4
Event-Study Analysis

ECONOMISTS ARE FREQUENTLY ASKED to measure the effect of an economic event on the value of a firm. On the surface this seems like a difficult task, but a measure can be constructed easily using financial market data in an event study. The usefulness of such a study comes from the fact that, given rationality in the marketplace, the effect of an event will be reflected immediately in asset prices. Thus the event's economic impact can be measured using asset prices observed over a relatively short time period. In contrast, direct measures may require many months or even years of observation.

The general applicability of the event-study methodology has led to its wide use. In the academic accounting and finance field, event-study methodology has been applied to a variety of firm-specific and economy-wide events. Some examples include mergers and acquisitions, earnings announcements, issues of new debt or equity, and announcements of macroeconomic variables such as the trade deficit.[1] However, applications in other fields are also abundant. For example, event studies are used in the field of law and economics to measure the impact on the value of a firm of a change in the regulatory environment,[2] and in legal-liability cases event studies are used to assess damages.[3] In most applications, the focus is the effect of an event on the price of a particular class of securities of the firm, most often common equity. In this chapter the methodology will be discussed in terms of common stock applications. However, the methodology can be applied to debt securities with little modification.

Event studies have a long history. Perhaps the first published study is Dolley (1933). Dolley examined the price effects of stock splits, studying nominal price changes at the time of the split. Using a sample of 95 splits

[1]We will further discuss the first three examples later in the chapter. McQueen and Roley (1993) provide an illustration using macroeconomic news announcements.

[2]See Schwert (1981).

[3]See Mitchell and Netter (1994).

from 1921 to 1931, he found that the price increased in 57 of the cases and the price declined in only 26 instances. There was no effect in the other 12 cases. Over the decades from the early 1930s until the late 1960s the level of sophistication of event studies increased. Myers and Bakay (1948), Baker (1956, 1957, 1958), and Ashley (1962) are examples of studies during this time period. The improvements include removing general stock market price movements and separating out confounding events. In the late 1960s seminal studies by Ball and Brown (1968) and Fama, Fisher, Jensen, and Roll (1969) introduced the methodology that is essentially still in use today. Ball and Brown considered the information content of earnings, and Fama, Fisher, Jensen, and Roll studied the effects of stock splits after removing the effects of simultaneous dividend increases.

In the years since these pioneering studies, several modifications of the basic methodology have been suggested. These modifications handle complications arising from violations of the statistical assumptions used in the early work, and they can accommodate more specific hypotheses. Brown and Warner (1980, 1985) are useful papers that discuss the practical importance of many of these modifications. The 1980 paper considers implementation issues for data sampled at a monthly interval and the 1985 paper deals with issues for daily data.

This chapter explains the econometric methodology of event studies. Section 4.1 briefly outlines the procedure for conducting an event study. Section 4.2 sets up an illustrative example of an event study. Central to any event study is the measurement of the abnormal return. Section 4.3 details the first step—measuring the normal performance—and Section 4.4 follows with the necessary tools for calculating the abnormal return, making statistical inferences about these returns, and aggregating over many event observations. In Sections 4.3 and 4.4 the discussion maintains the null hypothesis that the event has no impact on the distribution of returns. Section 4.5 discusses modifying the null hypothesis to focus only on the mean of the return distribution. Section 4.6 analyzes of the power of an event study. Section 4.7 presents a nonparametric approach to event studies which eliminates the need for parametric structure. In some cases theory provides hypotheses concerning the relation between the magnitude of the event abnormal return and firm characteristics. In Section 4.8 we consider cross-sectional regression models which are useful to investigate such hypotheses. Section 4.9 considers some further issues in event-study design and Section 4.10 concludes.

4.1 Outline of an Event Study

At the outset it is useful to give a brief outline of the structure of an event study. While there is no unique structure, the analysis can be viewed

as having seven steps:

1. *Event definition.* The initial task of conducting an event study is to define the event of interest and identify the period over which the security prices of the firms involved in this event will be examined—the *event window.* For example, if one is looking at the information content of an earnings announcement with daily data, the event will be the earnings announcement and the event window might be the one day of the announcement. In practice, the event window is often expanded to two days, the day of the announcement and the day after the announcement. This is done to capture the price effects of announcements which occur after the stock market closes on the announcement day. The period prior to or after the event may also be of interest and included separately in the analysis. For example, in the earnings-announcement case, the market may acquire information about the earnings prior to the actual announcement and one can investigate this possibility by examining pre-event returns.
2. *Selection criteria.* After identifying the event of interest, it is necessary to determine the selection criteria for the inclusion of a given firm in the study. The criteria may involve restrictions imposed by data availability such as listing on the NYSE or AMEX or may involve restrictions such as membership in a specific industry. At this stage it is useful to summarize some characteristics of the data sample (e.g., firm market capitalization, industry representation, distribution of events through time) and note any potential biases which may have been introduced through the sample selection.
3. *Normal and abnormal returns.* To appraise the event's impact we require a measure of the abnormal return. The abnormal return is the actual *ex post* return of the security over the event window minus the normal return of the firm over the event window. The normal return is defined as the return that would be expected if the event did not take place. For each firm i and event date τ we have

$$\epsilon^*_{i\tau} = R_{i\tau} - \mathrm{E}[R_{i\tau} \mid X_\tau], \tag{4.1.1}$$

where $\epsilon^*_{i\tau}$, $R_{i\tau}$, and $\mathrm{E}(R_{i\tau})$ are the abnormal, actual, and normal returns, respectively, for time period t. X_t is the conditioning information for the normal performance model. There are two common choices for modeling the normal return—the *constant-mean-return model* where X_t is a constant, and the *market model* where X_t is the market return. The constant-mean-return model, as the name implies, assumes that the mean return of a given security is constant through time. The market model assumes a stable linear relation between the market return and the security return.

4. *Estimation procedure.* Once a normal performance model has been selected, the parameters of the model must be estimated using a subset of the data known as the *estimation window.* The most common choice, when feasible, is to use the period prior to the event window for the estimation window. For example, in an event study using daily data and the market model, the market-model parameters could be estimated over the 120 days prior to the event. Generally the event period itself is not included in the estimation period to prevent the event from influencing the normal performance model parameter estimates.
5. *Testing procedure.* With the parameter estimates for the normal performance model, the abnormal returns can be calculated. Next, we need to design the testing framework for the abnormal returns. Important considerations are defining the null hypothesis and determining the techniques for aggregating the abnormal returns of individual firms.
6. *Empirical results.* The presentation of the empirical results follows the formulation of the econometric design. In addition to presenting the basic empirical results, the presentation of diagnostics can be fruitful. Occasionally, especially in studies with a limited number of event observations, the empirical results can be heavily influenced by one or two firms. Knowledge of this is important for gauging the importance of the results.
7. *Interpretation and conclusions.* Ideally the empirical results will lead to insights about the mechanisms by which the event affects security prices. Additional analysis may be included to distinguish between competing explanations.

4.2 An Example of an Event Study

The Financial Accounting Standards Board (FASB) and the Securities Exchange Commission strive to set reporting regulations so that financial statements and related information releases are informative about the value of the firm. In setting standards, the information content of the financial disclosures is of interest. Event studies provide an ideal tool for examining the information content of the disclosures.

In this section we describe an example selected to illustrate the event-study methodology. One particular type of disclosure—quarterly earnings announcements—is considered. We investigate the information content of quarterly earnings announcements for the thirty firms in the Dow Jones Industrial Index over the five-year period from January 1988 to December 1993. These announcements correspond to the quarterly earnings for the last quarter of 1987 through the third quarter of 1993. The five years of data for thirty firms provide a total sample of 600 announcements. For each firm and quarter, three pieces of information are compiled: the date of the announcement, the actual announced earnings, and a measure of the expected earnings. The source of the date of the announcement is Datastream, and the source of the actual earnings is Compustat.

If earnings announcements convey information to investors, one would expect the announcement impact on the market's valuation of the firm's equity to depend on the magnitude of the unexpected component of the announcement. Thus a measure of the deviation of the actual announced earnings from the market's prior expectation is required. We use the mean quarterly earnings forecast from the Institutional Brokers Estimate System (I/B/E/S) to proxy for the market's expectation of earnings. I/B/E/S compiles forecasts from analysts for a large number of companies and reports summary statistics each month. The mean forecast is taken from the last month of the quarter. For example, the mean third-quarter forecast from September 1990 is used as the measure of expected earnings for the third quarter of 1990.

In order to examine the impact of the earnings announcement on the value of the firm's equity, we assign each announcement to one of three categories: good news, no news, or bad news. We categorize each announcement using the deviation of the actual earnings from the expected earnings. If the actual exceeds expected by more than 2.5% the announcement is designated as good news, and if the actual is more than 2.5% less than expected the announcement is designated as bad news. Those announcements where the actual earnings is in the 5% range centered about the expected earnings are designated as no news. Of the 600 announcements, 189 are good news, 173 are no news, and the remaining 238 are bad news.

