Journal of Business Finance & Accounting, 19(4), June 1992, 0306 686X \$2.50

MODELLING ABNORMAL RETURNS: A REVIEW ARTICLE

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INTRODUCTION

This paper provides a guide to event study methodologies and outlines procedures for modelling abnormal returns and their associated problems. The introduction provides a brief overview of event studies and sets out the rest of the paper.

Event Studies

An *event study* is the name given to an empirical investigation of the relationship between security prices and economic events.[1] Most event studies have focused on the behaviour of share prices in order to test whether their stochastic behaviour is affected by the disclosure of *firm-specific events*. For this sort of study, the most general form of the null and alternative hypotheses are as follows (for a formal development see, for example, Gonedes, 1975):

$$\begin{aligned} H_N: f(\tilde{R}_j \mid y_i) - f(\tilde{R}_j) &= 0 \text{ for all } y_i \\ H_A: f(\tilde{R}_j \mid y_i) - f(\tilde{R}_j) &\neq 0 \text{ for at least one } y_i \end{aligned} \tag{1}$$

where $\tilde{R}_j$ is the return on security j in an event period of interest;

y_i is a signal from information structure η announced in the event period that potentially affects security j;

$f(\tilde{R}_j \mid y_i)$ is the distribution of $\tilde{R}_j$ conditional on the information signal y_i from the information structure η;

$f(\tilde{R}_j)$ is the marginal distribution of $\tilde{R}_j$.

In equation (1), y_i denotes the firm specific event of interest and is a drawing from the set of signals in the information structure η. The alternative hypothesis states that for a signal, y_i, from an information system to possess *information content*, the distribution of the rate of return on the share conditional on the signal y_i should differ from the marginal distribution. While equation (1) allows for any parameter of the return distribution to be affected by the

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information signal, the majority of studies have restricted attention to the expected value of the return distribution.[2] This gives the revised null and alternative hypotheses:

$$
\begin{aligned}
&\mathrm{H_N}\colon \mathrm{E}(\tilde{R}_j \mid y_i) - \mathrm{E}(\tilde{R}_j) = \mathrm{E}(\tilde{u}_j \mid y_i) = 0 \text{ for all } y_i \\
&\mathrm{H_A}\colon \mathrm{E}(\tilde{R}_j \mid y_j) - \mathrm{E}(\tilde{R}_j) = \mathrm{E}(\tilde{u}_j \mid y_i) \neq 0 \text{ for at least one } y_i.
\end{aligned} \tag{2}
$$

In equation (2), the alternative hypothesis states that for the information signal, y_i, to possess information content, the unexpected or *abnormal* return on security j conditional on the signal y_i, $\mathrm{E}(\tilde{u}_j \mid y_i)$, must be non-zero. It should be noted that the marginal expected return, $\mathrm{E}(\tilde{R}_j)$, is unconditional with respect to y_i, but will be conditioned on information available prior to the event period and, in some research designs, on the return on a market index in the event period.

The basic structure of the standard form of event study can be mapped out as in Figure 1. Step one is required because at any point in time only one event is observed for any one firm. In order to estimate $\mathrm{E}(\tilde{u}_j \mid y_i)$ a number of observations on different firms (and/or the same firm at different event dates) is required. Firms' returns are subsequently analysed in a common *event time*. In step two, the specification of the benchmark for calculating abnormal returns is a crucial part of the design of the event study. If the benchmark is not carefully specified then spurious inferences can result from subsequent statistical tests. Various abnormal return metrics have been used in research studies. In step three, the assumptions underlying statistical tests are important considerations in the calculation of abnormal returns. In addition, a number of methods for cumulating abnormal returns over the test period have been employed. The exercise represented in Figure 1 is commonly referred to as *residuals analysis* although some researchers reserve this term exclusively for empirical studies that employ the *market model*, to be discussed below.

Figure 1

Basic Structure of Residual Analysis

1. Identify event dates for a sample of firms subject to the disclosure item of interest (for example, earnings announcements), and group observations into a common *event time*.

2. Within the overall *test period* (TP) of interest, calculate the following (estimate of the) *abnormal return* for each firm and for each period around the announcement date:

$$\hat{u}_{jt} = R_{jt} - \mathrm{E}(R_{jt}) \qquad t \in \mathrm{TP}.$$

3. Compute the mean abnormal return across firms in the sample, possibly cumulated over the TP, as an estimate of $\mathrm{E}(\tilde{u}_j \mid y_i)$ and test whether $\mathrm{E}(\tilde{u}_j \mid y_i) = 0$ using a test statistic of the form:

$$\frac{\text{mean abnormal return}}{\text{standard deviation}}$$

In the next four sections this paper focuses on the following areas of concern in the design of event studies:

1. The main alternative methods for measuring abnormal returns.
2. Evidence on the efficiency and power of these methods.
3. Special problems concerned with estimation and test statistics.
4. Controlling for extra-market factors.

The first three areas comprise the approach to residuals analysis traditionally followed in the literature, while the final area reflects the increasing concern of researchers contained in more recent contributions to the literature.

ALTERNATIVE ABNORMAL RETURN METRICS

There are various dimensions along which the calculation of abnormal returns can vary.

