The Adjustment of Security Returns to the Disclosure of Replacement Cost Accounting Information

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1. INTRODUCTION

Continuing inflation in recent years has highlighted potential problems with historical cost (HC) accounting. Those who are concerned with the insufficiency of this measurement basis argue that during periods of substantive inflation. HC accounting data can give a false impression of earnings and financial strength that lead to the erosion of a firm's capital base [e.g. AICPA (1963), APB (1969), Gynther (1970), Sterling (1975), FASB (1979), Connor (1979)]. To corret this situation, the disclosure of replacement cost (RC) data (or similar current cost data) in financial reports has often been suggested [e.g., Edwards and Bell (1961), Wright (1975), Mathews (1968), Revsine (1970), Revine and Weygandt (1974), FASB (1979)].

In view of the alleged importance of RC accounting data in an inflationary economy, the Securities and Exchange Commission (SEC) amended Rule 3-17 of Regulation S-X in March 1976, (1) thus mandating the public disclosure of certain RC accounting data by its registrants. The details of the amended rule were published in the SEC's Accounting Series Release No. 190 (ASR 190).

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(1) Securities Act of 1933, Release no. 5695(March 23, 1976).

The new rule requires the following RC disclosures in the footnotes to the 10-K report for fiscal years ending on or after December 25, 1976: (a) current RCs of both inventories and productive assets(property, plant, and equipment) estimated at each fiscal year end: (b) depreciation expense and cost of sales (cost of goods sold) at current RC; and (c) descriptions of the methods used in determining the amounts of the above items.(2) According to ASR 190, the RC disclosures outlined above are not required unless the firm's inventories and productive assets aggregate more than $100 million and comprise more than 10% of total assets.

The SEC asserts that the RC data, if disclosed in compliance with the new rule, will enable investors to obtain more relevant information about the current economics of a business enterprise in an inflationary economy than the information provided by HC accounting alone.(3) The SEC also states that 'the Commission believes that such data are important and useful,' and that 'imprecision, if properly explained, will not make the data misleading.'(4) However, the management of firms required to file the RC data in compliance with ASR 190 have challenged the SEC position. They question the alleged usefulness of the RC data becauce of numerous measurement and implementation problems as outlined e.g., in several 1976 10-K reports.(5)

The SEC's assertion that the RC data are useful to investors implies that the data would convey information (beyond that which is available in HC

(2) See *B. Amendments Adopted* of ASR 190.
(3) See *A. General Statement* of ASR 190.
(4) *Ibid.*
(5) For example, General Motors in its 1976 10-K report (p. 75) questions the usefulness of RC data because of the subjectivity necessarily involved in estimating such data, and because of the unrealistic premise that all assets are replaced at one time. The subjective nature of RC data and the associated cautions to users are also found in PepsiCo's 1976 10-K report (p. F-7). The 1976 10-K report (p. 21) of Xerox Corporation cautions that the RC data portray only a partial (and, therefore, perhaps misleading) picture of the impact of inflation on a particular firm by ignoring the effects of inflation on assets other than those specified in ASR 190, as well as the effects on monetary liabilities. Because of the problems similar to those outlined above, U.S. Steel management warns in the 1976 10-K report (p. 41) that the RC data required by ASR 190 do little to assist investors in understanding either the current cost of operating the business or the economic investment in productive assets.

reports and/or through non-accounting sources) relevant to assessing the distribution of returns for firms disclosing such data. If this were true and if such data were previously not available through alternative sources, then (ignoring any externalities) one would expect to observe security return adjustments to the initial disclosure of RC data in 10-K reports. Alternatively, if the counterview questioning the potential usefulness of the RC data were true, one would expect to find no such return adjustments. The purpose of this study is to empirically examine whether such return adjustments actually took place.

The new RC disclosure rule may have affected security returns because of information in the RC data and/or compliance costs borne by affected firms. Allowing for this possibility, the present study investigates the effect of compliance costs separately from the effect of information in the RC data.

The next section discusses the effects of compliance costs including hypotheses and empirical tests. The third section presents the theoretical discussions, hypotheses, and empirical tests of information effects. The fourth section consolidates the findings with respect to the two distinct effects.

2. POSSIBIE EFFECTS OF COMPLIANCE COSTS ON SECURITY RETURNS

A number of events took place in the evolution of the RC disclosure which may have served as signals for revaluing the firms affected by ASR 190 (see table 1). Since the various events seemed to signal different implications regarding security returns, it would be unreasonable to assume that all events had the same effect on security returns. Thus the present study distinguishes two types of potential effects for investigation: the effect of ASR 190 compliance cost and the effect of information in the RC data disclosed under ASR 190.

Table 1. The SEC's ASR 190 related pronouncements.

(1) *ASR 190 proposal* (August 21, 1975)

A. The SEC proposed amendments to Regulation S-X requiring the following replacement cost (RC) disclosures in 10-K reports: (a) the current RCs of inventories and productive capacity; (b) cost of sales; (c) depreciation, depletion, or amortization expense; and (d) the methods used in determining the above RC data.

B. The proposal includes general guidelines for measuring the effects of inflation on a firm (especially on current business operations rather than the value of business assets), and proposes a definition of RC, inventory assets, and productive capacity.

C. The proposal also indicates that only those firms which meet a size (materiality) standard will eventually be subject to the proposed rule. No materiality standard is suggested yet other than an example, total sales) of $50 million.

(2) *ASR 190* (March 23, 1976)

A. The above proposal was formally adopted in ASR 190. The items of RC disclosure are the same as those originally proposed.

B. The new rule suggests the $100-million materiality standard for the RC disclosure.

C. SAB No. 7 and a proposal of a safe harbor rule (ASR 203 later) were issued concurrently with ASR 190.

(3) *SAB No. 7* (March 23, 1976)

A. This is the first SAB published to implement ASR 190. SABs are neither rules nor official views of the SEC; they are interpretations.

B. SAB No. 7 suggests a definition of RC, productive capacity ('special' assets inclusive), and inventories.

C. The Bulletin also provides guidelines for estimating RC data for inventories (allowing the use of FIFO and LIFO methods under certain conditions), productive capacity, depreciation (requiring the use straight-line method and the average current RC), and the cost of sales.

D. The Bulletin also briefly explains how to disclose the RC information in footnote to the 10-K report.

(4) *SAB No. 9* (June 17, 1976)

A. SAB No. 9 clarifies the scope of productive capacity and inventories beyond that discussed in SAB No. 7. Productive capacity includes certain assets for non-productive activities, not assets under non-capitalized financing leases and operating leases and the government-owned facilities. Land for resale is an inventory.

B. The Bulletin suggests guidelines for the size (materiality) test. Land, but not non-capitalized financing leases, is included in the test.

(5) *SAB No. 10* (July 27, 1976)

A. SAB No. 10 presents a change in the definition of productive capacity (i.e., non capitalized financing leases, if significant, as part of productive capactive capacity). It also provides new interpretations of productive capacity and RC, construction in progress and the business segment not intended to maintain are excluded from productive capacity. RC should be based

on current, not future, technology and environmental conditions.

B. The Bulletin suggests several specific guidelines for developing the RC data for inventories, productive capacity, and the cost of sales, including the use of indices in estimating RC.

C. The Bulletin recommends the following to be excluded from the materiality test; (a) inventories and productive capacity of unconsolidated subsidiaries and compamies accounted for under the equity method, and (b) land held for investment.

D. An example of a schedule of items to be included in and excluded from the RC disclosure is presented.

(6) *SAB No. 11* (September 3, 1976)

A. The Bulletin interprets operating leases as part of the lessor's productive capactive capacity, and fully depreciated assets as part of productive capacity if they are still in use and material.

B. The Bulletin also suggests four general RC measurement techniques: indexing, direct pricing, unit pricing, and functional pricing.

(7) *SAB No. 12* (November 10, 1976)

A. SAB No. 12 suggests that the use of the indexing method alone is not acceptable under certain conditions in estimating the RCs of productive assets. It also provides further guidelines for estimating the RC data for the 'limited-use' assets, productive capacity and depreciation.

B. Four complete examples of RC disclosures in footnote to the 10-K report are presented.

(8) *ASR 203*: *Safe Harbor Rule* (December 9, 1976)

A. On March 23, 1976, the SEC had proposed a safe harbor rule to protect persons involed in developing the RC data from potential legal liabities under certain conditions. The proposal was accepted in ASR 203 (Safe Harbor Rule) on December 9, 1976.

B. The SEC adopted the rule because of the imprecise of the RC data and its desire to encourage the development and disclosure of such data in good faith.

(9) *SAB No. 13* (January 4, 1977)

A. The Bulletin suggests that the FASB Statement No. 13 definition of capital lease may be used for financing leases under certain conditions in determining productive capacity. It also recommends certain repairs, materials, and supplies to be included in inventories for the RC disclosure.

B. Two examples of the RC discosures in the annual report to stockholders are presented. The Bulletin also suggests that RC disclosures for parent company financial statements are not required if RC data are provided for the consolidated financial statements.

