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EVENT STUDIES IN MANAGEMENT RESEARCH: THEORETICAL AND EMPIRICAL ISSUES

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We examined the use of event studies in management research and found that there was inadequate attention paid to theoretical and research design issues. This lack of attention may lead to false inferences regarding the significance of the events and the validity of the theories being tested. To illustrate the extent of this problem, we attempted to replicate three recent studies. To guide authors and reviewers, we outline procedures for appropriate use of the event study method.

The event study method is a powerful tool that can help researchers assess the financial impact of changes in corporate policy. Using this method, a researcher determines whether there is an "abnormal" stock price effect associated with an unanticipated event. From this determination, the researcher can infer the significance of the event. This method has been used extensively in accounting and finance, often to measure the impact of corporate control changes. In management, the framework has been used to judge the effects of endogenous corporate events such as divestiture from South Africa, corporate control changes, corporate refocusing, CEO turnover, the use of affirmative action programs, layoffs, plant closures, corporate illegalities, product recalls, customer service changes, diversification programs, strategic investment decisions, and the formation of joint ventures, as well as the effects of exogenous events such as the enactment of major legislation, the appointment of top executives to cabinet positions, and the deaths of CEOs.

The event study method has become popular because it obviates the need to analyze accounting-based measures of profit, which have been criticized because they are often not very good indicators of the true performance of firms. For example, managers can manipulate accounting profits because they can select accounting procedures (Benston, 1982). Stock prices, on the other hand, are not as subject to manipulation by insiders. Stock prices are supposed to reflect the true value of firms, because they are assumed to

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reflect the discounted value of all future cash flows and incorporate all relevant information. Therefore, event studies, which are based on stock price changes, should measure the financial impact of a change in corporate policy, leadership, or ownership more effectively than a methodology based on accounting returns. Furthermore, the event study method is relatively easy to implement, because the only data necessary are the names of publicly traded firms, event dates, and stock prices.

Given that this method is increasingly used to assess the impact of managerial decision making, it is important to consider whether it has been implemented correctly, whether results have been reported clearly, and whether the interpretation of results has been appropriate. It is well established that the usefulness of this analytical technique depends heavily on a set of rather strong assumptions (Brown & Warner, 1980, 1985). We review these assumptions in the section on event studies. If these assumptions are violated, the empirical results may be biased and imprecise, and, therefore, basing conclusions on them is problematic. Additionally, research design issues affect the results obtained with this framework. It is possible that some theories have been unjustifiably supported because of inappropriate technique. Given these concerns, it is important that researchers report the steps taken in implementing the methodology so that readers can have confidence in the inferences drawn.

One way to determine the extent to which research design issues affect the results of such studies is to reexamine the data using an alternative design. Doing so is difficult in management research, because management journals do not require authors to make their data available to others. We were able to replicate three recent studies, two because the necessary data were included in the papers and one because an author was willing to make his data available. The results of these replications show that our concerns about the validity of the assumptions and the implementation of the methodology are valid. To address this problem constructively, in our recommendations we outline a procedure for implementing an event study of managerial decisions.

We do not have any quarrel with the validity of the event study methodology or its use in management research, per se. Our primary concern involves the empirical implementation and the paucity of information provided to readers regarding research design and implementation issues. Neither is it our intention to criticize *all* event studies published in management journals. Many appear to have been well designed and executed. However, the lack of information regarding the validity of assumptions and several research design issues in some articles raises questions about the confidence that readers can place in the conclusions drawn.

The problem is that misuse by some is tainting the work of others who employ the method correctly. Given that management event studies have important organizational and public policy implications, it is critical that their research designs and implementations be flawless.

EVENT STUDIES: ASSUMPTIONS AND RESEARCH DESIGN ISSUES

The event study method was developed to measure the effect of an unanticipated event on stock prices. The standard approach is based on estimating a market model for each firm and then calculating abnormal returns. These abnormal returns are assumed to reflect the stock market's reaction to the arrival of new information. The method is as follows: The rate of return on the share price of firm i on day t is expressed as

$$R_{it} = \alpha_i + \beta_i R_{mt} + \varepsilon_{it},$$

where

R_{it} = the rate of return on the share price of firm i on day t,
R_{mt} = the rate of return on a market portfolio of stocks (such as Standard & Poor's 500 or a market index) on day t,
α = the intercept term,
β = the systematic risk of stock i,
and
ε_{it} = the error term, with $E(\varepsilon_{it}) = 0$.

From estimation of the above equation, the researcher derives estimates of daily abnormal returns (AR) for the ith firm using the following equation:

$$AR_{it} = R_{it} - (a_i + b_i R_{mt}),$$

where a_i and b_i are the ordinary least squares (OLS) parameter estimates obtained from the regression of R_{it} on R_{mt} over an estimation period (T) preceding the event, for example, 250 to 50 days prior to the event. The abnormal returns (AR_{it}) represent returns earned by the firm after the analyst has adjusted for the "normal" return process. That is, the rate of return on the stock is adjusted by subtracting the expected return from the actual return. Any significant difference is considered to be an abnormal, or excess, return.

Following Dodd and Warner (1983), many authors compute a standardized abnormal return (SAR), where the abnormal return is standardized by its standard deviation:

$$SAR_{it} = AR_{it}/SD_{it},$$

with

$$SD_{it} = \{S_i^2 \times [1 + 1/T\,(R_{mt} - R_m)^2 / \sum_{t=1}^{T} (R_{mt} - R_m)^2]\}^{0.5},$$

where S_i^2 is the residual variance from the market model as computed for firm i, R_m is the mean return on the market portfolio calculated during the estimation period, and T is the number of days in the estimation period.

The standardized abnormal returns can then be cumulated over a num-

ber of days, k (the event window), to derive a measure of the cumulative abnormal return (CAR) for each firm:[1]

$$CAR_i = (1/k^{0.5}) \sum_{t=1}^{k} SAR_{it}.$$

A standard assumption is that the values of CAR_i are independent and identically distributed. With this assumption, we convert these values to identically distributed variables by dividing the CAR_i by its standard deviation, which is equal to $[(T-2)/(T-4)]^{0.5}$.

Thus, the average standardized cumulative abnormal returns across n firms ($ACAR$) over the event window can be computed as:

$$ACAR_t = 1/n \times 1/[(T-2)/(T-4)]^{0.5} \sum_{i=1}^{n} CAR_{it}.$$

The test statistic used to assess whether the average cumulative abnormal return is significantly different from zero (its expected value) is:

$$Z = ACAR_t \times n^{0.5}.$$

If significant, the cumulative abnormal return is assumed to measure the average effect of the event on the value of the n firms. That is, the significance of the abnormal return allows the researcher to infer that the event had a significant impact on the values of the firms.

Readers can be confident that the conclusions from an event study are valid only if they can be confident that the researcher has truly identified the abnormal returns associated with the event. The inference of significance relies on the following assumptions: (1) markets are efficient, (2) the event was unanticipated, and (3) there were no confounding effects during the event window.[2] Therefore, it is appropriate to use this method when these assumptions are likely to be valid. The third assumption is critical, because the method, by definition, attributes the abnormal return to the event under consideration. If other financially relevant events are occurring during the event window, it is difficult to isolate the impact of one particular event. In addition to these assumptions, several research design issues are important in implementing the event study methodology. The critical issues are (1) sample size, (2) nonparametric tests to identify outliers, (3) the length of the

[1] Many authors do not report cumulative returns. Instead, they report daily returns (either standardized or unstandardized) and the associated t-statistics for the null hypothesis of a zero abnormal return.