1. *Calculation of returns*

The first choice is whether to calculate discrete returns or logarithmic returns:

$$\text{Discrete:} \quad R_{jt} = \frac{P_{jt} + D_{jt} - P_{jt-1}}{P_{jt-1}}$$

$$\text{Logarithmic:} \quad R_{jt} = \log[(P_{jt} + D_{jt})/P_{jt-1}],$$

where P_{jt} = the price of security j at the end of period t;
D_{jt} = dividends paid during period t;
P_{jt-1} = the price of security j at the end of period $t-1$, adjusted for any capitalisations in order to make it comparable to P_{jt}.

There are both theoretical and empirical reasons for preferring logarithmic returns. Theoretically, logarithmic returns are analytically more tractable when linking together sub-period returns to form returns over longer intervals (simply add up the sub-period returns). Empirically, logarithmic returns are more likely to be normally distributed and so conform to the assumptions of standard statistical techniques.

2. *The measurement interval*

Various measurement intervals have been employed in the literature for computing returns, the most popular being monthly, weekly and daily intervals. Morse (1984) has examined the econometric trade-off between the choice of monthly and daily data from an analytical perspective. Morse analyses the bias and efficiency of estimates of expected abnormal returns associated with an information event subject to a number of commonly encountered statistical and data problems. Except for the case where there is uncertainty over the precise announcement date of the information, Morse's results generally support the choice of a shorter measurement interval to detect information effects. These results are further supported by the simulation studies of Brown and Warner (1980), (1985) and Dyckman et al. (1984).

3. *The benchmark for abnormal returns*

A number of alternative specifications of the benchmark expected return have been used in the literature.

Model A: *Mean adjusted returns*

The mean adjusted returns benchmark assumes that the *ex ante* expected return for security j is a constant that can vary across firms:

$$E(\tilde{R}_j) = k_j \text{ for all } t. \tag{3}$$

This will be the case if interest rates, risk premia, and the security's risk are constant over time. Given equation (3), the *ex post* predicted return for security j in period t, in the absence of any news disclosure, is given by k_j and the predicted abnormal return is given by the difference between the actual return on security j and k_j, which is estimated from historic data:

$$\hat{u}_{jt} = R_{jt} - k_j. \tag{4}$$

Model B: *Market adjusted returns*

The market adjusted returns model assumes that *ex ante* expected returns are the same for all securities and therefore equal in any period to the expected market return in that period:

$$E(\tilde{R}_j) = E(\tilde{R}_m) \text{ for all } j. \tag{5}$$

The *ex post* abnormal return on security j in period t that controls for market affects is given by:

$$\tilde{u}_{jt} = R_{jt} - R_{mt}, \tag{6}$$

where the marginal expected return on security j in period $t \epsilon \mathrm{TP}$ is conditioned on the realisation of the market return in period t.

Model C: *Capital Asset Pricing Model benchmark*

The *Capital Asset Pricing Model* (CAPM) benchmark, as it is implemented in practice, might be more appropriately termed a one-factor *Security Market Line* benchmark. This model controls for security risk as well as for the market. The *ex ante* expected return for security j in period t is given as:

$$\mathrm{E}(\tilde{R}_{jt}) = (1-\beta_j)R_{ft} + \beta_j \mathrm{E}(\tilde{R}_{mt}), \tag{7}$$

where R_{ft} is the return on a risk-free security in period t (normally taken to be the return on Treasury Bills) and β_j is the systematic risk of security j relative to the market index.

To implement this method β_j must first be estimated. The predicted abnormal return is then given by:

$$\hat{u}_{jt} = R_{jt} - (1-\hat{\beta}_j)R_{ft} - \hat{\beta}_j R_{mt}. \tag{8}$$

Model C collapses to Model A if a security's systematic risk is constant and if R_{ft} and R_{mt} are constant over time. Model C collapses to model B if all securities have the same systematic risk as the market.

Model D: *The matched/control portfolio benchmark*

A variant of the CAPM benchmark is the matched or control portfolio benchmark, also known as the *difference in returns* benchmark. Most early versions of this method controlled a security's return for its *systematic risk* against the market. Under this procedure, the sample securities subject to the disclosure event are formed into a portfolio, p. A second portfolio, q, is drawn independently of the disclosure item of interest, or in some cases conditional on the portfolio securities not experiencing the disclosure event under study. The portfolios are weighted to have the same estimated β value, often constrained to unity. The abnormal return is the difference between the returns on portfolios p and q:

$$\hat{u}_{pt} = R_{pt} - R_{qt}. \tag{9}$$

Beaver (1981) compares this procedure with the CAPM benchmark.

Other versions of this method (for example, Vermaelen, 1981) calculate abnormal returns for *individual* securities as the difference between actual security returns and the return on a reference portfolio of securities in the same beta risk decile. More recent variants of this approach have been used to control for extra-market factors and will be considered further below.

