

$$b_v = \frac{b/Var(b) + b_i/Var(b_i)}{1/Var(b) + 1/Var(b_i)}$$

where b_v = the Vasicek adjusted beta

b = the cross-sectional mean of the OLS betas

$Var(b)$ = the variance of the cross-sectional distribution of OLS betas

b_i = the specific firm's OLS beta

$Var(b_i)$ = the specific firm's squared standard error of the OLS beta

Since betas were estimated for 26 different time periods, each OLS beta was adjusted using the appropriate chronological cross-sectional distribution.

A final decision relates to the selection of an index to serve as a proxy for the market portfolio. As mentioned in the next section, all tests of hypotheses have been carried out using beta estimates based upon both the CRSP Value Weighted Index and the CRSP Equal Weighted Index. Both indexes could not, of course, have been used in the matching process. There is, however, limited empirical evidence that beta estimates generated from the CRSP Equal Weighted Index are more consistent with certain properties implied by the CAPM than are betas generated

from the CRSP Value Weighted Index (see Elgers and Murray (1981b)). For this reason, the CRSP Equal Weighted Index was used in the matching process.

An attempt was made to match on one additional variable, fiscal year end. A number of news items regarding a firm are disclosed around its fiscal year end. Since the test period for both a change firm and its matched-pair is centered on the former's fiscal year end, it was thought important to match on this variable. Matching on this variable, however, would have resulted in a drastic reduction in the number of potential candidates for matching with each member of the change groups. This coupled with the use of a 10 month test period (nearly a year) prompted the dropping of fiscal year end as a matching variable.

The matching process was conducted as follows. For each change firm, all potential candidates for matching that were in the same industry were identified. For each of the firms so identified, a Mahalanobis distance measure was computed. The calculation took the form:

$$D^2 = (M_a - M_c)' W^{-1} (M_a - M_c)$$

where D^2 = the distance measure of firm a from change firm c

M_a = a four element vector of matching

variables for firm a

M_c = a four element vector of matching variables for firm c

W = the covariance matrix of the cross-section of matching variables

W is based upon the pooled cross-section of change and nonchange firms. The changes identified and used in this study covered an eight year time span; W was computed for each individual year.

All distance scores were arrayed in ascending order. Pairs were assigned based upon D^2. Starting with the smallest D^2, each score was examined to ascertain which change and nonchange firm gave rise to it. If the change firm had not yet been assigned a match and if the nonchange firm was not previously assigned to a change firm, a match was made. This process was repeated until all change firms were assigned a matched control group member.

The D^2 statistic can be thought of as a univariate measure of multidimensional differences. It is a natural extension of a Euclidean distance measure used by Martin et al. (1977), and is preferable to it in that the covariance structure among the matching variables is taken into consideration.

Dependent Variables

This study takes a multivariate approach in examining the market reaction to accounting changes. Three dependent variables are used: CARs, changes in systematic risk and trading volume reaction.

A later interpretation of the CARs will rest upon the jointly maintained hypotheses of semi-strong market efficiency and the descriptive validity of the Sharpe-Lintner Capital Asset Pricing Model (CAPM). Semi-strong market efficiency implies that security prices fully reflect all publicly available information. In operationalizing the concept of "fully reflect", one can assert that equilibrium prices (or expected returns) are established in accordance with the CAPM. The CAPM can be stated as:

$$E(R_{it}) = R_f + [E(R_{mt}) - R_f]B_i$$

where $E(R_{it})$ = the expected return on security i in period t

R_f = the risk free rate

$E(R_{mt})$ = the expected return on the market portfolio in period t

B_i = the systematic risk of security i

The assumptions that 1) market equilibrium can be stated in terms of expected returns and 2) equilibrium

expected returns fully reflect publicly available information have an important implication: trading systems based solely on publicly available information cannot result in expected returns in excess of the equilibrium expected returns. More formally, let

$$e_{it} = R_{it} - E(R_{it}/\Phi)$$

and $E(e_{it}) = 0$

where Φ = the information set available at time t-1.

Departures of e_{it} from its expected value of zero is an indication that the market has become aware of new information that necessitated a equilibrating price adjustment.

Operationally, the natural log form of the market model is used. The market model can be stated as :

$$R_{it} = a_i + b_i R_{mt} + e_{it}$$

where R_{it} = the continuously compounded realized rate of return on security i in month t

R_{mt} = the continuously compounded realized rate of return on the market portfolio

a_i = the intercept term

b_i = the estimate of systematic risk

e_{it} = a residual term that satisfies the OLS assumptions

This particular form of the market model was chosen for

several reasons. It has enjoyed widespread use and will therefore make the results of this study more comparable to the results of past research efforts. Additionally, it has been shown by Collins and McKeown (1979) that this form of the market model is relatively free of specification error.

Several studies (e.g., Abdel-khalik and Mckeown (1978b) and Elgers and Murray (1981a)) have shown that research results may be sensitive to the index used to serve as a proxy for the market portfolio. Accordingly, all tests were conducted twice using estimates of systematic risk generated by 1) the CRSP Equal Weighted Index (CEW) and 2) the CRSP Value Weighted Index (CVW).

For each firm, a residual in each month of the test period was computed as:

$$e_{it} = R_{it} - a_i - b_i R_{mt}$$

The test period consists of ten months: the month of the fiscal year end in the year that the firm (or its matched-pair) made the change, the four preceeding months and the five succeeding months. The choice of a test period is essentially an arbitrary one. The objective is to select the period during which it is likely that the market became aware of the accounting change and its

effects on the firm. Since firms often disclose accounting changes in third or fourth quarter earnings reports, the four month period prior to year end was included in the test period. Detailed information concerning a change is likely to appear in annual earnings announcements, 10-K reports and annual reports. Accordingly the five months after the year end is included in the test period.

For each of the two change and two nonchange groups, average residuals (ARs) and CARs were computed as follows:

$$AR_t = (1/N)\sum_{i=1}^{N} e_{it}$$

where N = the number of firms in the group and

$$CAR = \sum_{t=-4}^{5} AR_t$$

where t = 0 is the month of the fiscal year end in the year in which the change was made

Because of data availability problems, different estimation periods for the market model parameters were used for the two groups studied (i.e., the LIFO group and its associated matched control group and the SFAS No. 13 changes plus the ITC changes and their control group) [5]. Parameters for firms in Experimental Group 1 and Control

Group 1 were estimated over three different time periods: 1) 48 months prior to the test period, 2) 24 months both before and after the test period, and 3) 48 months after the test period. The use of different estimation periods is motivated by the consideration that the accounting principle change as well as other factors could result in an unstable beta coefficient. A comparison of the test period CARs generated from parameters based on the three estimation periods permits an assessment of the sensitivity of the CARs to the use of different estimation periods.

Only two estimation periods were used for Experimental Group 2 and Control Group 2. These periods consist of 1) 48 months before the test period and 2) 24 months both before and after the test period. Since changes in response to SFAS No. 13 occured in 1977 and 1978, 48 months of post-test period data is unavailable.

As was done in the matching process, all betas were adjusted using Vasicek's procedure. The cross-sectional distribution used in the adjustment procedure was based upon the total cross-section of firms examined in this study, that is, Experimental Groups 1 and 2 and Control Groups 1 and 2.

As indicated previously, there is the possibility that an accounting principle change may influence the market's perception of a firm's risk. For example, a change may indicate the entrance by a firm (or the expansion by a firm) in an industry. That is, management may be motivated to adopt the accounting principle that is dominant in that industry. Fama and Miller (1972) have developed a theory stating that expected return is a function of a firm's production-investment decisions (which vary from industry to industry). To the extent that the beta coefficient is considered to be a surrogate for these decisions, a change in industry may result in a change in systematic risk. Moreover, it is not unlikely that a common factor may be responsible for both the accounting principle change and the change in systematic risk. For example, an increase in inventory costs may be partly responsible for management's decision to switch to LIFO and a change in systematic risk. Finally, the change itself could be responsible for a change in systematic risk if it induces alterations in managerial behavior.

Accordingly, a reassessment by the market of a firm's systematic risk in response to an accounting change is another indication that the change has information content. Therefore, change in systematic risk is used as a second dependent variable. For Experimental and Control

Group 1, it is defined as the Vasicek adjusted beta based on the four year estimation period subsequent to the test period less the Vasicek adjusted beta based on the four year estimation period prior to the test period. Due to data availability problems, two year estimation periods were used for Experimental and Control Group 2.

The third dependent variable used in this study is trading volume. A significant CAR may be an indication of a homogeneous reassessment by the market of a firm's equilibrium price. An information item could elicit no equilibrating price change, but still possess information content because it causes various reactions across different capital market participants. With respect to an equilibrating price change, the various reactions may tend to cancel each other out resulting in the absence of a significant CAR. Several researchers (Beaver (1968) and Foster (1973) among others) have suggested examining trading volume as a measure of differential reaction to items potentially possessing information content. Accordingly, trading volume is selected as the third dependent variable to be used in this study.

In assessing the impact of an accounting principle change on trading volume, a time series model developed by Copeland (1979) was used. This model can be expressed as:

$$V_{it} = -c + dV_{mt} - fV_{mt-1} + gV_{it-1} + h_{it}$$

where V_{it} = the natural log of the number of shares traded of security i in month t divided by the number of trading days in month t

V_{mt} = the natural log of the number of shares traded on the New York Stock Exchange in month t divided by the number of trading days in month t

c,d,f and g = parameters of the model for firm i

h_{it} = ā residual term that satisfies the usual OLS assumptions

The above model has more appeal than the simple one-factor model developed by Beaver (1968) for several reasons. First, it is not ad hoc, as is Beaver's model. It is based upon assumptions regarding the effect of information arrival on trading volume. Additionally, there is evidence that the model employed here is less misspecified than Beaver's model (see Copeland (1979)).

Parameters of the model were estimated using 24 monthly observations both before and after the test period. For each month in the test period, a residual was computed as follows:

$$h_{it} = V_{it} - (-c + dV_{mt} - fV_{mt-1} + gV_{it-1})$$

Since h_{it} may vary from firm to firm simply due to differences in the number of shares of stock outstanding, it must be standarized in some manner. One option might have been to divide each V_{it} by the number of shares of firm i outstanding in month t. However, a measure of the number of shares outstanding is not available on a monthly basis. Therefore, the procedure used was to divide each h_{it} by its standard deviation. Since h_{it} is essentially a forecast error, its standard deviation is the square root of the forecast error variance (see Appendix C, expression (11)).

These standardized residuals were accumulated in the same manner that residuals from the market model were accumulated.

Trading volume on the New York Stock Exchange as well as the number of trading days in each month were obtained from Standard and Poor's Statistical Service.