2.1. Compliance costs

The potential costs of complying with ASR 190 fall into the following general categories: the out-of-pocket compliance costs, opportunity costs, and expected legal costs. The first two of these costs are involved in the preparation of RC data itself. The last one may be incurred as a consequence of disclosing the

RC data because of the data's imprecise nature resulting from measurement problems.

Costs of data preparation. Firms incur costs to produce and report the RC data in compliance with ASR 190. Examples of such costs include the opportunity cost of using internal staff and equipment, the cost of developing an accounting system, fees paid for external accountants, and asset appraisal fees. Among the events listed in table 1, the release of ASR 190 proposal and its adoption, as well as the six Staff Accounting Bulletins (SABs), might have changed the market's expectations of the costs of data preparation.

Since the proposal of ASR 190 was published on August 21, 1975, both the SEC and the financial community have expressed a great deal of concern about the costs of RC data preparation. For example, because RC data is difficult to measure, in its ASR 190 propsal the SEC recognized that the cost of developing the required (RC) information may be significant. Mr. Burton, then the Chief Accountant of the SEC, also said that small companies defined by the $100 million materiality standard were exempted from the RC disclosure requirement 'because of the cost and difficulty of computing the data' (*The Wall Street Journal*, March 25, 1976). In the same article, the *Journal* also reported that about 90% of the 390 letters of comments the SEC received on its ASR 190 proposal were negative because of the difficulty of developing the data. Despite the anticipated high cost of data preparation, the SEC's conclusion in ASR 190 was that 'the (RC) data are of such importance that the benefits of disclosure clearly outweigh the costs of data preparation.'

Evidence from several limited studies about the significance of the firms first-year (1976) compliance costs is somewhat conflicting. For example, Bastable (1977) investigated the ASR 190 compliance costs for the 1976 fiscal year using a questionnaire. Of the firms initially selected for the questionnaire, fourteen provided data for the costs of first-year compliance. The results suggest that the estimated cost of compliance ranged from $5.00 to as much as $800,000 per firm. The results also indicate that 'compliance was attained

largely by internal staffs at the expense of other productive activities', implying that opportunity costs were involved. Based on the evidence, Bastable sums up the results, saying that society has paid a high price for ASR 190.

Garsombke (1978) investigated the costs of first-year compliance with ASR 190 by surveying 244 financial executives of selected firms through a mail questionnaire. He found: (a) overall, the incremental dollar costs of complying with ASR 190 were relatively small (only 0.008% of cost of sales), but the opportunity costs were great; (b) more than 50% of the incremental dollar costs and a large portion of the opportunity costs were considered to be non-recurring over time; (c) 85% of the respondents felt the costs exceeded the benefits.

Reviewing the effects of recently released accounting rules on corporate annual reports, *The Wall Street Journal* (March 30, 1977) reported that ASR 190 caused headaches for corporate treasurers with many man-hours spent in gathering the RC data.

Evidence from the above studies should be interpreted carefully. It seems to support the fact that the out-of-pocket costs of compliance incurred by firms for the 1976 fiscal year was insignificant, relative to the size of such firms. However, the studies focused primarily on out-of-pocket compliance costs, omitting both opportunity costs and potential legal costs for investigation. Furthermore, their sample is small or may not represent the population, if those firms that responded to the questionnaires did so because of their extreme experience with ASR 190. In the latter case, there could exist a response bias.

Costs of legal exposure. As indicated earlier, RC data can be imprecise (or even misleading) so that users of such data may be misled by its implications. Thus, lawsuits by users against a reporting firm can result, causing the firm to bear various legal costs. Also, the SEC can take sanctions against complying firms for negligence in preparing and reporting the ASR 190 data.[6]

(6) There is no known court case involving the ASR 190 data. It remains yet to be seen what

Presumably, to reduce the firms' legal exposure from RC data, the SEC issued ASR 203 as a safe harbor rule on December 9, 1976. This rule, originally proposed concurrently with the adoption of ASR 190 on March 23, 1976, includes the provision that the firm (or person) involved in preparing the ASR 190 data would not be exposed to legal liabilities if the data were prepared with a reasonable basis and were disclosed in good faith. Bacause of the provision, it is conceivable that ASR 203 reduced the potential legal costs.

So far, discussions have been limited to the ASR 190 compliance costs *per se*, without considering their implications for security returns. Although the three types of compliance costs may be large in dollar magnitude, it is likely that they are insignificant relative to the value of complying firms. Thus, a prior expectation is that the effect of compliance costs on security returns would be minimal. Further discussions about the cost effects follow next.

2.2. Cost effect on security returns

If the hypothesized compliance cost effects are present, the resultant stock price changes are most likely to be observed at the time of the release of the ASR 190 proposal, its adoption, and subsequent modifications and interpretations.(7) The reason is that these events seem to hold the greatest potential for signaling the market exactly which firms will be affected and the likely magnitude of the compliance costs. The ASR 190 proposal did not suggest any formal materiality standard (other than total sales or assets of $50 million cited as an example of such a standard) for determining which firms would eventually be subject to the newly proposed rule. Furthermore, the proposal stated that the (proposed) rule could be extended to smaller registrants after more experience is gained in its implementation for larger registrants, thus giving the impression that firms meeting the $50 million potential criterion

kind of legal exposure the complying firms may encounter.

(7) If such compliance costs were passed on to the firm's customers through higher prices of the products, the negative effect of the compliance costs might not exist.

would eventually have to comply with the proposed rule. Note that the $ 50 million standard based on total assets or sales does not appear large, relative to the size of firms listed on NYSE, for example. Therefore, it is likely that investors perceived virtually all NYSE firms to be affected by the proposed rule, and to bear compliance costs. Thus, they might revalue all such firms adversely conditional upon the proposal.

When ASR 190 was adopted, the SEC suggested $ 100 million in the aggregation of inventories and productive assets as the formal materiality standard for determining whether a firm would have to comply with ASR 190. Thus, firms not meeting the materiality standard were exempted from the new rule and were relieved of the compliance costs. If the market agents were uncertain as to which firms would ultimately be exempted from the RC disclosure requirements, then one might anticipate that the formal adoption of ASR 190 which provided materiality guidelines for disclosure would resolve the uncertainty.

It is hypothesized that market agents revised expectations about the exempted firms, favorably upon the adoption of ASR 190. Thus, security returns for these firms would have upward adjustments at the time of that event. In contrast, security returns for complying firms should remain unchanged, or experience further downward adjustments, conditional upon the adoption of ASR 190.

Unlike the ASR 190 proposal and its adoption, it is somewhat difficult to predict the direction in which market agents revised expectations conditional upon a given SAB since the firms compliance costs could actually either increase or decrease due to a given SAB. For example, SAB No. 12 warned that under certain conditions using the indexing method alone would not be acceptable in estimating RCs of productive assets. This warning could increase data prepapation costs. At the same time, the Bulletin also presented four complete examples of RC disclosure in footnotes to the 10-K reports. These examples were likely to help firms reduce data presentation costs.

As discussed earlier, ASR 203 (Safe Harbor Rule) would likely reduce

potential legal costs of complying firms, thereby producing positive adjustments of security returns.

In short, because of the different cost implications of the various ASR 190 events, their effects on security returns could differ in extent and/or direction from event to event. Thus in the present study, the statistical tests investigating the hypothesized cost effect treat each event (including each SAB) separately, rather than pooling them in one test.

2.3. The sample firms

The sample for testing the cost effect consists of firms (treatment firms hereafter) required to comply with ASR 190 for the 1976 fiscal year and firms (control firms hereafter) not required to comply with ASR 190 for the same fiscal year because of the $100 million materiality standard.

A group of 750 firms (the 1975 *Fortune* 500 plus additional 250[8] from a list of the NYSE firms) was initially selected for mailing letters requesting a copy of their 1976 10-K report. Of the 750 firms, 235 responded promptly to the request.[9] Of these firms, 227 met the following requirements and were accepted as potential treatment firms: (a) reported RC data for 1976 in compliance with 190; (b) were listed on NYSE for the 172 week period(December 23, 1973—April 9, 1977); (c) had December 31 fiscal year end, and (d) had security returns for the period in the CRSP daily return files.

An initial list of potential control firms was developed from the *Value Line Data Base* tape (1976) by checking the $100 million materiality standard. The application of the materiality standard plus the above criteria, (a) through (d), applied to treatment firms produce 109 potential control firms.

Given the potential candidates for treatment and control firms, a pairing of the two types of firms was performed according to certain criteria (to be discussed

(8) These 250 firms were somewhat arbitrarily selected from the list of firms in both the CRSP monthly return files and the COMPUSTAT tapes by reading the firms' names and information about their affiliated stock market and fiscal year end (December 31).

(9) There might exist a potential response bias if those firms which sent the 10-K reports did so because they experienced a small (or large) effect of compliance with the ASR 190.

later). The pairing process generated 83 pairs which constitute the final treatment and control samples (83 firms in each)[10] for testing the cost effect. The firms are listed in appendix 1.