Model E: *The Market Model benchmark*

The Market Model (MM) has probably been the most popular benchmark employed in event studies. The MM makes no explicit assumption about how equilibrium security prices are established. Instead, it assumes that returns are generated according to the following mechanism:

$$\tilde{R}_{jt} = \alpha_j + \beta_h \tilde{R}_{mt} + \tilde{u}_{jt}, \tag{10}$$

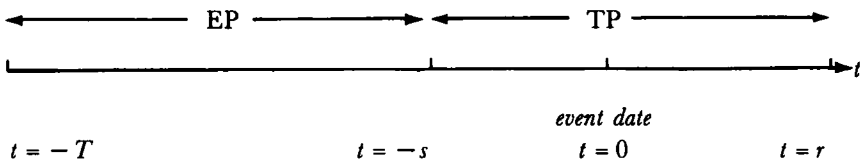
where $\tilde{u}_{jt}$ is a mean zero, independent disturbance term in period t.[3] Equation (10) partitions $\tilde{R}_{jt}$ into a systematic component linearly related to $\tilde{R}_{mt}$ and an unsystematic component, $\tilde{u}_{jt}$, which is uncorrelated with $\tilde{R}_{mt}$. The effect of firm-specific events is meant to be fully captured in the unsystematic component, the assumption being that the information signal and $\tilde{R}_{mt}$ are independent. Both α_j and β_j must be estimated here, resulting in a predicted abnormal return of:

$$\hat{u}_{jt} = R_{jt} - (\hat{\alpha}_j + \hat{\beta}_j R_{mt}). \tag{11}$$

Further motivation for employing the market model benchmark is that, in general, it results in smaller variances of abnormal returns (relative to raw returns), leading to more powerful statistical tests, and that it produces smaller correlations across security abnormal returns giving closer conformity to standard statistical tests (Beaver, 1981).

4. *Choice of estimation period and test period*

With all of the benchmarks A, C, D, and E above, the returns history for each security is usually divided into an *estimation period* (EP) and a *test period* (TP). The EP is used for estimating the parameters of the benchmark expected return. This allows predicted abnormal returns to be calculated within the TP. One form of this procedure is illustrated schematically below:



Here the EP spans $t = -T$ to $t = -s$ while the TP covers $t = -s, \ldots, 0, \ldots r$. Within this setting, the value of k_j in model A, for example, would be estimated over the $T-s$ observations in the EP.

In some studies the EP spans either side of the TP.[4] It is generally chosen as a period of time close to the TP but one in which the disclosure events under study are expected to have no effect on security prices. This is intended to allow parameter estimation to be made during a period when there are no persistent abnormal returns. (See Thompson II et al., 1988, for further discussion and possible refinements of this standard methodology.)

For estimation of the parameters of the market model, method D above, the number of observations used in practice has varied widely. For example, on daily data Lambert and Larcker (1985) used 60 observations, while Dodd et al. (1984) used 600 observations. On weekly data the number of observations tends to span between two and four years, while on monthly data five years is the norm. In practice, there is a trade-off between including more observations to increase statistical accuracy and not going too far forward or back from the TP in case the parameters of the return generating mechanism have shifted. Data availability considerations often constrain the choice.

Benchmark parameters are sometimes estimated unconditionally without excluding the TP (see Vermaelen, 1981, for an actual example using model D above). Thompson (1985) discusses the relationship between MM residuals analysis and classical econometric techniques and considers in detail whether market model parameters should be estimated conditionally or unconditionally. The appropriate procedure depends upon the presence of any causal link between market returns and event dates. Assuming that the true correlation between announcement dates and the market return is zero, estimating the MM parameters in non-announcement periods only, avoids introducing any spurious correlation into the estimates. Alternatively, if the true correlation is non-zero, then estimates of the MM parameters will be biased if the parameters are estimated unconditionally, excluding the TP. (Thompson, 1985, pp. 159–60).

5. *Choice of the market index*

With any of the methods B, C, D, or E for estimating abnormal returns, a market index must be selected. Market indices can differ in terms of the type and number of constituent securities, the weighting attached to securities, and how security prices are averaged to form the index. In addition some stock indices are just price indices while others incorporate a dividend adjustment and so are true return indices.

For methods employing a CAPM benchmark, the theoretically correct market index is a value-weighted index of the entire universe of capital assets. As pointed out by Roll (1977) such an index is practically unmeasurable.

The standard procedure adopted in practice has been determined largely by data availability, and involves selecting either a published value-weighted or equally-weighted arithmetic average index of equity securities. For example, Dimson and Marsh (1986) employ both the capitalisation weighted Financial Times Actuaries All Share Index, adjusted for dividend yield,[5] and an equally weighted index of returns on all UK listed shares.

6. *Accumulating abnormal returns over time*

Almost all event studies call for abnormal returns to be cumulated over a number of periods. This may be in order to fully capture the effect of an event on share prices, or to accommodate uncertainty over the exact date of the event. Alternatively, a number of studies analyse the relationship between accounting disclosures and the behaviour of security prices over a period leading up to the disclosure date. These studies are more accurately classified under the general area of *association studies* rather than as information content studies in that they are designed to test whether common information sets are reflected in the behaviour of security prices and the accounting measure disclosed. Apart from the choice of TP, the design of association studies is similar to the design of information content studies and, although this paper implicitly assumes the latter is the focus of interest, a number of the issues considered here apply equally to association studies.

