The Response of Insider Trading to Changes in Regulatory Standards

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Abstract

This study reconsiders the question addressed in Jaffe [10]: Does the trading behavior of insiders respond to changes in regulatory standards? Contrary to Jaffe's conclusion, this study finds that insiders adjusted their willingness to exploit negative, nonpublic information concurrent with major events in the Texas Gulf Sulfur (TGS) litigation. The difference in findings is attributable to two improvements upon Jaffe's study: the appropriate specification of the effect of the District Court's opinion in the TGS case on the profitability of insider trading and the separate analysis of the unexpected returns associated with purchase and sales transactions.

Introduction

Reports of the misuse of inside information by securities traders have increased dramatically in recent years. In response to these allegations, Congress and the Securities and Exchange Commission (SEC) enacted several changes to the Securities Laws and the procedures used to enforce them. Although a major objective of these changes was to reduce or eliminate the exploitation of the investing public by insiders, to date there is little empirical support for the proposition that insiders alter their trading behavior in response to changes in the regulatory environment. In the only detailed study of the effects of changes in regulatory standards on the trading behavior of corporate insiders, Jaffe [10] finds that neither the indictment nor the federal District Court's opinion in the landmark *SEC v. Texas Gulf Sulfur and Company* (TGS)[1] case affected the

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[1]The *Securities Acts of 1933* and the *Securities Exchange Act of 1934* provide the general legal framework governing the trading activity of individuals possessing inside information. The specific standards, however, were promulgated judicially in a series of actions brought by the Securities and Exchange Commission (SEC) in the middle and late 1960s. Most significant among these was *SEC v. Texas Gulf Sulfur and Company* (TGS). The legal principles established in this case continue to be guidelines for securities transactions by corporate officials.

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trading profits earned by insiders. The appropriateness of Jaffe's research design and, consequently, the validity of his inferences, however, are open to question.

This paper reports the results of a reexamination of the effects of the TGS litigation on the trading profits earned by corporate insiders. After incorporating several modifications to Jaffe's [10] research design, this study finds that an association did exist between specific events in the Texas Gulf Sulfur litigation and changes in the profitability of sales transactions by insiders. This association is consistent with the hypothesis that corporate insiders altered their trading behavior as the courts shifted the stringency of the material fact standard governing the significance of the nonpublic information required to be disclosed prior to corporate officials being able to trade legally in their firm's shares.

Jaffe's Study of *SEC v. Texas Gulf Sulfur*

The influence of one particular set of investors, corporate officials and beneficial owners (insiders),[2] on the securities markets is of particular concern to regulators and legal and financial researchers. This concern appears to arise from a belief that investor confidence in the fairness and integrity of these markets is reduced if corporate officials are not constrained from exploiting nonpublic information about their firms [22].

Beginning in the early 1960s, the SEC initiated a program of rigorous enforcement of the antifraud provisions of the *Securities Acts of 1933* and the *Securities Exchange Act of 1934*. A major goal of this program was to reduce, if not totally eliminate, speculative trading by corporate insiders [13]. Jaffe [10] investigates the effects on insider trading behavior of three important events in this program: the SEC's administrative ruling *In The Matter of Cady, Roberts and Co.*, the TGS indictment, and the Trial Court's opinion. Based on a series of pre- and postevent comparisons, he concludes that his results "do not suggest that regulatory changes had an effect on the profitability of insider trading" [10, p. 109].[3] Although Jaffe's results have been neither replicated nor extended, they

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[2]The term *corporate insiders* is used to distinguish this subgroup from the more common classification of insiders that includes any and all market agents, brokers, analysts, newspaper reporters, etc who come into possession of valuable nonpublic information in the normal course of their activities.

[3]Jaffe [10] also investigated the effects of changes in the regulatory environment on the volume of shares transacted by insiders. Because the linkage between exploitation of private information and volume of shares traded is tenuous, this dimension was not investigated in this study.

have been cited frequently as evidence that changes in regulatory standards do not affect the trading behavior of insiders [4, 21].

Jaffe's derives his research design from two critical, but perhaps invalid, assumptions. First, he assumes that each of the three regulatory events examined in his study had, if anything, a "deterrent effect on the profitability of insider trades" [10, p. 108]. Second, he assumes that events subsequent to the TGS Trial Court's decision "changed little the results reached in the first (Trial Court) case" [10, p. 119]. Many legal scholars argue that these assumptions may be invalid [2, 12, 13, 18, 22].

An Overview of the TGS Case: Enforcement Issues at Question and Sequence of Critical Events

There is a consensus among legal scholars that prior to 1961 the SEC was not actively enforcing the antifraud provisions of the Securities Laws against insider trading. Former SEC Commissioner Roberta Karmel [13] contends that the SEC's reluctance to aggressively prosecute illegal insider trading was largely philosophical. Beginning with its administrative ruling in November 1961 *In The Matter of Cady, Roberts and Co.* (enforcement event 1 in Figure 1), the SEC actively began to seek out and prosecute illegal trading by insiders. Because this ruling signalled a major turning point in the SEC's enforcement philosophy, it is not surprising that Jaffe hypothesizes that it had a deterrent effect on insider trading.

In their widely used textbook on securities regulation, Jennings and Marsh [12] argue that a second reason for the lack of enforcement activity was that the applicability of the Securities Laws to insider trading was an unsettled legal question. They identify two specific issues that they believe affected the ability of both the SEC and insiders to determine if and when trading activity was illegal:

> Enforcement Issue I: Did the provisions of *Securities Acts of 1933*, *Securities Exchange Act of 1934*, and Rule 10b-5 promulgated under them apply to open market (i.e., not face-to-face) transactions by insiders?[4]

[4]Section 10 of the Securities Exchange Act of 1934 and Rule 10b-5 promulgated under it make it illegal for any person to "omit to state a material fact necessary in order to make the statements made, in light of the circumstances under which they were made, not misleading" or to "engage in any act, practice . . . which operates or would operate as a fraud or deceit upon any person in connection with the purchase or sale of any security."

Enforcement Issue II: How important, i.e., material, did the facts known by an insider have to be before he or she was required to disclose or refrain from trading?

In its *Cady, Roberts* ruling, the SEC addressed each of these issues. The SEC held first that Rule 10b-5 was applicable to all transactions by insiders. Second, the SEC imposed a liberal material fact standard by ruling that virtually all information that came into possession of an insider during the course of his or her duties was material and thus subject to a duty to disclose or refrain from trading. Because *Cady, Roberts* was an administrative ruling, however, the SEC's position on each of these issues lacked the force of law.