With the announcements categorized, the next step is to specify the sampling interval, event window, and estimation window that will be used to analyze the behavior of firms' equity returns. For this example we set the sampling interval to one day; thus daily stock returns are used. We choose a 41-day event window, comprised of 20 pre-event days, the event day, and 20 post-event days. For each announcement we use the 250-trading-day period prior to the event window as the estimation window. After we present the methodology of an event study, we use this example as an illustration.

4.3 Models for Measuring Normal Performance

A number of approaches are available to calculate the normal return of a given security. The approaches can be loosely grouped into two categories—statistical and economic. Models in the first category follow from statistical assumptions concerning the behavior of asset returns and do not depend on any economic arguments. In contrast, models in the second category rely on assumptions concerning investors' behavior and are not based solely on statistical assumptions. It should, however, be noted that to use economic models in practice it is necessary to add statistical assumptions. Thus the potential advantage of economic models is not the absence of statistical assumptions, but the opportunity to calculate more precise measures of the normal return using economic restrictions.

For the statistical models, it is conventional to assume that asset returns are jointly multivariate normal and independently and identically distributed through time. Formally, we have:

(A1) Let $\mathbf{R}_t$ *be an* $(N\times 1)$ *vector of asset returns for calendar time period t.* $\mathbf{R}_t$ *is independently multivariate normally distributed with mean* μ *and covariance matrix* Ω *for all t.*

This distributional assumption is sufficient for the constant-mean-return model and the market model to be correctly specified and permits the development of exact finite-sample distributional results for the estimators and statistics. Inferences using the normal return models are robust to deviations from the assumption. Further, we can explicitly accommodate deviations using a generalized method of moments framework.

4.3.1 Constant-Mean-Return Model

Let μ_i, the ith element of μ, be the mean return for asset i. Then the constant-mean-return model is

$$\begin{aligned} R_{it} &= \mu_i + \xi_{it} \qquad (4.3.1)\\ \mathrm{E}[\xi_{it}] = 0 \qquad \mathrm{Var}[\xi_{it}] &= \sigma^2_{\xi_i}, \end{aligned}$$

where R_{it}, the ith element of $\mathbf{R}_t$, is the period-t return on security i, ξ_{it} is the disturbance term, and $\sigma^2_{\xi_i}$ is the (i, i) element of Ω.

Although the constant-mean-return model is perhaps the simplest model, Brown and Warner (1980, 1985) find it often yields results similar to those of more sophisticated models. This lack of sensitivity to the model choice can be attributed to the fact that the variance of the abnormal return is frequently not reduced much by choosing a more sophisticated model. When using daily data the model is typically applied to nominal returns. With monthly data the model can be applied to real returns or excess returns (the return in excess of the nominal riskfree return generally measured using the US Treasury bill) as well as nominal returns.

4.3.2 Market Model

The market model is a statistical model which relates the return of any given security to the return of the market portfolio. The model's linear specification follows from the assumed joint normality of asset returns.[4] For any security i we have

$$\begin{aligned} R_{it} &= \alpha_i + \beta_i R_{mt} + \epsilon_{it} \qquad (4.3.2)\\ \mathrm{E}[\epsilon_{it}] = 0 \qquad \mathrm{Var}[\epsilon_{it}] &= \sigma^2_{\epsilon_i}, \end{aligned}$$

where R_{it} and R_{mt} are the period-t returns on security i and the market portfolio, respectively, and ϵ_{it} is the zero mean disturbance term. α_i, β_i, and $\sigma^2_{\epsilon_i}$ are the parameters of the market model. In applications a broad-based stock index is used for the market portfolio, with the S&P500 index, the CRSP value-weighted index, and the CRSP equal-weighted index being popular choices.

The market model represents a potential improvement over the constant-mean-return model. By removing the portion of the return that is related to variation in the market's return, the variance of the abnormal return is reduced. This can lead to increased ability to detect event effects. The benefit from using the market model will depend upon the R^2 of the market-model regression. The higher the R^2, the greater is the variance reduction of the abnormal return, and the larger is the gain. See Section 4.4.4 for more discussion of this point.

4.3.3 Other Statistical Models

A number of other statistical models have been proposed for modeling the normal return. A general type of statistical model is the *factor model*. Factor models potentially provide the benefit of reducing the variance of the abnormal return by explaining more of the variation in the normal return. Typically the factors are portfolios of traded securities. The market model is an example of a one-factor model, but in a multifactor model one might include industry indexes in addition to the market. Sharpe (1970) and Sharpe, Alexander, and Bailey (1995) discuss index models with factors based on industry classification. Another variant of a factor model is a procedure which calculates the abnormal return by taking the difference between the actual return and a portfolio of firms of similar size, where size is measured by market value of equity. In this approach typically ten size groups are considered and the loading on the size portfolios is restricted

[4]The specification actually requires the asset weights in the market portfolio to remain constant. However, changes over time in the market portfolio weights are small enough that they have little effect on empirical work.

to unity. This procedure implicitly assumes that expected return is directly related to the market value of equity.

In practice the gains from employing multifactor models for event studies are limited. The reason for this is that the marginal explanatory power of additional factors beyond the market factor is small, and hence there is little reduction in the variance of the abnormal return. The variance reduction will typically be greatest in cases where the sample firms have a common characteristic, for example they are all members of one industry or they are all firms concentrated in one market capitalization group. In these cases the use of a multifactor model warrants consideration.

Sometimes limited data availability may dictate the use of a restricted model such as the *market-adjusted-return model.* For some events it is not feasible to have a pre-event estimation period for the normal model parameters, and a market-adjusted abnormal return is used. The market-adjusted-return model can be viewed as a restricted market model with α_i constrained to be 0 and β_i constrained to be 1. Since the model coefficients are prespecified, an estimation period is not required to obtain parameter estimates. This model is often used to study the underpricing of initial public offerings.[5] A general recommendation is to use such restricted models only as a last resort, and to keep in mind that biases may arise if the restrictions are false.

4.3.4 Economic Models

Economic models restrict the parameters of statistical models to provide more constrained normal return models. Two common economic models which provide restrictions are the Capital Asset Pricing Model (CAPM) and exact versions of the Arbitrage Pricing Theory (APT). The CAPM, due to Sharpe (1964) and Lintner (1965b), is an equilibrium theory where the expected return of a given asset is a linear function of its covariance with the return of the market portfolio. The APT, due to Ross (1976), is an asset pricing theory where in the absence of asymptotic arbitrage the expected return of a given asset is determined by its covariances with multiple factors. Chapters 5 and 6 provide extensive treatments of these two theories.

The Capital Asset Pricing Model was commonly used in event studies during the 1970s. During the last ten years, however, deviations from the CAPM have been discovered, and this casts doubt on the validity of the restrictions imposed by the CAPM on the market model. Since these restrictions can be relaxed at little cost by using the market model, the use of the CAPM in event studies has almost ceased.

Some studies have used multifactor normal performance models motivated by the Arbitrage Pricing Theory. The APT can be made to fit the cross-section of mean returns, as shown by Fama and French (1996a) and others, so a properly chosen APT model does not impose false restrictions on mean returns. On the other hand the use of the APT complicates the implementation of an event study and has little practical advantage relative to the unrestricted market model. See, for example, Brown and Weinstein (1985). There seems to be no good reason to use an economic model rather than a statistical model in an event study.

[5]See Ritter (1990) for an example.

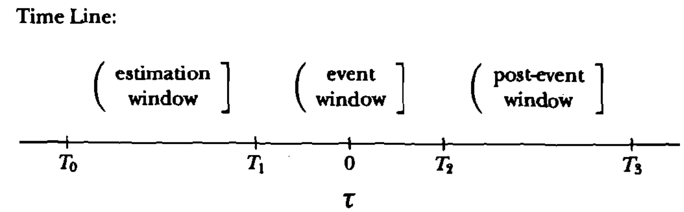

Figure 4.1. Time Line for an Event Study

4.4 Measuring and Analyzing Abnormal Returns

In this section we consider the problem of measuring and analyzing abnormal returns. We use the market model as the normal performance return model, but the analysis is virtually identical for the constant-mean-return model.

We first define some notation. We index returns in event time using τ. Defining $\tau = 0$ as the event date, $\tau = T_1 + 1$ to $\tau = T_2$ represents the event window, and $\tau = T_0 + 1$ to $\tau = T_1$ constitutes the estimation window. Let $L_1 = T_1 - T_0$ and $L_2 = T_2 - T_1$ be the length of the estimation window and the event window, respectively. If the event being considered is an announcement on a given date then $T_2 = T_1 + 1$ and $L_2 = 1$. If applicable, the post-event window will be from $\tau = T_2 + 1$ to $\tau = T_3$ and its length is $L_3 = T_3 - T_2$. The timing sequence is illustrated on the time line in Figure 4.1.

We interpret the abnormal return over the event window as a measure of the impact of the event on the value of the firm (or its equity). Thus, the methodology implicitly assumes that the event is exogenous with respect to the change in market value of the security. In other words, the revision in value of the firm is caused by the event. In most cases this methodology is appropriate, but there are exceptions. There are examples where an event is triggered by the change in the market value of a security, in which case the event is endogenous. For these cases, the usual interpretation will be incorrect.