[2] This is by no means an exhaustive set of issues. For example, there may be size and industry effects. Although the market model has its shortcomings, it is simply the best available model at this time. See Bromiley, Govekar, and Marcus (1988) for an excellent discussion of these issues and a critique of the event study methodology as a tool for measuring the impact of strategic decisions.

event window and justification of the length, (4) confounding effects, and (5) explanation of the abnormal returns.

To illustrate how these issues have been addressed in management studies, we summarize 29 recent event studies published in three top management journals, the *Academy of Management Journal* (*AMJ*), the *Strategic Management Journal* (*SMJ*), and the *Journal of Management* (*JOM*) in Table 1. The studies described are all of the event studies we could identify in these journals from 1986 through the spring of 1995. For each study, we report the topic addressed, the nature of the event, and how the authors handled the five critical research design issues. In the following sections, we draw on the data summarized in Table 1 to illustrate our arguments about the validity of assumptions and research design and implementation.

ASSUMPTIONS UNDERLYING IDENTIFICATION OF ABNORMAL RETURNS

Market Efficiency

The first assumption is that markets are efficient. A significant body of work in economics and finance has addressed the efficient markets hypothesis; in the management literature, Bromiley, Govekar, and Marcus (1988) summarized this work. Such attention is warranted because this assumption provides the basis for the use of the event study method. Market efficiency implies that stock prices incorporate all relevant information that is available to market traders. If this is true, then any financially relevant information that is newly revealed to investors will be quickly (instantaneously) incorporated into stock prices. Therefore, an event is anything that results in *new* relevant information. A researcher can identify significant events by their impact on the stock prices of firms. To do this, the researcher defines a period of days over which the impact of the event will be measured. This period is known as the event window.

The assumption of market efficiency is difficult to reconcile with the use of a long event window. The use of very long windows in many management studies implies that some researchers do not believe that the effects of events are quickly incorporated into stock prices. This can be interpreted as a violation of the assumption of market efficiency. In some circumstances, it may be reasonable to assume that information is revealed to investors slowly over a period of time. For example, when the event is an acquisition, information about the number of potential acquirers and their evaluation of the target may be revealed over a relatively long period. Where this is the case, it is the obligation of a researcher to explain why the effect would not be realized within a short period of time. Otherwise, the use of the event study method is inappropriate.

Unanticipated Events

The second assumption stated above is based on the idea that an event is announced in the press. The market previously did not have information

TABLE 1
A Summary of Recent Event Studies in the Management Literature, Sorted by Topic

Studies	Journal[a]	Topic[b]	Nature of Event(s)	Research Design Issues: (1) Sample Size(s)[c]	(2) Nonparametric Test for Outliers	(3a) Length of Event Window(s)	(3b) Justified Length of Window	(4) Checked for Confounding Effects	(5) Explained Excess Returns
Wright, Ferris, Hiller, and Kroll (1995)	*AMJ*	CSR	Labor department awards for exemplary affirmative action programs; firms found guilty of discrimination	$N = 35$ $n_1 = 34$	No	−10 to +10	No	No	No
Meznar, Nigh, and Kwok (1994)	*AMJ*	CSR	Withdrawal from South Africa	$N = 39$ $n_1 = 19$	No	Various lengths from −30 to +10	No	Only on the days before and after the event	No
Clinebell and Clinebell (1994)	*JOM*	CSR	Plant closings	$N = 98$ $n_1 = 34$	No	−1 to +1 −5 to +5	No	Yes	Yes
Davidson and Worrell (1992)	*SMJ*	CSR	Product recall announcements	$N = 133$ $n_1 = 20$	No	Various lengths from −90 to +90	No	Only on the days before and after the event	No
Worrell, Davidson, and Sharma (1991)	*AMJ*	CSR	Major layoff programs	$N = 117$ $n_1 = 30$	No	Various lengths from −90 to 90	No	No	No
Davidson and Worrell (1988)	*AMJ*	CSR	Corporate illegalities	$N = 96$	No	Various lengths from −90 to +90	No	No	No
Worrell, Davidson, and Glascock (1993)	*AMJ*	CG	Firing and hiring of key executives	$N = 62$ $n_1 = 26$	No	Various lengths from −30 to +30	No	No	No
Davidson, Worrell, and Dutia (1993)	*JOM*	CG	CEO successions in bankrupt firms	$N = 81$ $n_1 = 26$	No	−5 to 0	No	No	No
Mahoney and Mahoney (1993)	*SMJ*	CG	Antitakeover amendments	$N = 409$ $n_1 = 23$	No	−50 to +10	No	No	No

TABLE 1 (continued)

Studies	Journal[a]	Topic[b]	Nature of Event(s)	Research Design Issues					
				(1) Sample Size(s)[c]	(2) Nonparametric Test for Outliers	(3a) Length of Event Window(s)	(3b) Justified Length of Window	(4) Checked for Confounding Effects	(5) Explained Excess Returns
Turk (1992)	*JOM*	CG	Managerial response to takeover bids	$N = 232$ $n_1 = 29$	No	−50 to +5	Yes	No	Yes
Chatterjee, Lubatkin, Schweiger, and Weber (1992)	*SMJ*	CG	(Cultural differences associated with) mergers and acquisitions	$N = 30$	Yes	−10 to +5	No	No	Yes
Markides (1992)	*AMJ*	CG	Corporate refocusing	$N = 43$	Yes	Various lengths from −10 to +10	No	Yes	Yes
Davidson, Worrell, and Cheng (1990)	*JOM*	CG	Executive successions	$N = 367$ $n_1 = 35$	No	Various lengths from −90 to +90	No	No	Yes
Seth (1990)	*SMJ*	CG	Corporate acquisitions	$N = 104$ $n_1 = 27$	No	−40 to +5	Yes	Yes	No
Lubatkin, Chung, Rogers, and Owens (1989)	*SMJ*	CG	CEO successions	$N = 471$	No	Various lengths from −50 to +50	Yes	No	Yes
Friedman and Singh (1989)	*AMJ*	CG	CEO successions	$N = 130$ $n_1 = 9$	No	−2 to +2	Yes	Yes	Yes
Shelton (1988)	*SMJ*	CG	Corporate acquisitions	$N = 218$	No	−1 to +1	No	No	Yes
Beatty and Zajac (1987)	*SMJ*	CG	CEO successions	$N = 209$ $n_1 = 25$	No	−30 to +30	No	Yes	No
Lubatkin (1987)	*SMJ*	CG	Mergers	$N = 340$ $n_1 = 36$	No	Various lengths from −18 to +64 months	No	No	No

TABLE 1 (continued)

Studies	Journal[a]	Topic[b]	Nature of Event(s)	Research Design Issues: (1) Sample Size(s)[c]	(2) Nonparametric Test for Outliers	(3a) Length of Event Window(s)	(3b) Justified Length of Window	(4) Checked for Confounding Effects	(5) Explained Excess Returns
Singh and Montgomery (1987)	*SMJ*	CG	Corporate acquisitions	$N = 77$ $n_1 = 37$	No	−5 to +100	No	No	No
Worrell, Davidson, Chandy, and Garrison (1986)	*AMJ*	CG	Deaths of key executives	$N = 127$ $n_1 = 43$	No	Various lengths from −90 to +30	No	No	No
Chatterjee (1986)	*SMJ*	CG	Corporate acquisitions	$N = 17$ $n_1 = 9$	No	−49 to +50	No	No	No
Koh and Venkatraman (1991)	*AMJ*	JV	Joint venture formations	$N = 175$	Yes	−1 to +1	Yes	Yes	Yes
Madhavan and Prescott (1995)	*AMJ*	JV	Joint venture formations	$N = 108$ $n_1 = 36$	No	Various lengths from −5 to +5	Yes	Yes	No
Jacobson (1994)	*AMJ*	LEG	Enactment of health care cost containment legislation	$N = 38$ $n_1 = 18$	No	−10 to +1	Yes	Yes	Yes
Nayyar (1993)	*SMJ*	MISC	Diversification moves by service firms	$N = 163$ $n_1 = 33$	No	−1 to +1	No	No	No
Nayyar (1995)	*SMJ*	MISC	Customer service changes (increases and decreases)	$N = 262$ $n_1 = 2$	Yes	−1 to +10	Yes	No	No
Woolridge and Snow (1990)	*SMJ*	INV	Strategic investment decisions	$N = 767$ $n_1 = 93$	No	Various lengths from −1 to +10	No	No	No
McGuire, Schneeweis, and Naroff (1988)	*AMJ*	MISC	Appointments of top managers to cabinet positions	$N = 22$	No	−30 to +30	No	Yes	No

[a] *AMJ = Academy of Management Journal, SMJ = Strategic Management Journal,* and *JOM = Journal of Management.*

[b] CSR = Corporate social responsibility, CG = corporate governance, JV = joint ventures, LEG = legislation, INV = investment, and MISC = miscellaneous.