In April 1965 the SEC filed suit against several key officials of the Texas Gulf Sulfur Company (TGS) alleging that they had exploited nonpublic information to purchase shares in TGS.[5] The TGS indictment (enforcement event 2) was the first of a series of actions in which the SEC sought to have the substance of its *Cady, Roberts* ruling affirmed by the federal courts. Initially, the SEC was only partially successful in achieving this objective. The opinion issued by the United States District Court for the Southern District of New York (the Trial Court) in August 1966 (enforcement event 3 in Figure 1) upheld the SEC's position on trading by insiders over organized exchanges. To the disappointment of the SEC, however, the Trial Court also held that:

> the test of materiality must necessarily be a conservative one, particularly since actions under Section 10b are brought on the basis of hindsight [23].

The SEC immediately appealed this ruling. In August 1968 the United States Court of Appeals for the Second Circuit (the Appellate Court) issued what many legal scholars have termed a landmark decision [13, 22] (enforcement event 4 in Figure 1). In what Painter termed the "heart of majority opinion" [18, p. 209], the Appellate Court adopted a position unequivocally supporting the

[5]In *SEC v. Texas Gulf Sulfur*, it was alleged that officials of the TGS purchased shares in TGS on the basis of undisclosed information about the discovery of silver and copper deposits in Canada. Although the first signs of this discovery were known to company officials as early as mid-1959, TGS did not disclose its activities in the area publicly until April 12, 1964 and did not disclose publicly that it had made a major ore discovery until April 16, 1964, four days later. Although officials of TGS and other tippees purchased shares in TGS throughout the early 1960s, the Trial Court held that only purchases made in April 1964 violated the antifraud provisions (Rule 10b-5) of the 1933 and 1934 acts.

SEC's liberal material fact standard. In April 1969 the Supreme Court declined to review the Appellate Court's opinion (enforcement event 5), effectively establishing the Appellate Court's standard as legally binding.

Specification of the TGS Trial Court Litigation Effect Hypotheses

Jaffe [10] considers the association between insider trading profits and enforcement events 1 through 3. Either taken individually or cumulatively, he is unable to reject the statistical hypothesis that the profitability of insider trading did not decline[6] coincident with these events. Perhaps because he does not identify explicit enforcement issues, Jaffe's research design does not incorporate the fact that the material fact standard established by the TGS Trial Court differed substantially, both in terms of stringency and potential consequence, from that established in earlier enforcement events.

Many legal scholars argue that the establishment of a material fact standard for undisclosed information was the most important question at issue in the TGS litigation [2, 12, 13, 18, 22]. Because the stringency of this standard presumably affects judgments by insiders of the likelihood that a transaction will violate Rule 10b-5, it is possible that the Trial Court's conservative material fact finding reduced the deterrent effect of the Securities Laws.[7] Assuming that this conjecture is valid, even if some elements of the Trial Court's opinion had a deterrent effect, these effects may have been offset or overwhelmed by the effects of the court's material fact finding.[8] Consequently, the first litigation effect hypothesis investigated in this study is:

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[6]Jaffe [10] does not develop a clearly articulated alternative research hypothesis. The use of a one tailed significance level, as reported in his Table 3 (pg. 107), clearly indicates, however, that the alternative statistical hypothesis of interest is that the post-TGS litigation event period was associated with smaller average standardized residuals.

[7]Jaffe's [10] results show that the average eight month profitability of insider trading more than doubled from the period immediately before the *Cady, Roberts* ruling, event 1, to the period immediately following the Trial Court's opinion, event 3, 3.8 percent versus 8.4 percent.

[8]Based on a review of insider trading litigation prior to the *Cady, Roberts* decision, Jennings and Marsh conclude that little uncertainty existed about enforcement issue 1, the applicability of the securities laws to open market transactions by insiders. If this conclusion is valid, then Jaffe's tests of the effects of the Trial Court's opinion on insider trading activity reduces to a test of whether the opinion had an incremental impact on perceptions of the willingness of the SEC to seek and prosecute illegal trading activity by insiders.

H1: The Trial Court's finding increased the willingness of insiders to exploit proprietary information for trading purposes.

Specification of the TGS Appellate Court Litigation Effect Hypotheses

Jaffe [10] chose not to investigate the effects of enforcement event 4, the Court of Appeals decision, and enforcement event 5, the refusal by the Supreme Court to review it. As noted earlier, he explicitly argues that these events did not affect the regulatory environment established by the Trial Court's decision. Most legal scholars disagree with this presumption and consider the Appellate Court's decision to be the critical enforcement event [2, 4, 12, 13, 18, 22]. For example, in his treatise on insider trading, Painter argues that this "decision must be considered a major victory for the Commission for it (the conservative material fact standard) was perhaps the single aspect of the District Court's decision which was most disappointing (to the SEC) . . . " [18, p. 210]. Accordingly, Jaffe's inability to detect a significant deterrent effect may be attributable to his decision not to investigate these later events. Given the acknowledged importance of these events, this study investigates a second TGS litigation effect hypothesis:

H2: The Court of Appeals TGS opinion decreased the willingness of insiders to exploit proprietary corporate information for trading purposes.

Effects of Changes in the Material Fact Standard on the Profitability of Trading

Both TGS litigation effect hypotheses predict that a change in the material fact standard affects the willingness of insiders to trade in their firm's securities. Neither hypothesis, however, specifies how these changes affect the observed profitability of their transactions.[9]

The willingness of an insider to trade speculatively in his or her firm's securities is, presumably, an increasing function of the expected net trading profits. In an efficient capital market, trading profits will be realized only if and when the private information underlying the trade is revealed credibly to the

[9]Perhaps because he recognized the complexity of this issue, Jaffe [10] is careful not to state explicitly in his text how a deterrent effect will be evidenced.

market.[10] Thus, in order to undertake a speculative trading strategy successfully, the insider also must commit himself or herself to the disclosure of his or her private information. This means that, like a bank robbery, the act of illegal trading leaves a major clue that a crime has been committed. Given the ease with which corporate disclosure events can be detected, the SEC's antifraud enforcement problem is simplified. To prove that a violation of Rule 10b-5 has occurred, the SEC need establish only an *ex post* linkage between the event and specific transactions by the corporate officials.

For a given value of undisclosed information and expected gross trading profit, changes in the regulatory environment affect the trading behavior of insiders by altering the expected costs, p(K), of trading. The costs, K, include, at a minimum, the cost of defense should a violation of Rule 10b-5 be alleged and the penalties incurred if a violation is proved. These costs were not at issue, however, in the TGS litigation. Nonetheless, it is important to note that during the 1960s, the Securities Laws required that any trading profits earned by an insider from illegally trading in his or her firm's stock be returned to the firm.[11] Hence, the actual out-of-pocket costs of a violation of Rule 10b-5 never were less than the actual gross profit from the trade.