It is typical for the estimation window and the event window not to overlap. This design provides estimators for the parameters of the normal return model which are not influenced by the event-related returns. Including the event window in the estimation of the normal model parameters could lead to the event returns having a large influence on the normal return measure. In this situation both the normal returns and the abnormal returns would reflect the impact of the event. This would be problematic since the methodology is built around the assumption that the event impact is captured by the abnormal returns. In Section 4.5 we consider expanding the null hypothesis to accommodate changes in the risk of a firm around the event. In this case an estimation framework which uses the event window returns will be required.

4.4.1 Estimation of the Market Model

Recall that the market model for security i and observation τ in event time is

$$R_{i\tau} = \alpha_i + \beta_i R_{m\tau} + \epsilon_{i\tau}. \tag{4.4.1}$$

The estimation-window observations can be expressed as a regression system,

$$\mathbf{R}_i = \mathbf{X}_i\boldsymbol{\theta}_i + \boldsymbol{\epsilon}_i, \tag{4.4.2}$$

where $\mathbf{R}_i = [R_{iT_0+1} \cdots R_{iT_1}]'$ is an $(L_1 \times 1)$ vector of estimation-window returns, $\mathbf{X}_i = [\boldsymbol{\iota}\ \mathbf{R}_m]$ is an $(L_1 \times 2)$ matrix with a vector of ones in the first column and the vector of market return observations $\mathbf{R}_m = [R_{mT_0+1} \cdots R_{mT_1}]'$ in the second column, and $\boldsymbol{\theta}_i = [\alpha_i\ \beta_i]'$ is the (2×1) parameter vector. $\mathbf{X}$ has a subscript because the estimation window may have timing that is specific to firm i. Under general conditions ordinary least squares (OLS) is a consistent estimation procedure for the market-model parameters. Further, given the assumptions of Section 4.3, OLS is efficient. The OLS estimators of the market-model parameters using an estimation window of L_1 observations are

$$\hat{\boldsymbol{\theta}}_i = (\mathbf{X}_i'\mathbf{X}_i)^{-1}\mathbf{X}_i'\mathbf{R}_i \tag{4.4.3}$$

$$\hat{\sigma}^2_{\epsilon_i} = \frac{1}{L_1 - 2}\hat{\boldsymbol{\epsilon}}_i'\hat{\boldsymbol{\epsilon}}_i \tag{4.4.4}$$

$$\hat{\boldsymbol{\epsilon}}_i = \mathbf{R}_i - \mathbf{X}_i\hat{\boldsymbol{\theta}}_i \tag{4.4.5}$$

$$\mathrm{Var}[\hat{\boldsymbol{\theta}}_i] = (\mathbf{X}_i'\mathbf{X}_i)^{-1}\sigma^2_{\epsilon_i}. \tag{4.4.6}$$

We next show how to use these OLS estimators to measure the statistical properties of abnormal returns. First we consider the abnormal return properties of a given security and then we aggregate across securities.

4.4.2 Statistical Properties of Abnormal Returns

Given the market-model parameter estimates, we can measure and analyze the abnormal returns. Let $\hat{\boldsymbol{\epsilon}}^*_i$ be the $(L_2 \times 1)$ sample vector of abnormal returns for firm i from the event window, $T_1 + 1$ to T_2. Then using the market model to measure the normal return and the OLS estimators from (4.4.3), we have for the abnormal return vector:

$$\begin{aligned}\hat{\boldsymbol{\epsilon}}^*_i &= \mathbf{R}^*_i - \hat{\alpha}_i\boldsymbol{\iota} - \hat{\beta}_i\mathbf{R}^*_m \\ &= \mathbf{R}^*_i - \mathbf{X}^*_i\hat{\boldsymbol{\theta}}_i,\end{aligned} \tag{4.4.7}$$

where $\mathbf{R}^*_i = [R_{iT_1+1} \cdots R_{iT_2}]'$ is an $(L_2 \times 1)$ vector of event-window returns, $\mathbf{X}^*_i = [\boldsymbol{\iota}\ \mathbf{R}^*_m]$ is an $(L_2 \times 2)$ matrix with a vector of ones in the first column and the vector of market return observations $\mathbf{R}^*_m = [R_{mT_1+1} \cdots R_{mT_2}]'$ in the second column, and $\hat{\boldsymbol{\theta}}_i = [\hat{\alpha}_i\ \hat{\beta}_i]'$ is the (2×1) parameter vector estimate. Conditional on the market return over the event window, the abnormal returns will be jointly normally distributed with a zero conditional mean and conditional covariance matrix $\mathbf{V}_i$ as shown in (4.4.8) and (4.4.9), respectively.

$$\begin{aligned}\mathrm{E}[\hat{\boldsymbol{\epsilon}}^*_i \mid \mathbf{X}^*_i] &= \mathrm{E}[\mathbf{R}^*_i - \mathbf{X}^*_i\hat{\boldsymbol{\theta}}_i \mid \mathbf{X}^*_i] \\ &= \mathrm{E}[(\mathbf{R}^*_i - \mathbf{X}^*_i\boldsymbol{\theta}_i) - \mathbf{X}^*_i(\hat{\boldsymbol{\theta}}_i - \boldsymbol{\theta}_i) \mid \mathbf{X}^*_i] \\ &= 0. \qquad (4.4.8)\\ \mathbf{V}_i &= \mathrm{E}[\hat{\boldsymbol{\epsilon}}^*_i\hat{\boldsymbol{\epsilon}}^{*\prime}_i \mid \mathbf{X}^*_i] \\ &= \mathrm{E}\big[[\boldsymbol{\epsilon}^*_i - \mathbf{X}^*_i(\hat{\boldsymbol{\theta}}_i - \boldsymbol{\theta}_i)][\boldsymbol{\epsilon}^*_i - \mathbf{X}^*_i(\hat{\boldsymbol{\theta}}_i - \boldsymbol{\theta}_i)]' \mid \mathbf{X}^*_i\big] \\ &= \mathrm{E}[\boldsymbol{\epsilon}^*_i\boldsymbol{\epsilon}^{*\prime}_i - \boldsymbol{\epsilon}^*_i(\hat{\boldsymbol{\theta}}_i - \boldsymbol{\theta}_i)'\mathbf{X}^{*\prime}_i - \mathbf{X}^*_i(\hat{\boldsymbol{\theta}}_i - \boldsymbol{\theta}_i)\boldsymbol{\epsilon}^{*\prime}_i \\ &\qquad - \mathbf{X}^*_i(\hat{\boldsymbol{\theta}}_i - \boldsymbol{\theta}_i)(\hat{\boldsymbol{\theta}}_i - \boldsymbol{\theta}_i)'\mathbf{X}^{*\prime}_i \mid \mathbf{X}^*_i] \\ &= \mathbf{I}\sigma^2_{\epsilon_i} + \mathbf{X}^*_i(\mathbf{X}_i'\mathbf{X}_i)^{-1}\mathbf{X}^{*\prime}_i\sigma^2_{\epsilon_i}. \qquad (4.4.9)\end{aligned}$$

$\mathbf{I}$ is the $(L_2 \times L_2)$ identity matrix.

From (4.4.8) we see that the abnormal return vector, with an expectation of zero, is unbiased. The covariance matrix of the abnormal return vector from (4.4.9) has two parts. The first term in the sum is the variance due to the future disturbances and the second term is the additional variance due to the sampling error in $\hat{\boldsymbol{\theta}}_i$. This sampling error, which is common for all the elements of the abnormal return vector, will lead to serial correlation of the abnormal returns despite the fact that the true disturbances are independent through time. As the length of the estimation window L_1 becomes large, the second term will approach zero as the sampling error of the parameters vanishes, and the abnormal returns across time periods will become independent asymptotically.

Under the null hypothesis, H_0, that the given event has no impact on the mean or variance of returns, we can use (4.4.8) and (4.4.9) and the joint normality of the abnormal returns to draw inferences. Under H_0, for the vector of event-window sample abnormal returns we have

$$\hat{\epsilon}_i^* \sim \mathcal{N}(0, \mathbf{V}_i). \tag{4.4.10}$$

Equation (4.4.10) gives us the distribution for any single abnormal return observation. We next build on this result and consider the aggregation of abnormal returns.

4.4.3 Aggregation of Abnormal Returns

The abnormal return observations must be aggregated in order to draw overall inferences for the event of interest. The aggregation is along two dimensions—through time and across securities. We will first consider aggregation through time for an individual security and then will consider aggregation both across securities and through time.