Hypotheses and Statistical Analyses

For each of the two types of changes examined, separate analyses were conducted. The primary analysis consisted of a simultaneous comparison of the three dependent variables across the change and nonchange groups. Let d be a three element column vector of

differences in group centroids. The null hypothesis can be stated as:

$$H_o: d=0$$

That is, the null hypothesis states that there is no difference between the two groups when the three dependent variables are considered simultaneously.

Univariate t tests are inappropriate for use in this study for several reasons. Overall interpretation of a series of univariate t tests is difficult. One significant t test may not indicate an overall difference between the two groups. Moreover, a number of nearly significant t statistics may indicate overall group differences. Secondly, univariate tests fail to consider the covariance structure of the dependent variables.

This study utilized Hotelling's T^2 ,which is the multivariate analog of the t test, to assess the multiple dimensions of group differences. A two sample approach is inappropriate since the control group is matched with the change group and is not independent of it. A one sample approach is utilized. T^2 is calculated as:

$$T^2 = N(d' \bar{C}^1 d)$$

where N = the number of matched-pairs

C = the sample covariance matrix of the difference scores

Under the null hypothesis,

$$((N-p)/p(n-1))T^2 \sim F^{p}_{N-p}$$

where p = the number of dependent variables.

An important issue that arises when overall differences are found between two groups concerns the identification of the dependent variable(s) that contribute significantly toward the diffenence. As previously indicated, standard univariate t tests are inappropriate. These tests use the computed t statistic and the typical univariate t critical value (or its normal approximation). The multiple comparison procedure described in Morrison (1967) was used to assess the variables along which the groups differ. The test statistic used in this procedure is the univariate t statistic. However, the critical value used to reject the null hypothesis is modified so as to ensure that all individual tests on the dependent variables are simultaneously true at the desired Type I error level. The critical t value used in these tests is equal to the square root of the critical T^2 value used in the multivariate test.

The only assumption of a one sample Hotelling's T^2 test is that of multivariate normality. Mardia (1975) has shown that Hotelling's T^2 is robust with respect to this assumption, even with small sample sizes.

There is some uncertainty as to the meaning of a CAR when a firm has experienced a change in systematic risk. In the absence of a beta shift, an abnormally large CAR is an indication of an information item resulting in a equilibrating price change. However, with a concurrent beta shift, the interpretation of CARs becomes more difficult. It is unclear as to what portions of the CARs are due to information effects or to the beta change. Accordingly, additional T^2 tests were undertaken for stable beta firms only. These tests utilized two dependent variables: CARs and volume residuals. Unstable betas were detected by use of the Chow test. This procedure is described below when the ancillary tests are discussed.

In addition to the primary tests outlined above, additional tests were conducted on an individual firm basis. These tests consisted of assessments of 1) beta stability via the Chow test, 2) departures of cumulative market model residuals from zero and 3) departures of cumulative volume residuals from zero.

The Chow test (see Johnston (1972)) is designed to detect beta shifts by examining the sums of squared error terms from three regressions: one based on data from before the event of interest, one based on the data from after the event of interest and one based on both sets of data. For Experimental and Control Group 1, these periods consisted of 1) the 48 months preceeding the test period, 2) the 48 months subsequent to the test period and 3) the above two periods combined. The regression based upon the entire 96 observations was constructed so that the intercept term was allowed to vary from the first period to the second. The test statistic, which follows the F distribution, is:

$$F = (S_1 - S_2)/(S_2/(N-4))$$

where S_1 = the sum of squared error terms from the regression fitted on the data from before and after the test period

S_2 = the sum of squared error terms from the two separately fitted regressions

Due to lack of data, 24 month periods instead of 48 month periods were used in assessing beta stability for Experimental and Control Group 2.

In assessing departures of market model residuals and volume model residuals from zero, it is necessary to utilize the proper variance estimate. In each case, since the test period was excluded from the estimation period, the residuals are more in the nature of forecast errors, and it is the forecast error variance which should be employed. Moreover, since cumulative residuals are tested, the cumulative forecast error variance must be used. Appendix C derives expressions for the cumulative forecast error variance in both the simple and multiple regression frameworks.

A difficult econometrics issue has not yet been addressed. There is evidence that returns may be cross-sectionally correlated (King (1966)). In developing the CARs for each group, the residuals are added across firms. Because of the cross-sectional dependence, residuals might not be considered to be independent drawings and the statistical tests suggested above are not strictly appropriate. Cross-sectional correlation is not a substantial problem in this study for several reasons. Residuals, not returns, are being used. The common market factor is removed from the return and this should reduce the correlation (see Beaver (1980)). Additionally, the changes took place across a variety of industries. This factor should also limit the cross-sectional correlation.

One final aspect of the research design should reduce any cross-sectional correlation. Because the control groups were selected on the basis of several matching variables, the experimental and control groups are not independent and difference scores of the residuals were used. The use of difference scores reduces the cross-sectional correlation in the following manner. Cross-sectional correlation results from firms experiencing common market, industry and other factors. Since each matched-pair is similar on a variety of relevant dimensions, it is reasonable to suggest that the factors causing the cross-sectional correlation have similar impacts on each member of the matched-pair. By taking difference scores, the effects of these factors cancel; the difference scores do not reflect the impact of those factors causing the cross-sectional correlation and these scores approach independence.

Footnotes

1. This is the same definition as used by Basu (1978).

2. See, for example, Kessel (1956).

3. Bowman (1980) has provided evidence which does not support the hypothesized superiority of measuring debt at market value.

4. The test period is defined in the next section.

5. Hereafter, LIFO changes will be referred to as Experimental Group 1 and their matched-pairs as Control Group 1. ITC changes and SFAS No. 13 changes will be referred to as Experimental Group 2 and their matched-pairs as Control Group 2.

C H A P T E R V

RESULTS

Sample

Experimental Groups. 131 firms that switched to LIFO met the sample selection criteria of the study. However suitable matches could not be found for three firms. This resulted in a final sample of 128 firms. All change and nonchange firms that were examined in this study are listed in Appendix B. Table 2 contains information regarding the industry grouping of Experimental Group 1. As would be expected, the major portion of the sample consists of manufacturing firms. While a wide array of manufacturing groups are represented, certain groups dominate the sample. The most dominant groups are chemicals and allied products, paper and allied products and petroleum refining and related industries.

It is somewhat surprising that no members of the wholesale or retail industries are members of the sample. In part, this can be explained by the sample constraint that a firm switching to LIFO and making a change in response to SFAS No. 13 or changing to the flow-through method of accounting for the ITC be excluded from the study. A review of Table 4, which summarizes the industry

grouping of Experimental Group 2, indicates that a number of firms making changes in response to SFAS No. 13 were wholesalers of retailers.

Table 3 summarizes the chronological distribution of the LIFO changes. The vast majority of the LIFO switches took place in 1974. The grouping of many of the changes in one year tends to reduce the external validity of the study and also underscores the need for a careful matching.

Experimental Group 2 is comprised of 22 firms. Four firms changed to the flow-through method of accounting for the ITC and 18 changed their lease accounting in response to SFAS No. 13. The industry breakdown of Experimental Group 2 is provided in Table 4. The industry distribution is fairly broad-based. Manufacturers of food and kindred products account for the most dominant proportion of the sample.

The chronological distribution of Experimental Group 2 is provided in Table 5. All SFAS No. 13 changes took place in 1977 or 1978. As with Experimental Group 1, the chronological distribution of Group 2 is not as uniform over the years studied as would be desirable.

Control Groups. Matched firms for the control groups were selected based upon 1) appropriate industry membership and 2) the D^2 statistic described in Chapter IV. To provide a basis for assessment of the success of the matching process, Tables 6, 7 and 8 describe the cross-sectional characteristics of the matching variables for Experimental Groups 1 and 2 and all potential candidates for inclusion in the control group, respectively.

The mean leverage of the potential control group members seems high by historical standards. However, the time series behavior of leverage displayed in Table 8 reveals that this variable has generally increased over the eight year period of this study. Moreover, Bowman (1980) reports that the average leverage for his sample of 92 firms, based on 1973 data, was 1.44 which is not too far from the mean (or the median) of the 1973 data in Table 8.

Table 8 also reveals an average earnings yield that appears rather low. The Wall Street Journal, for example, reports average earnings yields for the 30 Dow Jones Industrials of .093 and .156 as of January 3, 1974 and January 3, 1975, respectively (The Wall Street Journal, January 6, 1975, p.25). This difference potentially can be attributed to COMPUSTAT bias. The firms on COMPUSTAT

are the older, more established business institutions that may be able to command a high premium for every dollar of earnings. However, the Dow Jones stocks are also older, established business institutions. One other explanation for the above differences may be that the COMPUSTAT data base includes a number of firms other than industrials which may result in noncomparable samples.

A comparison of Tables 6 and 8 reveals that Experimental Group 1 was characterized by larger unexpected earnings changes, higher earnings yield and lower leverage and systematic risk. Larger increases in earnings on the part of the LIFO group corroborates Brown's (1980) findings. These larger earnings probably also account for the higher earnings yield of the LIFO group.

As mentioned previously, 131 firms that switched to LIFO and that had data sufficient for purposes of this study were indentified. However, suitable matches could be found for only 128 firms. One firm was eliminated because no match was found and two firms were elimated because their best D^2 statistic was substantially greater than any other D^2 score of the firms included in the control group [1].

Table 9 summarizes the characteristics of the matching variables for Control Group 1. As can be seen, on each of the matching variables, Control Group 1 is closer to Experimental Group 1, in terms of both mean and median, than are all possible members of the control group based upon the data from all years.

Since over 90% of the LIFO changes took place in 1974, it may be more relevant to compare Tables 6 and 9 with the 1974 data in Table 8. This comparison results in the same conclusions as those reached in the preceeding paragraph.

With regard to Experimental Group 2, a comparison of Table 7 and the data based upon all years in Table 8 indicates that Experimental Group 2 has substantially larger unexpected earnings changes, lower earnings yield and leverage in terms of means but higher earnings yield and leverage in terms of medians, and lower systematic risk. Table 10 reports the matching variable statistics for Control Group 2. For all matching variables, the means of Control Group 2 are closer to the means of Experimental Group 2 than are the means of all potential control group members (Table 8). In terms of medians, the above conclusions hold for only unexpected changes in earnings and earnings yield.

Since most of Experimental Group 2 changes took place in one year, 1977, it may be more relevant to compare Tables 7 and 10 with the 1977 data in Table 8. This comparison yields the same conclusions as those reached in the above paragraph.

In summary, the above comparisons support the contention that the matching process yielded control groups more similar to the experimental groups in terms of the matching variables than would have been obtained by random sampling procedures. Additional comparisons were conducted, however, to further investigate the degree of similarity between the experimental and control groups. Hotelling's T^2 tests were conducted between each experimental group and its control group. These results are reported in Table 11. Usually in the case of two related groups, the one sample test is appropriate since this approach recognizes the correlation between the groups on the test variables. The one sample test for Group 1 is significant at less than the .01 level, indicating substantial differences between Experimental and Control Group 1 on the matching variables. Based upon this test, it seems that the matching procedure was not as successful as was hoped. The one sample test for Group 2 is not significant at the .05 level, indicating no significant differences between the two groups.