The impact, if any, of TGS is that it altered the probability, p, of enforcement action by the SEC. The material fact standard affects this probability in at least two ways. First, in the absence of outright prohibitions on insider trading, it defines the conditions under which trading is illegal or legal. There is, therefore, a point beyond which the market value of the nonpublic information and the prominence of the required public disclosure insures that the prior probability of enforcement action is equal to 1. This point is determined subjectively, however, and a substantial amount of ambiguity is likely to exist about its location. Short of this point, the prevailing material fact standard interacts with the enforcement philosophy of the SEC to determine for any given level of undisclosed information the prior probability of enforcement activity. Presumably, the more liberal the standard, the more likely it is that an observed information event will trigger an investigation by the SEC

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[10]The question here is whether the actual facts must be disclosed in order to obtain the required security price revision. Arguably, an insider may attempt to push the information content but not the actual facts into the public domain. In the moderate to long run, however, it seems unreasonable to assume that a corporate insider can suppress or disguise his or her firm's major, and potentially most economically valuable, information events.

[11]In the *Insider Trading Sanctions Act of 1984*, the penalties for violating Rule 10b-5 were changed to include a maximum damage penalty of three times the gross trading profit.

and the less difficult (and less costly) it is for the SEC to prove that a specific trade was illegal.

It generally is agreed [2, 4] that the likelihood of enforcement activity is an increasing function of value of nonpublic information underlying the trade, V.[12] For simplicity of presentation, assume that this relationship is described by the following piece-wise linear function:

$p = bV$ if $V < S$,
$p = 1$ if $V > S$

where:

p = the prior probability of enforcement action;
V = the value of the nonpublic information known to the insider;
b = an arbitrary constant defined so that p=1 if V=S and p=0 if V=0;
S = the material fact standard, measured in terms of the market value of undisclosed information, assumed to be the subjective point beyond which the probability of enforcement activity by the SEC is equal to 1.

Because both the costs of violation, K, and the probability of enforcement are increasing functions of the value of nonpublic information, the expected costs of violation, p(K), will be nonlinear in V. This relationship is depicted as curve ACT in Figure 2. This implies that an insider's trading opportunity set will lie between points A and C, the intersections of the expected gross trading profit line AC'DC and the expected cost curve ACT.

Given these assumptions, a shift in the material fact standard from conservative to liberal (S to S' in Figure 2) will cause the expected cost curve to rotate up and to the left, curve AC'T', and shift the expected zero profit point from C to C'. This shift in breakeven points, in turn, causes the insider's total trading opportunity set, as measured by gross trading profits, to narrow from AC to AC'. Thus, to the extent that an insider is willing to exploit a trading

[12]If the probability of enforcement is assumed to be independent of the expected gross trading profits, then a reduction in the costs of detecting insider transactions, holding both the material fact standard and the costs of legal defense constant, should result in insiders foregoing previously profitable low gross profit trading opportunities. Interestingly, this implies that average gross trading profits could increase subsequent to regulatory changes that had a deterrent effect on insider trading activity. The possibility of this condition arising was pointed out by a reviewer of an earlier version of this paper. This effect should be most pronounced when the sample of insider trades is relatively broad.

opportunity in the range C' to C given a conservative standard, all other things held constant, a shift to a liberal standard will cause the average gross profitability of the observable trading activity to fall.

As discussed earlier, the objective of the SEC's enforcement program was to make insiders adhere to the disclose-or-refrain-from-trading rule. If the TGS litigation achieved this objective, then it follows that any and all transactions observed subsequent to TGS were executed only when insiders did not possess proprietary information. Consequently, in a market in which prices rapidly adjust to reflect new information, insider trading activity in the post-TGS period would not be associated with abnormal trading profits.[13] Existing empirical evidence suggests that this extreme condition was not obtained, however, as significant abnormal returns have been detected as recently as 1978-1982 [8, 25].

In contrast to the perfect adherence hypothesis, Carlton and Fischel [2] and Easterbrook [4] both argue that it is unlikely that the TGS litigation had any effect on the observable trading activity of insiders. This conclusion follows from their supposition that the critical element in deterring speculative trading is the ability of the SEC to identify and prosecute illegal trading activity. In practice, given the enforcement technologies available to the SEC during the 1960s, this meant that only prominent, easily identifiable, i.e., bombshell [2, p. 866], public information events were scrutinized by the SEC for prior illegal trading activity. Consequently, if an insider was circumspect and willing to accept small trading profits, then it was unlikely that a violation of Rule 10b-5 would be alleged, much less proved.

Consistent with this line of argument, existing empirical evidence suggests that corporate insiders rarely trade in anticipation of major public information events such as earnings, dividend, and merger announcements [5, 8] and that corporate insiders restrict themselves to relatively small profit trading opportunities [7, 8, 25]. This evidence suggests that the subjective opportunity set for most insiders is much narrower (say between points A to D in Figure 2) than the total opportunity set, AC. If the subjective trading opportunity set of insiders is extremely narrow, then, on average, observable trading insider gross trading profits would not have been affected by a shift from a liberal to a conservative material fact standard.

Between the extremes of the perfect adherence and no effects conditions, the empirical consequences of the TGS litigation are problematic.[14] It is clear that

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[13]Although Jaffe [10] is vague about how changes in the regulatory environment affect the levels of profitability associated with reported insider transactions, his deterrent effect hypothesis and statistical tests appear to be conditioned on this scenario.

[14]The author appreciates the comments of a reviewer of an earlier version of this paper on this issue.

the TGS litigation affected the legal environment in which insiders make their trading decisions. The empirical question is whether, on average, the range of trading opportunity exploited by insiders prior to TGS was sufficiently large to have been affected significantly by shifts in the material fact standard.[15] Although it appears that this range is relatively narrow, it is also likely that the rotation of the expected cost line caused by the liberal material fact standard was large. Accordingly, conditional on the descriptive validity of the assumptions underlying Figure 2, the following alternative litigation effect hypotheses are posited.

A1: The Trial Court's finding imposing a conservative material fact standard was associated with an increase in the average profitability of insider trading.

A2: The Court of Appeals TGS opinion imposing a liberal material fact standard was associated with a decrease in the average profitability of insider trading.

Research Design, Variable Selection, and Event Classification

This study tests the litigation effect hypotheses by comparing levels of insider trading profitability before and after critical events in the TGS litigation. In this respect, the research design parallels that used in Jaffe [10]. The methodology and statistical procedures have been updated to reflect the results of recent empirical research into the relationship between abnormal returns and insider trading events.