2.4. Test periods

A period of 172 weeks beginning December 23, 1973 and ending April 9, 1977 was selected for investigation. The first 86 weeks ending August 16, 1975 were defined as the pre-test period over which no cost (and information) effects of the RC disclosure were expected (see fig. 1). The remaining 86 weeks were designated as the overall test period over which any such affects were expected to be revealed.

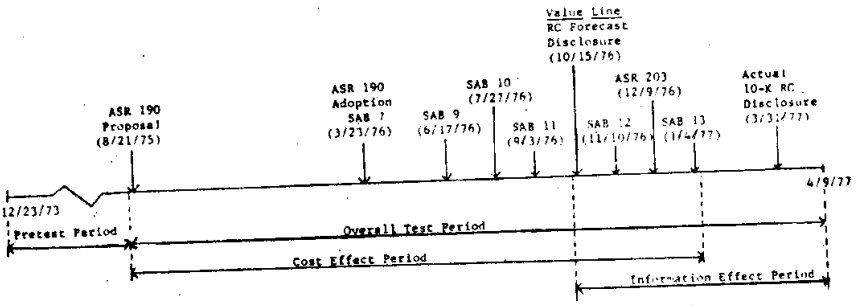

Fig. 1. ASR 190 related critical events.

At least ten ASR 190 events (see fig. 1) were believed to be potentially relevant to assessing the effects of the RC disclosure rule. Given the events, the overall test period was partitioned into various subperiods for conducting the statistical tests. A subperiod chosen for testing a cost effect was one week, the week in which an ASR 190 event occurred. A total of eight compliance

(10) A fair number of firms were lost in the process of constructing the treatment and control samples, which might cause a sample selection bias. Of the 235 firms that sent their 1976 10-K reports, eight were first deleted because they did not meet the requirement(s) discussed above. Then, an additional 144 firms were lost in the process of pairing treatment and control firms due to the relatively strict criteria (to be discussed shortly) employed. Also, there may exist a sample construction bias because of difference in certain non-controlled financial profiles (e.g., size) between paired firms [see Foster (1980) and Banz (1979) for discussions about such a bias]. The firm profiles controlled in the present study will be discussed later.

cost events (the adoption of ASR 190, the publication of six SABs and the issuance of ASR 203) were examined separately (or combined if two events occurrently) for testing the cost effect. The other events (see fig. 1) were excluded from consideration for the cost effect for reasons to be discussed later. As indicated earlier, the compliance cost events cannot be pooled in one statistical test since the direction of their effects on the firms compliance costs seem to differ. For this reason and given market efficiency, using one week as a test period for investigating the cost effect of each event is believed to be reasonable.

2.5. Pairing firms

The potential cost effect was investigated using a matched pair design between treatment and control firms.[11] Control firms were individually paired with treatment based on four dimensions: (a) the 'good' and 'bad' news based on the signs ('+' for good and '−' for bad) of differences in HC earnings per share (EPS) between the two fiscal years, 1975 and 1976. (b) the systematic risk (β) of security return; (c) the week of 1976 10-K report release;[12] and (d) the firms' industry membership (if possible). The results of matching firms according to the four criteria are presented in appendices 2 and 3.

The matched pair design has at least three advantages. First, the design can help control the effects of numerous unspecified factors affecting security returns both before and after the initiation of the RC disclosure to the extent that they affected treatment and control firms alike. Second, the design accounts for contemporaneous cross-sectional correlations of return data by differencing

(11) Because of the materiality threshold, treatment firms systematically differ in size from control firms if the aggregation of inventory and productive assets is a measure of the size. An attempt was made to control for potential confounding effects of the firms' size difference on the results of the tests (to be described later), but with no success. Given the research design employing control firms, the problem of size difference necessarily exists and appears difficult to resolve satisfactorily (see footnote 10).

(12) The release week is the week that includes the date at which the firms filed the 1976 10-K reports with the SEC. This date was assumed to be the date stamped with 'FEE RECEIVED' by the SEC on the cover page of the 10-K report.

the returns of paired treatment and control firms. Third, the design can virtually eliminate the potential problem of the regression phenomenon inherent to the estimation of the firm's systematic risk (the phenomenon that high βs tend to be overestimated and that low βs tend to be underestimated). This is true as long as the extent of the regression phenomenon is the same between paired firms.

The first criterion, (a), was considered in pairing firms in order to eliminate the effects of information in HC annual earnings on security returns. Since the HC annual income numbers were released in the same event period as the ASR 190 data in the 10-K reports, the control for the market reaction to unexpected changes in HC accounting income was necessary to isolate the effect of the RC disclosure rule.

The systematic risk of individual firms was estimated via the market model below using weekly return observations for 86 weeks ending August 16, 1975, which was one week prior to the SEC's proposal (August 21, 1975) for RC disclosure,

$$R_{jt}=\alpha_j+\beta_j R_{Mt}+\varepsilon_{jt},$$

where

$R_{jt}=$ the rate of return[13] on security j for week t developed from the CRSP daily return tapes,

$R_{Mt}=$ the value weighted rate of return on the market portfolio developed from the CRSP daily market return tapes, adjusted for dividends,

$\alpha_j, \beta_j=$ regression coefficients,

$\varepsilon_{jt}=$ error term.

The details of pairing based on $\hat{\beta}$s are found in appendix 2. Summary statistics of $\hat{\beta}$s are presented in table 2. As revealed, at the aggregate group level the mean (0.891) and the variance (0.111) of $\hat{\beta}$s for the treatment

(13) Weekly returns are defined as $R_t=\log_e\{(P_t+D_t)/P_{t-1}\}$, where R_t is the rate of return on common stock for week t, P_t is the price per common stock share at the end of week t, and D_t is dividends paid during week t.

group are similar to the mean (0.879) and the variance (0.125) of $\hat{\beta}$s for the control group, suggesting that differences in the systematic risk between paired firms have been successfully controlled.

Table 2. Summary statistics of beta estimates (βs) for treatment and control firms.

Group	Mean[a]	Variance[a]	$\overline{SE}(\beta)$[b]	$\bar{R}^{2c}$
Treatment (83 firms)	0.891	0.111	0.149	0.312
Control (83 firms)	0.879	0.125	0.200	0.201

[a]The means and the variances of βs are computed using individual βs in appendix 2.
[b]The average standard errors of individual βs in each group.
[c]The average R^2 for individual βs in each group.

2.6. Testing hypotheses and procedures

The effect of compliance costs was investigated with respect to each of the eight ASR 190 events mentioned before, by comparing returns for treatment and control firms. The cost effect of the ASR 190 proposal was judged to be difficult to investigate since the effect (if any) was believed to be market wide as discussed earlier. That is, due to its potential cost implications for virtually all NYSE-listed firms, it was hypothesized that the proposal could affect both treatment and control firms adversely. In this case, control firms are, in fact, not control firms. For this reason, the test for the cost effect of the ASR 190 proposal using control firms is likely not to reveal significant differences of security returns. Thus the event was considered only for comparisons with the other event (the adoption of ASR 190) in terms of results of the tests conducted later.

When the proposal was adopted in ASR 190, the formal materiality standard of $100 million of inventories *and* productive assets was established. Therefore if any significant cost burden was expected at the time of the ASR 190 proposal, the new materiality standard relieved control firms from bearing these costs. As a result, it was hypothesized that security returns for control firms would have upward adjustmets to the adoption of ASR 190, while security returns for treatment firms should remain unchanged (or fall further since the

probability of adopting the proposal increased to one).

As discussed earlier, SABs could either increase or decrease the compliance costs imposed on firms and pooling all SABs in one statistical test might cause positive return adjustments to cancel negative return adjustments. On the other hand, ASR 203 (Safe Harbor Rule) may have benefited treatment firms, reducing potential legal costs involved in the ASR 190 data. If this were the case, one should observe upward adjustments of security returns for the firms conditional upon ASR 203.

The hypotheses for testing the cost effects of the ASR 190 events are stated as follows:

Null	Alternative
$\mu_d \geqq 0$	$\mu_d < 0$ for ASR 190 adoption
$\mu_d = 0$	$\mu_d \neq 0$ for six SABs
$\mu_d \leqq 0$	$\mu_d > 0$ for ASR 203

where μ_d is the (population) mean of weekly return differences between paired firms. The actual weekly return difference was calculated as $(R_1 - R_0)$ for each pair where T and C represent treatment and control firms, respectively.