When two related groups are examined, generally the relationship between the two groups arised from matching on variables other than the test variables. In the present case however, the matching variables are the test variables. The matching process virtually assures a high correlation between the two groups on the test variables. This has the effect of reducing the magnitude of the inverse of the variance-covariance structure, thus making the test more sensitive. In fact, the more successful the matching process is, the more difficult it is to conclude, based upon a one sample test, that the experimental and control groups do not differ. Since the matching process may be biasing the one sample test in the direction of rejecting the null hypothesis of no difference, the two sample test might be more appropriate. This test indicates that neither set of groups differs on the matching variables at any reasonable significance level.

The final comparison made to assess the success of the matching process relates to the D^2 statistic. Table 12 presents a summary of the D^2 scores. In terms of mean, median and maximum value, the D^2 scores for each set of experimental and control groups is substantially below the corresponding figure for the entire cross-section of D^2 scores. Based upon this and the preceeding comparison, each experimental group and its control group seem to be

fairly similar in terms of the matching variables.

The variables which served as the basis for the computation of the D^2 scores were selected in large part for the impact they might have on the CARs. To investigate this issue, cross-sectional regressions were performed with CARs as the dependent variable and the matching variables as the independent variables. Separate regressions were run for Experimental and Control Group 1 and Experimental and Control Group 2. These results are reported in Table 13. The results of these regressions are not impressive for either set of firms. The R-square for Group 1 amounts to .048 while the R-square for Group 2 is .173. While the Group 1 regression is significant at less than the .05 level, the strength of the relationship in terms of explained variation is rather weak.

It would have been more reassuring had the matching variables explained more of the variation in the CARs. Such a result would have strengthened the support for matching on these variables. In any event, the matching process does help ensure that any differences found between an experimental and control group with respect to the CARs cannot be attributed to differences in the matching variables.

While the matching variables were selected primarily because of the impact they potentially could have upon the CARs, it is quite plausible that they could also affect changes in systematic risk. Blume (1971) shows, for example, that the magnitude of systematic risk is related to the stability of systematic risk. Accordingly, changes in systematic risk were also regressed on the matching variables for each group of firms. These results are reported in Table 14. The explanatory power of the regressions is fairly high; regressions for Group 1 and Group 2 have R-squares of .187 and .203, respectively. In both cases, systematic risk possesses significant marginal explanatory power. It seems, therefore, that with respect to the beta change dependent variable it was important to undertake the matching process.

Market Model Regressions

Table 15 contains the results of the market model regressions for Experimental and Control Group 1. For each of the two groups, six regressions were run. These six regressions resulted from varying the market index (CVW or CEW) and the estimation period (48 months before the test period, 24 months on either side of the test period and 48 months after the test period). All the statistics reported in Table 14 are in line with the results of other studies. R-squares of approximately .3

are typical of the explanatory power usually achieved by the market model. Also notice that betas generated by using the CVW index are uniformly greater than their CEW index counterparts. Similar results have been found by Elgers and Murray (1981b).

One other interesting aspect of the results reported in Table 15 relates to the R-squares. The R-squares generated by using the CEW index are uniformly larger than those resulting from the use of the CVW index. This result again corroborates the findings of Elgers and Murray (1981b) and lends support for the use of CEW betas in the matching process.

The results of the market model regressions for Experimental and Control Group 2 are reported in Table 16. Eight regressions were run for each group. These eight regressions resulted from varying the index (CVW or CEW) and the time period used for parameter estimation. The four periods were: 1) 48 months prior to the test period, 2) 24 months on either side of the test period, 3) 24 months prior to the test period and 4) 24 months after the test period. Regressions based upon 48 months of post-test period data could not be run because of data availability problems. The two regressions based on two years of pre-test period data and two years of post-test

period data were run because of the need to base the beta change dependent variable on them.

The comments made above regarding Table 15 generally hold for the results reported in Table 16. The only surprising aspect of Table 16 relates to the R-squares. They are generally quite high, particularly for the regressions based upon the two year time span subsequent to the test period. It's likely that these results are a function of 1) the particular firms in the sample and 2) the specific time period used.

Trading Volume Model

The regression results of Copeland's volume model are reported in Table 17. The model's derivation implies that the sign of the intercept term and coefficient f should be negative whereas the sign of the other two coefficients should be positive. With the exception of the intercept terms for Experimental and Control Group 2, all coefficients have the appropriate sign. Moreover, the model did a fairly good job of explaining the variability in trading volume; the R-squares ranged from .355 to .424. These figures are not quite as good as Copeland (1979) reports, but he was using weekly data. The R-squares obtained in this study do exceed those usually obtained using Beaver's (1968) model.

Tests of Hypotheses

Experimental and Control Group 1. The degree of dissimilarity between the sample firms changing to LIFO and their matched-pairs on the three dependent variables was assessed by using Hotelling's T^2. The primary results are reported in Table 18. As Table 18 indicates, Hotelling's T^2 was performed six times by varying the estimation period for the betas used to generate the CARs and also by employing both the CVW and CEW index. Only one of these six tests yielded a significant test statistic. That test employed the CEW index and an estimation period consisting of the 24 months both before and after the test period.

The null hypoythesis of no information content being associated with a LIFO switch cannot be rejected in five of the six cases reported in Table 18 and can be rejected in one of them. It appears that the research results of this study are sensitive to both the index selected to represent the market portfolio and the time period used to estimate beta. The sensitivity of the results to the index can partially be explained by a difference in the beta movements of the experimental vs. the control group within each of the two indexes. When the CVW index is used, the experimental and the control group exhibit an approximately equal decrease in beta (.093 and .075,

respectively) which resulted in a univariate t value of -.53. However, when the CEW index was employed, the groups moved in opposite directions. The beta of the experimental group decreased by .004 while the beta of the control group increased by .035. This difference resulted in a univariate t value of -1.46. The difference between the beta change of each group is greater when the CEW is used and this is likely to contribute toward obtaining statistically significant differences when using the multivariate T^2 test.

The above is also likely to explain the sensitivity of the results to the time period used to estimate the betas employed to generate the CARs. For both indexes, the separation between the CARs of the experimental and control group is the greatest for the estimation period of 24 months both before and after the test period. For the CEW index, this separation plus the difference between the groups with respect to the beta change was sufficient to yield a significant T^2 test. Also note that while the magnitude of the difference in the CARs for the two groups varies over the three estimation periods for each index, the pattern of the movement is quite similar for each index. That is, the largest difference in the CARs occurred in the estimation period composed of two years both before and after the test period for both indexes.

In the one case where a significant difference between the two groups was found, multiple comparison tests were conducted. A significant T^2 ensures that at least one linear compound of differences in group centroids is significantly different from zero. Since the objective here is to assess which of the dependent variables promoted the significant T^2, only simple compounds were tested. That is, three multiple comparison tests were conducted; in each test, weight was given to only one dependent variable. None of these tests was significant at an alpha level of .05. Additional multiple comparisons were not undertaken since the linear compounds which would serve as the basis of these tests would lack interpretability. Accordingly, univariate t statistics were examined to obtain a sense of which dependent variables were most responsible for the significant T^2. From this examination it is apparent that the CARs were "driving" the T^2 statistic to significance. The univariate t statistic on the CARs was -2.19 while the t statistics for the beta change and the volume residuals were -1.46 and .62, respectively.

The conventional wisdom would posit that a LIFO change would be accompanied by a favorable stock market reaction. This favorable reaction would be due to the positive impact that a LIFO switch would have on the

firm's cash flow because of the tax consequences of the accounting change. However, that is not what the results in Table 18 indicate. In all of the tests reported upon in Table 18, the CARs of the experimental group are negative and lower than the CARs of the control group. These results directly conflict with those of Ball (1972) and Sunder (1973; 1975). They do, however, agree with the results of Brown (1980) who, unlike Ball and Sunder, examined LIFO switches occurring during the same time frame as those in this study. A subsequent section of this chapter provides a rationale for a negative market reaction to a LIFO switch and reports the results of an empirical test of that proposition.

Because of the difficulty in interpreting a CAR when systematic risk is unstable, additional T^2 tests on the CARS and volume residuals were conducted using only those firms exhibiting stable betas. Stability was assessed using the Chow test. The results of the T^2 tests on this sample of firms are reported in Table 19 and are quite similar to the results reported in Table 18. Only one of the six tests, the test using the CEW index and an estimation period of 24 months both before and after the test period, yielded a T^2 significant at the .05 level. Again as before, although the multiple comparison procedure applied to each dependent variable resulted in

no statistically significant test statistic, an examination of the univariate t values indicates that it is the CARs which primarily contributed to the significant T^2.

Experimental and Control Group 2. Hotelling's T^2 tests were used to assess the degree of dissimilarity between Experimental Group 2 and Control Group 2 on the three dependent variables. Four tests were performed. The data for these tests were generated by varying the market index (CVW and CEW) and the period used to estimate the betas employed to generate the CARs (four years predceeding the test period and two years on either side of the test period). These results are presented in Table 20. As expected, none of the tests are significant. Moreover, not one of the univariate tests are significant.

The results of conducting T^2 tests on stable beta firms only are presented in Table 21. No significant T^2 was found.

Based upon the above statistics, the null hypothesis of no information content being associated with Experimental Group 2 changes cannot be rejected. Changes of this type elicited no observable stock market reaction.

Note that the analyses of Groups 1 and 2 are not

directly comparable since the beta change dependent variable is based on a four year estimation period for Group 1 and a two year estimation period for Group 2. Additional tests were conducted on Group 1 data using a two year estimation period to compute systematic risk changes. No important differences were found between these tests and those reported upon in Tables 18 and 19.

SFAS No. 13 Changes. Tests identical to the ones conducted for Group 2 were separately applied to the 18 firms in Group 2 making changes in response to SFAS No. 13. The results of these tests are reported in Tables 22 and 23. Not one of the T^2 test statistics is significant at an alpha level of .05. These results indicate that the null hypothesis of no information content cannot be rejected.

An analysis of the univariate t statistics does, however, reveal one significant statistic at the .05 level (when significance is assessed in the univariate manner). This t value of -2.25 is reported in Table 23 and arises from a test of equality of the CARs of stable beta firms by employing the CEW index and an estimation period consisting of two years on either side of the test period. The CARs of the change group are negative and significantly lower than those of the control group. This

is some indication of an adverse market reaction to changes induced by SFAS No. 13.