[c] Sample sizes are for overall sample (N) and smallest subsample (n_1) if events are disaggregated.

on the event, and traders gain information from the announcement. Abnormal returns can then be assumed to be the result of the stock market's reacting to new information. It is possible that an event will have been anticipated or information leaked to the market in advance of a formal announcement. Such leakages make use of the event study methodology problematic, as it is difficult to determine when traders became aware of the new information. For example, information on corporate control changes and top-level management turnover may actually be revealed to the market before the events are officially announced (cf. Beatty & Zajac, 1987; Chatterjee, 1986; Mahoney & Mahoney, 1993; Turk, 1992; Seth, 1990). For other types of events commonly examined in the management literature, it may also be important to examine the validity of the assumption that the events were unanticipated. However, management researchers have rarely addressed this issue in studies that do not focus on corporate control issues.

Confounding Effects

The third assumption is based on the claim that a researcher has isolated the effect of an event from the effects of other events. This is perhaps the most critical assumption of the methodology. It is assumed that there are no confounding effects from other events. Confounding events can include the declaration of dividends, announcement of an impending merger, signing of a major government contract, announcement of a new product, filing of a large damage suit, announcement of unexpected earnings, and change in a key executive. Any of these events might have an impact on the share price during an event window. The longer the event window, the more difficult it is for researchers to claim that they have controlled for confounding effects. Management scholars do not appear to have been sensitive to this issue. The authors of many studies employing long windows have not stated whether they controlled for confounding effects.

RESEARCH DESIGN AND IMPLEMENTATION

Sample Size

Sample size is a concern because the test statistics used in the event study framework are based on normality assumptions associated with large samples. Unfortunately, small samples are quite common in the management literature, especially when events are disaggregated along many dimensions (see Table 1). When using a small sample, researchers are prudent to use "bootstrap" methods, which do *not* require the normality assumptions that are relied upon with large samples (Barclay & Litzenberger, 1988). An example of the application of bootstrap tests follows.

A researcher has computed daily average abnormal returns (*AR*) and the proportion of negative returns (*PRNEG*) for a sample of 15 firms over a 200-day estimation period (the period before the event that is used to estimate the parameters, α and β). Thus, 15 sets of 200 daily excess returns have been generated. One excess return from each of these 15 distributions is

randomly drawn and the *AR* and *PRNEG* are derived. The researcher repeats this process 3,000 times (15 × 200), providing bootstrap distributions of the *ARs* and *PRNEGs*. The significance tests for the *ARs* and *PRNEGs* are based on a comparison of their probability values with the bootstrap distribution:

$$\textit{Probability value } (AR) = (\textit{number of ARs} \leq AR_t)/3{,}000$$

and

$$\textit{Probability value } (PRNEG_t) = (\textit{number of PRNEGs} \geq PRNEG_t)/3{,}000.$$

These statistics should have been reported in several of the studies we examined. For example, Nayyar (1995) examined the abnormal returns associated with customer service changes involving only two companies. Jacobson (1994) examined the abnormal returns associated with health care cost containment legislation for samples of 18 and 20 firms. McGuire, Schneeweis, and Naroff (1988) used a sample of 22 firms to examine the effects of having a top manager appointed to a cabinet position. Clearly, in many instances imposing normality assumptions would be quite heroic. Given that the sample sizes are generally quite small, it is troubling that only 4 out of the 29 studies reported additional statistics on the distribution of abnormal returns (beyond the mean abnormal return) and none reported bootstrap tests.

Nonparametric Tests to Identify Outliers

The test statistics employed in event studies tend to be quite sensitive to outliers,[3] and a small sample magnifies the impact of any one firm's returns on the sample statistic. Hence, with small samples, interpretation of significance is problematic. It becomes crucial to assess whether the results are driven by outliers. In the management literature, authors have seldom identified outliers or adjusted the methodology to take their influence into account. However, it is clear that researchers should adjust the event study technique, or be especially careful to identify outliers, when dealing with small samples.

The identification of outliers raises the issue of what to do about them. Many researchers simply eliminate them from their samples, assuming that these data points reflect noise or measurement error. However, deleting outlying observations is a drastic approach. It is possible that outliers provide an important signal of the existence of confounding effects. One important control for outliers is for researchers to report nonparametric test statistics. One to include is the following binomial Z statistic, which tests whether the proportion of positive to negative returns exceeds the number expected from the market model:

[3] The estimates of abnormal returns in event studies are based on OLS regressions. It is well known that least squares parameter estimates, based on a quadratic loss function, are highly sensitive to outliers. One solution to this problem, reported by Jacobson (1994: 446), is to identify influential outliers using measures proposed by Cook (1977, 1979).

$$Z_p = (PRNEG_t - p^*)/[(p^*)(1 - p^*)/N]^{1/2},$$

where $PRNEG_t$ is the proportion of negative excess returns on day t, p^* is the expected value of $PRNEG_t$, and N is the number of firms (Kumar, Sen, & Shome, 1993).[4] A second nonparametric statistic to report is the Wilcoxon signed rank test, which considers both the sign and the magnitude of abnormal returns (Kohler, 1985).

Length of the Event Window

Possibly the most crucial research design issue is the length of the event window used in an event study. Many management studies are based on long event windows. There are two problems with this practice. First, as Brown and Warner (1980, 1985) showed, using a long event window severely reduces the power of the test statistic, Z_t. This reduction leads to false inferences about the significance of an event.

In addition, it has been empirically demonstrated that a short event window will usually capture the significant effect of an event (Ryngaert & Netter, 1990). For example, Dann, Mayers, and Raab (1977) found that the market price of a stock fully adjusts within *15 minutes* of the release of firm-specific information. Mitchell and Netter (1989) found that the stock market reacted within *90 minutes* of news wire stories announcing proposed federal tax legislation. Because it is much more difficult to control for confounding effects when long windows are used, an event window should be as short as possible. It should be long enough to capture the significant effect of the event, but short enough to exclude confounding effects.[5]

The nature of the event being studied should determine the length of the event window used (Ryngaert & Netter, 1990: 257). For example, where it can be shown that leakage of information is likely, the window should include some time prior to the announcement of the event so that abnormal returns associated with the leakage will be captured. In the absence of uncertainty about when information is actually revealed to the market, it is difficult to justify a long window. As noted earlier, the assumption of market efficiency implies almost instantaneous adjustment in stock price to the arrival of new information.

Table 1 shows that the event windows in management studies have generally been quite long. In fact, 181-day event windows (which means 181 *trading days,* or approximately nine months) are not uncommon! Only about one-quarter of the studies include justification of the lengths of the windows used. Those that have justified the length (e.g., Seth, 1990; Turk, 1992) have

[4] The idea behind this test is that, if the event has no significant effect on shareholder returns, then abnormal returns will be normally distributed—that is, half the companies will experience positive abnormal returns and the other half, negative abnormal returns.