The hypotheses for the cost effects were tested using the standard paired t-test. The tests were conducted for each cost event week separately. If the cost effects of the ASR 190 events were present, the costs might be greater for treatment firms with more diversified lines of business (LOB) and/or with a larger composition of inventories and productive assets in total assets than for treatment firms with less diversified LOB and/or with a smaller composition of such assets. The reason is that the RC data for the former firms would be more difficult (expensive) to prepare than the RC data for the latter firms. In other words, the cost burden borne by the former firms might be relatively greater than that borne by the latter firms, implying that the extent of the hypothesized cost effect might differ between the two types of firms differentiated by the two criteria. This possibility of differential cost effects was also

investigated as part of the statistical tests, by partitioning firms into high and low subgroups according to the median number of LOBs and the median value of the asset composition ratio.[14]

2.7. Results of the cost effect tests

The results of the tests for the cost effect are presented in table 3 and 4, along with the cumulative average return differences (CARD)[15] plotted in fig. 2 over the entre 172-week period (including additional 12 weeks in the

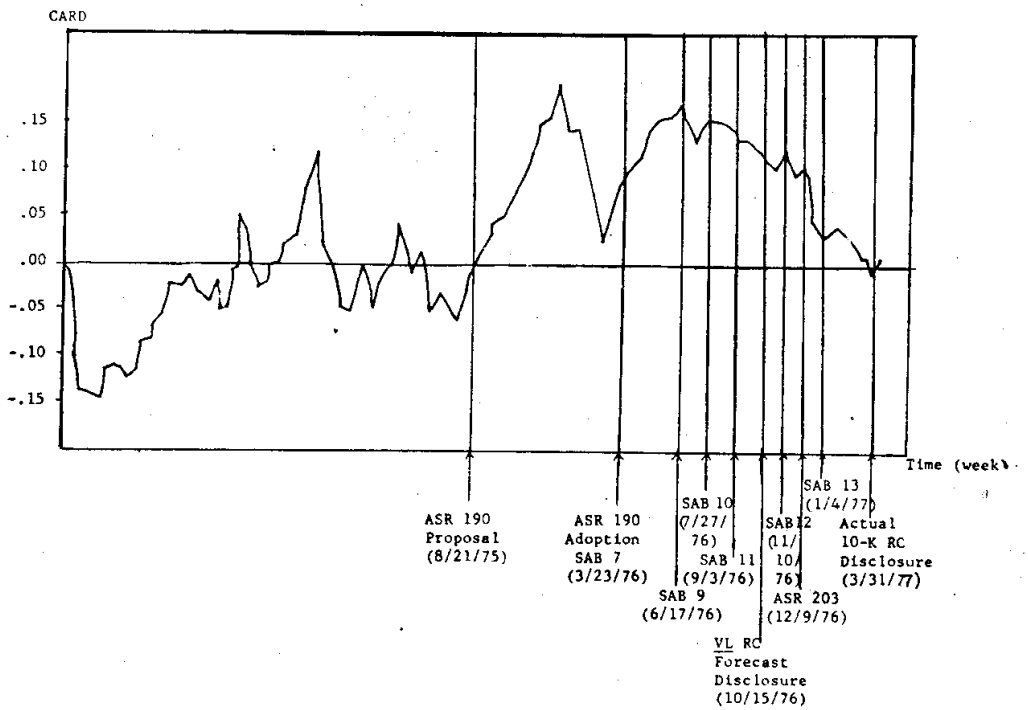

Fig. 2. Cumulative average return differences (CARD) for 83 pairs of firms over the entire (172 weeks) period examined.

(14) There is the possibility that both the LOB tests and the tests based on the asset composition may be correlated with firm size (e.g., larger firms tend to have more LOB) and thus the tests may confound size with LOB (or asset composition).

(15) The CARD curve is constructed as follows:

$$CARD_\tau=\sum_{t=1}^{\tau}\bar{d}_t=\sum_{t=1}^{\tau}\sum_{j=1}^{n}(R_{jt_T}-R_{jt_C})/n, \quad \tau=1, 2, \ldots, 172,$$

where $\bar{d}_t$=the average return difference for week t, n=the total number of pairs (83), R_j= the rate of return on the jth paired common stock, and T, C=treatment and control firms respectively.

test period for the information effect).

The t-values in the second column of table 3 are for the case where all 83 pairs of firms were included in conducting the t-tests. The t-values in the third and fourth columns are for the two subgroups of firms differentiated by the number of LOB as reported in 10-K reports. These t-values were calculated to see whether the cost effect differed depending upon the extent to which the

Table 3. The values of paired t-statistics[a] for testing the effect of compliance costs.

Event	Overall group (83 pairs)	Line of business(LOB)[c] High subgroup (44 pairs)	Line of business(LOB)[c] Low subgroup (39 pairs)	Asset composition[d] High subgroup (41 pairs)	Asset composition[d] Low subgroup (42 pairs)
ASR 190 proposal(8/21/1975)	1.000	0.412	1.114	0.123	1.150
ASR 190[b] and No. 7(3/23/1976)	2.858*	2.428*	1.513	0.735	8.022*
SAB No. 9(6/17/1976)	1.717	1.293	1.116	0.983	1.405
SAB No. 10(7/27/1976)	1.782	1.992	0.633	1.930	0.620
SAB No. 11(9/3/1976)	−0.133	0.050	−0.227	1.115	0.791
SAE No. 12(11/10/1976)	−0.066	0.264	−0.468	−0.910	0.928
ASR 203(Safe Harbor)[b] (12/9/1976)	0.041	1.504	−1.504	−0.020	−0.078
SAB No. 13(1/4/1977)	−3.515*	−2.641*	−2.301*	−2.504*	−2.579*

[a]The t-values are based on the weekly tests for the cost events. The degrees of freedom (d.f.) and the critical t-values (approximate values indicated by asterisks) at the probability level of 0.05 are:

Cases	d.f.	Critical t-value One-tailed	Critical t-value Two-tailed
83 pairs	82	1.666*	1.993*
44 pairs	44	1.682*	2.018*
39 pairs	38	1.687*	2.025*
41 pairs	40	1.684	2.021
42 pairs	41	1.683*	2.020*

[b]The tests for these two events were one-tailed. All other tests were two-tailed.

[c]The high (and low) LOB represents pairs whose treatment firms had highly (and less highly) diversified lines of business (LOB) and, thus, were assumed to have relatively high (and low) costs of the RC data preparation. Treatment firms in the high LOB group are those (44 firms) with four or more LOB as reported in 10-K, while treatment firms in the low LOB group are those (39 firms) with less than four LOB.

[d]The asset composition ratio was computed as follows: $(I_{HC}+NPP_{HC})/TA_{HC}$, where I stands for inventories, NPP for net property, plant and equipment, and equipment, and TA for total assets, Treatment firms (i.e., the pairs) were divided into the high low groups based on the median value (0.5503) of the ratio. There are 42 firms in the high and low groups, respectively.

*Denotes a statistically significant t-value at the 0.05 probability level.

complying firms' assets subject to the RC disclosure were heterogeneous as measured by the number of diversified business lines. Similarly, in the last two columns are the t-values calculated in order to investigate whether the cost effect varied according to the relative composition of inventories and productive assets in total assets.

As revealed in table 3, the t-values for all of the ASR 190 events, other than ASR 190 (and SAB No. 7) and SAB No. 13, are insignificant at the 0.05 probability level.[16] The t-values for ASR 190 are significant and positive in sign, contrary to what was expected (see the hypotheses for ASR 190). Note that return differences were calculated as $(R_T - R_C)$ where T and C stand for treatment and control firms respectively. The prior expectation was that security returns for control firms would have upward adjustments to the event, while security returns for treament firms should remain unchanged, because of the $100 million materiality standard. Thus, this standard exempting control firms from the burden of compliance costs expected at the time of the ASR 190 proposal should have produced a negative t-value, rather than the positive t-values which were observed. Therefore, assuming that expectations changed in the ASR 190 week, we cannot attribute significant t-valuse to the cost effect of ASR 190.

Finding a statistically significant t-value for ASR 190 and SAB No. 7 is consistent with the hypothesis that the mandate to disclose RC data yielded benefits that exceeded the costs to the disclosing firms. However, for reasons to be discussed below, random events may be producing this result.

(16) Note that both ASR 190 and ASR 203 were examined by conducting the one-tailed tests, while the other cost events were investigated by conducting the two-tailed tests (see the hypotheses stated earlier). There were eight control firms (included in the tests) which did not meet the tentative materiality standard ($50 million) in the ASR 190 proposal in terms of either total sales or assets. It is likely that these firms were not affected due to expected ASR 190 compliance costs even at the time of the proposal. In order to investigate whether these firms confounded the test results for the case of 83 pairs, additional tests were conducted using the remaining 75 pairs of firms after deleting eight pairs associated with the eight control firms. The results of these additional tests were similar to those for 83 pairs reported here, suggesting that the eight firms did not cause the hypothesized confounding.

The t-value for SAB No. 13 also is significant and negative in sign. This event was examined by conducting a two-tailed test with no prediction of sign (see the hypotheses stated earlier), since it was not clear whether SAB No. 13 increased or decreased the compliance costs.

The significant t-values for ASR 190 (and SAB No. 7) and SAB No. 13 may be attributed to random factors or a problem with matching firms. In order to obtain an insight into question, the t-tests were also conducted for each of the non-event weeks in both the pre-test and test periods. A summary of the results for these additional tests is presented in table 4.