A further reason for computing abnormal returns over a longer interval arises in some event studies from the need to specify an expectations benchmark for the accounting disclosure. For example, in examining the price reaction to earnings announcements any market reaction will only be in response to unexpected earnings. The period over which abnormal returns are analysed should therefore coincide with the period over which expectations concerning earnings are revised. A number of earnings announcement studies adopt a random walk model for the earnings generating mechanism taking the previous year's earnings as the expectations benchmark. Implicit within this assumption is that information about actual earnings may be gradually disclosed at any time over the prior twelve months but that *all* information is disclosed by the date of the announcement. This means that under the null hypothesis the expectations benchmark is only specified correctly one year prior to the earnings announcement. The appropriate period for analysing abnormal returns is therefore the entire twelve month period up to and including the earnings announcement.[6] In effect, this was the approach pioneered by Ball and Brown (1968).

The two most popular methods of accumulating abnormal returns over time are the Cumulative Abnormal Returns (CAR) and the Abnormal Performance Index (API). These are calculated as follows:

$$\text{CAR} = \sum_{t \in \text{TP}} \frac{1}{N} \sum_{j} \hat{u}_{jt} \tag{12}$$

(for an early example, see Fama et al., 1969),

$$\text{API} = \frac{1}{N} \sum_{j} \prod_{t \in \text{TP}} (1 + \hat{u}_{jt}) - 1 \tag{13}$$

(for an early example, see Ball and Brown, 1968).

The interpretation of these measures depends upon whether returns have been measured in continuous or discrete time (Ohlson, 1978; and Watts and Zimmerman, 1986). In continuous time the CAR represents the abnormal return on a portfolio that is rebalanced every period to give equal weighting in each security. In discrete time the API gives the abnormal return from initially investing equally in each security and then holding these securities over the cumulation period. (Roll, 1983; Blume and Stambaugh, 1983; and Dimson and Marsh, 1986, discuss the possible biases introduced by using the CAR method.)

EVIDENCE ON THE EFFICIENCY AND POWER OF THE METHODS

There have been a number of simulation studies of the various event study methodologies. The most influential of these have been a pioneering study by Brown and Warner (1980) on monthly data, and articles by Brown and Warner (1985), Dyckman et al. (1984) and Jain (1986), all extending the original Brown and Warner study to daily data.

In a typical simulation study abnormal returns are artificially added into the actual returns of securities. The researcher then analyses the likelihood that, in detecting abnormal returns, alternative event study methodologies lead to Type I and Type II errors; that is, that they will reject the null hypothesis when the abnormal return is zero, and that they will fail to reject the null when the abnormal return is non-zero. A first requirement for the use of any methodology is that it be well-specified, in the sense that Type I error rates should not be too far above or below the chosen significance level for the test.[7] Tests that result in excessive Type I errors are said to be *misspecified*. *Ceteris paribus*, a more *powerful* test is preferred (one that has a lower Type II error rate for a given Type I error rate).

Simulation studies have analysed the effect of varying a number of dimensions of event study designs including: (i) the expectations benchmark; (ii) the sample size of securities subject to the event; (iii) the size of the event window for cumulating abnormal returns; (v) the length of the EP; (vi) the weighting of the market index; (vii) the choice of statistic for testing information content, for example, whether it should be parametric or non-parametric; (viii) the measurement interval for computing returns; (ix) the degree of clustering of event dates in calendar time; and (x) the degree of clustering across other extra-market dimensions. All the published simulation studies to be considered below have been carried out on US data. Some circumspection may therefore be needed in applying the results to a UK setting.

On monthly data the main conclusion of Brown and Warner (BW) was the following:

> . . . a simple methodology based on the market model performs well under a wide variety of conditions. In some situations, even simpler methods which do not explicitly adjust for marketwide factors or for risk perform no worse than the market model.

In addition BW established the following results for their simulations:

(i) with a sample size of 50 and an abnormal return of one per cent the event's impact is unlikely to be detected using monthly data whatever the method used;
(ii) if the exact month of the event cannot be identified so that some form of event window has to be used (such as with CARs) then the power of any method to detect abnormal performance is drastically reduced;
(iii) using an equally-weighted index leads to more powerful tests than using a value-weighted index;

(vi) t-tests are reasonably well-specified; but certain non-parametric tests are not.

The third result above is due to the fact that on the randomly selected sample of securities that Brown and Warner study, returns are more highly correlated with an equally weighted index. This means that greater precision is achieved in measuring systematic risk against the market and in measuring residuals, with the result that abnormal performance is easier to detect.

On daily data BW conclude,

> . . . methodologies based on the OLS market model and using standard parametric tests are well-specified under a variety of conditions.

In addition, BW find that although daily security returns and abnormal returns typically depart from normality, mean abnormal returns across securities converge to normality as the sample size increases. Further results of Brown and Warner (1985) are referred to in the following section.

The findings of Dyckman, Philbrick and Stephan (DPS) on daily data reinforce the findings of BW. In particular, DPS find a slight preference for the market model over other procedures and they find that any non-normality of daily abnormal returns has little effect on event study tests.