[5] Another reason to be concerned about long event windows is that an inherent assumption of this model is that the α and β terms remain constant during the event window. This assumption may be problematic with long event windows. Therefore, with very long windows, researchers should estimate pre-event and postevent parameters separately.

usually explained why leakages were expected. Many other studies, however, have reported multiple windows with no justification for any of the lengths used.

Confounding Effects

A second problem with using long event windows is that they greatly exacerbate the difficulty of controlling for confounding effects. Since many of the firms under examination are large, diversified, multinational firms, it is likely that significant events occur quite frequently. With a short event window, a researcher can be reasonably confident that an abnormal return is due to an event, because it is relatively easy to identify confounding effects. Because this methodology is based on the assumption that the researcher is calculating the returns that result from the event being studied, failing to control for confounding events causes serious doubts about the validity of the empirical results and calls into question any conclusions drawn.

For example, Meznar, Nigh, and Kwok (1994) reported that they checked for confounding effects during the 3-day window immediately surrounding their event. They found confounding events for 37 percent of the firms in their original sample and eliminated those firms from the sample estimated. They specifically did *not,* however, check for confounding events over any other window length, because doing so "would have eliminated so many events from the pool" (1994: 1639).[6] An examination of the results in Meznar and colleagues' publication shows that there were no significant abnormal returns during the 3-day window when confounding effects were controlled for. Only during longer (13-, 21-, 31-, and 41-day) windows, when confounding effects were *not* controlled for, did they find significant results.

There are methods that allow researchers to control for confounding events. Foster (1980) discussed several of them, such as (1) eliminating firms that have confounding events, (2) partitioning a sample by grouping firms that have experienced the same confounding events, (3) eliminating a firm from the sample on the day that it experiences a confounding event, and (4) subtracting the financial impact of the confounding effect when calculating the abnormal returns. In this regard, Salinger (1992) used a technique that subtracted the impact of confounding events in a study of the financial impact of the Bhopal disaster on Union Carbide.

Table 1 shows that many articles in management journals do not clearly state whether confounding events are controlled for.[7] It is possible that many

[6] Meznar and colleagues stated that "the protection period selected represents an attempt to compromise between internal and external validity issues" (1994: 1639). This statement is inconsistent with the advice of Cook and Campbell, who cautioned that ". . . jeopardizing internal validity for the sake of increasing external validity usually entails a minimal gain for a considerable loss . . . internal validity is the *sine qua non* of causal inference" (1979: 84).

[7] One notable exception is Markides, who stated that he included only those firms for which "no major confounding announcements (earnings, dividends, share repurchases) were made within five days before or after the announcement day" (1992: 403).

researchers do control for confounding events but do not report this information in their published articles. In Wright, Ferris, Hiller, and Kroll (1995), the authors stated that they did check for confounding events; however, our check of the *Wall Street Journal* index found 189 confounding events during the reported windows for their two studies. Tables 3 and 4 list these confounding events, which are discussed in the section of this article on replication.

Explanation of Abnormal Returns

A final issue of note concerns explanation of abnormal returns. After determining the significance of the CARs, in a second stage of the analysis, a researcher should explain the abnormal returns by showing that the cross-sectional variation in the returns across firms is consistent with a given theory. For example, the theory may predict that there should a positive correlation between the size of the abnormal return and the extent of firm diversification. Thus, in the second stage of the analysis, the researcher should regress the abnormal returns on some measure of firm diversification and report the parameter estimates. For example, Friedman and Singh (1989) regressed abnormal returns on several variables describing organizational context and precursor events in a study of the effect of CEO succession.

Other management studies that have explained abnormal returns include Clinebell and Clinebell (1994), Jacobson (1994), Turk (1992), Chatterjee, Lubatkin, Schweiger, and Weber (1992), Markides (1992), Koh and Venkatraman (1991), Davidson, Worrell, and Cheng (1990), Lubatkin, Chung, Rogers, and Owens (1989), and Shelton (1988). Demonstrating that the pattern of abnormal returns is consistent with established theory, a standard practice in other disciplines, lends considerable credibility to the empirical findings of a study.[8]

We also see from Table 1 that few of the published works contain an explanation of the abnormal returns. In the management literature, some researchers have not even predicted a priori the *direction* of the effect of events. For example, in a study examining layoff announcements, the authors stated that "we believed that such reactions will generally be negative, but they could be positive or neutral" (Worrell, Davidson, & Sharma, 1991: 664). And, in an article examining the effect of having a top corporate manager receive a cabinet appointment, McGuire and colleagues (1988) surmised that the event "may be detrimental of the stock market returns" or "may . . . raise the value of the firm's stock" or "may have little effect on a firm's market value" (1988: 202). Where there is no underlying theory to test, it is not surprising that excess returns are not explained.

[8] A reviewer noted that there may be a problem with using the abnormal returns as dependent variables in an OLS regression, because they are essentially residuals that may be heteroskedastic and correlated with the independent variables. However, Karafiath (1994) showed that the OLS estimator performs surprisingly well under these conditions and that there appears to be little benefit in using more complex estimation procedures.

Having outlined the assumptions of the model and the important research design issues, we examine three recently published event studies that tested theories of corporate social responsibility.

REPLICATIONS OF SELECTED EVENT STUDIES

Event Studies of Corporate Social Responsibility

The issues discussed above may be particularly significant in the area of corporate social responsibility (CSR). This is an area in which researchers desire to have an impact on public policy decision making, but it has been difficult for them to do so because of the problems involved in measuring the impact of managerial decisions. The primary model that has emerged in this area is stakeholder theory (Donaldson & Preston, 1995). According to stakeholder theory, implementing socially responsible decisions involves a trade-off. The trade-off is between profit enhancement (for the benefit of stockholders) and something of benefit to other stakeholders. This posited relationship has led some researchers to infer that the impact of socially responsible decisions can be estimated by examining their effect on stock prices. The presumed importance, and ease of determination, of stock prices may explain why researchers have used the event study methodology to test theories pertaining to corporate social responsibility.

Several of the articles in our sample, reported in Table 1, deal with CSR. The issues examined include affirmative action programs (Wright et al., 1995); discrimination suits (Wright et al., 1995); withdrawal from South Africa (Meznar et al., 1995); plant closings (Clinebell & Clinebell, 1994); product recall announcements (Davidson & Worrell, 1992); layoff programs (Worrell et al., 1991); and corporate illegalities (Davidson & Worrell, 1988). In all of these articles, the researchers inferred from their empirical results that the events being studied had significant impacts on the stock prices of the firms and that those findings supported some aspect of stakeholder theory.

We reexamined three corporate social responsibility hypotheses from two publications, Nigh, Meznar, and Kwok (1994) and Wright, Ferris, Hiller, and Kroll (1995), to see if the research designs the authors employed materially affected the conclusions they drew. We selected these studies because they were the most recent corporate social responsibility studies using the event study method that had appeared in *AMJ*. Because the authors were willing to provide firm names and dates, we were able to reanalyze the data using an alternative research design.[9] The research design issues that we focused on in this analysis were the length of the event windows used and the presence of confounding effects. For each of these studies, we estimated 2-, 3-, 5-, and 11-day windows as well as the longest window reported

[9] It is interesting to note that the paper by Wright and colleagues is the only management study to provide firm names and event dates in the text.

in each study. For each window, we eliminated firms for which there were confounding effects, defined as other economically relevant events. These were events reported in the *Wall Street Journal* during the event window.