The results reveal that the t-values are also significant for certain non-event weeks in both the pretest and test periods. The percentages of such significant t-values to the total number of t-values computed for the non-event weeks are

Table 4. Frequency of statistically significant weekly return differences in pre-test, test, and ASR cost event periods.

Period	Overall (83 pairs)	No. of LOB		Asset composition	
		High	Low	High	Low
Pre-test period(86 weeks)					
Total number of t-values computed	86	86	86	86	86
No. of significant t-values at 0.05[a]	14 (14.3)	8 (9.3)	7 (8.1)	10 (11.6)	12 (14.0)
Mean of absolute t-values	1.156	0.988	0.975	1.037	1.037
Test period(74 weeks)					
Total number of t-values computed	74	74	74	74	74
No. of significant t-values at 0.05[a]	14 (18.9)	2 (16.2)	6 (8.1)	6 (8.1)	10 (13.5)
Mean of absolute t-values	1.130	1.050	0.889	0.902	0.985
The 8 event weeks[b]					
Total number of t-values computed	8	8	8	8	8
No. of significant t-values at 0.05[a]	2 (25.0)	2 (25.0)	1 (12.5)	1 (12.5)	2 (25.0)
Mean of absolute t-values	1.389	1.323	1.114	1.040	1.321

[a]Percentages given in parentheses.

[b]See table 3.

relatively high and are, in general, not substantially different from the frequency of the significant t-values for the eight cost event weeks. Percentages of the significant t-values are about two to three times higher than one would expect by chance with probability of 0.05. Therefore, the significant t-values in those for ASR 190 and SAB No. 13, may be attributed to random factors and/or problems with the methodology employed here.

The observed t-values in column 3 through column 6 of table 3 appear to support the above interpretations. If any cost effect indeed existed, as revealed in the significant t-values for ASR 190 and SAB No. 13, one would expect the effect to vary depending upon the heterogeneity of the complying firms' assets measured by the number of LOR or the relative proportion of inventories and productive assets in total assets. However, no such tendency is observed. This finding may, in part, be explained by the possible confounding(with LOB) of size difference between treatment and control firms.

Based on the above evidence, one cannot reject the hypothesis that ASR 190 imposed significant compliance costs on firms.

3. INFORMATION EFFECTS OF REPLACEMENT COST DISCLOSURE

3.1. Potential information content

As a first step in assessing the information effect of RC data on security returns, one should identify a model and its parameters that describe the relationship between the operating and financial risk characteristics of firms and the equilibrium pricing of securities. One must then demonstrate how RC data are relevant to estimating such parameters. However, because of the RC disclosure rule's complex implications for security returns, it seems difficult to find (or develop) such a model. Therefore, *a priori* it is not clear what systematic information effect the RC disclosure might have on security returns.

For the above reason the following discussion focuses on how the RC dis-

closure would affect security returns. Conceptual discussions about the potential usefulness of RC data in general are found in the previous studies[17] [e.g., Edwards and Bell (1961), Sprouse and Moonitz (1962), Wright (1965), Mathews (1968), Revsine (1970), Revsine and Weygandt (1974), Abdel-khalik and McKeown (1978), and FASB (1979)].

Despite the alleged measurement and implementation problems, the RC accounting data disclosed under ASR 190 may be used by investors in various ways. Although little evidence is available on how the RC data specifically is used, it is conceivable that investors can compute current operating income adjusted for RCs of inventories and productive assets. Information on this income may be used in assessing the firm's operating profitability and its growth, prospective cash flows from operation, and dividend policy, during periods of changing prices. Many of the studies cited above assert that such an income number is more useful for users' economic decisions than HC income.

Although it can be hypothesized that the ASR 190 data were used by investors in assessing the risk return prospects of firms, one cannot predict, *a priori*, that information provided by the data would induce downward revaluations of firms. Given alternative sources of information and the market agents rational expectations behavior, the effects of the RC disclosure would have been anticipated and impounded in security prices well in advance of the release of RC numbers under ASR 190. If so, it is possible that the RC disclosure made later under ASR 190 could induce investors to revise their previous expectations in either direction. Therefore, on average, the effect (if any) of the RC disclosure on firms could have been either positive or negative.

(17) Drake and Dopuch (1965) and Prakash and Sunder (1979) question the usefulness of dichotomizing income into current operating income and holding gains measured under RC as alleged by Edwards and Bell (1961) and others and conclude that such a dichotomy does not make income data more useful. Nor is the income dichotomy useful in predicting firms' future income streams. However, as Drake and Dopuch note in their study, the conclusions may not be viewed as the rejection of RC accounting itself since the acceptance of RC accounting and the dichotomization of income for reporting purposes are two separate issues. Usefulness of RC data cannot be judged according to the method of income reporting.

However, with an appropriate grouping of firms and using certain forecasted RC data as a proxy for the market's expectations of ASR 190 RC data, it is possible to formulate a hypothesis of the systematic effect of the RC disclosure on security returns. A research design for investigating such a hypothesis is discussed next.

3.2. Assessing the information effect

The effect of RC data on security returns was investigated over the 26-week period (October 10, 1976 through April 9, 1977) encompassing the *Value Line* forecasted RC disclosure for 1976 and the actual 10-K RC disclosure (see fig. 1 above). The *Value Line Investment Survey* began publishing the income effects of certain forecasted RC data for individual firms in its October 15, 1976, issue. The forecasted RC data for the sample firms for this study was released over the 16-week period ending January 29, 1977. The 1976, 10-K RC data for more than 90% of the sample firms were disclosed during the week of March 27 through April 2, 1977 (see appendix 2). Therefore, if the forecasted and actual RC data had any information, its effect on security returns most likely would be observed over the 26-week period.

As indicated earlier, given alternative sources of information and the market agents' rational expectations behavior, the market could assess the information content of RC data prior to the release of the 1976 10-K reports. Thus, the Value Line RC forecasts were assumed to be proxy for the market's expectations of ASR 190 RC data.

Since the forecasted RC data would be either under- or overestimated relative to the actual RC data disclosed under ASR 190, the market might subsequently have revised prior expectations conditional upon the disclosure of actual RC data in either direction. To control for such potential cancellations, a special grouping procedure was employed for this study.

3.3. Grouping of matched pairs

The sample for testing the information effect consists of 78 pairs developed from the 83 pairs of firms for testing the cost effect previously. Five pairs[18] were lost in the process of grouping pairs on the basis of unexpected RC earnings. To group the matched pairs, the following 1976 RC income forecast error was first computed:

$$\Delta EPS_{RC}=(RC_A-76HC_A)-(RC_F-HC_F),$$

where

HC_F =the Value Line forecast of 1976 EPS under HC,

RC_F =the Value Line forecast of 1976 EPS adjusted for estimated RC,

$76HC_A$ =actual 1976 EPS under HC,

RC_A =actual 1976 EPS adjusted for actual RC,

The first term (RC_A-76HC_A) is the adjustments for actual RC depreciation and cost of sales. The second term is the corresponding forecasted adjustments. Hence, the difference is the unexpected change in the adjustment of actual vs. forecasted RC income data.

Given ΔESP_{RC}, treatment firms (along with the paired control firms) were grouped into two (good-news and bad-news) subgroups according to the signs of ΔEPS_{RC}. If a treatment firm's ΔEPS_{RC} had a positive (or negative) sign, then the matched pair was classified as a good (or bad) news pair in the sense that the adverse impact of actual RC data on the pair's earnings was less (or greater) than the impact previously estimated by the market (i.e., the Value Line).[19] The above grouping procedure produced 23 'good-news' pairs

(18) Two treatment firms (and, hence, the related pairs) were deleted as outliers because they had unusually large unexpected HC income numbers, while three more pairs were excluded because one firm in each of the three pairs had a negative HC earnings forecast for 1976.

(19) For example, suppose that the Value Line forecasted the 1976 HC-based *EPS* of a firm to be \$1.50 (HC_F) and a decrease in the firm's *EPS* number by 30% if the number would be adjusted for the forecasted depreciation expense under RC. Further suppose that the firm's actual HC-based *EPS* for 1976 was \$1.80($76HC_A$) and \$1.54 (RC_A), if adjusted for actual RC depreciation expense. Then, the firm's ΔEPS_{RC} for the year would be \$0.19 implying that the firm would be classified as a good-news firm.

($+\Delta EPS_{RC}$) and 55 'bad-news' pairs($-\Delta EPS_{RC}$), respectively.

After the matched pair return differences were grouped into good and bad news groups, average weekly portfolio return differences were computed for each of the 26 weeks by averaging individual component return differences in each subgroup. Finally, a difference between the two portfolio return differences (i.e., the good-news firms' portfolio return difference minus the bad-news firms' portfolio return difference) was computed for each week. Thus, there were a total of 26 such return differences which were treated as a single sample for conducting the test for the hypothesized information effect. This second round of differencing was necessary to avoid the cancellation of positive and negative return adjustments to unexpected RC income numbers (EPS_{RC}).