Both Brown and Warner (1985) and Dyckman et al. (1984) find daily data result in more powerful test statistics than are found for the monthly data simulations in Brown and Warner (1980). For example comparing Table 3 in Brown and Warner (1980) with Table 3 in Brown and Warner (1985) it can be seen that with no event date uncertainty the rejection rates for the null hypothesis when the abnormal return is one per cent are of the order of three times greater with daily data than with monthly data.

The findings of BW and DPS on event studies employing monthly and daily data clearly show that accurately identifying announcement dates and concentrating on abnormal returns in as small an event window as possible, results in much more powerful hypothesis tests. However, this is not too surprising given the design of these simulation studies where an abnormal return is seeded into the actual return on a specific day.

TESTING PROCEDURES AND SPECIAL PROBLEMS CONCERNED WITH ESTIMATION

With the availability of daily stock return data, increased attention has been paid to the possible measurement problems involved in estimating market model parameters over shorter measurement intervals. These problems arise from the difficulty of observing 'true' stock prices and market index levels that are synchronous at the end of each measurement interval being used. In the late 1970s and early 1980s, a number of studies attempted to provide solutions to this problem.

Recent literature has also paid more careful attention to the assumptions implicitly underlying alternative statistical tests of information content and to the possible violation of these assumptions in particular applications. A large part of this literature has been devoted to the problem raised by cross-sectional correlation of the security abnormal performance measures. Both of these areas are considered in this section.

Thin Trading and the Intevalling Effect on Estimated Betas

Numerous studies have shown that the explanatory power of the market model regression equation and the mean cross-sectional value of beta, estimated from value-weighted indices, rise as the measurement interval increases. Dimson (1979) finds for the UK that the estimated betas of infrequently traded shares rise as the interval increases, while, to a lesser extent, the opposite holds for frequently traded shares. Similar results have been found for other countries.

Explanations for these effects have centred on price-adjustment delays and trading frictions which cause the observed returns on securities to depart from their true values. This can be as a result of infrequent trading, so that reported returns reflect dated transactions and are therefore non-synchronous across securities, or because frictions in the trading process cause adjustment lags in quotation prices. More detailed discussion of these effects is contained in Cohen et al. (1986). Price-adjustment delays result in an *error-in-variables* problem in the ordinary least squares market model regression equation resulting in biased and inconsistent beta estimates. In particular, infrequently traded shares have a beta estimate that is biased downwards, while for frequently traded shares the bias is upwards. This problem of bias will inevitably be exacerbated as the return measurement interval is reduced and will therefore be greatest with daily data. Biased beta estimates have the potential for resulting in biased estimates of abnormal returns and consequently misspecified test statistics in event studies. A number of methods for correcting for this bias have been proposed in the literature (Scholes and Williams, 1977; Dimson, 1979; and Cohen et al., 1983).

The *Scholes-Williams* (SW) *beta estimator* assumes that although trades are non-synchronous, a transaction takes place in every measurement interval; in addition it is assumed that price-adjustment delays arise only through non-synchronous trading so that an observed transaction price is the true price at the time of the transaction. SW derive the following estimator:

$$\hat{\beta}_{SW} = \frac{\hat{\beta}^{-1} + \hat{\beta}^{0} + \hat{\beta}^{+1}}{1 + 2\hat{\rho}_m}, \qquad (14)$$

where $\hat{\beta}^n$ is an estimator of the slope coefficient in a simple regression of the return on the security in period t against the return on the market in period $t + n$;

$\hat{\rho}_m$ is an estimate of the first-order serial correlation coefficient for the market index.

Under the stated assumptions, the SW beta estimator is a consistent estimator of the true beta.

The *Dimson aggregate coefficients* (DAC) *estimator* does not require that a trade takes place in every return interval. Dimson's formula is as follows:

$$\hat{\beta}_D = \sum_{k=-n}^{n} \hat{\beta}_k. \tag{15}$$

Here $\hat{\beta}_k$, $k = -n, \ldots, 0, \ldots, n$, are estimates of the slope coefficients in a multiple regression of the return on the security in period t against the return on the market in periods $t-n, \ldots, 0, \ldots, t+n$. Dimson and Marsh have employed this method with $k = -1, \ldots, 5$.[8]

Thin Trading and Event Studies

Both BW and DPS, in the studies previously referred to, report results for the impact on simulated event studies using daily data of correcting for thin trading. BW replicate all their experiments using the SW and DAC methods and also report results for a population of less frequently traded securities. They find that using either the SW or the DAC procedures results in reduced biases in OLS estimates of beta but results in no improvement in either the specification or the power of event study tests.

DPS perform an event study simulation separately on low-, medium- and high-trading volume populations. They find that the SW and DAC methods do not increase the ability to detect abnormal performance on daily returns for thinly traded securities. However, all of their samples remain biased towards larger, more actively traded firms.