We introduce the cumulative abnormal return to accommodate multiple sampling intervals within the event window. Define $\text{CAR}_i(\tau_1, \tau_2)$ as the cumulative abnormal return for security i from τ_1 to τ_2 where $T_1 < \tau_1 \leq \tau_2 \leq T_2$. Let γ be an $(L_2 \times 1)$ vector with ones in positions $\tau_1 - T_1$ to $\tau_2 - T_1$ and zeroes elsewhere. Then we have

$$\widehat{\text{CAR}}_i(\tau_1, \tau_2) \equiv \gamma' \hat{\epsilon}_i^* \tag{4.4.11}$$

$$\text{Var}[\widehat{\text{CAR}}_i(\tau_1, \tau_2)] = \sigma_i^2(\tau_1, \tau_2) = \gamma' \mathbf{V}_i \gamma. \tag{4.4.12}$$

It follows from (4.4.10) that under H_0,

$$\widehat{\text{CAR}}_i(\tau_1, \tau_2) \sim \mathcal{N}(0, \sigma_i^2(\tau_1, \tau_2)). \tag{4.4.13}$$

We can construct a test of H_0 for security i from (4.4.13) using the standardized cumulative abnormal return,

$$\widehat{\text{SCAR}}_i(\tau_1, \tau_2) = \frac{\widehat{\text{CAR}}_i(\tau_1, \tau_2)}{\hat{\sigma}_i(\tau_1, \tau_2)}, \tag{4.4.14}$$

where $\hat{\sigma}_i^2(\tau_1, \tau_2)$ is calculated with $\hat{\sigma}_{\epsilon_i}^2$ from (4.4.4) substituted for $\sigma_{\epsilon_i}^2$. Under the null hypothesis the distribution of $\widehat{\text{SCAR}}_i(\tau_1, \tau_2)$ is Student t with $L_1 - 2$ degrees of freedom. From the properties of the Student t distribution, the expectation of $\widehat{\text{SCAR}}_i(\tau_1, \tau_2)$ is 0 and the variance is $(\frac{L_1-2}{L_1-4})$. For a large estimation window (for example, $L_1 > 30$), the distribution of $\widehat{\text{SCAR}}_i(\tau_1, \tau_2)$ will be well approximated by the standard normal.

The above result applies to a sample of one event and must be extended for the usual case where a sample of many event observations is aggregated. To aggregate across securities and through time, we assume that there is not any correlation across the abnormal returns of different securities. This will generally be the case if there is not any clustering, that is, there is not any overlap in the event windows of the included securities. The absence of any overlap and the maintained distributional assumptions imply that the abnormal returns and the cumulative abnormal returns will be independent across securities. Inferences with clustering will be discussed later.

The individual securities' abnormal returns can be averaged using $\hat{\epsilon}_i^*$ from (4.4.7). Given a sample of N events, defining $\bar{\epsilon}^*$ as the sample average of the N abnormal return vectors, we have

$$\bar{\epsilon}^* = \frac{1}{N} \sum_{i=1}^{N} \hat{\epsilon}_i^* \tag{4.4.15}$$

$$\text{Var}[\bar{\epsilon}^*] = \mathbf{V} = \frac{1}{N^2} \sum_{i=1}^{N} \mathbf{V}_i. \tag{4.4.16}$$

We can aggregate the elements of this average abnormal returns vector through time using the same approach as we did for an individual security's vector. Define $\overline{\text{CAR}}(\tau_1, \tau_2)$ as the cumulative average abnormal return from τ_1 to τ_2 where $T_1 < \tau_1 \leq \tau_2 \leq T_2$ and γ again represents an $(L_2 \times 1)$ vector with ones in positions $\tau_1 - T_1$ to $\tau_2 - T_1$ and zeroes elsewhere. For the cumulative average abnormal return we have

$$\overline{\text{CAR}}(\tau_1, \tau_2) \equiv \gamma' \bar{\epsilon}^* \tag{4.4.17}$$

$$\text{Var}[\overline{\text{CAR}}(\tau_1, \tau_2)] = \bar{\sigma}^2(\tau_1, \tau_2) = \gamma' \mathbf{V} \gamma. \tag{4.4.18}$$

Equivalently, to obtain $\overline{\text{CAR}}(\tau_1, \tau_2)$, we can aggregate using the sample cumulative abnormal return for each security i. For N events we have

$$\overline{\text{CAR}}(\tau_1, \tau_2) = \tfrac{1}{N} \sum_{i=1}^{N} \widehat{\text{CAR}}_i(\tau_1, \tau_2) \tag{4.4.19}$$

$$\text{Var}[\overline{\text{CAR}}(\tau_1, \tau_2)] = \bar{\sigma}^2(\tau_1, \tau_2) = \tfrac{1}{N^2} \sum_{i=1}^{N} \sigma_i^2(\tau_1, \tau_2). \tag{4.4.20}$$

In (4.4.16), (4.4.18), and (4.4.20) we use the assumption that the event windows of the N securities do not overlap to set the covariance terms to zero. Inferences about the cumulative abnormal returns can be drawn using

$$\overline{\mathrm{CAR}}(\tau_1, \tau_2) \sim \mathcal{N}\left(0, \bar{\sigma}^2(\tau_1, \tau_2)\right), \tag{4.4.21}$$

since under the null hypothesis the expectation of the abnormal returns is zero. In practice, since $\bar{\sigma}^2(\tau_1, \tau_2)$ is unknown, we can use $\hat{\bar{\sigma}}^2(\tau_1, \tau_2) = \frac{1}{N^2}\sum_{i=1}^{N}\hat{\sigma}_i^2(\tau_1, \tau_2)$ as a consistent estimator and proceed to test H_0 using

$$J_1 = \frac{\overline{\mathrm{CAR}}(\tau_1, \tau_2)}{\left[\hat{\bar{\sigma}}^2(\tau_1, \tau_2)\right]^{\frac{1}{2}}} \overset{a}{\sim} \mathcal{N}(0, 1). \tag{4.4.22}$$

This distributional result is for large samples of events and is not exact because an estimator of the variance appears in the denominator.

A second method of aggregation is to give equal weighting to the individual SCAR_i's. Defining $\overline{\mathrm{SCAR}}(\tau_1, \tau_2)$ as the average over N securities from event time τ_1 to τ_2, we have

$$\overline{\mathrm{SCAR}}(\tau_1, \tau_2) = \frac{1}{N}\sum_{i=1}^{N}\widehat{\mathrm{SCAR}}_i(\tau_1, \tau_2). \tag{4.4.23}$$

Assuming that the event windows of the N securities do not overlap in calendar time, under H_0, $\overline{\mathrm{SCAR}}(\tau_1, \tau_2)$ will be normally distributed in large samples with a mean of zero and variance $\left(\frac{L_1-2}{N(L_1-4)}\right)$. We can test the null hypothesis using

$$J_2 = \left(\frac{N(L_1-4)}{L_1-2}\right)^{\frac{1}{2}} \overline{\mathrm{SCAR}}(\tau_1, \tau_2) \overset{a}{\sim} \mathcal{N}(0, 1). \tag{4.4.24}$$

When doing an event study one will have to choose between using J_1 or J_2 for the test statistic. One would like to choose the statistic with higher power, and this will depend on the alternative hypothesis. If the true abnormal return is constant across securities then the better choice will give more weight to the securities with the lower abnormal return variance, which is what J_2 does. On the other hand if the true abnormal return is larger for securities with higher variance, then the better choice will give equal weight to the realized cumulative abnormal return of each security, which is what J_1 does. In most studies, the results are not likely to be sensitive to the choice of J_1 versus J_2 because the variance of the CAR is of a similar magnitude across securities.

4.4.4 *Sensitivity to Normal Return Model*

We have developed results using the market model as the normal return model. As previously noted, using the market model as opposed to the constant-mean-return model will lead to a reduction in the abnormal return variance. This point can be shown by comparing the abnormal return variances. For this illustration we take the normal return model parameters as given.

The variance of the abnormal return for the market model is

$$\begin{aligned}\sigma_{\epsilon_i}^2 &= \mathrm{Var}[R_{it} - \alpha_i - \beta_i R_{mt}] \\ &= \mathrm{Var}[R_{it}] - \beta_i^2\,\mathrm{Var}[R_{mt}] \\ &= (1 - R_i^2)\,\mathrm{Var}[R_{it}],\end{aligned} \tag{4.4.25}$$

where R_i^2 is the R^2 of the market-model regression for security i.

For the constant-mean-return model, the variance of the abnormal return ξ_{it} is the variance of the unconditional return, $\mathrm{Var}[R_{it}]$, that is,

$$\sigma_{\xi_i}^2 = \mathrm{Var}[R_{it} - \mu_i] = \mathrm{Var}[R_{it}]. \tag{4.4.26}$$

Combining (4.4.25) and (4.4.26) we have

$$\sigma_{\epsilon_i}^2 = (1 - R_i^2)\,\sigma_{\xi_i}^2. \tag{4.4.27}$$

Since R_i^2 lies between zero and one, the variance of the abnormal return using the market model will be less than or equal to the abnormal return variance using the constant-mean-return model. This lower variance for the market model will carry over into all the aggregate abnormal return measures. As a result, using the market model can lead to more precise inferences. The gains will be greatest for a sample of securities with high market-model R^2 statistics.

In principle further increases in R^2 could be achieved by using a multifactor model. In practice, however, the gains in R^2 from adding additional factors are usually small.