Once the matched pairs were grouped on the basis of the sign of unexpected RC income (ΔEPS_{RC}), two sets of summary statistics were calculated in order to check the efficacy of the control procedures. The first set of summary statistics are the percentage change in HC earnings between 1975 and 1976 for treatment and control firms in the $+\Delta EPS_{RC}$ (good-news) group and the $-EPS_{RC}$ (bad-news) group. Table 5 presents the medians, means, and standard deviations of the treatment and control firms in the two groups. From table 5 we see that the treatment and control firms in each group are reasonably well matched in terms of mean and median change in HC earnings over 1975 and 1976. The only slight problem is in the $+\Delta EPS_{RC}$ group where the mean and median change of the treatment group is slightly greater than for the control firms. To the extent this is a problem, this group's average weekly return difference is

Table 5. Summary statistics of unexpected historical cost income numbers (ΔEPS_{HC}) between 1975 and 1976[a]

Group	Treatment firms			Control firms		
	Median	Mean	Std. dev.	Median	Mean	Std. dev.
Overall (78 firms)	0.289	0.527	0.956	0.284	0.479	0.720
$+\Delta EPS_{RC}$ (23 firms)	0.389	0.614	0.777	0.274	0.443	0.432
$-\Delta EPS_{RC}$ (55 firms)	0.258	0.492	1.026	0.284	0.494	0.814

[a] $\Delta EPS_{HC}=(76HC_A-75HC_A)/75HC_A$.

biased upwards over the 52 weeks preceding the announcement of 1976 HC earnings.

Table 6. Summary statistics of historical cost earnings forecast errors (ΔEPS_{HC}) for 1976.[a]

Group	Treatment firms			Control firms		
	Median	Mean	Std. dev.	Median	Mean	Std. dev.
Overall (78 firms)	−0.010	−0.043	0.241	0.014	0.030	0.339
$+EPS_{RC}$ (23 firms)	0.000	−0.032	0.208	−0.009	−0.011	0.311
$-\Delta EPS_{RC}$ (55 firms)	−0.013	−0.047	0.255	0.014	0.047	0.351

[a] $\Delta EPS_{HC}=(76HC_A-HC_F)/HC_F$.

The second set of summary statistics (table 6) presents the Value Line HC percentage earnings forecast errors. Value Line's forecasts of earnings for 1976 (HC_F) for both treatment and control firms were compared to the actual 1976 HC earnings ($76HC_A$). The test for information in RC data is made using the period from Value Line's forecasts of both HC and RC earnings through the disclosure of actual HC and RC earnings, a 26-week period. One check if unexpected historical cost earnings are being controlled (and hence any security adjustments can be attributable to the RC data) is the equality of the Value Line HC forecast errors of the treatment and control firms in each portfolio grouping ($\pm\Delta EPS_{RC}$). Table 6 suggests that Value Line HC forecast errors are not equal in the both groups. Again means, medians, and standard deviations are reported. Within the $-\Delta EPS_{RC}$ group the treatment firms have a mean (median) HC earnings forecast error of −0.0047 (−0.013) while the control firms have a mean (median) forecast error of 0.047 (0.014). The difference in means (medians) within this group is −0.094 (0.027). Not controlling for these HC forecast errors will, *ceteris paribus*, cause the difference in returns between treatment and control firms in the ΔEPS_{RC} group to be understated, thus biasing the results towards rejecting the null hypothesis of no information in RC disclosures. We will later see that this is likely occurring.

3.4. Testing the hypotheses

Given the grouping procedure, it was hypothesized that the behavior of security returns would differ systematically between the good-news ($+\Delta EPS_{RC}$) and bad-news ($-\Delta EPS_{RC}$) pairs, if actual 10-K RC data had information. Based on the return differences, the hypotheses for testing the information effect are stated as follows:

Null: $\mu_d \leqq 0$,

Alternative: $\mu_d > 0$,

where μ_d is the (population) average return difference whose estimate ($\bar{d}$) was computed as follows:

$$\bar{d} = \frac{1}{26} \sum_{t=1}^{26} (d_G{}^t - d_B{}^t),$$

where d_t is the portfolio return difference between treatment and control firms for week t, and G and B stand for the good-news and bad-news firms, respectively.

The hypotheses were tested again using the standard paired t-test.

3.5. Results of the information effect tests

The results of the information effect test for the 26-week period (October 15, 1976 through April 9, 1977) are presented in tables 7 and 8 and in fig. 3. In addition, the same test was also conducted over the 10-week (January 30 through April 9, 1977) periods, respectively, in order to see how sensitive the results of the test are to a change in a test period. The 10-week test excludes the disclosure period of Value Line RC forecasts, thereby encompassing only the release period of 1976 annual accounting numbers including the ASR 190 data. The 52-week test covers the release period of 1975, as well as 1976, annual accounting numbers. The results of those additional tests are also presented for comparisons in the last two columns of table 7.

As revealed, the observed t-value for this 26-week period is 1.6250, which

Table 7. Results of the information effect tests.

Description	26-week period	10-week period	52-week period
Cumulative average return difference[a]	0.1035	0.0627	0.1303
Mean of average weekly return differences[b]	0.0039	0.0063	0.0025
Standard deviation of average weekly return differences	0.0122	0.0015	0.0140
Standard error of average weekly return differences	0.0024	0.0047	0.0019
t-value	1.6250	1.3338	1.3158

[a]The cumulative average return difference was computed as: $CARD_N = \sum_{t=1}^{N}(d_t^G - d_t^B)$; where d^G and d^B are the average return differences for the good-news and bad-news subgroups, respectively, and N increases from 1 to 26.

[b]The average return differences were computed as the good-news subgroup's weekly return difference minus the bad-news subgroup's weekly return difference.

Table 8. Average weekly return differences for testing the information effect.

Week	Good-news firms (1)	Bad-news firms (2)	Differences (1)−(2)
1	−0.00463	0.00004	−0.00467
2	−0.00235	0.00675	−0.00910
3	0.00196	−0.00872	0.01068
4	−0.00614	−0.01325	0.00711
5	−0.00108	−0.00112	0.00004
6	0.01573	0.01740	−0.00167
7	−0.00185	−0.00062	−0.00123
8	−0.02519	−0.01289	−0.01230
9	0.00802	−0.00834	0.01636
10	0.00370	0.00033	0.00337
11	0.00610	0.00140	0.00470
12	0.01800	−0.01148	0.02948
13	−0.02737	−0.02591	−0.00146
14	0.00147	−0.00467	0.00614
15	−0.00336	−0.00301	−0.00035
16	−0.00539	0.00086	−0.00625
17	−0.01046	0.00948	−0.01994
18	0.00977	−0.00397	0.01374
19	0.01452	−0.00528	0.01980
20	−0.01024	0.00388	−0.01412
21	0.01749	−0.00989	0.02738
22	−0.01012	−0.00761	−0.00251
23	0.00082	−0.00763	0.00845
24	−0.00698	−0.01023	0.00325
25	0.01212	−0.00349	0.01561
26	0.00017	−0.01088	0.01105

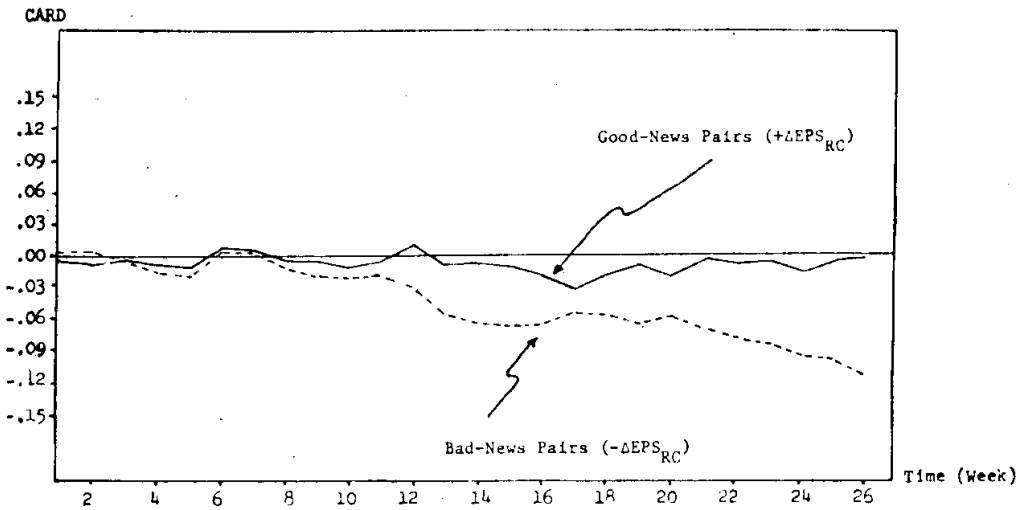

Fig. 3. Cumulative average return difference (*CARD*) curves for good-news and bad-news matched pairs.

is significant at the probability level of 0.10.[20] Thus the null hypothesis of no information effect is rejected. However, the statistically significant security return difference can be due to either unexpected RC data or unexpected HC data. In table 6, Value Line HC forecast errors differed between treatment and control firms in the $-\Delta EPS_{RC}$ group producing a potential bias. If this bias, due to unexpected HC earnings, is in fact driving the results in table 7, then the $-\Delta EPS_{RC}$ group should be dominating the results.