Empirical Issues

Tables 2, 3, and 4 list the confounding events that fell within the longest window, by firm, for the firms in the three studies. Column 1 in these tables lists the firm name (for those firms for which we found confounding events), column 2 contains the event date, and the remaining columns contain the dates of the confounding events. The column headings for columns 3 through 13 define the confounding events.

We identified 178 confounding events for the Meznar et al. sample of firms that withdrew from South Africa, 69 for the Wright et al. sample of firms that received awards for exemplary affirmative action programs, and 120 for the Wright et al. sample of firms that were found guilty of discrimination. These 367 confounding events included major executive changes (17), restructuring or divestitures (29), acquisition activity (39), joint ventures (18), major litigation or labor unrest (43), forecasted changes in sales or earnings (32), major contracts (34), and announcements of earnings or dividends (78). All of these events have been shown to generate significant abnormal returns (Becker, 1987; Becker & Olson, 1986; Hite & Owers, 1983; Kalay & Loewenstein, 1985; Kaplan & Weisbach, 1992; McConnell & Nantell, 1985; McWilliams, Turk, & Zardkoohi, 1993; Reinganum, 1983; Rogerson, 1989).

Many of these confounding events were quite noteworthy. During the event window for the Meznar and colleagues study, we found Allegis fighting a battle with the pilots' union for control of the company; Pepsi reacting to the introduction of New Coke; Revlon engaged in a hostile takeover battle; Unisys involved in a major corruption scandal; Apple losing its founder-CEO, Steve Jobs; and a substantial number of earnings and dividend announcements. During the event window for Wright and colleagues' study of affirmative action award winners, we found Polaroid announcing a new film on the day of the event studied; McDonnell Douglas receiving a $298 million contract on the day of the "event" and three additional large contracts during the window; Scott Paper's earnings announcement on the day of the "event"; Tenneco entering a major reorganization; EG&G announcing a dividend increase; and Philip Morris attempting to sell 7-Up. During the event window for the Wright and colleagues' study of firms found guilty of discrimination, we found Northwest Airlines' entry into a price war on the day of the "event"; a strike at USX; Pan Am's attempt to avert a strike; General Motors announcing plant shutdowns, a strike, and a confrontation with Ross Perot; and Apple reporting a decline in earnings and expected future earnings.

It is interesting that the affirmative action awards, whose announcements were the events studied, do not seem to have been noteworthy to the financial community. In fact, none of these awards was reported in the *Wall*

TABLE 2
Confounding Events Occurring during Meznar, Nigh, and Kwok's Longest Event Window[a]

		Event Type												
Company Affected	Event Date	Restructuring/ Divestiture	Price Changes	New Products	Dividend/ Earnings Announcements	Joint Venture	Acquisition Activity	Litigation/ Labor Unrest	Major Executive Changes	Major Initiatives by Rivals	Forecasted Changes in Earnings or Sales	Layoffs	Debt or Equity Related Event	Contract Awards
Allegis	8/24/87				7/31		7/27, 8/14	7/31						8/27
American Brands	5/01/87	5/7					4/9						5/12	
Apple	8/05/85	6/27		7/9	7/19						7/25, 8/16		8/2	
Ashland	10/29/86				10/23, 11/7						11/6		11/4	
BBDO	8/21/85				7/24, 8/27									
Black and Decker	1/17/87				1/27				12/22				1/27	
Bundy	12/09/86				11/25									
Chase Manhattan	8/01/85				7/16, 7/23		7/2, 7/24, 8/2		7/25					
Control Data	11/11/88	10/10, 11/16			10/25, 11/15						11/14	10/10		11/7, 10/31
CPC International	4/03/87	3/25, 4/2			3/18, 4/13						4/13			
Dow Chemical	2/14/87		2/3, 2/4		1/30				2/6	1/22, 1/28, 2/3				
Dun & Bradstreet	12/10/86	11/3, 12/18												
Eastman Kodak	11/20/86				11/13, 11/17	10/17, 11/25, 11/6	11/18				11/13		11/24	
Emery Air	7/02/87								6/2					
Exxon	12/30/86	12/11, 12/12, 12/23									12/11			
Federal Mogul	9/16/88				9/29									
Firestone	5/19/87	5/15			5/22			5/5, 5/15			5/1			
Fluor	12/06/86	10/14			12/10									11/25
Foster Wheeler	2/23/87				1/27, 2/24	1/20								
Goodyear	6/08/89				5/3			5/12						
IBM	10/22/86	9/12	9/19, 9/24, 10/7, 10/8		10/14	10/7			10/29, 10/30	10/2, 10/3, 10/23, 10/27	10/6, 10/8	10/9		9/11
International Harvester	8/31/85				8/16	9/13						8/19		
Johnson Controls	11/24/86				11/28, 10/31									

TABLE 2 (continued)

Company Affected	Event Date	Event Type: Restructuring/ Divestiture	Price Changes	New Products	Dividend/ Earnings Announcements	Joint Venture	Acquisition Activity	Litigation/ Labor Unrest	Major Executive Changes	Major Initiatives by Rivals	Forecasted Changes in Earnings or Sales	Layoffs	Debt or Equity Related Event	Contract Awards
McGraw-Hill	2/27/87				1/29, 2/3				1/22					
Motorola	10/09/85				10/14							10/2		10/1, 10/11
NCNB	2/20/85				1/21		1/14							
NCR	4/05/89				4/18									
Newmont Mining	3/31/88	4/5	3/22										3/23	
Norton	3/04/87				2/13, 3/2		1/27							
Pepsi	5/19/85	5/18, 5/21	5/21		4/26, 5/2		4/30	4/30, 5/21		4/23, 4/24, 5/8, 5/16	5/22		5/2	
Perkin Elmer	2/26/85			2/5	2/20, 2/22									2/5
Procter & Gamble	9/26/86							9/5			10/2			
Raychem	8/29/89				7/28, 7/31									
Revlon	12/05/86						11/14, 11/17, 11/18 11/19, 11/20 11/25, 11/26	10/29, 11/28, 12/3						
Square D	2/27/87				1/29, 2/27									
Stanley Works	5/05/86				4/17									
Tambrands	11/28/87				10/28, 10/29									
Unisys	8/20/88		8/30		7/15		8/11	7/13, 7/13, 7/15, 7/20	7/20					7/15, 7/25, 8/31
Westinghouse	3/13/87					2/2, 3/3	1/30, 2/7, 2/10		3/11		2/5	2/2		2/10, 3/16, 3/19
Totals (178)		(17)	(9)	(2)	(46)	(8)	(21)	(16)	(9)	(11)	(13)	(5)	(7)	(14)

[a] The window lasted from day −30 through day +10.