4.4.5 *CARs for the Earnings-Announcement Example*

The earnings-announcement example illustrates the use of sample abnormal returns and sample cumulative abnormal returns. Table 4.1 presents the abnormal returns averaged across the 30 firms as well as the averaged cumulative abnormal return for each of the three earnings news categories. Two normal return models are considered: the market model and, for comparison, the constant-mean-return model. Plots of the cumulative abnormal returns are also included, with the CARs from the market model in Figure 4.2a and the CARs from the constant-mean-return model in Figure 4.2b.

The results of this example are largely consistent with the existing literature on the information content of earnings. The evidence strongly

Table 4.1. *Abnormal returns for an event study of the information content of earnings announcements.*

Event Day	Market Model						Constant-Mean-Return Model					
	Good News		No News		Bad News		Good News		No News		Bad News	
	$\bar{\epsilon}^*$	$\overline{CAR}$	$\bar{\epsilon}^*$	$\overline{CAR}$	$\bar{\epsilon}^*$	$\overline{CAR}$	$\bar{\epsilon}^*$	$\overline{CAR}$	$\bar{\epsilon}^*$	$\overline{CAR}$	$\bar{\epsilon}^*$	$\overline{CAR}$
−20	.093	.093	.080	.080	−.107	−.107	.105	.105	.019	.019	−.077	−.077
−19	−.177	−.084	.018	.098	−.180	−.286	−.235	−.129	−.048	−.029	−.142	−.219
−18	.088	.004	.012	.110	.029	−.258	.069	−.060	−.086	−.115	−.043	−.262
−17	.024	.029	−.151	−.041	−.079	−.337	−.026	−.086	−.140	−.255	−.057	−.319
−16	−.018	.011	−.019	−.060	−.010	−.346	−.086	−.172	.039	−.216	−.075	−.394
−15	−.040	−.029	.013	−.047	−.054	−.401	−.183	−.355	.099	−.117	−.037	−.431
−14	.038	.008	.040	−.007	−.021	−.421	−.020	−.375	−.150	−.266	−.101	−.532
−13	.056	.064	−.057	−.065	.007	−.414	−.025	−.399	−.191	−.458	−.069	−.601
−12	.065	.129	.146	.081	−.090	−.504	.101	−.298	.133	−.325	−.106	−.707
−11	.069	.199	−.020	.061	−.088	−.592	.126	−.172	.006	−.319	−.169	−.876
−10	.028	.227	.025	.087	−.092	−.683	.134	−.038	.103	−.216	−.009	−.885
−9	.155	.382	.115	.202	−.040	−.724	.210	.172	.022	−.194	.011	−.874
−8	.057	.438	.070	.272	.072	−.652	.106	.278	.163	−.031	.135	−.738
−7	−.010	.428	−.106	.166	−.026	−.677	−.002	.277	.009	−.022	−.027	−.765
−6	.104	.532	.026	.192	−.013	−.690	.011	.288	−.029	−.051	.030	−.735
−5	.085	.616	−.085	.107	.164	−.527	.061	.349	−.068	−.120	.320	−.415
−4	.099	.715	.040	.147	−.139	−.666	.031	.379	.089	−.031	−.205	−.620
−3	.117	.832	.036	.183	.098	−.568	.067	.447	.013	−.018	.085	−.536
−2	.006	.838	.226	.409	−.112	−.680	.010	.456	.311	.294	−.256	−.791
−1	.164	1.001	−.168	.241	−.180	−.860	.198	.654	−.170	.124	−.227	−1.018
0	.965	1.966	−.091	.150	−.679	−1.539	1.034	1.688	−.164	−.040	−.643	−1.661
1	.251	2.217	−.008	.142	−.204	−1.743	.357	2.045	−.170	−.210	−.212	−1.873
2	−.014	2.203	.007	.148	.072	−1.672	−.013	2.033	.054	−.156	.078	−1.795
3	−.164	2.039	.042	.190	.083	−1.589	−.088	1.944	−.121	−.277	.146	−1.648
4	−.014	2.024	.000	.190	.106	−1.483	.041	1.985	.023	−.253	.149	−1.499
5	.135	2.160	−.038	.152	.194	−1.289	.248	2.233	−.003	−.256	.286	−1.214
6	−.052	2.107	−.302	−.150	.076	−1.213	−.035	2.198	−.319	−.575	.070	−1.143
7	.060	2.167	−.199	−.349	.120	−1.093	.017	2.215	−.112	−.687	.102	−1.041
8	.155	2.323	−.108	−.457	−.041	−1.134	.112	2.326	−.187	−.874	.056	−.986
9	−.008	2.315	−.146	−.603	−.069	−1.203	−.052	2.274	−.057	−.931	−.071	−1.056
10	.164	2.479	.082	−.521	.130	−1.073	.147	2.421	.203	−.728	.267	−.789
11	−.081	2.398	.040	−.481	−.009	−1.082	−.013	2.407	.045	−.683	.006	−.783
12	−.058	2.341	.246	−.235	−.038	−1.119	−.054	2.354	.299	−.384	.017	−.766
13	−.165	2.176	.014	−.222	.071	−1.048	−.246	2.107	−.067	−.451	.114	−.652
14	−.081	2.095	−.091	−.312	.019	−1.029	−.011	2.096	−.024	−.475	.089	−.564
15	−.007	2.088	−.001	−.314	−.043	−1.072	−.027	2.068	−.059	−.534	−.022	−.585
16	.065	2.153	−.020	−.334	−.086	−1.159	.103	2.171	−.046	−.580	−.084	−.670
17	.081	2.234	.017	−.317	−.050	−1.208	.066	2.237	−.098	−.677	−.054	−.724
18	.172	2.406	.054	−.263	.066	−1.142	.110	2.347	.021	−.656	−.071	−.795
19	−.043	2.363	.119	−.144	−.088	−1.230	−.055	2.292	.088	−.568	.026	−.769
20	.013	2.377	.094	−.050	−.028	−1.258	.019	2.311	.013	−.554	−.115	−.884

The sample consists of a total of 600 quarterly announcements for the thirty companies in the Dow Jones Industrial Index for the five-year period January 1989 to December 1993. Two models are considered for the normal returns, the market model using the CRSP value-weighted index and the constant-mean-return model. The announcements are categorized into three groups, good news, no news, and bad news. $\bar{\epsilon}^*$ is the sample average abnormal return for the specified day in event time and $\overline{CAR}$ is the sample average cumulative abnormal return for day −20 to the specified day. Event time is measured in days relative to the announcement date.

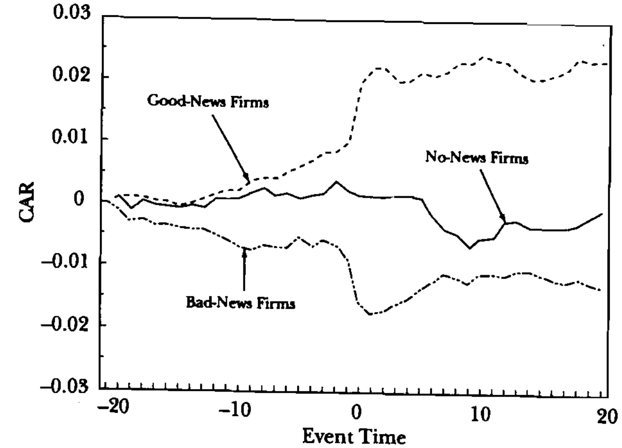

Figure 4.2a. *Plot of Cumulative Market-Model Abnormal Return for Earning Announcements*

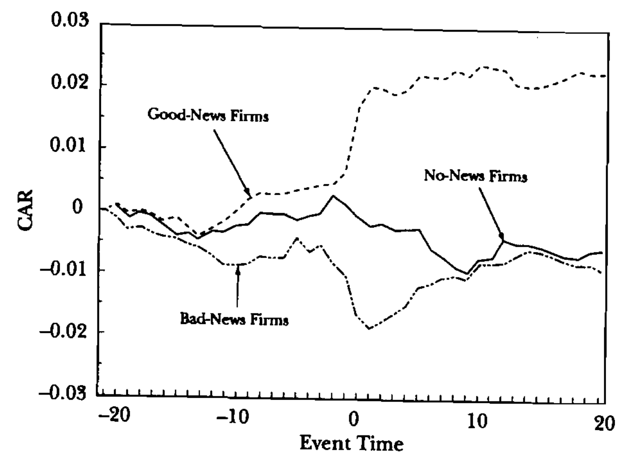

Figure 4.2b. *Plot of Cumulative Constant-Mean-Return-Model Abnormal Return for Earning Announcements*

supports the hypothesis that earnings announcements do indeed convey information useful for the valuation of firms. Focusing on the announcement day (day zero) the sample average abnormal return for the good-news firm using the market model is 0.965%. Since the standard error of the one-day good-news average abnormal return is 0.104%, the value of J_1 is 9.28 and the null hypothesis that the event has no impact is strongly rejected. The story is the same for the bad-news firms. The event day sample abnormal return is −0.679%, with a standard error of 0.098%, leading to J_1 equal to −6.93 and again strong evidence against the null hypothesis. As would be expected, the abnormal return of the no-news firms is small at −0.091% and, with a standard error of 0.098%, is less than one standard error from zero. There is also some evidence of the announcement effect on day one. The average abnormal returns are 0.251% and −0.204% for the good-news and the bad-news firms respectively. Both these values are more than two standard errors from zero. The source of these day-one effects is likely to be that some of the earnings announcements are made on event day zero after the close of the stock market. In these cases the effects will be captured in the return on day one.