These results may be peculiar to the sampling procedures employed in the two studies and so should perhaps be treated with some caution. The sample used in actual event studies will typically be non-random and correcting for thin-trading may affect the results. However, the insensitivity of event study results to the method used to estimate parameters has been reported by a number of researchers (Gheyara and Boatsman, 1980, p. 111; Dodd and Warner, 1983, p. 413 fn. 24; Linn and McConnell, 1983, p. 376 fn. 15; and Dopuch et al., 1986, p. 97 fn. 5). Moreover, as pointed out by Brown and Warner (1985), these findings are not inconsistent with the analyses of Scholes and Williams, Dimson, and Cohen et al. Although the OLS market model abnormal return may be biased for an individual security, in an event study, the bias in conditional abnormal returns may average out to zero in the sample.

Alternative Testing Procedures and Violation of Independence Assumptions

The most naive test procedure would be to calculate the average abnormal return and its standard error across event securities to give a t-statistic as follows:

$$\frac{\bar{u}_t}{\mathrm{SE}(\bar{u}_t)} = \frac{\frac{1}{N}\sum_{i=1}^{N} \hat{u}_{it}}{\left(\frac{1}{N-1}\sum_{i=1}^{N} (\hat{u}_{it} - \bar{u}_t)^2\right)^{1/2}} \sim t(N-1). \tag{16}$$

The corresponding t-test assumes independent drawings from an identically distributed normal population. It is therefore implicitly assumed that the mean effect of the event is identical across securities. In addition, no allowance is made for variances of abnormal returns being unequal across securities or for cross-correlation in abnormal returns. If abnormal returns exhibit either heteroscedasticity or cross-sectional dependence, then equally weighting abnormal returns as in equation (16) above, leads to inefficient estimates of the mean abnormal return, and calculating standing errors assuming cross-sectional independence leads to biases in the estimated standard errors; as a result, statistical significance tests are misspecified.

A more refined test procedure which has been used in a number of event studies is due to Patell (1976), and is often referred to as the *Patell Standardised Residual* (PSR) *Test*. The PSR is based on the market model and is constructed as follows. The abnormal return for security i in period $t\epsilon$TP is calculated according to the market model. Patell notes that when the parameters of the market model are estimated from observations outside the TP, abnormal returns are prediction errors rather than true residuals and should therefore be standardised according to the following formula:

$$V_{it} = \frac{\hat{u}_{it}}{s_i\sqrt{C_{it}}}, \tag{17}$$

where $s_i^2 = \dfrac{\sum_{t=1}^{T} \hat{u}_{it}^2}{T-2}$ is an estimate of the variance of the residuals during the EP;

$C_{it} = 1 + \dfrac{1}{T} + \dfrac{(R_{mt} - \bar{R}_m)^2}{\sum_t (R_{mt} - \bar{R}_m)^2}$ reflects the standard econometric adjustment for the increase in variance for prediction outside the EP;

T = the number of observations in the EP; and

$\bar{R}_m = \dfrac{1}{T}\sum_{t=1}^{T} R_{mt}.$

Summing the standardised abnormal returns across securities, a normalised sum can be formed which is distributed unit normal for large N:

$$Z_{Vt} = \frac{\sum_{i=1}^{N} V_{it}}{\left(\sum_{i=1}^{N} \frac{T_i - 2}{T_i - 4} \right)^{1/2}} \sim \mathrm{N}(0,1), \tag{18}$$

where T_i = the number of EP observations for security i.

A similar test can be constructed on the cumulative abnormal returns:

$$\mathrm{CAR}_i = \frac{1}{\sqrt{L}} \sum_{t=1}^{L} \frac{u_{it}}{s_i \sqrt{C_{it}}}, \tag{19}$$

where L = the number of observations cumulated in the TP (see Patell, 1976, pp. 256–7).

The PSR test explicitly recognises the possibility of different residual variances across securities, and weights the abnormal returns accordingly. But, as Patell notes, the PSR test continues to assume cross-sectional independence of abnormal returns and no change in residual variances between the EP and the TP.

Evidence that an assumption of unchanging residual variances may be unrealistic for events such as earnings releases is contained in Beaver (1968), Patell and Wolfson (1979) and Daley et al. (1988). Collins and Dent (1984) suggest a procedure that allows for a constant multiplicative change in residual variance from the EP to the TP.

Cases where cross-sectional dependencies in abnormal returns are *not* likely to pose inference problems occur in event studies where the event of interest is spread diffusely over a long period in calendar time for different securities. Averaging the abnormal returns across securities in event time in this case effectively eliminates any residual price variation that is unrelated to the particular type of event of interest. However, even in this case, some care should be taken over the design of the benchmark for measuring abnormal returns. Failure to make market or risk adjustments or to control for other factors in the return generating mechanism whilst continuing to assume cross-sectional independence of abnormal returns could induce spurious inferences in particular samples.

The situation where an assumption of cross-sectional independence of abnormal returns is most likely to be violated is where firms have contemporaneous event dates in calendar time. This is often referred to as the problem of *event-date clustering*. Such a phenomenon arises naturally in certain studies such as tests of the information content of regulatory changes that (potentially) affect all event securities at the same time. The problem may be exacerbated when the event securities are clustered along a further dimension such as industry or size.