Insider trading data were obtained from a computer readable condensation of the *Official Summary of Security Transactions and Holdings*. This document is published monthly by the SEC and summarizes the transactions reported under the provisions of the Securities Act of 1934. This data base, previously used in Finnerty [7], resulted in a more diverse sample of insider transactions than Jaffe [10, 11]. The number of insiders who transacted, the number of shares they

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[15]It is also important to recognize that individual judgments about the true location of the material fact standard and individual risk tolerances also play an important role. For example, it is possible that all insiders perceived that the expected cost curve shifted, but that insiders with low risk tolerance ceased trading and high risk tolerance insiders did not conclude that their opportunity set had shifted. Consequently, observed levels of gross trading profits may have increased. The author appreciates the comments of the reviewer on this issue.

traded, and the type of transaction in which they engaged (open market purchase or sale) were collected for 479[16] New York Stock Exchange firms for each month in each of four different calendar periods. These periods were September 1963 to March 1965 (pre-TGS period 1), May 1963 to March 1965 (pre-TGS period 2), September 1966 to March 1968 (the post-Trial Court period), and May 1969 to March 1971 (the post-Appellate Court period).

In his study, Jaffe [10] chose to disregard the type of transaction in which the insider engaged. His decision to treat purchase and sales transactions as equivalent events reflects an assumption that changes in the legal environment would affect them identically. There is neither a theoretical nor an empirical justification for this presumption. Ronen [20], for example, argues that both the incentives to generate and exploit nonpublic information and the institutional constraints governing trading by insiders, e.g., the prohibition of short selling by insiders, differ markedly between good and bad nonpublic information. Consequently, the decision process leading to a purchase transaction differs from that leading to a sales transaction. Consistent with Ronen's hypothesis, Finnerty [7] finds that both the timing and the levels of abnormal returns differ between the two types of trading event. In this study, purchase and sales transactions are considered both individually and jointly (aggregate trading).

Because he finds that significant abnormal returns only occur subsequent to intensive trading events, Jaffe [10] only uses this subsample of trading events in his tests. By construction, this procedure results in a sample of firms that experienced extraordinary amounts of trading activity within a calendar month. To the extent that extraordinary numbers of transactions better reflect the possession of special information by a firm's insiders, measurement error in identifying speculative trading events is reduced. Furthermore, if changes in the regulatory environment mainly affect the exploitation of bombshell information [2], this approach also increases the power of the statistical tests.

Recent research [7, 21] indicates that significant unexpected returns are associated with a broader set of trading events. In order to determine if the effects of changes in the material fact standard were widespread or restricted to extraordinary trading events, two transaction identification procedures are used in this study. The first, the low intensity rule, classifies a firm as purchase, sales, or no trade in accordance with the net number of shares traded by the insiders of that firm during a calendar month. The second is a modification of Jaffe's

[16]To be included in the sample, a firm was required to have experienced at least one insider transaction in the period January 1960 to November 1971. The resulting sample of NYSE firms then was narrowed to 479 firms by the requirement that each firm have return data on the CRSP monthly tape from January 1958 to December 1972.

intensive trading rule. A firm is classified as experiencing a high intensity purchase (sales) event in event month t if the number of separate purchase (sales) transactions exceeds the number of sales (purchase) transactions by at least three in that calendar month.

Insider trading profitability is measured by the unexpected returns to a portfolio of firms experiencing similar trading events in the same event month. The unexpected portfolio return for month t+i is the equally weighted sum of the market model prediction errors i months subsequent to the month of portfolio formation for each firm, j, in the portfolio formed in month t.[17] The market model prediction error for firm j, i months after portfolio formation is calculated as:

$$MPE_{j,t+i} = R_{j,t+i} - [a_j + b_j R_{m,t+i}]$$

where:

$MPE_{j,t+i}$ = the market model prediction error (unexpected return) for firm j at i months subsequent to portfolio formation month t

$R_{m,t+i}$ = the return of CRSP NYSE equally weighted index in month t+i

a_j, b_j = the firm-specific parameters of the market model estimated over the sixty months immediately prior to portfolio formation month t

Statistical Methodology

In his study Jaffe [10] uses a two sample t-statistic to test the equality of abnormal returns before and after TGS enforcement events. This methodology has two potential weaknesses. First, it is difficult to specify precisely how changes in the regulatory environment will affect the levels and timing of trading profits. In particular, although it is hypothesized that trading profits were reduced by the imposition of a liberal material fact standard, it is not clear whether these profits will be affected in the short or long term.[18] This issue is

[17]This contrasts with the two factor Fama and MacBeth [6] estimation procedure used by Jaffe [10].

[18]In addition to the possibility that the level of trading profits was affected by the TGS litigation, it also can be conjectured that insiders adjusted to alterations in the legal environment by changing the patterns of corporate information releases rather than by foregoing profitable trading opportunities. For example, the materiality of the facts known by the insider could be masked from the SEC by releasing the information in small bits over a relatively long period. Given an assumption of informationally efficient markets, if the timing

particularly important, as the results of recent research show that the abnormal returns associated with insider transaction persist for at least one year subsequent to the date of trade [1, 7, 8, 25]. Accordingly, this study uses a vector of abnormal returns as its dependent variable and compares patterns, as well as totals, of unexpected returns over the 12 months subsequent to insider trading events.

The second potential problem is that recent research [1, 7, 25] on the empirical behavior of the abnormal returns associated with insider trading events suggests that the distribution of abnormal returns subsequent to these events is not independent and identical. Consequently, a univariate statistical test using abnormal returns summed over a period of time as a dependent variable may result in a weak test of the litigation effect hypotheses. This problem is elaborated in Appendix A.

Given the need to compare both patterns and totals of insider trading profits, this study employs a two sample Hotelling's T^2 statistic in its tests. Formally, this procedure tests the null hypothesis that the 12 element vector of mean month-by-month unexpected returns is equal prior to and subsequent to changes in the regulatory environment. Rejection of this hypothesis indicates that at least one linear combination of the monthly mean unexpected returns differs between the sample periods. Included in these linear combinations are both the case of only the first month following portfolio formation being important (Jaffe's [10] first case X=0) and the case of an equally weighted linear combination across any sequence of holding periods being important (Jaffe's second X=1 case and third X=7 case). Moreover, because this statistic uses the entire estimated variance-covariance matrix, the statistical properties of the T^2 test are not affected by either differences in the distribution of abnormal returns across holding periods or by serial dependence in abnormal returns within the 12 month observation period.

Effects of the District Court's Decision

The first hypothesis investigated was A1 (the announcement of a conservative material fact standard by the District Court in August 1966 was followed by an increase in profitability of insider trading). The postlitigation period in these tests was September 1966 through March 1968 (the Trial Court period). The pre-TGS base line period was the 19 month identical calendar month period occurring between the announcement of the *Cady Roberts* decision

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of corporate disclosures consciously was altered, then the timing of the impoundment of this information in security prices also would be shifted, although the cumulative level of insider trading profitability would not change.

and the TGS indictment, September 1963 through March 1965 (pre-TGS litigation period 1).