TABLE 3
Confounding Events Occurring during Wright, Ferris, Hiller, and Kroll's Longest Event Window for Affirmative Action Awards[a]

		Event Type												
Company Affected	Event Date	Restructuring/ Divestiture	Price Changes	New Products	Dividend/ Earnings Announcements	Joint Venture	Acquisition Activity	Litigation/ Labor Unrest	Major Executive Changes	Major Initiatives by Rivals	Forecasted Changes in Earnings or Sales	Layoffs	Debt or Equity Related Event	Contract Awards
Glaxo	9/17/92			9/4	9/11, 9/15									
Motorola	9/17/92			9/9		9/23								
Pfizer	9/17/92			9/24		9/15								
Society Corp.	9/17/92						9/22							
Anheuser Busch	9/19/91											9/17		
Polaroid	9/19/91			9/19										
Tenneco	9/19/91	9/12			9/12									
Marriott	10/23/90	10/12											10/12, 10/15	
Potomac Electric	10/23/90				10/29									
Schering Plough	10/23/90				10/19, 10/24			10/10						
U.S. West	10/23/90				10/19	10/23								
Westinghouse	10/23/90				10/11		10/31							10/24
Barnett Banks	12/18/89										12/13			
Procter & Gamble	12/18/89			12/8, 12/14			12/14	12/28			12/5			
Southern New England Telecom	12/18/89				12/13									
Texas Instruments	12/18/89	12/19												12/27
Allied Signal	11/15/88			11/2								11/14		11/21, 11/23
Duke Power	11/15/88											11/10		
EG & G	11/15/88				11/23		11/2							
McDonnell Douglas	11/15/88	11/1												11/15, 11/16, 11/17, 11/22
Quaker Oats	11/15/88				11/10		11/10					11/3		
Syntex	11/15/88				11/22									
Scott Paper	7/22/87			7/23	7/22									
Xerox	7/22/87				7/14, 7/24		7/15							
Bank of America	7/09/86				7/17									
General Mills	7/09/86	7/23			6/24, 7/11									
Johnson & Johnson	7/09/86				7/22, 7/24									
Raytheon	7/09/86				6/26, 7/11		6/26				7/7			
Phillip Morris	7/09/86	6/25, 7/14			7/18			7/16, 7/22						
Totals (69)		(7)	(0)	(8)	(23)	(3)	(7)	(4)	(0)	(0)	(3)	(4)	(2)	(8)

[a] The window lasted from day −10 through day +10.

TABLE 4
Confounding Events Occurring during Wright, Ferris, Hiller, and Kroll's Event Window for Firms Found Guilty of Discrimination[a]

		Event Type												
Company Affected	Event Date	Restructuring/ Divestiture	Price Changes	New Products	Dividend/ Earnings Announcements	Joint Venture	Acquisition Activity	Litigation/ Labor Unrest	Major Executive Changes	Major Initiatives by Rivals	Forecasted Changes in Earnings or Sales	Layoffs	Debt or Equity Related Event	Contract Awards
Coca-Cola	9/24/92			9/29							9/23, 9/28			
IBM	7/06/92	6/25		7/10, 6/22	7/20	6/22, 7/2, 7/7, 7/13, 7/14		6/25 6/29 7/15		6/26	6/22 7/13, 7/17			6/23
Apple Computer	4/22/91		4/10		4/16			4/10, 4/17		5/3	4/19, 5/1, 5/2			
Northwest Airlines	8/19/91		8/19, 8/20, 8/22		8/12		8/22	8/8		8/13				
Southwestern Bell	11/04/91					11/14								
USX	2/27/91							2/19, 3/1						
General Electric	10/04/89				10/13			10/13	9/26					9/25, 9/29 10/2, 10/5 10/9, 10/12

TABLE 4 (continued)

Company Affected	Event Date	Event Type: Restructuring/ Divestiture	Price Changes	New Products	Dividend/ Earnings Announcements	Joint Venture	Acquisition Activity	Litigation/ Labor Unrest	Major Executive Changes	Major Initiatives by Rivals	Forecasted Changes in Earnings or Sales	Layoffs	Debt or Equity Related Event	Contract Awards
Pan Am	2/04/88							2/4	1/22					
CBS	8/05/87							8/14	8/17					
Con Agra	9/04/87						9/11		8/26		9/17			
General Motors	4/08/87			4/9				3/26 3/30, 4/6	4/7		4/3, 4/6 4/13, 4/16		4/3, 4/14	3/25 4/14, 4/17
3M	7/27/87				7/23				7/22					7/21, 7/23
Anheuser Busch	5/15/86						5/7, 5/12					7/2		
Chase Manhattan	2/21/86	2/20, 3/6							3/5	5/27				
Du Pont	7/10/86	6/30					7/9, 7/21					7/2	7/22	
Ford Motor Company	5/02/86		4/22, 5/5	4/18	4/25			4/18, 5/12		5/15	4/21, 5/14		4/28	
Goodyear	5/21/86				5/7, 5/9									
Pacific Telesis	12/05/86						12/10				12/3			
TWA	6/20/86							6/24	6/12		6/9			
United Airlines	7/02/86						7/2, 7/8 7/11, 7/14							
USX	8/05/86		8/19		7/30			7/22, 7/25, 7/28, 7/29, 8/1, 8/4, 8/5		8/8, 8/11, 8/12			7/24	
Westinghouse	3/28/86	3/26				3/26		4/2						
Totals (120)		(5)	(7)	(5)	(9)	(7)	(11)	(25)	(8)	(8)	(16)	(2)	(5)	(12)

[a] The window lasted from day −10 through day +10.

Street Journal. This is notable because the *Wall Street Journal* is considered the newspaper of record for financially relevant events.

It is also interesting to note that the authors of the studies we reexamined claimed quite dramatic value effects. Meznar and colleagues' stated that "by day 10, the stocks of the studied firms were, on the average, almost 5.5 percent lower than they would have been had the firms not withdrawn from South Africa" (1994: 1641). If this inference is legitimate, it indicates that managers' socially responsible actions can be quite costly for stockholders. Most of the firms in these studies were very large in terms of employment, geographic scope, and market value (which was generally in the billions of dollars). A decline of 5.5 percent would thus represent an *average* drop in value in the hundreds of millions of dollars. Alternatively, a positive cumulative abnormal stock return of 1 percent (Wright et al., 1995) would increase value by millions of dollars ($100 million for a $10 billion firm).[10] Thus, the rewards for managers whose compensation is tied to stock price return would be quite enticing. The size of these reported effects warrants further examination and consideration of the empirical and theoretical problems associated with using event studies to test CSR theory.

We reexamined each of the three studies using both short and long windows. Table 5 presents our results. Several things are worth noting. After we controlled for confounding events, the magnitudes of the abnormal returns were greatly diminished for all windows. More importantly, after controlling for confounding events, we found that the abnormal returns for all three studies were *all* statistically insignificant. Not surprisingly, eliminating confounding events dramatically reduced the sample size, especially for the long windows.[11] For the Meznar et al. study, the sample size was reduced to zero for the longest window. That is, for a 41-day window, all of the firms had at least one confounding event. For the Wright et al. award winners, the sample for the longer window (21 days) was reduced to 5 firms, and for the firms found guilty of discrimination, the sample was reduced to 13 firms. We also report (in the last column) the proportion of negative excess returns, which is an important nonparametric statistic, especially for a small sample. We tested whether these proportions were significantly different from their expected values. In all instances, these test statistics were insignificant.

Our conclusion is that we find no support for the hypothesis that the

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[10] In an earlier paper that also used the event study methodology to test the effects of these same affirmative action awards, Hiller and Ferris speculated that the financial rewards may be a result of the "promotion of good management techniques [including] increasing opportunities, attracting talented individuals, removing artificial barriers, encouraging diverse opinions, and learning to deal effectively with different personalities" (1993: 797).

[11] Although Wright and colleagues reported that they "scanned these sources [the *Wall Street Journal Index* and the *Dow Jones News Retrieval Service*] for the 90 days before and after each occurrence to identify the occurrence of other, economically relevant events" (1995: 277), we found a total of 189 economically relevant events reported in the *Wall Street Journal* during the two 21-day windows they reported.