Firm Level Analysis

Analyses were also conducted on a firm by firm basis. These tests assessed 1) beta stability, 2) departures of CARs from zero and 3) departures of cumulative volume residuals from zero. The results are displayed in Table 24. Experimental and Control Group 1 seem to differ on two accounts. Five firms from Experimental Group 1 exhibited CARs significantly less than zero while only two firms from its control group did. This result tends to support the results of the cross-sectional analysis. Additionally, firms changing to LIFO were characterized by a higher degree of beta instability. Thirteen experimental and only three control firms were characterized by unstable betas. No firms in either group were characterized by abnormally large volume residuals.

The results for Experimental and Control Group 2 are identical, while the results for SFAS No. 13 changes and their control group are almost identical. These results again support the cross-sectional analysis.

Declining Profit Margin Hypothesis

As an earlier section of this chapter indicated, firms in the sample that switched to LIFO were characterized by lower CARs than were the control group. This is an indication of an adverse reaction on the part of capital market participants. This runs counter to the widely held belief that a rational investor would react favorably to a LIFO switch because of the improvement in the firm's cash flow which results from the tax benefits of the change. If the hypothesis of market efficiency is still maintained, other factors might explain the market's negative reaction to a LIFO switch.

Tax benefits will only accrue to a firm if its per unit inventory costs are rising. It therefore seems reasonable to assume that firms switching to LIFO either are or are anticipating experiencing increases in their unit inventory costs. To the extent that the competitive position of the firm permits it to correspondingly increase its unit selling price, the firm may not have incurred any substantial negative result due to the price rise. However, negative consequences would result for the firm if it is unable to sufficiently modify its unit selling price in the face of rising inventory costs.

The above forms the basis of a potential explanation of the negative market reaction to LIFO changes. A LIFO change may have been an indication to the market that management anticipates sizable increases in unit inventory costs. Armed with this signal and their view of the competitive position of the firm, capital market particiapnts may have concluded that many of the firms switching to LIFO would not be able to sufficiently adjust their unit selling prices in response to increased inventory costs.

The above proposition was tested in two ways. First the average change in the cost of goods sold percentage (CGS %) for the LIFO group was compared with the same variable for the control group via a one sample t test. The CGS % is reflective of the spread between average unit cost and average unit selling price. A cost increase that is not offset by a proportional rise in the selling price would result in an increased CGS %. The objective of this test is to ascertain if the LIFO group experienced an increase in its costs which it could (or would) not pass along to its customers via increased selling prices. The test was done twice, once where the change in the CGS % was measured as the CGS % in the year before the change less the CGS % in the year of the change and once more where the change in the CGS % was measured as the CGS % in

the year of the change less the CGS % in the year after the change [2]. These results are reported in Table 25. A one sided test was used since the a priori expectation is that the experimental group experienced greater increases in the CGS % than the control group. Note that given the way in which the variables were defined, a negative change in the CGS % implies an increase in the CGS % from one year to the next. In both periods, the experimental group's CGS % increased significantly more than did the control group's percentage at better than the .05 level. These results are strong indications that the firms switching to LIFO experienced increased inventory costs that they were unable to pass along to their customers via increased selling prices.

One further test was performed to assess the degree to which the market utilized the signals regarding changes in the CGS %. If the market is using this information in valuing securities, some degree of positive association would be expected between CARs and changes in the CGS %. Accordingly, CARs were correlated with CGS % changes on a cross-sectional basis for those firms switching to LIFO. Since Deakin (1976) has shown that certain accounting ratios do not follow the normal distribution, both Pearson and rank-order correlations were calculated. These correlations were calculated three times, one for each

definition of the change in the CGS % and once where the two measures of the change in the CGS % was combined. The latter approach was accomplished by 1) regressing the CARs on both CGS % change variables and 2) correlating the CARs with the estimated values from this regression. The results are reported in Table 26. In the first period, the Pearson correlation coefficient has the appropriate sign but is not significant at the .05 level. Both rank-order correlations are significant at the .05 level. The results for the second period are quite consistent across all three correlations. The correlations between the CARs and the change in the CGS % from the year of the change to the year following the change are .415, .375, and .263 for the Pearson, Spearman, and Kendall correlations, respectively. They are all significant at the .001 level. All correlations from the combined model are also significant at the .001 level. This can be interpreted as evidence supporting the notion that capital market participants utilized the signal from the LIFO switch in forming expectations regarding movements in the CGS % and valued securities based upon these expectations. Competing explanations will be discussed in the following chapter.

The fact that the market reacted negatively to the LIFO changes because of their signal regarding increased

inventory costs does not necessarily rule out the possibility that the market reacted positively to the tax benefits associated with LIFO switches. A favorable response to the tax benefits may have been overwhelmed by the adverse reaction to increased factor costs. To investigate this possibility, a covariance analysis was attempted where the change in CGS % served as the covariate and CARs served as the dependent variable. A necessary condition of covariance analysis is that the experimental and control groups respond in a uniform manner to the covariate. Unfortunetly, this was found not to be the case; use of covariance analysis in the present situation is therefore inappropriate.

Footnotes

1. These two firms had D^2 scores of approximately 25 and 90. Table 12 provides statistics on the D^2 scores obtained in this study.

2. In order for the CGS % to be computed via a consistent inventory method, the CGS % for each LIFO change firm was adjusted to reflect the effect of the inventory change when the change in the CGS % was measured as the CGS % in the year before the change less the CGS % in the year of the change.

C H A P T E R VI

CONCLUSIONS

Summary

Objective. A number of previous studies have shown that financial statement users do not respond in a uniform manner to all types of accounting principle changes. However, APB Opinion No. 20 requires essentially uniform disclosures for all changes in accounting principles. Since financial statement users do not view all accounting principle changes as being equivalent, the proposition that all changes should be accounted for indentically is not compelling.

An alternative approach would be to tailor the disclosure requirements for each type of change so that the information that is most relevant to financial statement users for each type of change is disclosed. A necessary element of this approach is the identification of a typology of accounting principle changes. The typology should be structured so that an a priori argument can be asserted as to why each type of change might have relevance for financial statement users. The delineation of the reasons why a change might have relevance for financial statement users can then be the basis for

accounting disclosure recommendations.

The typology used in this study employed the economic significance/no economic significance dichotomy in classifying accounting principle changes. It was suggested that since financial statement users are concerned with making economic decisions, only accounting principle changes possessing economic significance would be relevant to them. If this is the case, then only disclosures related to these accounting principle changes need be made.

There are substantial costs incurred by firms in generating, accumulating and communicating accounting disclosures to financial statement users. The users, in turn, incur substantial information processing costs. A rationale often used to justify costly accounting disclosures is that of information content. If it can be shown that certain types of accounting principle changes are not relevant to financial statement users, then requiring disclosures related to these changes must rest on other grounds.

It was also suggested that an accounting principle change could have economic significance for four reasons: 1) the change itself has economic significance, 2) the

change has an indirect economic impact via its effect on managerial behavior, 3) the change is associated with events having economic significance or 4) the change provides financial statement users with accounting numbers that were previously unavailable and that are useful to capital market participants in assessing the risk-return relationship of the firm.

The objective of this research was to assess the relevance (information content) to financial statement users of certain accounting principle changes which on an a priori basis were thought to belong to one of the two elements of the dichotomy. LIFO changes were thought to have economic significance due to the tax consequences associated with a LIFO switch. Changes to the flow-through method of accounting for the ITC and changes induced by SFAS No. 13 were thought not to be relevant to financial statement users because they are not characterized by any of the four scenarios described above as to why a change might have economic significance.

A number of earlier studies have investigated the changes examined here. Confidence in the results of these studies is somewhat limited due to shortcomings in their research designs. Two elements of the research design employed in this study largely circumvented the problems

encountered in the previous projects. One measure taken was a matching process that helped ensure that the change and nonchange groups were similar on a variety of dimensions that may have affected the dependent variables. Additionally, a multivariate test of significance was used to assess differences between the change and nonchange groups on the three dependent variables.

Results and Implications. The results of Hotelling's T^2 and univariate t tests indicated that the market reacted adversely to the LIFO changes examined in this study. This result conflicts with the a priori expectation that financial statement users would react positively to LIFO changes because of the associated tax savings. Significant correlations between the CARs and changes in the CGS % suggest that the LIFO switches provided signals to capital market participants regarding current and future increases in inventory costs and that it was these signals that accounted for the negative market reaction.

The above results indicate that LIFO changes possess information content for capital market participants. However, the source of the information content is not related solely to tax savings. The relevance of the LIFO changes to capital market participants was, in part, a function of the changes being signals regarding increased

factor costs which could not be passed along to the consumer. Thus LIFO changes were found to have information content and therefore to be worthy of disclosure, but not for the reasons originally expected.

None of the tests conducted on Experimental Group 2 indicated a market reaction. The absence of a market reaction is indicative of the irrelevance of Group 2 changes to capital market participants. However, when only the SFAS No. 13 changes were examined, one significant t statistic (in a univariate sense) was found. This statistic resulted from a test of the equality of the CARs of stable beta firms. Moreover, in all tests involving SFAS No. 13 changes, the CARs of the change group were below those of the matched control group. This provides a limited amount of evidence indicating an adverse market reaction. If a negative reaction does in fact exist, one possible cause is that the market may have anticipated altered managerial behavior as a result of SFAS No. 13. Certain leases were required to be capitalized as a result of SFAS No. 13 that were not previously capitalized. Because of this, some firms may be close to (or actually in) violation of debt covenant restrictions. Management may be motivated to change the firm's capital structure or to undertake other measures in order to avoid the covenant violations.

On a broader level, the results of this research pose serious questions to those conducting capital market based research. This area of research has evolved such that a large number of essentially arbitrary decisions must be made, all of which may influence the results of a study. Some of the issues that needed to be addressed in this study were:

1. identification of suitable matching dimensions and an appropriate matching process to be used in the selection of control groups
2. choice of the market model
3. length of the estimation period
4. choice of beta adjustment technique (if any)
5. choice of market proxy
6. choice of the test period
7. positioning of the estimation period with respect to the test period.

Decisions regarding the first four issues were made by appealing to the literature in the area. The literature concerning the other three issues is, at best, quite sparce. Choice of the test period was based upon knowledge of the timing of corporate earnings announcements and annual report issuance dates. There is some evidence that market proxy can affect research conclusions but only limited evidence regarding the preferability of market indexes. The safest approach in

this situation was to employ two widely used indexes. It was found that in terms of the LIFO T^2 tests, the results were sensitive to the choice of index. Similarly, the LIFO T^2 tests were found to be sensitive to the selection of the estimation period.

The dangers that these results imply for capital market researchers are the following. The greater the number of arbitrary decisions to which research inferences are sensitive, the less comparable are a cross-section of studies if these studies have taken different paths in these situations. For example, the results of a study using the CEW index may not be comparable to the results of a study using the CVW index. Unfortunetly, the situation is not much improved if researchers follow the course of conducting their analyses simultaneously under a variety of options. It is likely that the results will not be consistent across options. This will hamper the possibility of drawing unambiguous conclusions from the results. In the absence of theoretical or empirical guidance concerning the choices outlined above, there does not seem to be an appealing solution to the problem.