Table 1 summarizes the results for both the high intensity trading (Panel A), and low intensity trading (Panel B) samples. Each panel is organized by type of insider transaction portfolio: aggregate trading, purchase transactions only, and sales transactions only.

Because the high intensity/aggregate transaction sample selection criterion used in this study most closely replicates the sample selection criterion employed by Jaffe [10], the results for this subsample are presented first. These results are consistent with those reported by Jaffe. The nine month cumulative average unexpected returns (CURs) for this sample are somewhat larger than the eight month cumulative average returns reported by Jaffe: 3.8 percent for Jaffe's sample versus 6.7 percent during the pre-TGS period 1 and 8.4 percent for Jaffe's sample versus 12 percent during the post-Trial Court period. It is possible that these differences may be attributable to differences in sample size (278 pre-event and 112 postevent versus 286 and 582, respectively, in this study) and observation periods (January 1964 through March 1965 pre-event and September 1966 through March 1967 postevent versus September 1963 through March 1968, respectively, in this study).

To provide a comparison between Jaffe's results and those reported in this paper, a univariate, two sample t-test of the 12 month CURs for the high intensity/aggregate transaction sample was conducted. Based on his tests, Jaffe concluded that the Trial Court's TGS decision did not reduce the profitability of insider trading. The current results are consistent with the conclusion that insider profitability increased subsequent to this event. As shown in the right column of Table 1, the 12 month cumulative trading profits were significantly larger subsequent to the issuance of the Trial Court's opinion.[19] Visual comparison of the pre- and post-Trial Court period CURs at varying intervals subsequent to the trading event suggests further that the postevent CURs were uniformly greater and that the difference in CURs increased as the holding period increased. It was therefore surprising that the Hotelling's T^2 two sample F-statistic was too small to reject the hypothesis of no period-to-period difference in levels and/or timing of unexpected returns ($F = 1.72$).[20]

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[19]As pointed out earlier, the appropriateness of this test is open to question. Moreover, as Jaffe notes [10, p. 108], his standardization procedures may cause his statistical results to differ from the simple two sample test of mean CURs reported in this table.

[20]It should be noted that the two sample t-test results are particularly sensitive to the problem of cross-sectional dependence in returns, causing the estimated standard deviations to be understated. In contrast, both Jaffe's

Segregation of the high intensity sample by the type of transactions, purchase or sale, affected the nature of the results.[21] In either instance, significant period-to-period differences in the levels and/or timing of unexpected returns were found, using both the multivariate or univariate test statistics. Consistent with A1, the profitability of sales transactions increased from 5 percent in the prelitigation period to 20 percent in the post-Trial Court period.[22] Contrary to A1, however, the average profitability of high intensity purchase events was significantly lower in the post-Trial Court period.

The low intensity trading event sample tests (Panel B) were consistent with those of the high intensity sample both in terms of significance and pattern. The post-Trial Court period was characterized by higher profitability of sales transactions and lower profitability of purchase transactions. Thus, contrary to Carlton and Fischel's [2] bombshell conjecture, the differences in levels and/or timing of unexpected returns do not appear to have been restricted to extraordinary trading events.

The results of the TGS Trial Court tests are puzzling. Although it was anticipated that purchase and sales activity might be differentially affected by this decision, the finding that the profitability of one type of transaction increased while the other decreased was not anticipated. Consequently, a set of additional tests based on an alternative metric of unexpected insider trading profits was conducted. Before presenting these tests and their results, it is useful to report and discuss the results of the investigation of the Court of Appeals decision using the CUR metric.

Post-Appeals Court Results

The Appellate Court litigation effect hypothesis was tested by comparing the pattern of trading profits during the period immediately following the

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standardized return procedure and the Hotelling's T^2 procedure are alternative ways to reduce the effects of this dependence on the validity of the test statistics.

[21]The number of portfolios used in the tests is less than the maximum possible 19 due to a requirement that a portfolio contain no fewer than three firms. This requirement was imposed to insure some diversification within portfolios.

[22]It should be noted that the results show that insider trading events were no less prevalent in the post-Trial Court period. If anything, frequency of sales events increased substantially. Moreover, this trend appears to be about the same for either the high and low intensity sample. This evidence is inconsistent with the conjecture that average trading profits increased because tolerant low risk, low profit traders were deterred by the Trial Court's decision, but high risk, high profit traders were not.

Supreme Court's TGS decision (enforcement event 5) to that in the comparable calendar month period immediately prior to the filing of the TGS suit (enforcement event 2). The postevent period used was May 1969 through March 1971, and the comparable pre-event period was May 1963 through March 1965. This resulted in a maximum of 23 observations (portfolios) in both the pre- and postintervention periods.

Table 2 summarizes the results for both high intensity trading events (Panel A) and low intensity trading events (Panel B). Only the results for the purchase and sales portfolio transaction samples are reported. Consistent with A2, the low intensity/sales event sample exhibited a statistically significant period-to-period decrease in the profitability of insider sales transactions coincident with the culmination of the TGS litigation. Although the trend in levels of unexpected returns for high intensity sales events was also consistent with A2, neither the Hotelling's T^2 F-statistic nor the two sample t-statistic were large enough to reject the hypothesis of no period-to-period difference. The null hypothesis of no period-to-period difference in unexpected returns could not be rejected for either high or low intensity purchase events.

The Material Fact Reversal Effect

Taken together, the results reported in Tables 1 and 2 are consistent with the inference that the Trial Court's decision affected the trading behavior of insiders, but that this effect was either reversed or did not persist in the Appellate Court period. In order to provide direct support for this conclusion, two additional sets of analyses were conducted. The first analysis provides an alternative test of the hypothesis that insiders altered their trading behavior in response to shifts in the material fact standard. The second analysis was conducted to ensure that the effects identified in the research were not attributable to omitted time period-specific factors.

As discussed earlier, predicting the direction of the effect of a change in the material fact standard on observed trading profits is problematic. Nonetheless, consistency of effect requires that the direction of the effect associated with a change to a conservative be different from the effect of a change to a liberal material fact standard. Moreover, a reversal of effects should be observed if the stringency of the material fact standard was the critical enforcement issue at question in TGS. Accordingly, an alternative and, perhaps stronger, test of the hypothesized effects of changes in the material fact standard on insider trading behavior can be achieved by testing the hypothesis that the Appellate Court's ruling reversed the effects of the Trial Court's decisions [14]. This hypothesis was tested by comparing vectors of unexpected returns in the post-Trial Court to

those in the post-Appellate Court period. The results of these tests, both univariate and multivariate, are reported in Panel A of Table 3.