A number of studies when faced with the problem of cross-sectional correlation due to contemporaneous event dates have adopted a procedure originally employed by Jaffe (1974) and Mandelker (1974). Under this procedure, for each time period an equally weighted portfolio is formed of those securities that are subject to an event during that calendar time period. A portfolio abnormal return is calculated for each period in the TP as:

$$\hat{u}_{pt} = \frac{1}{N} \sum_{i \epsilon p} \hat{u}_{it} . \tag{20}$$

The TP will include all calendar periods in which any sample security experienced an event. The TP portfolio abnormal returns are then standardised by dividing by their estimated standard deviation calculated over the EP as follows:

$$\mathrm{SE}(\hat{u}_{pt}) = \sqrt{\frac{1}{T-1} \sum_{t \epsilon \mathrm{TP}} (\hat{u}_{pt} - \bar{u}_p)^2} \tag{21}$$

where $\bar{u}_p = \frac{1}{T} \sum_t \hat{u}_{pt} \quad t = 1, \ldots, T \epsilon \mathrm{TP}.$

This estimate of the standard deviation of the portfolio residuals directly takes into account cross-sectional dependence between residuals of securities within each portfolio in a given period (for a formal development of this see Collins and Dent, 1984). If abnormal returns across different portfolios are assumed to be cross-sectionally uncorrelated, the standardised portfolio abnormal return can then be averaged across N portfolios to give a t-statistic (or z-statistic for large N).

Recent Developments

Recent empirical and analytical approaches to the problem of cross-sectional dependence in abnormal returns have tended increasingly to adopt so-called *systems methods* involving the pooling of time series and cross-sectional data using joint generalised least squares (GLS) estimation techniques in a multiple regression approach to event studies (see, for example, Binder, 1985; Thompson, 1985; Malatesta, 1986; McDonald, 1987; and Schipper and Thompson, 1983). Collins and Dent (1984) provide an earlier analysis while Bernard (1987) provides the most complete discussion of the problem in this context. In applications, the choice of technique involves a trade-off between, on the one hand, the degree of violation of the OLS assumptions, where GLS is more efficient than OLS if the residual covariance is known, and on the other hand, the problem of having to estimate the residual covariance matrix and subsequently employing estimated GLS. Preliminary evidence from simulation studies (Malatesta, 1986; and McDonald, 1987) suggests that OLS works as well as estimated GLS.[9,10]

A small number of papers have recently adopted a testing procedure based on *empirical distributions* (see, for example, Foster et al., 1984; Marais, 1984; and, for an example within a simulation study, Kothari and Wasley, 1989). Significance tests based on the empirical distribution of abnormal returns for the sample securities do not require any distributional assumption such as normality, and they take into account cross-sectional dependencies and differences in residual variances across securities.

CONTROLLING FOR EXTRA-MARKET FACTORS

In practice the majority of event studies have computed abnormal returns using a CAPM or market model benchmark. Implicit in the use of either of these benchmarks is an assumption that the only systematic factor involved in the security generating mechanism is the return on the market. Support for this approach on randomly selected samples of securities has been provided by the simulation studies of BW and DPS.

However, since the mid-1970s various studies have noted a series of non-market regularities in security returns suggesting the possibility that extra-market factors may be present in the return generating process and that standard event study benchmarks may be misspecified.

As well as the size effect, to be considered further below, extra-market factors that have been documented as possibly entering the return generating mechanism are dividend yield, industry classification, systematic (beta) risk, and residual risk. Event studies may give rise to spurious results to the extent that the sample securities are unrepresentative relative to the market index across any of these dimensions. Other empirical regularities or anomalies that could potentially affect event studies are seasonal effects such as the January effect (the tendency for the size effect to be disproportionately concentrated in January) found for the US, and the weekend effect, found for a number of stock markets.

The empirical regularity that has received most attention as having a potential impact upon event studies is the *size effect*. This refers to the finding for a number of countries that smaller capitalisation stocks have higher risk-adjusted returns, on average, than large capitalisation stocks. In the presence of a size effect, event studies that focus on smaller firms are likely to register positive abnormal returns relative to the market index, even in the absence of an event; the opposite result would hold for larger firms.

Dimson and Marsh (1986 and 1988) provide a clear and comprehensive analysis of the potential impact of the size effect on event study methodologies. Their approach to controlling for the size effect is also used by Foster et al. (1984) and receives further support from the recent simulation study of Kothari and Wasley (1989). Dimson and Marsh (1986) point out that if the benchmark expected return fails to reflect the size effect then estimates of abnormal returns will be biased for both small and large capitalisation stocks. Despite this, the bias in the mean abnormal return across securities may be eliminated if the event securities match the market index in terms of capitalisation. In addition, if the TP for cumulating abnormal returns is very short (a few days or a single month), the bias introduced by misspecifying the benchmark is likely to be small relative to any event-related return and noise.