Limitations

As with any empirical research project, certain compromises were necessitated in the execution of this

study which resulted in various limitations. Perhaps the most significant limitation is one that characterizes all ex post studies, the effect of uncontrolled confounding variables. While a great deal of effort was taken to eliminate a number of potentially confounding variables via the matching process, it is far from certain that all such variables were controlled for. In fact, it is likely that not all confounding variables were identified and controlled.

This limitation has important implications for the results of the LIFO analysis. Earlier it was concluded that LIFO changes acted as a signal regarding current and future increases in inventory costs that could not be passed along to customers. However, the possibility exists that the market became aware of the inventory cost increases from other information sources. For example, commodity prices as well as the outlook for these prices frequently appear in The Wall Street Journal. Therefore, the adverse market reaction that was found in this study is not a conclusive indication of signals being conveyed to the market via the accounting changes.

Another limitation relates to the time specific nature of the results of this study. Each of the two groups of changes examined in this study occurred during a

very short period of time. Most of the LIFO changes took place in the 1974-1975 time period and all of the SFAS No. 13 changes took place in 1977 or early 1978. The time period during which the LIFO changes took place was characterized by a recession, high interest rates and substantial inflation. It is likely that capital market participants were highly sensitive to issues involving increasing costs during this period. As the high association between the CARs and changes in the CGS % indicates, financial statement users used the signal from the LIFO switch in forming expectations regarding increases in factor costs which could not be passed along to consumers. It is not clear that the same result would hold in an environment characterized by lower inflation and, quite likely, lessened sensitivity to inflationary issues on the part of capital market participants.

Another limitation is associated with Experimental Group 2 and the SFAS No. 13 changes. The size of both groups was small, 22 and 18, respectively. This greatly reduces the likelihood of discovering any difference between the experimental and control groups when, in fact, they do exist.

One final limitation characterizes this study as well as much of the capital market based research. An

examination of CARs and changes in systematic risk is an analysis of aggregate behavior. It does not consider the effect of accounting disclosures on individual investor behavior. To the extent that accounting policy makers wish to base their decisions on the impact of accounting numbers on individual financial statement users, market based studies are not particularly relevant to them.

Suggestions for Future Research

There are two obvious avenues for future research regarding LIFO changes. One would be the examination of LIFO changes taking place in time periods other than the one studied here. Ball (1972) and Sunder (1973; 1975) examined LIFO changes in earlier periods. When sufficient market based data becomes available, examination of more recent changes would be desirable.

Another direction which future research could take is the expansion of the set of matching variables used to select the control group. One additional variable that could be used is the change in the CGS %. The possibility exists that the market did react positively to the tax benefits associated with the LIFO changes, but this reaction could have been overshadowed by the adverse reaction to increased factor costs. By matching on the change in the CGS %, this proposition could be tested.

With respect to SFAS No. 13 changes, this study found some faint evidence of a market reaction. Since there is the possibility that management may have altered their decisions as a result of SFAS No. 13, and since this managerial reaction may be the basis of a market reaction, the impact of SFAS No. 13 on managerial behavior is an issue worth addressing.

Another path of research worth pursuing is an investigation of other accounting principle changes in order to ascertain where in the typology described in Chapter III they belong. For example, do changes in depreciation method primarily affect managerial behavior or do they merely provide a new view of economic reality to financial statement users? Additionally, more thought should be given to the nature of the disclosures that should be made for the various types of accounting principle changes.

One final comment is in order regarding empirical research projects whose objective is to assess the information content of various accounting disclosures. In the face of criticisms regarding their external validity, laboratory experiments have receded from view and have been replaced by capital market based projects as the primary research mode used to assess the impact of

accounting numbers on financial statement users. Perhaps the pendulum has swung too far. While market based studies rank high on the external validity scale, they are also characterized by severe limitations: uncontrolled variables, a focus on aggregate behavior and an examination of only one group of financial statement users (common stockholders). Since in large part the strengths (weaknesses) of laboratory studies are the weaknesses (strengths) of market based studies, these two approaches complement one another and both, therefore, deserve a place as a viable option open to accounting researchers.

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APPENDIX A

Table 1

Comparison of Earnings Per Share 1972-1976

Year	Change Companies 1974	Change Companies 1975	Non change Companies	Significance Level (1974 Firms)
1972	$1.81	--	$1.53	NS
1973	2.43	$2.38	1.95	.05
1974	2.94	2.36	2.27	.05
1974*	3.56	--	2.27	.001
1975	2.55	1.99	2.10	NS
1975*	--	2.25	--	--
1976	--	3.74	--	--

*Earnings per share assuming that change companies had not changed to LIFO.

Source: Brown (1980)

Table 2

Experimental Group 1 - Industry Composition

Industry Group	Number of Firms
Metal Mining	3
Bituminous Coal and Lignite Mining	1
Oil and Gas Extraction	1
Construction Other Than Building Construction - General Contractors	1
Construction - Special Trade Contractors	1
Manufacturing	
Food and Kindred Products	13
Tobacco	1
Textile Mill Products	14
Apparel and Other Finished Products Made from Fabrics and Similar Materials	3
Lumber and Wood Products, except Furniture	3
Furniture and Fixtures	1
Paper and Allied Products	15
Printing, Publishing and Allied Industries	5
Chemicals and Allied Products	27
Petroleum Refining and Related Industries	15
Rubber and Miscellaneous Plastic Products	13
Leather and Leather Products	3
Stone, Clay, Glass and Concrete Products	7
Transportation Equipment	1
TOTAL	128

Table 3

Experimental Group 1 - Chronological Distribution of the Changes

Year	Number of Firms
1970	1
1971	1
1972	0
1973	4
1974	112
1975	10
TOTAL	128

Table 4

Experimental Group 2 - Industry Classification

Industry Group	Number of Firms
Manufacturing	
Food and Kindred Products	5
Apparel and Other Finished Products Made from Fabrics and Similar Materials	2
Lumber and Wood Products, except Furniture	1
Paper and Allied Products	1
Petroleum Refining and Related Industries	2
Fabricated Metal Products, except Machinery and Transportation Equipment	1
Machinery, except Electrical	1
Electrical and Electronic Machinery, Equipment, and Supplies	1
Wholesale and Retail Trade	
Building Materials, Hardware, Garden Supply and Mobile Home Dealers	1
General Merchandise Stores	3
Food Stores	2
Transportation by Air	1
Electric, Gas and Sanitary Services	1
TOTAL	22

Table 5

Experimental Group 2 - Chronological Distribution of the Changes

Year	Number of Firms
1972	2
1973	1
1974	0
1975	1
1976	0
1977	14
1978	4
TOTAL	22

Table 6

Summary Statistics of Matching Variables - Experimental Group 1

Variable	Mean	Median	Standard Deviation
Unexpected Change in Earnings*	22.628	5.100	62.286
Earnings Yield	.153	.143	.084
Leverage	1.685	1.170	1.554
Systematic Risk	.926	.910	.315

*In millions

Table 7

Summary Statistics of Matching Variables - Experimental Group 2

Variable	Mean	Median	Standard Deviation
Unexpected Change in Earnings*	25.871	9.921	43.796
Earnings Yield	-.027	.104	.465
Leverage	2.316	1.519	2.397
Systematic Risk	.943	.990	.357

*In millions

Table 8

Summary Statistics of Matching Variables
Potential Control Group Members

Year	Variable	Mean	Median	Standard Deviation
1970	Unexpected Change in Earnings*	-1.403	.080	29.977
	Earnings Yield	.058	.064	.063
	Leverage	1.807	.810	3.257
	Systematic Risk	1.051	1.053	.381
1971	Unexpected Change in Earnings*	2.684	.464	35.695
	Earnings Yield	.039	.063	.145
	Leverage	2.216	1.019	3.817
	Systematic Risk	1.043	1.035	.385
1972	Unexpected Change in Earnings*	3.007	.821	14.458
	Earnings Yield	.043	.058	.106
	Leverage	2.100	.929	3.616
	Systematic Risk	1.067	1.082	.290
1973	Unexpected Change in Earnings*	5.171	.729	35.439
	Earnings Yield	.058	.070	.107
	Leverage	2.256	1.051	3.846
	Systematic Risk	1.059	1.082	.404

*In millions

Table 8 (cont.)

Year	Variable	Mean	Median	Standard Deviation
1974	Unexpected Change in Earnings*	1.308	.276	48.693
	Earnings Yield	.097	.126	.501
	Leverage	4.298	2.073	6.805
	Systematic Risk	1.014	1.017	.305
1975	Unexpected Change in Earnings*	-1.094	.334	34.340
	Earnings Yield	.041	.179	1.151
	Leverage	7.610	3.447	11.796
	Systematic Risk	.993	.983	.348
1976	Unexpected Change in Earnings*	7.065	1.185	51.425
	Earnings Yield	.040	.126	.596
	Leverage	5.521	2.300	9.430
	Systematic Risk	1.004	.989	.350
1977	Unexpected Change in Earnings*	4.813	1.159	37.116
	Earnings Yield	.043	.114	.683
	Leverage	4.728	1.784	15.454
	Systematic Risk	1.010	.998	.336

*In millions

Table 8 (cont.)

Year	Variable	Mean	Median	Standard Deviation
All Years	Unexpected Change in Earnings*	2.781	.589	37.837
	Earnings Yield	.053	.087	.566
	Leverage	3.910	1.504	8.819
	Systematic Risk	1.024	1.018	.368

*In millions

NOTE: Although a few changes took place in 1978, no data was needed on the matching variables for this year. This is due to the fact that all 1978 changes took place in January and the Compustat convention of treating the data of firms with fiscal year ends of January through May as being associated with the previous calendar year.

Table 9

Summary Statistics of Matching Variables - Control Group 1

Variable	Mean	Median	Standard Deviation
Unexpected Change in Earnings*	17.103	1.443	62.203
Earnings Yield	.128	.139	.126
Leverage	1.721	1.257	1.609
Systematic Risk	.940	.924	.318

*In millions

Table 10

Summary Statistics of Matching Variables - Control Group 2

Variable	Mean	Median	Standard Deviation
Unexpected Change in Earnings*	13.000	1.931	29.514
Earnings Yield	.019	.104	.344
Leverage	1.429	.933	1.292
Systematic Risk	.940	.921	.343

*In millions

Table 11

Hotelling T^2 Tests on Matching Variables

Experimental Group 1 - Control Group 1	F Value	P Value
One sample test	3.635	.008
Two sample test	1.164	.327
Experimental Group 2 - Control Group 2		
One sample test	2.515	.078
Two sample test	1.320	.280

Table 12

Summary of D^2 Scores

Group	Mean	Median	Maximum Value
All D Scores	5.640	1.241	548.161
Experimental Group 1 - Control Group 1	.337	.072	9.686
Experimental Group 2 - Control Group 2	1.032	.127	9.134

Table 13

Regressions of CARs on Matching Variables

	Experimental and Control Group 1	Experimental and Control Group 2
t values (p values in parentheses)		
Unexpected Change in Earnings	1.563 (.120)	-1.883 (.067)
Earnings Yield	-2.858 (.005)	- .254 (.801)
Leverage	- .701 (.484)	.314 (.755)
Systematic Risk	1.367 (.173)	1.065 (.293)
R^2	.048	.173
Adjusted R^2	.032	.087
F value (p value in parentheses)	3.114 (.016)	2.028 (.109)

NOTE: The CARs used in the above regressions were obtained by use of beta generated by employing the CEW index and an estimation period consisting of the 48 months preceding the test period. Results similar to those reported above were obtained by using the other index and other estimation periods.