For either trading portfolio formation rule, low or high intensity, the results show that the levels and/or timing of the insider trading profits associated with either purchase or sales events changed significantly concurrent with the Appellate Court's decision to reimpose a liberal material fact standard. Contrary to the implication of the litigation effect hypothesis, however, the average profitability of purchase transaction samples exhibited a period-to-period increase. Except for the high intensity purchase sample, the results of two sample t-tests of 12 month cumulative returns paralleled the multivariate results.

Time Period-Specific Effects

Because the Trial Court ruled in favor of a conservative material fact standard and the Appellate Court ruled in favor of a liberal standard, it was surprising to find that the profitability of purchase transactions respectively decreased and increased subsequent to these decisions. Additional tests were conducted to determine if this result was spurious. In these tests, attention was given to the possibility that time period-specific factors systematically had affected the forecast accuracy of the market model.

Research by Pettit and Westerfield [19] suggests that the predictive accuracy of the market model varies across calendar periods. Moreover, it is widely known that the market model does not capture all time period-specific systematic effects on security returns [15]. To improve the validity of the research, a new measure of the profitability of insider trading events, net unexpected returns (NUR), was developed.[23]

Net unexpected returns were calculated using a three step procedure. First, a unique 50 firm control portfolio was formed in each month t of the relevant sample periods. These firms were selected randomly from the set of sample firms that did not experience any insider trading event during the seven month period centered on month t. Next, the 12 month unexpected returns to each control portfolio were calculated using the methodology outlined earlier.[24] Finally, NURs were calculated by subtracting the unexpected return to the

[23]Rozeff and Zaman [21] recently have reported that controlling for size and EPS effects reduces the level of estimated insider trading profitability.

[24]On average, the control portfolios had a 12 month CUR of -2.0 percent in the post-Trial Court period and of +4.5 percent in the post-Appellate Court period. Consequently, within the sample employed in this study, the market model systemically overestimated actual returns in the post-Trial Court period and underestimated actual returns during the post-Appellate Court period.

control portfolio in month t+i from the unexpected return to the appropriate trading event portfolio in the month t+i. Assuming that the control portfolio captures the effects of time period systematic market model estimation error, the NUR is a pure measure of trading profits earned from exploiting firm-specific information.

The NUR metric first was used to retest the material fact reversal hypothesis. These results, reported in Panel B of Table 3, support two conclusions. First, the decline in sales transactions trading profits subsequent to the Appellate Court's decision is invariant to the profitability measure employed. As reported in the right side of Panel B, using either a multivariate or univariate test, both the high and low intensity sales transaction samples exhibited significant declines in average net unexpected returns subsequent to the Appellate Court's decision.

Second, it appears that time period-specific effects substantially account for the previously reported period-to-period increase in the profitability of purchase transactions. As shown in the left side of Panel B of Table 3, once the tendency of the market model to overpredict (underpredict) was controlled for, the profitability of purchase transactions did not differ significantly between the post-Appellate Court and post-Trial Court periods. The impact of the change in dependent variable measure is illustrated best by the high intensity/purchase sample results. Using raw unexpected returns, this sample exhibited a significant period-to-period increase in profitability. Using NURs, the sample exhibited a weak period-to-period decline.

As a follow-up to these results, a series of single sample Hotelling T^2 tests was conducted. This procedure first was used to test the hypothesis that the NURs associated with sales transactions were not significantly different from zero, i.e., insider trading was not abnormally profitable, in any one observation period. Three observation periods were examined: pre-TGS litigation, Trial Court and Appellate Court. As shown in Panel B of Table 4, for either the low or high intensity samples, the 12 month NURs associated with sales transactions were significantly different from a vector of zeros in both the prelitigation and post-Trial Court periods.[25] Thus, the conclusion that insiders

[25]A two sample Hotelling's T^2 test comparing pre-TGS litigation low intensity sales transaction net unexpected returns to those in the post-Trial Court period was not significant. (F statistic of 1.61 with 12 and 25 degrees of freedom.) The same test for the high intensity sample was highly significant. (F statistic of 2.25 with 12 and 23 degrees of freedom.)

A two sample Hotelling's T^2 test comparing pre-TGS litigation low intensity sales transaction NURs to those in the post-Appellate Court period was not significant. (F statistic of 1.27 with 12 and 33 degrees of freedom.) The same test for the high intensity sample also was not significant. (F statistic of .484 with 12 and 33 degrees of freedom.)

earned trading profits when they sold shares was robust to the choice of trading profit metric. Conversely, and consistent with the stated intent of the SEC, the hypothesis of month by month trading profits equal to zero could not be rejected in the post-Appellate Court period. Based on these results, it appears that insiders did alter their willingness to exploit nonpublic bad news about their firms coincident with the conclusion of the TGS litigation.

Single sample T^2 tests also were conducted for purchase transactions samples. These tests confirmed the important of time-specific effects on the observed levels of unexpected returns associated with these transactions. As shown in Panel A of Table 4, only the pre-TGS period was characterized by profitable purchase activity by insiders. Moreover, even during this period, the NURs associated with the high intensity sample were not statistically different from a vector of zeros, even though their 12 month sum was relatively large. During the other two periods, the 12 month vector of NURs for purchase transactions were cumulatively small and not significantly different from a vector of zeros.

Because both the pattern of the test results and a visual inspection of the cumulative NURs suggested that purchase trading profits declined concurrent with the TGS litigation, a two sample Hotelling's T^2 test comparing NURs in the pre-TGS period for the low intensity purchase transaction sample to those in the post-Appellate Court period was conducted. This test, however, did not result in rejection of the hypothesis of no period-to-period difference (F statistic of 1.26 with 12 and 33 degrees of freedom). Thus, the NUR results for purchase transactions were consistent with the no litigation effects hypothesis.

On whole, after controlling for time period-specific systematic market model estimation error, only weak statistical support was found for the conclusion that the TGS litigation affected the level of trading profits earned by insiders by purchasing shares in their firms. Instead, it appears that in the time periods examined in this study, insiders earned little, if any, trading profits on their purchase transactions. Because this finding indicates that corporate insiders, on average, rarely exploited high market value (i.e., bombshell) nonpublic good news for trading purposes, it is probable that, as suggested by Carlton and Fischel [2], insider purchase activity may be relatively insensitive to changes in the regulatory environment.[26]

Conversely, the test results reported in this section find that the profitability of insider sales transactions exhibited a significant change coincident with the reversal of the District Court's conservative material fact standard by the Court of Appeals. This finding, when coupled with the observation that the levels of

[26]Alternatively, the possibility exists that purchase behavior was affected at the time of the SEC's *Cady, Roberts* decision. Data were not collected to investigate this possibility.

unexpected returns declined between periods, is consistent with the conclusion that imposition of a liberal material fact standard had a deterrent effect on the decision of insiders to sell shares in their firms.