TABLE 5
Cumulative Abnormal Returns for Meznar, Nigh, and Kwok and Wright, Ferris, Hiller, and Kroll before and after Controlling for Confounding Events[a]

	Before			After				
Intervals	***CAR***	***Z***	***N***	***CAR***	***Z***	***N***	***PRNEG***[b]	Z_p[c]
(a) Meznar, Nigh, and Kwok (1994): Effects of Withdrawal from South Africa								
−1 to 0	−.0005	−0.28	39	−.0005	−0.28	39	52.6	0.08
−1 to +1	−.0039	−0.65	39	−.0026	−0.45	32	53.1	0.02
−2 to +2	−.0057	−0.94	39	+.0022	+0.62	29	51.7	0.01
−5 to +5	−.0243	−2.68**	39	−.0103	−0.86	19	58.4	0.05
−30 to +10	−.0546	−2.58**	39	n.a.	n.a.	0	n.a.	
(b) Wright, Ferris, Hiller, and Kroll (1995): Effects of Affirmative Action Awards								
−1 to 0	+.0048	n.r.	34	+.0008	+0.21	28	48.2	−0.04
−1 to +1	+.0098	n.r.	34	+.0004	+0.08	22	51.5	0.04
−2 to +2	+.0108	n.r.	34	−.0024	−0.32	18	52.2	0.02
−5 to +5	+.0106	n.r.	34	−.0117	−0.77	11	58.7	0.46
−10 to +10	+.0118	+1.54	34	−.0343	−1.14	5	56.2	0.34
(c) Wright, Ferris, Hiller, and Kroll (1995): Effects of Being Found Guilty of Discrimination								
−1 to 0	−.0037	n.r.	35	+.0074	+1.37	24	56.3	0.33
−1 to +1	−.0047	n.r.	35	+.0023	+0.31	21	60.3	0.68
−2 to +2	−.0046	n.r.	35	−.0002	−0.02	15	60.0	0.53
−5 to +5	−.0028	n.r.	35	−.0068	−0.36	12	63.6	0.73
−10 to +10	−.0017	−1.12	35	−.0040	−0.14	11	58.0	0.45

[a] Data are reported to the fourth decimal place because it is critical to have units of measurement that are as detailed as possible. This is necessary because even small changes in share prices for companies with billions of dollars in market value may constitute a large financial impact. The entry n.a. = not applicable; n.r. = not reported.

[b] *PRNEG* = percentage of cumulative abnormal returns.

[c] Z_p = binomial *z* statistic for testing the significance of the proportion of negative cumulative abnormal returns.

** $p < .01$

announcement of these socially responsible decisions had an impact on the stock prices of firms. We do not conclude that this is evidence that socially responsible decisions have no impact on the stock price (or performance) of a firm. Our conclusion is that we cannot infer, on the basis of this technique, that stockholders benefit or are harmed by decisions that may reflect corporate social responsibility. This is not surprising, because most of the firms examined in these studies are large, diversified companies, whose managers frequently make important decisions, making it difficult to isolate the effect of any one decision.

Our results clearly demonstrate that event studies of corporate social responsibility may be quite sensitive to research design issues, especially the length of the event windows used and confounding economically relevant

events.[12] This sensitivity is important because most of the CSR studies reported in Table 1 have long windows (including three with 181-day windows) and because most of the authors did not report controlling for confounding events throughout those windows. In fact, only one CSR study (Clinebell & Clinebell, 1994) had a relatively short window and appropriate controls for confounding events during the entire window.[13] Given that we have demonstrated that there are no statistically significant effects in the three studies we replicated, the implication is that these issues should be taken into account in designing and reviewing event studies.

Theoretical Issues

There are also theoretical concerns with the use of this methodology. An analysis of stock price effects may not be the most appropriate method for testing the impact of corporate social responsibility. Using the event study method implies that such behavior can be recognized by its stock price effect, which is interpreted as a proxy for economic performance (and a measure of shareholder wealth). Two contrary views of the impact of socially responsible decisions emerge from the studies we replicated. These alternatives can be stated as propositions:

> *Proposition 1. Socially responsible behavior has a negative impact on economic performance (and stockholder wealth), constituting a redistribution of benefit from a firm's shareholders to other stakeholders (Meznar et al., 1994).*
>
> *Proposition 2. Socially responsible behavior both enhances economic performance (and stockholder wealth) and creates benefits for other stakeholders and thus produces social gains. Similarly, socially irresponsible behavior decreases performance, as well as creating harm for other stakeholders (Wright et al., 1995).*

It is important to note that a methodology based on an examination of the stock price effects of corporate social responsibility alone does not allow us to discriminate between these two views. For example, showing that affirmative action awards lead to higher stock prices does not prove that there is no redistribution from other stakeholders. Consumers could be bearing the cost of the affirmative action programs, or there might be redistribution of benefits from workers to stockholders. Therefore, event study results do not enable a researcher to conclude that the second view is correct if the first view is incorrect. A similar conclusion can be drawn from an event study

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[12] A good illustration of this sensitivity to research design is Posnikoff's (1997) study. She found a *positive* and significant stock price effect for firms withdrawing from South Africa, while Meznar and colleagues found a negative and significant effect for the same event.

[13] Clinebell and Clinebell also used a relatively large sample and explained the excess returns generated.

finding that withdrawal from South Africa decreased shareholder wealth. Withdrawal may not have created net benefits for other stakeholders, as many who opposed divestiture argued (Meznar et al., 1995: 1636–1637).

To test these propositions, a researcher should measure the impact of socially responsible decisions on other stakeholder groups. For example, if a researcher wanted to present evidence that there is a net social gain to affirmative action programs, a more direct approach would be to measure (1) the efficiency (total factor productivity) of firms before and after the start of these programs and (2) the impact of these programs on other stakeholders such as employees, consumers, and communities.

These concerns have been addressed in other areas of research in which event study methodology is used extensively. For example, a large body of empirical evidence on the combined market values of acquiring and acquired firms suggests that takeovers have a positive net effect on stockholder wealth (Jensen & Ruback, 1983). This finding is often cited as evidence that these transactions are socially desirable, because the market reaction is ostensibly consistent with the view that firms are more efficient after these events occur. However, even if takeovers create shareholder value, researchers should also consider whether these gains are the result of increased efficiency, or merely transfers from one stakeholder group to another. For example, Shliefer and Summers (1988) hypothesized that takeovers may have harmful effects on other stakeholders through layoffs, plant closings, lower wages, unexpected reductions in orders for suppliers, and lower tax revenues.

Viewed from this perspective, shareholder gains may be offset by losses to other corporate stakeholders, resulting in a net social loss. For example, Hitt, Hoskisson, Ireland, and Harrison (1991) demonstrated that acquisitions reduced the intensity of R&D investment at the firm level, which may lead to lower social welfare. Lichtenberg and Siegel (1987, 1990), using plant-level analysis, examined the impact of takeovers on total factor productivity, capital expenditures, plant closures, and wages. They found that changes in ownership resulted in higher plant-level productivity, without accompanying declines in capital expenditures or in wages for blue-collar workers and without increases in plant closures. In an analysis of individual workers, Gokhale, Groshen, and Neumark (1995) found that hostile takeovers led to employment and wage reductions for older workers. Considered together, the results of these studies demonstrate that examining stock price alone is not an appropriate way to assess net social benefits, and, by implication, not the best way to test corporate social responsibility. We propose that a more desirable methodological approach is to examine the net, or social, effects of decisions viewed as representing CSR. Doing so involves simultaneously measuring effects on firm performance and benefits or losses experienced by other stakeholder groups.

An additional caveat regarding the use of event studies is that the use of this methodology constrains researchers to an analysis of a strictly firm-level measure of performance. This constraint may be problematic when manag-

ers engage in socially responsible behavior that influences only a particular plant or unit of a firm. For example, affirmative action awards are often given to individual plants or divisions of firms such as General Electric, which has thousands of plants worldwide. Although the impact of an award may be quite significant at the plant level, it might be negligible at the firm level. This fact biases the analysis *against* showing an impact to these types of decisions. Therefore, behavior that should be encouraged and that could have a firm-level impact if practiced in multiple plants or units may go unrecognized and unrewarded. When examining plant-level or divisional decisions or outcomes, researchers need to use alternative methods, such as those discussed above.