Table 14

Regressions of Beta Changes on Matching Variables

	Experimental and Control Group 1	Experimental and Control Group 2
t values (p values in parentheses)		
Unexpected Change in Earnings	-2.536 (.012)	-1.236 (.224)
Earnings Yield	.220 (.826)	- .559 (.579)
Leverage	2.079 (.039)	- .123 (.902)
Systematic Risk	-7.496 (.000)	-3.003 (.005)
R^2	.187	.203
Adjusted R^2	.174	.121
F value (p value in parentheses)	14.421 (.000)	2.486 (.059)

NOTE: Beta change measures used in the above regressions were generated by using the CEW index. Similar results were obtained when the CVW index was used.

Table 15

Market Model Regressions - Experimental and Control Group 1

Experimental Group 1	R-Square	OLS Beta	Vasicek Beta
CVW Index			
4 years prior to the test period	.272	1.231	1.204
2 years on either side of the test period	.274	1.152	1.142
4 years after the test period	.298	1.124	1.111
CEW Index			
4 years prior to the test period	.316	.921	.906
2 years on either side of the test period	.321	.895	.886
4 years after the test period	.318	.910	.902
Control Group 1			
CVW Index			
4 years prior to the test period	.237	1.221	1.193
2 years on either side of the test period	.235	1.126	1.114
4 years after the test period	.264	1.152	1.118
CEW Index			
4 years prior to the test period	.283	.943	.914
2 years on either side of the test period	.286	.920	.888
4 years after the test period	.303	1.001	.949

Table 16

Market Model Regressions - Experimental and Control Group 2

Experimental Group 2	R-Square	OLS Beta	Vasicek Beta
CVW Index			
4 years prior to the test period	.290	1.097	1.088
2 years on either side of the test period	.336	1.202	1.156
2 years prior to the test period	.238	1.072	1.036
2 years after the test period	.457	1.341	1.230
CEW Index			
4 years prior to the test period	.336	.920	.891
2 years on either side of the test period	.359	.929	.897
2 years prior to the test period	.265	.896	.831
2 years after the test period	.475	.992	.947
Control Group 2			
CVW Index			
4 years prior to the test period	.297	1.125	1.130
2 years on either side of the test period	.296	1.209	1.166
2 years prior to the test period	.250	1.006	1.074
2 years after the test period	.388	1.303	1.216

Table 16 (cont.)

Control Group 2	R-Square	OLS Beta	Vasicek Beta
CEW Index			
4 years prior to the test period	.330	.942	.918
2 years on either side of the test period	.315	.935	.904
2 years prior to the test period	.261	.838	.837
2 years after the test period	.407	.956	.933

Table 17

Trading Volume Regressions - All Groups

Statistic	Experimental Group 1	Control Group 1	Experimental Group 2	Control Group 2
t value means				
coefficient d	1.045	2.855	2.915	2.415
coefficient f	- .303	- .953	-1.413	-1.335
coefficient g	.388	3.509	3.419	2.986
intercept	-1.162	- .731	.388	.000
R^2	.424	.409	.443	.355
F value mean	13.299	12.968	15.207	9.843

Table 18

Hotelling's T^2 Tests - Experimental Group 1 and Control Group 1

	CVW			CEW		
	Means		t values	Means		t values
Dependent Variables	Experimental Group 1	Control Group 1		Experimental Group 1	Control Group 1	
Beta estimation period for CARs: four years prior to the test period						
CARs	- .015	.033	-1.33	- .075	-.025	-1.40
Beta Change	- .093	-.075	- .53	- .004	.035	-1.46
Volume CARs	- .114	-.150	.62	- .114	-.150	.62
T^2	2.422			4.619		
F value (p value)	.795 (.499)			5.516 (.214)		
Beta estimation period for CARs: two years on either side of the test period						
CARs	- .055	.023	-2.17	- .074	.006	-2.19
Beta Change	- .093	-.075	- .53	- .004	.035	-1.46
Volume CARs	- .114	-.150	.62	- .114	-.150	.62
T^2	5.993			8.265		
F value (p value)	1.966 (.122)			2.712 (.048)		

Table 18 (cont.)

Beta estimation period for CARs: four years after the test period

Dependent Variables	CVW Means Experimental Group 1	CVW Means Control Group 1	CVW t values	CEW Means Experimental Group 1	CEW Means Control Group 1	CEW t values
CARs	- .068	-.023	-1.24	- .047	.006	-1.48
Beta Change	- .093	-.075	- .53	- .004	.035	-1.46
Volume CARs	- .114	-.150	.62	- .114	-.150	.62
T^2	2.689			5.675		
F value (p value)	.882 (.452)			1.862 (.139)		

Table 19

Hotelling's T^2 Tests, Stable Beta Firms* - Experimental Group 1 and Control Group 1

Beta estimation period for CARs: four years prior to the test period

	CVW			CEW		
	Means		t values	Means		t values
Dependent Variables	Experimental Group 1	Control Group 1		Experimental Group 1	Control Group 1	
CARs	- .017	.047	-1.59	- .075	-.012	-1.69
Volume CARs	- .136	-.181	.70	- .122	-.183	.99
T^2	3.320			4.305		
F value (p value)	1.629 (.201)			2.133 (.123)		

Beta estimation period for CARs: two years on either side of the test period

CARs	- .050	.033	-2.05	- .071	.010	-2.08
Volume CARs	- .136	-.181	.70	- .122	-.183	.99
T^2	5.578			6.953		
F value (p value)	2.737 (.069)			3.445 (.035)		

Table 19 (cont.)

Beta estimation period for CARs: four years after the test period

Dependent Variables	CVW Means Experimental Group 1	CVW Means Control Group 1	CVW t values	CEW Means Experimental Group 1	CEW Means Control Group 1	CEW t values
CARs	- .072	-.021	-1.26	- .049	.008	-1.46
Volume CARs	- .136	-.181	.70	- .122	-.183	.99
T^2	2.767			4.405		
F value (p value)	1.358 (.262)			2.182 (.118)		

*Beta stability was assessed at the .05 level. T^2 tests based upon firms exhibiting a stable beta at the .01 level resulted in no important differences from the statistics reported here. T^2 tests were also conducted on firms exhibiting a stable regression plane (as opposed to a stable slope coefficient). Again no important differences were found.

NOTE: Sample sizes for the CVW and CEW tests were 109 and 112, respectively.

Table 20

Hotelling's T^2 Tests - Experimental and Control Group 2

Beta estimation period for CARs: four years prior to the test period

	CVW			CEW		
	Means		t values	Means		t values
Dependent Variables	Experimental Group 2	Control Group 2		Experimental Group 2	Control Group 2	
CARs	.008	.050	-.80	-.037	.002	-.78
Beta Change	.194	.142	.32	.116	.096	.17
Volume CARs	-.115	-.087	-.30	-.115	-.087	-.30
T^2	.877			.745		
F value (p value)	.265 (.850)			.225 (.878)		

Beta estimation period for CARs: two years on either side of the test period

CARs	.053	.091	-.87	-.041	-.001	-.81
Beta Change	.194	.142	.32	.116	.096	.17
Volume CARs	-.115	-.087	-.30	-.115	-.087	-.30
T^2	.958			.774		
F value (p value)	.286 (.835)			.234 (.872)		

Table 21

Hotelling's T^2 Tests, Stable Beta Firms* - Experimental Group 2 and Control Group 2

Beta estimation period for CARs: four years prior to the test period

	CVW			CEW		
	Means		t values	Means		t values
Dependent Variables	Experimental Group 2	Control Group 2		Experimental Group 2	Control Group 2	
CARs	.036	.067	-.62	-.018	.031	-.89
Volume CARs	-.036	-.113	.72	-.036	-.042	.05
T^2	1.114			.820		
F value (p value)	.520 (.606)			.383 (.689)		

Beta estimation period for CARs: two years on either side of the test period

CARs	.085	.097	-.21	.015	.066	-.95
Volume CARs	-.036	-.113	.72	-.036	-.042	.05
T^2	.740			1.044		
F value (p value)	.346 (.714)			.487 (.624)		

*Beta stability was assessed at the .05 level. T^2 tests based upon firms exhibiting a stable beta at the .01 level resulted in no important differences from the statistics reported here. T^2 tests were also conducted on firms exhibiting a stable regression plane (as opposed to a stable slope coefficient). Again no important differences were found.

NOTE: Sample size for the CVW and the CEW tests was 16.

Table 22

Hotelling T^2 Tests - SFAS No. 13 Changes Only

Beta estimation period for CARs: four years prior to the test period

Dependent Variable	CVW Means: Change Group	CVW Means: Control Group	CVW t values	CEW Means: Change Group	CEW Means: Control Group	CEW t values
CARs	-.006	.053	-1.24	-.056	-.002	-1.15
Beta Change	.161	.240	- .44	.045	.129	- .65
Volume CARs	-.131	-.061	- .65	-.131	-.061	- .65
T^2	2.175			2.345		
F value (p value)	.640 (.601)			.690 (.572)		

Beta estimation period for CARs: two years on either side of the test period

CARs	.032	.108	-1.73	-.056	.019	-1.72
Beta Change	.161	.240	- .44	.045	.129	- .65
Volume CARs	-.131	-.061	- .65	-.131	-.061	- .65
T^2	3.108			3.478		
F value (p value)	.914 (.458)			1.023 (.410)		

Table 23

Hotelling T^2 Tests, Stable Beta Firms* - SFAS No. 13 Changes Only

Beta estimation period for CARs: four years prior to the test period

	CVW			CEW		
	Means		t values	Means		t values
Dependent Variable	Change Group	Control Group		Change Group	Control Group	
CARs	.021	.095	-1.92	-.043	.054	-2.10
Volume CARs	-.044	-.109	.51	-.043	-.021	- .16
T^2	5.202			4.407		
F value (p value)	2.384 (.138)			2.020 (.179)		

Beta estimation period for CARs: two years on either side of the test period

CARs	.069	.128	-1.04	-.029	.077	-2.25
Volume CARs	-.044	-.109	.51	-.043	-.021	- .16
T^2	2.150			5.633		
F value (p value)	.985 (.404)			2.582 (.120)		

*Beta stability was assessed at the .05 level. T^2 tests based upon firms exhibiting a stable beta at the .01 level resulted in no important differences from the statistics reported here. T^2 tests were also conducted on firms exhibiting a stable regression plane (as opposed to a stable slope coefficient). Again no important differences were found.