Summary and Conclusion

This study has reconsidered the question originally addressed in Jaffe [10]: Does the trading behavior of corporate insiders respond to changes in the legal environment? Contrary to the conclusion reached by Jaffe, the results of this study suggest that insiders did adjust their trading behavior coincident with critical events in the Texas Gulf Sulfur litigation. In particular, it appears that insiders altered their willingness to exploit negative, nonpublic information about their firms in response to changes in the prevailing material fact standard. The difference in findings appears to be attributable to three extensions of Jaffe [10]. First, based on a detailed analysis of the legal questions at issue in the TGS litigation, the current study hypothesizes that the District Court's opinion would be associated with an increase rather than a decrease in the profitability of insider trading. Second, based on the review of legal issues, this study includes the Court of Appeals decision as a critical event. Finally, insider purchase transactions are examined separately from insider sales transactions.

Little evidence is found that events in the TGS litigation had a significant effect on the profitability of purchase activity by insiders. This result, however, appears to be attributable to the more general finding that, after controlling for the effects of market model misestimation bias, insiders generally were not exploiting nonpublic good news during the period considered in this study.

The research design used in the study attempted to control for the effects of nonspeculative trading, i.e., transactions that are not intended to exploit the nonpublic information, by identifying firms that experienced high intensity trading events. This sample selection procedure, however, resulted in substantially reduced sample sizes and event portfolio diversification. As a consequence, a relatively large pre-TGS litigation to post-Appellate Court decline in observed trading profits for the high intensity sample was not statistically significant at conventional confidence levels. Thus, the inability of the study to detect a significant relation between purchase transaction trading profits and critical events in the TGS litigation may be a result of an inability to control or eliminate the effects of nonspeculative trading.

Alternatively, the fact that the findings differed between transaction types may indicate that differences between the incentives of management to trade on good and bad news affect the response of insider trading behavior to regulatory action. Recently Dye [3] and Trueman [26] have developed analytic models that link insider trading to the contractual agreement between managers and

shareholders. It is probable that this linkage differs with the type of news, good or bad, known by the manager. It is also probable that optimal contractual relationships are altered by regulatory changes. Accordingly, future research may want to explore in more detail the issue of whether the effects of regulatory change differ systematically with the type nonpublic information known by the insider.

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Table 1
TGS Litigation Effects: Trial Court Period
Pre-TGS Litigation Period (9/63-3/65) Compared to Post-Trial Court Period (9/66-3/68)

Event	Number of Portfolios	Number of Firms	Cumulative Average Unexpected Return 3 Month	6 Month	9 Month	12 Month	Two Sample Multivariate F Statistic	Two Sample Univariate t-Statistic 12 Month CUR
Panel A High Intensity Trading Sample								
Aggregate Trading								
Pre-Litigation Period	19	286	.02050	.04533	.06767	.07731	1.72	3.09**
Trial Court Period	19	582	.04648	.07679	.12044	.16052	(12,25)[a]	(36)
Purchase Transactions								
Pre-Litigation Period	16	103	.02791	.07358	.11141	.12939	1.98**	2.65*
Trial Court Period	18	111	.00931	.01653	.01427	.01374	(12,21)	(32)
Sales Transactions								
Pre-Litigation Period	19	179	-.01909	-.02611	-.04449	-.05437	3.07**	5.70**
Trial Court Period	18	462	-.06182	-.09894	-.15359	-.20370	(12,24)	(35)

Table 1 (continued)
TGS Litigation Effects: Trial Court Period
Pre-TGS Litigation Period (9/63-3/65) Compared to Post-Trial Court Period (9/66-3/68)

Event	Number of Portfolios	Number of Firms	Cumulative Average Unexpected Return 3 Month	6 Month	9 Month	12 Month	Two Sample Multivariate F Statistic	Two Sample Univariate t-Statistic 12 Month CUR
Panel B Low Intensity Trading Sample								
Aggregate Trading								
Pre-Litigation Period	19	2489	.01018	.01805	.02416	.03097	2.48**	4.76**
Trial Court Period	19	3005	.02452	.04166	.05875	.07140	(12,25)	(36)
Purchase Transactions								
Pre-Litigation Period	19	1162	.01296	.02346	.02795	.03251	3.52**	5.56*
Trial Court Period	19	1146	.00004	-.01164	-.02452	-.03937	(12,25)	(36)
Sales Transactions								
Pre-Litigation Period	19	1327	-.00883	.01421	-.02208	-.03060	8.43**	9.43**
Trial Court Period	19	1859	-.04052	-.07358	-.10832	-.13518	(12,25)	(36)

* statistically significant at at least the 10 percent level
** statistically significant at at least the 5 percent level
[a]degrees of freedom

Table 2
TGS Litigation Effects: Appellate Court Opinion
Pre-TGS Litigation Period (5/63-3/65) Compared to Post-Appellate Court Period (5/69-3/71)

Event	Number of Portfolios	Number of Firms	Cumulative Average Unexpected Return 3 Month	6 Month	9 Month	12 Month	Two Sample Multivariate F Statistic	Two Sample Univariate t-Statistic 12 Month CUR
Panel A: High Intensity Trading Sample								
Purchase Transactions								
Pre-Litigation Period	18	111	.02224	.07650	.08814	.09880	1.14	1.12
Appellate Court Period	18	147	.01326	.03658	.05180	.04154	(12,13)[a]	(34)
Sales Transactions								
Pre-Litigation Period	23	229	-.02244	-.02554	-.04195	-.05176	.57	1.40
Appellate Court Period	23	608	-.01071	-.01141	-.01824	-.01973	(12,33)	(44)
Panel B: Low Intensity Trading Sample								
Purchase Transactions								
Pre-Litigation Period	23	1352	.01408	.02745	.03508	.04010	.59	.20
Appellate Court Period	23	1343	.01966	.03329	.04253	.04216	(12,33)	(44)
Sales Transactions								
Pre-Litigation Period	23	1627	-.00741	-.01037	-.01909	-.02724	2.71*	4.29*
Appellate Court Period	23	2702	-.00339	-.00203	.00419	.00621	(12,33)	(44)

* statistically significant at at least the 10 percent level
** statistically significant at at least the 5 percent level
[a]degrees of freedom

Table 3
Material Fact Reversal Tests
Post-Trial Court Period (9/66-3/68) Versus Post-Appellate Court Period (5/69-3/71)