The conclusions using the abnormal returns from the constant-mean-return model are consistent with those from the market model. However, there is some loss of precision using the constant-mean-return model, as the variance of the average abnormal return increases for all three categories. When measuring abnormal returns with the constant-mean-return model the standard errors increase from 0.104% to 0.130% for good-news firms, from 0.098% to 0.124% for no-news firms, and from 0.098% to 0.131% for bad-news firms. These increases are to be expected when considering a sample of large firms such as those in the Dow Index since these stocks tend to have an important market component whose variability is eliminated using the market model.

The CAR plots show that to some extent the market gradually learns about the forthcoming announcement. The average CAR of the good-news firms gradually drifts up in days −20 to −1, and the average CAR of the bad-news firms gradually drifts down over this period. In the days after the announcement the CAR is relatively stable, as would be expected, although there does tend to be a slight (but statistically insignificant) increase for the bad-news firms in days two through eight.

4.4.6 *Inferences with Clustering*

In analyzing aggregated abnormal returns, we have thus far assumed that the abnormal returns on individual securities are uncorrelated in the cross section. This will generally be a reasonable assumption if the event windows of the included securities do not overlap in calendar time. The assumption allows us to calculate the variance of the aggregated sample cumulative abnormal returns without concern about covariances between individual sample CARs, since they are zero. However, when the event windows do

overlap, the covariances between the abnormal returns may differ from zero, and the distributional results presented for the aggregated abnormal returns are not applicable. Bernard (1987) discusses some of the problems related to clustering.

When there is one event date in calendar time, clustering can be accommodated in two different ways. First, the abnormal returns can be aggregated into a portfolio dated using event time, and the security level analysis of Section 4.4 can be applied to the portfolio. This approach allows for cross correlation of the abnormal returns.

A second way to handle clustering is to analyze the abnormal returns without aggregation. One can test the null hypothesis that the event has no impact using unaggregated security-by-security data. The basic approach is an application of a multivariate regression model with dummy variables for the event date; it is closely related to the multivariate F-test of the CAPM presented in Chapter 5. The approach is developed in the papers of Schipper and Thompson (1983, 1985), Malatesta and Thompson (1985), and Collins and Dent (1984). It has some advantages relative to the portfolio approach. First, it can accommodate an alternative hypothesis where some of the firms have positive abnormal returns and some of the firms have negative abnormal returns. Second, it can handle cases where there is partial clustering, that is, where the event date is not the same across firms but there is overlap in the event windows. This approach also has some drawbacks, however. In many cases the test statistic has poor finite-sample properties, and often it has little power against economically reasonable alternatives.

4.5 Modifying the Null Hypothesis

Thus far we have focused on a single null hypothesis—that the given event has no impact on the behavior of security returns. With this null hypothesis either a mean effect or a variance effect represents a violation. However, in some applications we may be interested in testing only for a mean effect. In these cases, we need to expand the null hypothesis to allow for changing (usually increasing) variances.

To accomplish this, we need to eliminate any reliance on past returns in estimating the variance of the aggregated cumulative abnormal returns. Instead, we use the cross section of cumulative abnormal returns to form an estimator of the variance. Boehmer, Musumeci, and Poulsen (1991) discuss this methodology, which is best applied using the constant-mean-return model to measure the abnormal return.

The cross-sectional approach to estimating the variance can be applied to both the average cumulative abnormal return $(\overline{\mathrm{CAR}}(\tau_1, \tau_2))$ and the average standardized cumulative abnormal return $(\overline{\mathrm{SCAR}}(\tau_1, \tau_2))$. Using the cross section to form estimators of the variances we have

$$\widehat{\mathrm{Var}}[\overline{\mathrm{CAR}}(\tau_1, \tau_2)] = \frac{1}{N^2}\sum_{i=1}^{N}\left(\mathrm{CAR}_i(\tau_1, \tau_2) - \overline{\mathrm{CAR}}(\tau_1, \tau_2)\right)^2 \quad (4.5.1)$$

$$\widehat{\mathrm{Var}}[\overline{\mathrm{SCAR}}(\tau_1, \tau_2)] = \frac{1}{N^2}\sum_{i=1}^{N}\left(\mathrm{SCAR}_i(\tau_1, \tau_2) - \overline{\mathrm{SCAR}}(\tau_1, \tau_2)\right)^2. \quad (4.5.2)$$

For these estimators of the variances to be consistent we require the abnormal returns to be uncorrelated in the cross section. An absence of clustering is sufficient for this requirement. Note that cross-sectional homoskedasticity is not required for consistency. Given these variance estimators, the null hypothesis that the cumulative abnormal returns are zero can then be tested using large sample theory given the consistent estimators of the variances in (4.5.2) and (4.5.1).

One may also be interested in the impact of an event on the risk of a firm. The relevant measure of risk must be defined before this issue can be addressed. One choice as a risk measure is the market-model beta as implied by the Capital Asset Pricing Model. Given this choice, the market model can be formulated to allow the beta to change over the event window and the stability of the beta can be examined. See Kane and Unal (1988) for an application of this idea.

4.6 Analysis of Power

To interpret an event study, we need to know what is our ability to detect the presence of a nonzero abnormal return. In this section we ask what is the likelihood that an event-study test rejects the null hypothesis for a given level of abnormal return associated with an event, that is, we evaluate the power of the test.

We consider a two-sided test of the null hypothesis using the cumulative-abnormal-return-based statistic J_1 from (4.4.22). We assume that the abnormal returns are uncorrelated across securities; thus the variance of $\overline{\mathrm{CAR}}$ is $\bar{\sigma}^2(\tau_1, \tau_2)$, where $\bar{\sigma}^2(\tau_1, \tau_2) = 1/N^2 \sum_{i=1}^{N} \sigma_i^2(\tau_1, \tau_2)$ and N is the sample size. Under the null hypothesis the distribution of J_1 is standard normal. For a two-sided test of size α we reject the null hypothesis if $J_1 < \Phi^{-1}(\alpha/2)$ or if $J_1 > \Phi^{-1}(1-\alpha/2)$ where $\Phi(\cdot)$ is the standard normal cumulative distribution function (CDF).

Given an alternative hypothesis H_A and the CDF of J_1 for this hypothesis, we can tabulate the power of a test of size α using

$$\begin{aligned} P(\alpha, \mathrm{H}_A) &= \Pr\left(J_1 < \Phi^{-1}\left(\tfrac{\alpha}{2}\right) \mid \mathrm{H}_A\right) \\ &\quad + \Pr\left(J_1 > \Phi^{-1}\left(1-\tfrac{\alpha}{2}\right) \mid \mathrm{H}_A\right). \end{aligned} \quad (4.6.1)$$

With this framework in place, we need to posit specific alternative hypotheses. Alternatives are constructed to be consistent with event studies using data sampled at a daily interval. We build eight alternative hypotheses using four levels of abnormal returns, 0.5%, 1.0%, 1.5%, and 2.0%, and two levels for the average variance of the cumulative abnormal return of a given security over the sampling interval, 0.0004 and 0.0016. These variances correspond to standard deviations of 2% and 4%, respectively. The sample size, that is the number of securities for which the event occurs, is varied from 1 to 200. We document the power for a test with a size of 5% ($\alpha = 0.05$) giving values of -1.96 and 1.96 for $\Phi^{-1}(\alpha/2)$ and $\Phi^{-1}(1-\alpha/2)$, respectively. In applications, of course, the power of the test should be considered when selecting the size.

The power results are presented in Table 4.2 and are plotted in Figures 4.3a and 4.3b. The results in the left panel of Table 4.2 and in Figure 4.3a are for the case where the average variance is 0.0004, corresponding to a standard deviation of 2%. This is an appropriate value for an event which does not lead to increased variance and can be examined using a one-day event window. Such a case is likely to give the event-study methodology its highest power. The results illustrate that when the abnormal return is only 0.5% the power can be low. For example, with a sample size of 20 the power of a 5% test is only 0.20. One needs a sample of over 60 firms before the power reaches 0.50. However, for a given sample size, increases in power are substantial when the abnormal return is larger. For example, when the abnormal return is 2.0% the power of a 5% test with 20 firms is almost 1.00 with a value of 0.99. The general results for a variance of 0.0004 is that when the abnormal return is larger than 1% the power is quite high even for small sample sizes. When the abnormal return is small a larger sample size is necessary to achieve high power.