NOTE: Sample size for the CVW and the CEW tests was 13.

Table 24

Firm Level Analysis

	CARs		Volume CARs	Significant* Beta Change
	Significantly* Greater Than Zero	Significantly* Less Than Zero	Significantly* Greater Than Zero**	
Experimental Group 1	5	5	0	13
Control Group 1	4	2	0	3
Experimental Group 2	0	0	0	3
Control Group 2	0	0	0	3
SFAS No. 13 Changes	0	0	0	2
SFAS No. 13 Control	0	0	0	3

*All tests were conducted at the .05 level
**One sided test

NOTE: The above tests are based upon the CEW index. The CARs were generated from Vasicek adjusted betas estimated from the 48 monthly observations preceding the test period. Tests using the other index and other estimation periods produced results similar to the ones reported here.

Table 25

Tests on Changes in the Cost of Goods Sold Percentage

Period	Means		t value	p value*
	Experimental	Control		
Year prior to the change less year of the change	-2.34	-.48	3.16	.001
Year of the change less year after the change	-3.00	-.88	3.71	.000

*One sided test

NOTE: Due to missing data on the COMPUSTAT Tapes, the above tests were performed on a sample of 92 matched pairs.

Table 26

Correlations Between CARs and Changes in CGS% - LIFO Changes

Period	Correlations (p values) Pearson	Spearman	Kendall
Year prior to the change less year of the change	.159 (.065)	.243 (.013)	.161 (.012)
Year of the change less year after the change	.415 (.001)	.375 (.001)	.263 (.001)
Both periods combined	.465 (.001)	.457 (.001)	.320 (.001)

NOTE: The CARs used here were generated by using the CEW Vasicek adjusted beta estimated based upon the 48 monthly observations preceding the test period. Correlations were calculated using Vasicek adjusted betas from other estimation periods and the other index. No important differences were found.

Due to missing data on the COMPUSTAT Tapes, the above correlations were based on a sample of 92 firms.

APPENDIX B

LIFO Changes

	Change Companies		Nonchange Companies
1.	Cleveland-Cliffs Iron Co.	1.	Heleca Mining Co.
2.	Phelps Dodge Corp.	2.	Newmont Mining Corp.
3.	Homestake Mining	3.	Dome Mines LTD
4.	St. Joe Minerals Corp.	4.	Eastern Gas & Fuel Assoc.
5.	Occidental Petroleum Corp.	5.	Getty Oil Co.
6.	Halliburton Co.	6.	Fluor Corp.
7.	Anthony Industries Inc.	7.	Dynalectron Corp.
8.	Anderson, Clayton & Co.	8.	Bartons Candy Corp.
9.	Campbell Soup Co.	9.	Ralston Purina Co.
10.	General Mills Inc.	10.	Pepsico Inc.
11.	Tasty Baking Co.	11.	General Foods Corp.
12.	Amstar Corp.	12.	Holly Sugar Corp.
13.	Amalgamated Sugar Co.	13.	Hormel (Geo. A.) & Co.
14.	Michigan Sugar	14.	Int'l Multifoods Corp.
15.	Hershey Foods Corp.	15.	Borden Inc.
16.	MacAndrews & Forbes	16.	Iowa Beef Processors
17.	Publicker Industries Inc.	17.	General Cinema Corp.
18.	Coca-Cola Bottling Co of NY	18.	Tootsie Roll Industries Inc.
19.	Pepcom Industries	19.	United Foods Inc.
20.	Wometco Enterprises Inc.	20.	Cagle's Inc.
21.	Reynolds (R. J.) Inds.	21.	Philip Morris Inc.
22.	Avondale Mills	22.	Adams-Millis Corp.
23.	Burlington Industries Inc.	23.	Alba-Waldensian Inc.
24.	Cone Mills Corp.	24.	Fabien Corp.
25.	Dan River Inc.	25.	Lowenstein (M) Corp.
26.	Fab Industries Inc.	26.	Idmos Corp.
27.	Fieldcrest Mills	27.	Belding Heminway
28.	Graniteville Co.	28.	American Mfg. Co.
29.	Mount Vernon Mills Inc.	29.	Huyck Corp.
30.	Reeves Brothers Inc.	30.	Collins & Aikman Corp.
31.	Riegel Textile Corp.	31.	National Spinning Co.
32.	Russell Corp.	32.	Stevens (J. P.) & Co.
33.	Springs Mills Inc.	33.	Crompton Co. Inc.
34.	Standard Coosa-Thatcher	34.	Opelika Mfg. Corp.
35.	Maslanc (C. H.) & Sons	35.	Compo Inds.
36.	Cluett, Peabody & Co.	36.	V. F. Corp.
37.	Hart Schaffner & Marx Co.	37.	Decorator Industries Inc.
38.	Munsingwear Inc.	38.	Wilson Brothers
39.	Georgia-Pacific Corp.	39.	Tech-Sym Corp.
40.	Masonite Corp.	40.	Golden West Homes
41.	Conchemco Inc.	41.	Vintage Enterprise Inc.

42.	Mohasco Corp.	42.	New Idria Inc.
43.	Chesapeake Corp. of Va.	43.	Potlatch Corp.
44.	Crown Zellerbach	44.	American Israeli Paper Mills
45.	Diamond Int'l Corp.	45.	Simkins Industries
46.	Great Northern Nekoosa	46.	Int'l Paper Co.
47.	Hammermill Paper Co.	47.	Boise Cascade Corp.
48.	Kimberly-Clark Corp.	48.	Bowater Corp. LTD-ADR
49.	St. Regis Paper Co.	49.	Domtar Inc.
50.	Union Camp Corp.	50.	Kleer-Vu Industries Inc.
51.	Bemis Co.	51.	Brown Co.
52.	Dennison Mfg. Co.	52.	APL Co.
53.	Sorg Paper Co.	53.	Federal Paper Board Co.
54.	Technical Tape Inc.	54.	Ludlow Corp.
55.	Connelly Containers Inc.	55.	Whippany Paperboard
56.	Maryland Cup Corp.	56.	Papercraft Corp.
57.	Paramount Packaging	57.	Clevepak Corp.
58.	Times Mirror Co.	58.	Capital Cities Communication
59.	Meredith Corp.	59.	Arcata Corp.
60.	Donnelley (R.R.) & Sons	60.	Gannett Co.
61.	Ennis Business Forms	61.	Time Inc.
62.	Safeguard Inds. Ind.	62.	Filmways Inc.
63.	American Cyanamid Co.	63.	Air Products & Chemicals Inc.
64.	EMC Corp.	64.	Johnson Products
65.	Grace (W. R.) & Co.	65.	Williams Cos.
66.	Hercules Inc.	66.	Colgate-Palmolive Co.
67.	Monsanto Co.	67.	Imperial Chem. Inds. LTD-ADR
68.	Olin Corp.	68.	U. S. Radium Corp.
69.	Pennwalt Corp.	69.	First Mississippi Corp.
70.	Stauffer Chemical Co.	70.	National Distillers & Chemical
71.	Essex Chemical Corp.	71.	Del Laboratories Inc.
72.	Ethyl Corp.	72.	Crompton & Knowles Corp.
73.	Great Lakes Chemical Corp.	73.	Nestle-Lemur Co.
74.	Reichhold Chemicals Inc.	74.	Insilco Corp.
75.	Pfizer Inc.	75.	Bristol-Myers Co.
76.	Upjohn Co.	76.	Lilly (Eli) & Co.
77.	Proctor & Gamble Co.	77.	American Home Products Corp.
78.	Stepan Chemical Co.	78.	Dutch Boy Inc.
79.	Avon Products	79.	Economics Laboratory Inc.
80.	Carter-Wallace Inc.	80.	Mary Kay Cosmetics
81.	Guardsman Chemicals Inc.	81.	Gillette Co.
82.	Pratt & Lambert Inc.	82.	Valspar Corp.
83.	Fairmount Chemical Co Inc	83.	Morton-Norwich Products
84.	Koppers Co.	84.	Int'l Minerals & Chemical
85.	Nalco Chemical Co.	85.	Rorer Group
86.	Dexter Corp.	86.	Sherwin-Williams Co.
87.	Ferro Corp.	87.	Purex Industries Inc.
88.	Park Chemical Co.	88.	Oakite Products.
89.	Sun Chemical Corp.	89.	Helene Curtis Industries
90.	Ashland Oil Inc.	90.	American Petrofina
91.	Crown Central Petroleum	91.	Walter (Jim) Corp.

92.	Crystal Oil Co.	92.	Quaker State Oil Refining
93.	Earth Resources Co.	93.	Gulf Canada LTD
94.	Husky Oil LTD	94.	Tesoro Petroleum Corp.
95.	Kerr-McGee Corp.	95.	Imperial Oil LTD
96.	Marathon Oil Co.	96.	Union Cil Co. of Cal.
97.	Murphy Oil Corp.	97.	Belco Petroleum Corp.
98.	Pennzoil Co.	98.	OKC Corp.
99.	Shell Oil Co.	99.	Sun Co.
100.	Standard Cil Co. (Calif)	100.	British Petroleum Co. LTD
101.	Standard Oil Co. (Ohio)	101.	Clark Oil & Refining Corp.
102.	Texaco Inc.	102.	Royal Dutch Petroleum Co.
103.	Witco Chemical Corp.	103.	Holly Corp.
104.	Certain-Teed Corp.	104.	Amerada Hess Corp.
105.	Armstrong Rubber	105.	Uniroyal Inc.
106.	Carlisle Corp.	106.	Aegis Corp.
107.	Cooper Tire & Rubber	107.	Cetec Corp.
108.	Firestone Tire & Rubber Co	108.	Glasrock Products
109.	General Tire & Rubber Co.	109.	Dart Industries
110.	Goodrich (B. F.) Co.	110.	Hoover Universal Inc.
111.	Great American Industries	111.	Alliance Tire & Rubber Co.
112.	Vulcan Corp.	112.	Dayco Corp.
113.	Armstrong Cork Co.	113.	Rogers Corp.
114.	Chelsea Industries Inc.	114.	Cellu-Craft Inc.
115.	Clopay Corp.	115.	Wellco Enterprises
116.	Crest-Foam Corp.	116.	Robintech Inc.
117.	Rubbermaid Inc.	117.	O'Sullivan Corp.
118.	Barry (R. G.)	118.	Weyenberg Shoe Mfg. Co.
119.	Brown Group Inc.	119.	Stride Rite Corp.
120.	McDonough Co.	120.	U. S. Shoe Corp.
121.	Anchor Hocking Corp.	121.	Lone Star Industries
122.	Brockway Glass Co.	122.	Texas Industries Inc.
123.	Corning Glass Works	123.	National Gypsum Co.
124.	Dorsey Corp.	124.	Seagrave Corp.
125.	Owen-Illinois Inc.	125.	Interpace Corp.
126.	Ideal Basic Inds. Inc.	126.	Johns-Manville Corp.
127.	Southdown Inc.	127.	Republic Gypsum Co.
128.	Cessna Aircraft Co.	128.	Lear Siegler Inc