However, the implication of Schwert (1983) that employing the market model will automatically control for the size effect is unlikely to hold true. Market model alphas capture the mean non-market return on a security. Therefore only if the size effect is constant through time and the size of the event study securities are constant between the estimation and test periods will market model alphas control for the size effect. But evidence suggests that the size effect has not been constant (Keim, 1983; and Levis, 1985). If there is a systematic difference between the estimation and test periods in their coverage of calendar time, or if event security sizes exhibit non-stationarity between the EP and TP then biased abnormal return measures will result even with the market model. The bias can be expected to be greater; (i) the longer the TP for cumulating abnormal returns; (ii) the larger or more volatile the size effect; (iii) the more unrepresentative the event securities are in size or weighting from the market index; or (iv) if a CAPM rather than a market model benchmark is employed.

Controlling for Size

Both Foster et al. (1984) and Dimson and Marsh (1986) employ a size control portfolio as a benchmark for computing abnormal returns. With this approach the predicted abnormal return for security j in period t is given as:

$$\hat{u}_{jt} = R_{jt} - R_{pt}, \tag{22}$$

where R_{pt} is the equally weighted mean return on a portfolio of stocks in the same size decile as firm j in period t. Dimson and Marsh (1986) also adjust this benchmark for differences in beta risk but this has a negligible effect on their results.

Kothari and Wasley (1989) have performed a simulation study of event study methodologies when the event securities are drawn from a population of firms that is exclusively small or exclusively large. They find that conventional t-tests using market-adjusted or market model measures of abnormal performance result in excessive Type I errors and so are misspecified when there is event date clustering and sample securities are exclusively large (small) firms. However, when applying the size control portfolio approach to this situation they conclude as follows:

> . . . For event studies in which control for the firm size effect is warranted . . . a conventional t-test based upon size control portfolio abnormal returns is valid [well-specified] and of equal or greater power than alternative testing procedures.

CONCLUSIONS

From this review of event study methodologies, the broad conclusions that can be drawn about the current state of the art in event-study research design are as follows:

1. If the sample securities have no unrepresentative exposure to extra-market factors and event dates are diffusely spread out in calendar time for the sample securities, then calculating abnormal returns using the ordinary least squares market model and using standard parametric statistical tests appears to be a well-specified procedure.
2. Where event-date clustering is a problem then some correction for cross-sectional dependence should be used.
3. Special care should be taken to check whether the sample of event securities is unrepresentative across any extra-market dimension, in particular firm size, and, if necessary, some form of control portfolio approach should be employed.
4. The ability to detect information content in an event study may be considerably enhanced if the precise event day for the sample securities can be established. Simultaneously, this reduces the possible effects of omitting non-market factors from the security return generating mechanism. In many event studies in practice, accuracy of event dates is likely to be more important than sophistication in modelling or statistical techniques.

NOTES

1 The empirical accounting literature has also seen a smaller number of studies that analyse the trading volume reaction to events (see, for example, Beaver, 1986; Morse, 1981; Bamber, 1986 and 1987; and for a review, Yadav, 1991). Event studies might therefore be more accurately separated into price based and trading volume based event studies. However, price based event studies represent the vast majority of published research and are the focus of event studies in this paper.

2 A number of studies, in testing for information content, have considered the variance of the return distribution (for example, Beaver, 1968; and Gheyara and Boatsman, 1980). Much of this paper applies equally to such studies.

3 The absence of any explicit equilibrium assumption raises problems in interpreting residual returns as 'abnormal' (see, for example, Ohlson, 1978). Alternatively, the MM can be interpreted within the framework of a one-factor (equilibrium) arbitrage pricing model.

4 In studies employing the MM, for example, this procedure is sometimes adopted as a crude control for the possibility of shifts in β over time.

5 The dividend yield corresponding to the All Share Index that is supplied by the Financial Times Actuaries is calculated using the last declared annual dividends of the constituent companies (FTBI, 1982). It is not therefore an accurate measure of the gross dividend yield of companies in the All Share Index that have gone *ex div* in the period over which returns are being calculated. Instead it is an artificially smoothed dividend yield series.

6 Similar considerations arise for example in event studies of takeovers where, to capture the full share price effect of a takeover on either the bidder or target company, the TP should start at the date where the bid was first anticipated by the market. This suggests a further problem,

which may particularly affect studies of events such as takeovers, of choosing the precise day that the bid was 'announced' to the market. If there is any difference across companies in the degree of anticipation or knowledge of the event by the market, then lining up companies' test period abnormal returns relative to the 'official' announcement date will inevitably weaken the power of any test to detect significant abnormal returns.

7 The criterion usually chosen is that Type I error rates are within a 95 per cent confidence interval for the nominal significance level of the test.

8 The formula given by equation (15) has been shown to contain an error (Cohen et al., 1983; and Fowler and Rorke, 1983). Cohen et al. (1986) have derived a generalised version of the SW estimator which applies where the price-adjustment delay lasts *n* periods and which also corrects the DAC estimator.

9 See Froot (1989) for more recent results on estimating the OLS covariance matrix consistently in the presence of cross-sectional dependence and heteroscedasticity.

10 Chandra and Balachandran (1990) synthesise the theoretical issues involved in event studies subject to cross-sectionally correlated data (see also the discussion by Bernard, 1990). They also provide evidence that apparently more sophisticated GLS methods are particularly sensitive to model misspecification and are therefore less robust than simpler testing procedures.

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