In the right panel of Table 4.2 and in Figure 4.3b the power results are presented for the case where the average variance of the cumulative abnormal return is 0.0016, corresponding to a standard deviation of 4%. This case corresponds roughly to either a multi-day event window or to a one-day event window with the event leading to increased variance which is accommodated as part of the null hypothesis. Here we see a dramatic decline in the power of a 5% test. When the CAR is 0.5% the power is only 0.09 with 20 firms and only 0.42 with a sample of 200 firms. This magnitude

Table 4.2. *Power of event-study test statistic J_1 to reject the null hypothesis that the abnormal return is zero.*

Sample Size	Abnormal Return				Abnormal Return			
	0.5%	1.0%	1.5%	2.0%	0.5%	1.0%	1.5%	2.0%
		$\sigma = 2\%$				$\sigma = 4\%$		
1	0.06	0.08	0.12	0.17	0.05	0.06	0.07	0.08
2	0.06	0.11	0.19	0.29	0.05	0.06	0.08	0.11
3	0.07	0.14	0.25	0.41	0.06	0.07	0.10	0.14
4	0.08	0.17	0.32	0.52	0.06	0.08	0.12	0.17
5	0.09	0.20	0.39	0.61	0.06	0.09	0.13	0.20
6	0.09	0.23	0.45	0.69	0.06	0.09	0.15	0.23
7	0.10	0.26	0.51	0.75	0.06	0.10	0.17	0.26
8	0.11	0.29	0.56	0.81	0.06	0.11	0.19	0.29
9	0.12	0.32	0.61	0.85	0.07	0.12	0.20	0.32
10	0.12	0.35	0.66	0.89	0.07	0.12	0.22	0.35
11	0.13	0.38	0.70	0.91	0.07	0.13	0.24	0.38
12	0.14	0.41	0.74	0.93	0.07	0.14	0.25	0.41
13	0.15	0.44	0.77	0.95	0.07	0.15	0.27	0.44
14	0.15	0.46	0.80	0.96	0.08	0.15	0.29	0.46
15	0.16	0.49	0.83	0.97	0.08	0.16	0.31	0.49
16	0.17	0.52	0.85	0.98	0.08	0.17	0.32	0.52
17	0.18	0.54	0.87	0.98	0.08	0.18	0.34	0.54
18	0.19	0.56	0.89	0.99	0.08	0.19	0.36	0.56
19	0.19	0.59	0.90	0.99	0.08	0.19	0.37	0.59
20	0.20	0.61	0.92	0.99	0.09	0.20	0.39	0.61
25	0.24	0.71	0.96	1.00	0.10	0.24	0.47	0.71
30	0.28	0.78	0.98	1.00	0.11	0.28	0.54	0.78
35	0.32	0.84	0.99	1.00	0.11	0.32	0.60	0.84
40	0.35	0.89	1.00	1.00	0.12	0.35	0.66	0.89
45	0.39	0.92	1.00	1.00	0.13	0.39	0.71	0.92
50	0.42	0.94	1.00	1.00	0.14	0.42	0.76	0.94
60	0.49	0.97	1.00	1.00	0.16	0.49	0.83	0.97
70	0.55	0.99	1.00	1.00	0.18	0.55	0.88	0.99
80	0.61	0.99	1.00	1.00	0.20	0.61	0.92	0.99
90	0.66	1.00	1.00	1.00	0.22	0.66	0.94	1.00
100	0.71	1.00	1.00	1.00	0.24	0.71	0.96	1.00
120	0.78	1.00	1.00	1.00	0.28	0.78	0.98	1.00
140	0.84	1.00	1.00	1.00	0.32	0.84	0.99	1.00
160	0.89	1.00	1.00	1.00	0.35	0.89	1.00	1.00
180	0.92	1.00	1.00	1.00	0.39	0.92	1.00	1.00
200	0.94	1.00	1.00	1.00	0.42	0.94	1.00	1.00

The power is reported for a test with a size of 5%. The sample size is the number of event observations included in the study, and σ is the square root of the average variance of the abnormal return across firms.

of abnormal return is difficult to detect with the larger variance of 0.0016. In contrast, when the CAR is as large as 1.5% or 2.0% the 5% test still has reasonable power. For example, when the abnormal return is 1.5% and

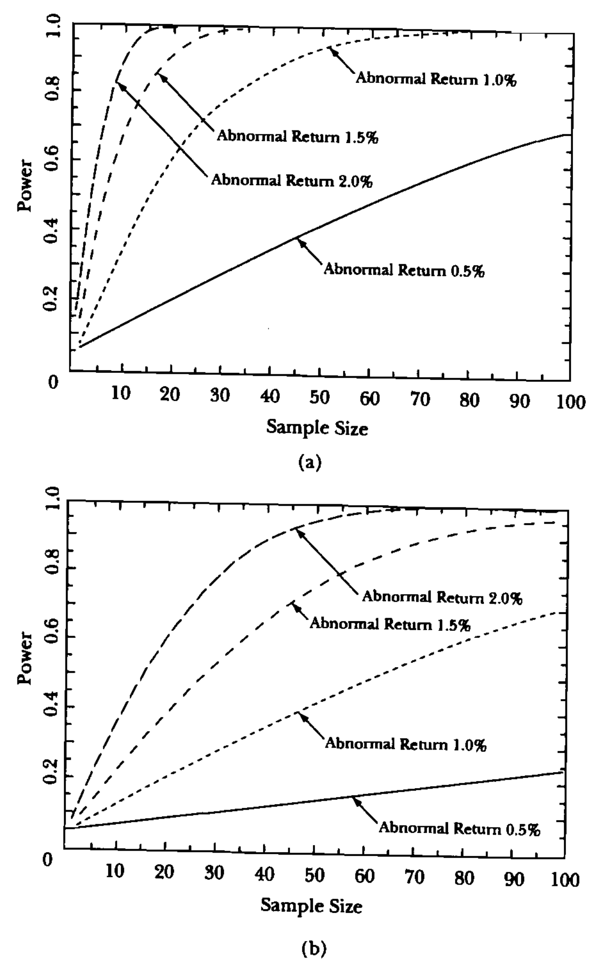

Figure 4.3. *Power of Event-Study Test Statistic J_1 to Reject the Null Hypothesis that the Abnormal Return Is Zero, When the Square Root of the Average Variance of the Abnormal Return Across Firms is (a) 2% and (b) 4%*

there is a sample size of 30, the power is 0.54. Generally if the abnormal return is large one will have little difficulty rejecting the null hypothesis of no abnormal return.

We have calculated power analytically using distributional assumptions. If these distributional assumptions are inappropriate then our power calculations may be inaccurate. However, Brown and Warner (1985) explore this issue and find that the analytical computations and the empirical power are very close.

It is difficult to reach general conclusions concerning the the ability of event-study methodology to detect nonzero abnormal returns. When conducting an event study it is necessary to evaluate the power given the parameters and objectives of the study. If the power seems sufficient then one can proceed, otherwise one should search for ways of increasing the power. This can be done by increasing the sample size, shortening the event window, or by developing more specific predictions of the null hypothesis.

4.7 Nonparametric Tests

The methods discussed to this point are parametric in nature, in that specific assumptions have been made about the distribution of abnormal returns. Alternative nonparametric approaches are available which are free of specific assumptions concerning the distribution of returns. In this section we discuss two common nonparametric tests for event studies, the sign test and the rank test.

The sign test, which is based on the sign of the abnormal return, requires that the abnormal returns (or more generally cumulative abnormal returns) are independent across securities and that the expected proportion of positive abnormal returns under the null hypothesis is 0.5. The basis of the test is that under the null hypothesis it is equally probable that the CAR will be positive or negative. If, for example, the null hypothesis is that there is a positive abnormal return associated with a given event, the null hypothesis is $H_0: p \leq 0.5$ and the alternative is $H_A: p > 0.5$ where $p = \Pr(CAR_i \geq 0.0)$. To calculate the test statistic we need the number of cases where the abnormal return is positive, N^+, and the total number of cases, N. Letting J_3 be the test statistic, then asymptotically as N increases we have

$$J_3 = \left[\frac{N^+}{N} - 0.5\right]\frac{N^{1/2}}{0.5} \sim \mathcal{N}(0,1)\,.$$

For a test of size $(1-\alpha)$, H_0 is rejected if $J_3 > \Phi^{-1}(\alpha)$.

A weakness of the sign test is that it may not be well specified if the distribution of abnormal returns is skewed, as can be the case with daily data. With skewed abnormal returns, the expected proportion of positive abnormal returns can differ from one half even under the null hypothesis. In response to this possible shortcoming, Corrado (1989) proposes a nonparametric rank test for abnormal performance in event studies. We briefly describe his test of the null hypothesis that there is no abnormal return on

event day zero. The framework can be easily altered for events occurring over multiple days.

Drawing on notation previously introduced, consider a sample of L_2 abnormal returns for each of N securities. To implement the rank test it is necessary for each security to rank the abnormal returns from 1 to L_2. Define $K_{i\tau}$ as the rank of the abnormal return of security i for event time period τ. Recall that τ ranges from T_1+1 to T_2 and $\tau = 0$ is the event day. The rank test uses the fact that the expected rank under the null hypothesis is $\frac{L_2+1}{2}$. The test statistic for the null hypothesis of no abnormal return on event day zero is:

$$J_4 = \frac{1}{N}\sum_{i=1}^{N}\left(K_{i0} - \frac{L_2+1}{2}\right)\Big/ s(L_2) \qquad (4.7.1)$$

$$s(L_2) = \sqrt{\frac{1}{L_2}\sum_{\tau=T_1+1}^{T_2}\left(\frac{1}{N}\sum_{i=1}^{N}\left(K_{i\tau} - \frac{L_2+1}{2}\right)\right)^2} \qquad (4.7.2)$$

Tests of the null hypothesis can be implemented using the result that the asymptotic null distribution of J_4 is standard normal. Corrado (1989) gives further details.