FASB No. 13 Changes

1. Pillsbury Co.	1. Campbell Taggart Inc.
2. General Host Corp.	2. Tobin Packing Co. Inc.
3. Greyhound Corp.	3. Smucker (J. M.) Co.
4. Rath Packing Co.	4. Pittsburgh Brewing
5. Levi Strauss & Co.	5. U. S. Industries
6. Champion Int'l Corp.	6. Pope & Talbot Inc.
7. Mead Corp.	7. Rexham Corp.
8. Phillips Petroleum Co.	8. Mobil Corp.
9. Standard Oil Co. (Indiana)	9. Atlantic Richfield Co.
10. Continental Group.	10. Raymond Industries Inc.
11. Pitney-Bowes Inc.	11. Foster Wheeler Corp.
12. Lynch Corp.	12. Craig Corp.
13. Evans Products Co.	13. Mobile Home Industries
14. Goldblatt Brothers	14. Almy Stores Inc.
15. May Department Stores	15. Allied Stores
16. Sears, Roebuck & Co.	16. K Mart Corp.
17. Albertson's	17. Weis Markets Inc.
18. National Tea Co.	18. Pueblo International Inc.

ITC Changes

1. Consolidated Foods Corp.	1. Carnation Co.
2. Fairfield-Noble Corp.	2. Schrader (Abe) Corp.
3. Continental Air Lines Inc.	3. PSA Inc.
4. Panhandle Eastern Pipe Line	4. Union Electric Co.

APPENDIX C

CUMULATIVE FORECAST ERROR VARIANCE DERIVATION

The purpose of this appendix is to derive expressions for a cumulative forecast error variance in both the simple and multiple regression contexts. These variances are needed for the firm level analysis.

Simple Regression Context

Consider the linear relationship

$$Y = a + bX + e \qquad (1)$$

where X and Y are the independent and dependent variables respectively, a and b are estimated parameters of the relationship and e is a disturbance terms which satisfies the usual OLS assumptions.

The variance of an individual forecast error, FEV, can be expressed as (see Kmenta (1976), p. 241):

$$FEV = T_e^2\left[1 + \frac{1}{N} + \frac{(X_o - \bar{X})^2}{\sum_{i=1}^{N} (X_i - \bar{X})^2}\right] \qquad (2)$$

where T_e^2 is the variance of the disturbance term in (1)

N is the number of observations over which the parameters of (1) are estimated

X_i^s are values of the independent variable in the estimation period

$\bar{X}$ is the mean of the independent variable in the estimation period, and

X_o is the value of the independent variable for which a forecast is sought

In deriving the cumulative forecast error variance (CFEV), it is convenient to view a cumulative forecast error (CFE) as the difference between 1) the sum of the actual observations and 2) the sum of the estimated values. Let Y_L refer to the sum of the actual observations and $\hat{Y}_L$ refer to the sum of the forecasts.

$$Y_L = y_1 + y_2 + \ldots + y_L \tag{3}$$

$$\hat{Y}_L = \hat{y}_1 + \hat{y}_2 + \ldots + \hat{y}_L \tag{4}$$

where y_i's are individual observations

$\hat{y}_i$'s are individual forecasts and

L is the number of periods in the forecast

Then

$$CFE = \hat{Y}_L - Y_L \tag{5}$$

Since Y_L and $\hat{Y}_L$ are independent [1]

$$CFEV = Var(Y_L) + Var(\hat{Y}_L) \tag{6}$$

Assuming zero serial correlation [2] among observations, the first term on the right hand side of (6) can be expressed as:

$$VAR(Y_L) = Var(y_1) + Var(y_2) + \ldots + Var(y_L)$$

Invoking the homoskedasticity assumption yields

$$Var(Y_L) = LT_e^2 \tag{7}$$

Turning to the second term on the right hand side of (6), $\hat{Y}_L$ can be written as:

$$\hat{Y}_L = a+bX_1 + a+bX_2 + \ldots + a + bX_L$$

$$= La + (X_1 + X_2 + \ldots + X_L)b$$

$\hat{Y}_L$ therefore is the weighted sum of two random variables, a and b, and $Var(\hat{Y}_L)$ can be expressed as:

$$Var(\hat{Y}_L) = L^2\, Var(a) + (X_1 + X_2 + \ldots + X_L)^2\, Var(b) + 2L(X_1 + X_2 + \ldots + X_L)\, Cov(a,b)$$

Substituting for Var(a), Var(b) and Cov(a,b) (see Kmenta (1976), pp. 217-220) results in

$$Var(\hat{Y}_L) = L^2T_e^2\left[\frac{1}{N} + \frac{\bar{X}^2}{\sum_{i=1}^{N}(X_i - \bar{X})^2}\right] + (X_1 + X_2 + \ldots X_L)^2\, T_e^2\left[\frac{1}{\sum_{i=1}^{N}(X_i - \bar{X})^2}\right] - 2L(X_1 + X_2 + \ldots + X_L)\,\bar{X}T_e^2\left[\frac{1}{\sum_{i=1}^{N}(X_i - \bar{X})^2}\right] \quad (8)$$

Finally, substituting expressions (7) and (8) in expression (6) yields:

$$CFEV = LT_e^2 + L^2T_e^2\left[\frac{1}{N} + \frac{\bar{X}^2}{\sum_{i=1}^{N}(X_i - \bar{X})^2}\right] + (X_1 + X_2 + \ldots + X_L)^2\, T_e^2\left[\frac{1}{\sum_{i=1}^{N}(X_i - \bar{X})^2}\right] - 2L(X_1 + X_2 + \ldots + X_L)\,\bar{X}T_e^2\left[\frac{1}{\sum_{i=1}^{N}(X_i - \bar{X})^2}\right] \quad (9)$$

Multiple Regression Context

This section will follow the notation of the previous section as closely as possible. Consider the linear relationship

$$Y = a + b_1X_1 + \ldots + b_KX_K + e \quad (10)$$

The variance of an individual forecast error, FEV, can be expressed as (see Kmenta (1976), p. 375):

$$FEV = T_e^2 + T_e^2/N + \sum_{k=1}^{K} (X_{ok} - X_k)^2 \, Var(b_k)$$
$$+ 2 \sum_{j<k}^{K} (X_{oj} - \bar{X}_j)(X_{ok} - \bar{X}_k) \, Cov(b_j, b_k) \qquad (11)$$
$$j,k = 1,\ldots, K \; , \; j<k$$

As before, express the cumulative forecast error, CFE, as the difference between the summation of the actual observations and the summation of the forecasts:

$$CFE = Y_L - \hat{Y}_L \qquad (12)$$

Since Y_L and $\hat{Y}_L$ are independent

$$CFEV = Var(Y_L) + Var(\hat{Y}_L) \qquad (13)$$

Assuming zero serial correlation among the observations, the first term on the right hand side of (13) can be expressed as:

$$Var(Y_L) = Var(y_1) + Var(y_2) +\ldots+ Var(y_L)$$
$$= LT_e^2 \qquad (14)$$

Rewriting $\hat{Y}_L$ we have

$$\hat{Y}_L = La + \sum_{k=1}^{K} (X_{1k} + X_{2k} +\ldots+ X_{Lk})b_k$$

Letting A, $B_1,\ldots B_K$ represent the true parameters of expression (10), the second term on the right hand side of (13) can be expressed as:

$$Var(\hat{Y}_L) = E[\hat{Y}_L - E(\hat{Y}_L)]^2$$
$$= E[La + \sum_{k=1}^{K} (X_{1k} + X_{2k} +\ldots+ X_{Lk})b_k - LA -$$
$$\sum_{k=1}^{K} (X_{1k} + X_{2k} +\ldots X_{Lk})B_k]^2$$
$$= E[L(a-A) + \sum_{k=1}^{K} (X_{1k} + X_{2k} +\ldots X_{Lk})(B_k - b_k)]^2 \qquad (15)$$

Since $\sum_{i=1}^{N} y_i = \sum_{i=1}^{N} \hat{y}_i$ in the estimation period, we know that

$$a + b_1\bar{X}_1 + \ldots + b_K\bar{X}_K = A + B_1\bar{X}_1 + \ldots + B_K\bar{X}_K$$

$$a - A = B_1X_1 - b_1X_1 + \ldots + B_K\bar{X}_K - b_K\bar{X}_K + \bar{e}$$

$$a - A = -(b_1 - B_1)\bar{X}_1 - \ldots - (b_K - B_K)\bar{X}_K + \bar{e} \qquad (16)$$

Substituting (16) in (15) results in

$$\mathrm{Var}(\hat{Y}_L) = E\left[L\left(-\sum_{k=1}^{K}(b_1 - B_k)\bar{X}_k + \bar{e}\right) + \sum_{k=1}^{K}(X_{1k} + \ldots + X_{Lk})(b_k - B_k)\right]^2$$

$$= E\left[L\bar{e} + \sum_{k=1}^{K}(X_{1k} + \ldots + X_{Lk} - L\bar{X}_k)(b_k - B_k)\right]^2$$

$$= L^2T_e^2/N + \sum_{k=1}^{K}(X_{1k} + \ldots + X_{Lk} - L\bar{X}_k)^2\,\mathrm{Var}(b_k)$$

$$+ 2\sum_{j<k}^{K}(X_{1j} + \ldots + X_{Lj} - L\bar{X}_j)(X_{1k} + \ldots + X_{Lk} - L\bar{X}_k)\mathrm{Cov}(b_j b_k) \qquad (17)$$

$$j,k = 1,\ldots,K \qquad j<k$$

Substituting (14) and (17) into (13) we have

$$\mathrm{CFEV} = LT_e^2 + L^2T_e^2/N + \sum_{k=1}^{K}(X_{1k} + \ldots + X_{Lk} - L\bar{X}_k)^2\mathrm{Var}(b_k)$$

$$+ 2\sum_{j<k}^{K}(X_{1j} + \ldots + X_{Lj} - L\bar{X}_j)(X_{1k} + \ldots + X_{Lk} - L\bar{X}_k)\mathrm{Cov}(b_j, b_k) \qquad (18)$$

Footnotes

1. Y_L and $\hat{Y}_L$ are independent since they are each functions of different sets of disturbance terms (see Kmenta (1976), p. 240).

2. Fama (1976) provides evidence concerning the lack of serial correlation among monthly security returns.