DISCUSSION

The event study framework provides a true measure of the financial impact of an event only if a set of assumptions regarding the nature of the empirical experiment is valid and if the research design is properly executed. The crucial assumptions are that: (1) markets are efficient, (2) the event was unanticipated, and (3) there were no confounding effects during the event window. Our examination of 29 recent studies in top management journals leads us to conclude that management articles often do not provide enough information to allow readers to judge whether these assumptions are valid. Management studies rarely address the issue of market efficiency, but they often employ very long windows. These windows imply that their authors believe market adjustment to new information is not immediate—or even quick. Few management studies, except those examining changes in corporate control, include discussions of whether events were anticipated. However, several have windows that extend backward for several weeks before their events. Again, the use of long windows has implications—in this case, that the events may have been anticipated. Finally, it appears that few management studies report checking for confounding events. With long windows, it is highly likely that firms have experienced confounding events.

In addition to the above assumptions, several research design issues are critical to the proper implementation of this method. These include sample size, identification of outliers, length of the event window, confounding effects, and explanation of the abnormal returns. Our replication of three recent studies demonstrates that these issues have not been uniformly addressed in management studies, particularly in the area of corporate social responsibility research.

Sample sizes as small as two firms have been reported. None of the articles that reported results for small samples reported using bootstrapping techniques or identified outliers and discussed their impact on results. Many studies employed unusually long windows without justifying their length. For example, Davidson and Worrell (1988) used a 181-day window in a study of the effects of firms' being found guilty of illegal acts; McGuire and colleagues (1988) used a 61-day window in a study of appointments of top

managers to cabinet positions; and Meznar and colleagues used a 41-day window in a study of withdrawal from South Africa. Several studies in our sample did justify the use of long windows, including Turk (1992), Seth (1990), Lubatkin et al. (1989), Madhavan and Prescott (1995), Jacobson (1994), and Nayyar (1995).

Most of the studies identified in Table 1 did not report checking for confounding events. Only 11 of the 29 studies reported such a check, and of these, 2 checked only on the days before and after the event, not for the entire window. Only 10 of the 29 studies reported an explanation of the distribution of abnormal returns, and not all of these included an econometric test of the explanation.

Our replication of 3 recent studies demonstrates that these issues matter. Upon replicating the studies using appropriate research design and implementation techniques, we found that our results differed from those presented in the published studies. Although the authors reported significant results for all of the studies, we found that none of them had significant results for a short window immediately surrounding the event. Therefore, our conclusions differed significantly from those published. We found no support for the hypothesis that decisions based on corporate social responsibility had an impact on stock prices. We also noted that this methodology may not be appropriate for testing theories of CSR, because it is important to consider the impact of such decisions on multiple stakeholders, not just shareholders.

On the basis of our examination of existing studies, we conclude that there does not appear to have been a great deal of sensitivity to important design and implementation issues in event studies in management journals. Given the paucity of information on the validity of the assumptions underlying choice of the method and the research design used to implement it, readers cannot be confident that researchers have drawn the correct inferences about the significance of events. To ensure that design, implementation, and reporting are appropriate and sufficient, we suggest that researchers and journals consider the following recommendations for future event studies.

RECOMMENDATIONS

In Exhibit 1, we outline the appropriate procedures for an event study. The first step is to determine when it is appropriate to use the event study method. When an event is likely to have a financial impact, is unanticipated by the market, and provides new information to the market, it is appropriate to use the method. The second step is to outline a theory that justifies a financial response to this new information. This step would include the *a priori* prediction of the sign of the effect, based on the theory outlined. The third step would be to identify the event's dates and a set of firms that experienced the event.

The fourth step would be to choose an appropriate event window. For

EXHIBIT 1
Steps for Implementing an Event Study

Step 1:	Define an event that provides new information to the market.
Step 2:	Outline a theory that justifies a financial response to this new information.
Step 3:	Identify a set of firms that experience this event and identify the event dates.
Step 4:	Choose an appropriate event window and justify its length, if it exceeds two days.
Step 5:	Eliminate or adjust for firms that experience other relevant events during the event window.
Step 6:	Compute abnormal returns during the event window and test their significance.
Step 7:	Report the percentage of negative returns and the binomial Z or Wilcoxon test statistic.
Step 8:	For small samples, use bootstrap methods and discuss the impact of outliers.
Step 9:	Outline a theory that explains the cross-sectional variation in abnormal returns and test this theory econometrically.
Step 10:	Report firm names and event dates in data appendix.

events that were clearly unanticipated and that took place on the date identified, the appropriate window should be very short, from 1 to 2 days. For an unanticipated event, the first day on which the market can trade on the information is the event day itself. For example, the crash of an airplane is an unanticipated event. The news of the crash is released quickly, and the market can be expected to react very quickly if the news is judged to be relevant information. The window for this event would be 1 day (assuming this is a trading day). Most news items are given to the *Wall Street Journal* the day before they appear in print. Therefore, some traders may receive information on a given event the day prior to its public announcement. For such events, trading may take place the day before the event. Because a researcher may not be able to identify when the news was released, the standard event window is 2 days, the day of the event and the day prior to it, assuming these are trading days.

If a window exceeds the standard 2-day length, it should be justified. For example, an event might be the announcement of an acquisition, such as the takeover of First Interstate by Wells Fargo. Because takeovers usually result from a strategic planning process, the information of the impending merger may have been leaked to or predicted by some traders. In this instance, a window that included the planning period before the merger might be justified. Researchers should include this justification, including an explanation of the length chosen, in their text. Windows that extend beyond the event date should be justified in terms of uncertainty about the impact of the event. For example, traders might view an announcement of an intent to engage in a hostile takeover with some skepticism. The final outcome would be viewed as having some probability greater than 0 but less than 1. In this circumstance, the "news" may come in pieces as the developments of the negotiation take place (cf. Salinger, 1992).

The fifth step would be to eliminate firms from a sample if other events

financially relevant to them occurred during the chosen window. Relevant events include such things as unexpected dividend or earnings announcements, takeover bids, merger negotiations, changes in key executives, restructuring, joint ventures, major contract awards, significant labor disputes, significant liability suits, and announcements of major new products. When long windows can be justified because of uncertainty about when information was revealed, techniques can be used to control for confounding events. Foster (1980) and Salinger (1992) describe these. Such techniques are to be used only when longer windows are necessary and properly justified, however.

The sixth step is for researchers to compute the daily (or cumulative) abnormal returns accrued during the event window, using the standard methodology that we outlined in the Methods section of this article, and to test the significance of the abnormal return. The seventh step is to report the percentage of negative returns and the binomial Z or the Wilcoxon test statistic, or both. The eighth step is to include additional information if a sample of fewer than 30 firms was employed. This additional information includes the identification and measurement of the influence of outliers and the results of bootstrapping techniques. The ninth step is to outline a theory that explains the cross-sectional variation in abnormal returns and to test this theory econometrically. Finally, the last step is to report firm names and event dates in a data appendix, to facilitate replication and extension. It is important to note that some of the researchers whose studies we have included in our sample may have followed these procedures. However, this information was not reported, at least in the published versions of the articles.

Careful implementation will lead to results that can be confidently interpreted. The results, if statistically significant, provide an estimate of the average abnormal return (as a percentage of stock price) for a firm that experienced the event in question during the sample time period. By multiplying the abnormal return by the stock price and number of outstanding shares, researchers can estimate the overall financial impact of an event. When the event is the result of a managerial decision, the effect of this decision on the value of the firm can be inferred. Therefore, the methodology is a powerful tool for assessing managerial decisions and for prescribing the course of managerial behavior, if it is the result of a well-designed and well-executed empirical analysis.

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