	Purchases Transactions				Sales Transactions			
	12 Month Cumulative Return				12 Month Cumulative Return			
Sample	Trial Period	Appeals Period	Two Sample Multivariate F Statistic	Two Sample Univariate t-Statistic 12 Month CUR	Trial Period	Appeals Period	Two Sample Multivariate F Statistic	Two Sample Univariate t-Statistic 12 Month CUR
Panel A: Unexpected Returns								
High Intensity	.0137 (16)[a]	.0415 (18)	3.28** (12,21)[b]	.61 (32)	-.2037 (18)	-.0197 (23)	8.00** (12,28)	9.40** (39)
Low Intensity	-.0394 (19)	.0422 (23)	4.66** (12,29)	6.11** (40)	-.1352 (19)	-.0062 (23)	16.97** (12,29)	12.96** (40)
Panel B: Net Unexpected Returns								
High Intensity	.0332 (16)	-.0054 (18)	1.47 (12,21)	.72 (32)	-.1827 (18)	-.0646 (23)	2.78** (12,28)	4.51** (39)
Low Intensity	-.0195 (19)	-.0027 (23)	.30 (12,29)	.85 (40)	-.1153 (19)	-.0387 (23)	1.96* (12,29)	3.89** (40)

* statistically significant at at least the 10 percent level
** statistically significant at at least the 5 percent level
[a]number of portfolios
[b]degrees of freedom

Table 4
Net Unexpected Return Tests for Abnormal Profitability During Event Periods

	Pre-TGS Litigation (5/63-3/65)		Post-Trial Court (9/63-3/65)		Post-Appellate Court (5/69-3/71)	
	12 Month Cumulative NUR	Single Sample Multivariate F Statistic	12 Month Cumulative NUR	Single Sample Multivariate F Statistic	12 Month Cumulative NUR	Single Sample Multivariate F Statistic
			Panel A Purchase Transactions			
High Intensity	.1007 (17)[a]	2.25 (12,5)[b]	.0332 (16)	2.77 (12,4)	-.0054 (18)	.25 (12,6)
Low Intensity	.0326 (23)	2.76* (12,11)	-.0195 (19)	2.64 (12,7)	-.0027 (23)	.41 (12,11)
			Panel B Sales Transactions			
High Intensity	-.0593 (23)	4.48** (12,11)	-.1827 (18)	11.17** (12,7)	-.0646 (23)	2.67 (12,11)
Low Intensity	-.0347 (23)	2.85** (12,11)	-.1153 (19)	4.04** (12,7)	-.0387 (23)	1.71 (12,11)

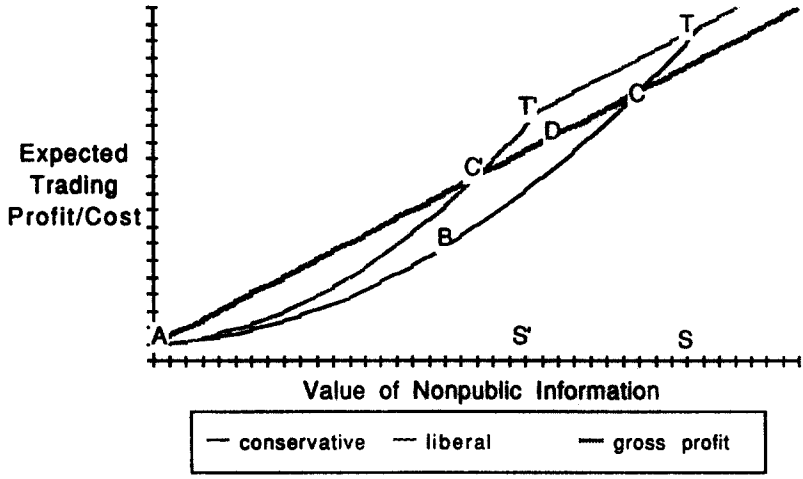
* statistically significant at at least the 10 percent level
** statistically significant at at least the 5 percent level
[a]number of portfolios
[b]degrees of freedom.

Figure 1
Chronology of SEC Antifraud Enforcement Activity: Issues and Events

	Enforcement Event	Date	Included in Jaffe's Study	Enforcement Issue I Applicability of Rule 10b-5	Enforcement Issue II Material Fact Standard
1	Ruling *In the Matter of Cady, Roberts and Co.*	November 1961	Yes	Applies	Liberal
2.	TGS Indictment	April 1965	Yes	Applies	Liberal
3	Trial Court Decision	August 1966	Yes	Applies	Conservative
4	Court of Appeals Decision	August 1968	No	Applies	Liberal
5	Supreme Court Declines to Review Lower Court Ruling	April 1969	No	Applies	Liberal

Figure 2

Effects of Increased Probability of Enforcement of Insider Trading Behavior



Appendix A

In his tests, Jaffe's employs the abnormal return to his event portfolios in month t+1, the month immediately subsequent formation, as the dependent variable. Tests are run for three alternative selection rules to form the month t portfolio: 1. an intensive trading event in month t (X=0); 2. an intensive trading event in month t or t-1 (X=1); or 3. an intensive trading event in month t or any of the seven months prior to t (X=7). The first rule assumes that the effect of changes in the regulatory environment is most pronounced at or near the actual date of trade. The other two samples provide a basis for assessing the assumption that insiders altered their trading behavior in response to changes in the enforcement environment by extending the time interval between trading and public disclosure of corporate information.

In the latter two cases, each observation of the dependent variable in Jaffe's tests is an average of unexpected returns measured at differing intervals between the date of trade and the month in which security returns are observed. At the extreme, one test observation might be generated from firms whose most recent trading event occurred six or seven months prior to the event date, while another observation might be generated from firms whose only trading events occurred in the event date. Statistically, this procedure is appropriate only if the assumption that the returns are uniformly and identically distributed across the months subsequent to trading events is valid. Existing empirical evidence is, however, inconsistent with this assumption. As discussed earlier, recent empirical evidence finds that the levels and timing of abnormal returns differ between purchase and sales events. Thus, Jaffe's inclusion of both types of events in the same portfolio violates the uniform distribution assumption. Further, the evidence also suggests that both the size and variance of unexpected returns fluctuate with the interval between the trading event and the month in which they are calculated [1, 7, 10, 25].

To some extent this problem in testing for multiperiod litigation effects can be overcome if the distributions of unexpected returns across months subsequent to trading events are independent. Conditional on this assumption, portfolios could be formed of firms experiencing similar trading event in month t and the abnormal returns to this portfolio cumulated a multimonth holding period. This metric then could be the dependent variable in the t-tests. Recent research, however, finds that unexpected returns subsequent to insider trading events are serially correlated; unexpected returns subsequent to purchase (sales) transactions tend to be positive (negative). Moreover, because the research design requires that trading events portfolios be formed in consecutive months, the sums of unexpected returns are not cross-sectionally independent because, by

construction, the same firm may be included in more than one transaction event portfolio.