Table 2 and fig. 3 reveal that the statistically significant difference in returns between the good-news and bad-news groups is due entirely to the negative abnormal returns of the bad-news group. The good-news group remains around zero over the entire 26-week period. Thus, care must be exercised in attributing the statistically significant return behavior to RC data. It is likely due to unexpected HC earnings.

(20) The *t*-test was repeated for the 26-week period using 81 pairs of firms by adding back to 78 pairs those three pairs which were deleted before since one of the paired firms had a negative denominator of ΔEPS_{HC} (see footnote 18). The *t*-value of this test was 1.3478, not substantially different from the *t*-value (1.6250) based on 78 pairs. This finding confirms that the exclusion of the three pairs from the 78-pair sample did not produce a serious problem with respect to the test results reported here.

To further explore the question of bias due to unexpected HC earnings, two additional test periods were examined: the entire 52 weeks preceding the filing of the 10-K's which would capture any security adjustment over the year after controlling for the change in HC earnings and the 10 weeks subsequent to Value Line forecast disclosures. These 10 weeks began after the last Value Line forecast was made, thus avoiding capturing any information in the Value Line disclosures. Both of these tests produce similar, statistically insignificant t-value (1.3338 and 1.3158) at the 0.10 probability level (table 7).

To investigate whether the small sample size, coupled with the parametric t-test based on restrictive assumptions about the sampling distribution of return data, was a methodological problem, the Wilcoxon matched pairs signed ranks (no-parametric) test was also conducted. The Wilcoxon test was run only for the 26-week period since the observed t-value (1.6250) of this period was found to be the largest of all t-values computed, suggesting that the methodological problem most likely could be revealed in that period, if one existed. The result of this test was insignificant with the related probability level of 0.535.

Based on the evidence and the unsatisfactory control for unexpected HC earnings, it seems reasonable to conclude that the null hypothesis of no information effect is *not* rejected. However, this conclusion should be carefully interpreted and further research is warranted.

A failure of detecting strong evidence for the information effect is consistent with the conclusions reached by Comiskey et al. (1978), Abdelkhalik and McKeown (1978), Eskew and Ro (1979), Beaver et al. (1980) and Gheyara and Boatsman (1980), although the results are not directly comparable because of the different return data and/or the different testing procedures employed. Comiskey et al. investigated the effect of the same SEC-mandated RC diclosure on security returns using the T^2 procedure applied to monthly return data. The result was that no such effect was detected. Focusing on the Value Line forecasted EPS effect of RC adjustments for estimated realized holding gains,

Abdel-khalik and McKeown examined a relationship between the EPS effect forecasts and the relative risk of firms. They found no clear evidence of such a relationahip. Eskew and Ro examined the impact of the SEC-mandated RC disclosure on both weekly returns and transaction volumes using the multivariate canonical correlation technique. The results did not reveal any evidence for an association between the accounting ratios adjusted for the ASR 190 data and any of the market variables investigated. Using daily returns, Beaver et al. and Gheyara and Boatsman investigated the information conter of ASR 190 and its RC data and found the results consistent with those of the above studies.

4. SUMMARY AND ANALYSIS OF THE RESULTS

This study reports an investigation of whether the SEC-mandated RC disclosure rule had any impact on common equity security returns. Using the matched pair design that has several advantages including a control for the cross-sectional correlations of return data, two distinct effects were investigated: cost effects and information effects.

The findings from the results of both cost and information effect tests can be consolidated as follows: There exists no evidence for the effect of ASR 190 compliance cost on common security returns. There is at best only very weak evidence of the information effect. However, this weak evidence may have resulted due to reasons other than information in the ASR 190 data, in particular the systematic difference in the market's over-estimation of historical cost earnings.

The lack of evidence for the cost effect implies that the actual burden of ASR 190 compliance cost borne by the complying firms for the 1976 fiscal year was not large relative to the value of the firms. As indicated earlier, Garsombke (1978) reports that, on average, the actual dollar costs of complying with ASR 190 were only 0.008% of the cost of sales for the firms included in his study. Although the management of certain complying firms expressed concern over the opportunity costs of complying with ASR 190, our results

suggest these costs were not significant to warrant the investors revaluation of such firms. A similar explanation also seems to apply to potential legal costs associated with the ASR 190 data. Alternatively, the failure to observe evidence of the effect may be attributed to potential limitations of the methodology employed here. Discussions about such potential problems are addressed later in this section.

The lack of strong evidence for the information effect implies that the 1976 RC data disclosed under ASR 190 did not provide new information about the potential impact of changing prices on the firms. Several reasons for this finding may be posted. First, information of the type provided by the ASR 190 data may have been largely impounded in stock prices through alternative sources, possibly when investors assessed the effects of inflation in general (not necessarily the ASR 190 data or events) on firms even prior to the initiation of the new RC disclosure requirement.

Second, it is likely that investors view the ASR 190 RC data as irrelevant to making investment decisions because of numerous measurement problems and the assumption that assets are to be replaced simultaneously (see footnote 5). Alternatively, the data may be irrelevant since the data do not measure the capacity to command financial resources [see Chambers (1969, 1966)].

Finally, the lack of strong evidence for information effects may have been caused by certain methodological limitations of the present study. Examples of such limitations include (a) the size difference of paried firms, (b) a bias in selecting and/or grouping firms, (c) the small sample size, and (d) the incompleteness of Value Line RC data as a proxy for the market's expectations of ASR 190 data.

The first problem, (a), can exist since treatment and control firms systematically differ in size, thus causing a 'size effect' problem [see footnote 11 and Banz (1979)]. Despite the efforts to control for differences in firm profiles in selecting and grouping firms, treatment ant control groups still differ in HC income forecast errors for the $-\Delta EPS_{RC}$ subgroup in table 6. Therefore, various

biases can result [see Foster (1980)]. The sample for the present study consists of only about six to eight percent of the population firms affected by ASR 190.

Regarding problem (d), the Value Line forecasted RC data may be an incomplete proxy for the market's expectations of ASR 190 since the data are based only on realized holding gains, not adjusted for unrealized holding gains. Furthermore, the Value Line data do not reflect the impact of inflation on monetary items of accounting information. Therefore, the data might reflect only a partial impact of changes in specific and general prices on firms.

The results of this study should be carefully interpreted because of its potential methodological limitations outlined above. An improvement of such limitations opens avenues for future research. In addition, at least three potential issues were not addressed in this study and, hence, demand for further research. First, the study on the RC disclosure of a single year (1976). Second, it investigated a change in the mean of the return distribution, leaving unaddressed the question of whether the variance of the return distribution changed due to the RC disclosure. Third, common equity security return is only one of the possible variables reflecting an impact of ASR 190 and its RC data on the capital market. Whether the behavior of any other market variable changed because of the RC disclosure is an open question.

Appendix 1: List of the sample firms

No.	Treatment firms	Paired control-firms
1	AMF	Milton Bradley
2	Abbot Labs.	Far West Financial
3	Akzona	AmpcoPittsburgh
4	Allis-Chalmers	Dentsply International
5	American Bakeries	Peter Paul
6	American Brands	Macandrews & Forbes
7	American Can	Weatherhead
8	American Telephone & Telegraph	Weyenberg Shoe Mfg.
9	Anchor Hocking	Rosario Resources Corp.
10	Armco Steel	Houghton-Mifflin

No.	Treatment firms	Paired control-firms
11	Arvin Industries	Myers, L.E.
12	Baxter Travenol Labs.	Capital Cities Communication
13	Brockway Glass	Lenox
14	Brunswick	Franklin Mint
15	CBS	Cowles Communications
16	Carborundum	American Sterilizer
17	Castle & Cook	Sonesta International Hotels
18	Chromalloy	America International Mining
19	Cincinnati Milacron	Mesta Machine
20	Crown Cork & Seal	Swank, Inc.
21	Dan River	Wayne-Gossard
22	Dart Industries	Gable, Inds.
23	Diamond International	Kroehler Manufacturing
24	Donnelly, R.R. & Sons	Blair, John
25	Evans Products	Electronic Memories & Management
26	FMC	Amtel, Inc.
27	Federal Mogul	Lamson & Sessions
28	Fieldcrest Mills	James. Fred S.
29	Gardner Denver	Watkins-Johnson
30	General Host	Warner Co.
31	General Motors	Murray Ohio Manufacturing
32	Grumman	Foote. Cone & Belding
33	Handy & Harmon	Bayuk Cigars
34	Hanna Mining	United Park City Mines
35	Harsco	Belden Corporation
36	Honeywell	EG&G
37	Insilco	Filtrol Corp.
38	IBM	Leesona
39	International Paper	Overnight Transportation
40	Kaiser Aluminum & Chemical	Woods Corp.
41	Libby-Owens Ford	Basic, Inc
42	Marathon Oil	Crompton & Knowels
43	Martin Marietta	Harcourt. Brace & Jovanovich
44	McDonnell Douglas	Monarch Machine Tool
45	McLouth Steel	Sav-On-Drugs
46	Midland Ross	Portec Ind.
47	NL Inds.	Bliss & Laughlin
48	Nabisco	Industrial National
49	Nalco Chemical	Stone & Webster

No.	Treatment firms	Paired control firms
50	Nashua	Venco Co.
51	National Distillers & Chemical	MEI
52	Northrup	Pittsburgh Forgings
53	Norton	Neptune International
54	Owens-Corning Fiberglass	Great Western Financial
55	Owens-Illinois	Lynch Communications System
56	PPG Industries	Dexter Corp.
57	Pennzoil	Financial Federation
58	Potlatch	Rexham Corp.
59	RCA	Dorr-Oliver
60	Revere Copper & Brass	Hospital Affiliates
61	Robertson, H.H.	Campbell Red Lake Mines
62	Roper	Brown & Sharpe Mfg.
63	Schering-Plough	Household Finance
64	Scovill Manufacturing	Automation Inds.
65	Sherwin-Williams	Northgate Explorations
66	S.W. Forest Inds.	Emery Air Freight
67	Square-D	Thomas & Betts
68	Standard Brands	Arkansas Best
69	Stanley Works	Lionel
70	Stauffer Chemical	Faberge
71	Studebaker Worthington	CTS
72	Sundstand	Cox Broadcasting
73	Tenneco	Unarco Inds.
74	Taxas Instruments	Dial Corp
75	Thiokol	Bandag, Inc.
76	Timken	Sunshine Mining
77	U.S. Inds.	Lehigh Valley Inds.
78	Warnaco	Munsingwear
79	Weyerhaeser	Papercraft
80	Wheelabratory-Frye	McKee. Corp.
81	White Cos. Inds.	United Industrial
82	Wrigly. Wm. Jr.	Mohawk Rubber
83	Zenith Radio	Electronic Assoc.

Appendix 2: Results of matching firms

Pair no.	Treatment firms			Control firms			
	$\hat{\beta}$	1976 10-K release week[a]	ΔEPS_{HC} sign	$\hat{\beta}$	1976 10-K release week[a]	ΔEPS_{HC} sign	Industry membership
1	0.3249	0	−	0.3069	0	−	
2	0.3421	0	−	0.3038	0	−	m
3	0.4052	0	+	0.2273	0	+	x
4	0.4346	0	+	0.4026	0	+	m
5	0.4889	0	−	0.4620	0	−	x
6	0.5236	0	+	0.3606	0	+	x
7	0.5681	0	+	0.5035	0	+	m
8	0.5710	0	+	0.5167	0	+	
9	0.5748	0	+	0.5230	0	+	
10	0.5783	0	+	0.5266	0	+	x
11	0.5808	0	−	0.5638	0	−	m
12	0.5840	0	+	0.5068	0	+	m
13	0.5856	0	+	0.5823	0	+	x
14	0.5918	0	+	0.4842	0	+	
15	0.5957	0	+	0.6064	0	+	
16	0.5980	0	−	0.6073	0	−	
17	0.6125	1(or 0)	−	0.5941	0	−	m
18	0.6120	0	−	0.4733	0	−	x
19	0.6366	0	+	0.6787	0	+	
20	0.6425	0	+	0.6540	0	+	
21	0.6481	1(or 0)	+	0.5820	0	+	m
22	0.6565	0	+	0.6626	0	+	m
23	0.6609	0	+	0.6028	0	+	x
24	0.6622	1(or 0)	−	0.6107	0	−	m
25	0.6661	0	+	0.6373	1	+	m
26	0.6753	0	−	0.6753	0	−	x
27	0.6802	0	+	0.6896	0	+	m
28	0.6879	0	+	0.6964	0	+	
29	0.6958	0	+	0.6532	0	+	x
30	0.6963	0	+	0.7427	0	+	x
31	0.6999	0	+	0.6768	0	+	x
32	0.7044	0	+	0.7122	0	+	m
33	0.7190	1(or 0)	+	0.7374	0	+	x
34	0.7274	0		0.7214		+	
35	0.7338	0	−	0.7484		−	
36	0.7421	0	+	0.7669	0	+	x

Pair no.	Treatment firms			Control firms			
	$\hat{\beta}$	1976 10-K release week[a]	ΔEPS_{HC} sign	$\hat{\beta}$	1976 10-K release week[a]	ΔEPS_{HC} sign	Industry membership[b]
37	0.7497	0	+	0.7660		+	
38	0.7588	0	+	0.7873		+	
39	0.7664	0		0.7762		+	x
40	0.7756	0	+	0.7658	0	+	x
41	0.7911	0	+	0.7280	0	+	x
42	0.7923	0	+	0.8301	0	+	x
43	0.8010	0	+	0.7335	0	+	x
44	0.8108	0	+	0.8695	1(or 0)	+	x
45	0.8151	0	+	0.8168	0	+	m
46	0.8204	0	+	0.9022	0	+	x
47	0.8246	1	+	0.8367	1	+	x
48	0.8544	0	+	0.9449	0	+	x
49	0.8654	0	+	0.8676	?	+	
50	0.8755	0	+	0.8763	0	+	
51	0.8774	0	+	0.8914	0	+	x
52	0.9060	0	+	0.8683	0	+	x
53	0.9289	0	+	0.9497	0	+	
54	0.9288	0	+	0.8603	0	+	x
55	0.9479	0	+	1.0005	0	+	x
56	0.9862	0	+	0.9924	0	+	x
57	1.0178	0	−	0.8741	0	−	x
58	1.0350	0	+	1.0363	0	+	
59	1.0397	0	+	1.0491	0	+	x
60	1.0415	0	+	0.9324	?	+	x
61	1.0598	0	+	1.0669	0	+	
62	1.0681	0	+	1.0628	0	+	x
63	1.0895	0	+	1.0767	0	+	x
64	1.0947	0	+	1.0996	0	+	x
65	1.2018	0	+	1.2291	0	+	x
66	1.2188	0	+	1.2015	0	+	
67	1.2954	0	+	1.2953	0	+	
68	1.2963	0	+	1.3078	0	+	
69	1.3104	0	+	1.3150	1(or 0)	+	
70	1.3139	0	+	1.3408	0	+	
71	1.3221	0	−	1.3299	0	−	
72	1.3364	0	+	1.3185	0	+	x

Pair no.	Treatment firms			Control firms			Industry membership
	$\hat{\beta}$	1976 10-K release week[a]	ΔEPS_{HC} sign	$\hat{\beta}$	1976 10-K release week[a]	ΔEPS_{HC} sign	
73	1.3482	0	+	1.3996	0	+	x
74	1.3725	0	+	1.2723	0	+	x
75	1.3720	0	+	1.3666	0	+	m
76	1.4122	4(or 3)	+	1.6507	3	+	
77	1.4151	1	−	1.6141	0	−	x
78	1.4530	0	+	1.4136	0	+	x
79	1.4769	0	+	1.3068	0	+	x
80	1.5253	0	+	1.4770	0	+	
81	1.5425	0	+	1.5590	0	+	m
82	1.5809	0	+	1.5570	0	+	x
83	1.9081	0	+	1.9240	1(or 0)	+	x

[a]Week 0: March 27—April 2, 1977. Week 1: March 20—March 26, 1977. Week 2: March 13—March 19, 1977. Week 3: March 6—March 12, 1977. Week 4: February 27—March 5, 1977. Week '1 (or 0)' means that the 10-K report was released on Friday of week 0 but that the case is treated as week 1 for purposes of matching firms here. The week '4 (or 3)' is similarly interpreted. The question mark means the 10-K release date was not indicated or clear.

[b]Symbol 'x' stands for the successful matching of firms by industry: 'm' means the firms are paired across different branches of manufacturing industry. For details see appendix 3.

Appendix 3: Result of pairing firms by industry

Industry	No. of pairs
Mining	1
Manufacturing	
Foods and beverages	3
Textile	3
Forest and paper products	3
Publishing and printing	1
Chemicals, drugs and cosmetics	3
Containers—metal and glass	2
Steel, copper, aluminum and metal fabricating	2
Machinery, equipment and tools	17
Transportation vehicles, equipment and parts	2
Toys and leisure time goods	2
All other manufacturing branch industries	15[a]
Communications	1
Wholesale and retail stores	1

Impossible-to-pair firms by industry	27[b]
Total	83

[a]This number is equivalent to the number of 'm' marked in the industry membership column in appendix 2.

[a]This number is equivalent to the number of blanks left in the in the industry membership column in appendix 2.

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