Typically, these nonparametric tests are not used in isolation but in conjunction with their parametric counterparts. The nonparametric tests enable one to check the robustness of conclusions based on parametric tests. Such a check can be worthwhile as illustrated by the work of Campbell and Wasley (1993). They find that for daily returns on NASDAQ stocks the nonparametric rank test provides more reliable inferences than do the standard parametric tests.

4.8 Cross-Sectional Models

Theoretical models often suggest that there should be an association between the magnitude of abnormal returns and characteristics specific to the event observation. To investigate this association, an appropriate tool is a cross-sectional regression of abnormal returns on the characteristics of interest. To set up the model, define $\mathbf{y}$ as an $(N\times 1)$ vector of cumulative abnormal return observations and $\mathbf{X}$ as an $(N\times K)$ matrix of characteristics. The first column of $\mathbf{X}$ is a vector of ones and each of the remaining $(K-1)$ columns is a vector consisting of the characteristic for each event observation. Then, for the model, we have the regression equation

$$\mathbf{y} = \mathbf{X}\boldsymbol{\theta} + \boldsymbol{\eta}, \qquad (4.8.1)$$

where $\boldsymbol{\theta}$ is the $(K\times 1)$ coefficient vector and $\boldsymbol{\eta}$ is the $(N\times 1)$ disturbance vector. Assuming $E[\mathbf{X}'\boldsymbol{\eta}] = 0$, we can consistently estimate $\boldsymbol{\theta}$ using OLS.

For the *OLS* estimator we have

$$\hat{\theta} = (\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'\mathbf{y}. \tag{4.8.2}$$

Assuming the elements of η are cross-sectionally uncorrelated and homoskedastic, inferences can be derived using the usual OLS standard errors. Defining σ^2_{η} as the variance of the elements of η we have

$$\text{Var}[\hat{\theta}] = (\mathbf{X}'\mathbf{X})^{-1}\sigma^2_{\eta}. \tag{4.8.3}$$

Using the unbiased estimator for σ^2_{η},

$$\hat{\sigma}^2_{\eta} = \frac{1}{(N-K)}\hat{\eta}'\hat{\eta}, \tag{4.8.4}$$

where $\hat{\eta} = \mathbf{y} - \mathbf{X}\hat{\theta}$, we can construct *t*-statistics to assess the statistical significance of the elements of $\hat{\theta}$. Alternatively, without assuming homoskedasticity, we can construct heteroskedasticity-consistent *z*-statistics using

$$\text{Var}[\hat{\theta}] = \frac{1}{N}(\mathbf{X}'\mathbf{X})^{-1}\left[\sum_{i=1}^{N}\mathbf{x}_i\mathbf{x}_i'\hat{\eta}_i^2\right](\mathbf{X}'\mathbf{X})^{-1}, \tag{4.8.5}$$

where $\mathbf{x}_i'$ is the *i*th row of $\mathbf{X}$ and $\hat{\eta}_i$ is the *i*th element of $\hat{\eta}$. This expression for the standard errors can be derived using the Generalized Method of Moments framework in Section A.2 of the Appendix and also follows from the results of White (1980). The use of heteroskedasticity-consistent standard errors is advised since there is no reason to expect the residuals of (4.8.1) to be homoskedastic.

Asquith and Mullins (1986) provide an example of this approach. The two-day cumulative abnormal return for the announcement of an equity offering is regressed on the size of the offering as a percentage of the value of the total equity of the firm and on the cumulative abnormal return in the eleven months prior to the announcement month. They find that the magnitude of the (negative) abnormal return associated with the announcement of equity offerings is related to both these variables. Larger pre-event cumulative abnormal returns are associated with less negative abnormal returns, and larger offerings are associated with more negative abnormal returns. These findings are consistent with theoretical predictions which they discuss.

One must be careful in interpreting the results of the cross-sectional regression approach. In many situations, the event-window abnormal return will be related to firm characteristics not only through the valuation effects of the event but also through a relation between the firm characteristics and the extent to which the event is anticipated. This can happen when investors rationally use firm characteristics to forecast the likelihood of the event occurring. In these cases, a linear relation between the firm characteristics and the valuation effect of the event can be hidden. Malatesta and Thompson (1985) and Lanen and Thompson (1988) provide examples of this situation.

Technically, the relation between the firm characteristics and the degree of anticipation of the event introduces a selection bias. The assumption that the regression residual is uncorrelated with the regressors, $\text{E}[\mathbf{X}'\eta] = 0$, breaks down and the OLS estimators are inconsistent. Consistent estimators can be derived by explicitly allowing for the selection bias. Acharya (1988, 1993) and Eckbo, Maksimovic, and Williams (1990) provide examples of this. Prabhala (1995) provides a good discussion of this problem and the possible solutions. He argues that, despite misspecification, under weak conditions, the OLS approach can be used for inferences and the *t*-statistics can be interpreted as lower bounds on the true significance level of the estimates.

4.9 Further Issues

A number of further issues often arise when conducting an event study. We discuss some of these in this section.

4.9.1 Role of the Sampling Interval

If the timing of an event is known precisely, then the ability to statistically identify the effect of the event will be higher for a shorter sampling interval. The increase results from reducing the variance of the abnormal return without changing the mean. We evaluate the empirical importance of this issue by comparing the analytical formula for the power of the test statistic J_1 with a daily sampling interval to the power with a weekly and a monthly interval. We assume that a week consists of five days and a month is 22 days. The variance of the abnormal return for an individual event observation is assumed to be $(4\%)^2$ on a daily basis and linear in time.

In Figure 4.4, we plot the power of the test of no event-effect against the alternative of an abnormal return of 1% for 1 to 200 securities. As one would expect given the analysis of Section 4.6, the decrease in power going from a daily interval to a monthly interval is severe. For example, with 50 securities the power for a 5% test using daily data is 0.94, whereas the power using weekly and monthly data is only 0.35 and 0.12, respectively. The clear message is that there is a substantial payoff in terms of increased power from reducing the length of the event window. Morse (1984) presents detailed analysis of the choice of daily versus monthly data and draws the same conclusion.

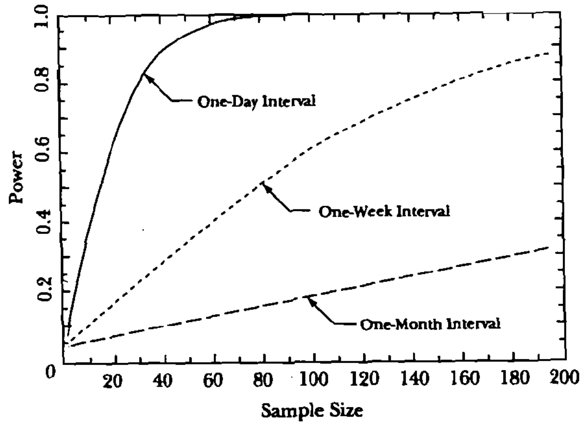

Figure 4.4. Power of Event-Study Test Statistic J_1 to Reject the Null Hypothesis that the Abnormal Return is Zero, for Different Sampling Intervals, When the Square Root of the Average Variance of the Abnormal Return Across Firms Is 4% for the Daily Interval

A sampling interval of one day is not the shortest interval possible. With the increased availability of transaction data, recent studies have used observation intervals of duration shorter than one day. The use of intra-daily data involves some complications, however, of the sort discussed in Chapter 3, and so the net benefit of very short intervals is unclear. Barclay and Litzenberger (1988) discuss the use of intra-daily data in event studies.

4.9.2 Inferences with Event-Date Uncertainty

Thus far we have assumed that the event date can be identified with certainty. However, in some studies it may be difficult to identify the exact date. A common example is when collecting event dates from financial publications such as the *Wall Street Journal.* When the event announcement appears in the newspaper one can not be certain if the market was informed before the close of the market the prior trading day. If this is the case then the prior day is the event day; if not, then the current day is the event day. The usual method of handling this problem is to expand the event window to two days—day 0 and day +1. While there is a cost to expanding the event window, the results in Section 4.6 indicate that the power properties of two-day event windows are still good, suggesting that it is worth bearing the cost to avoid the risk of missing the event.

Ball and Torous (1988) investigate this issue. They develop a maximum-likelihood estimation procedure which accommodates event-date uncertainty and examine results of their explicit procedure versus the informal procedure of expanding the event window. The results indicate that the informal procedure works well and there is little to gain from the more elaborate estimation framework.

4.9.3 Possible Biases

Event studies are subject to a number of possible biases. Nonsynchronous trading can introduce a bias. The nontrading or nonsynchronous trading effect arises when prices are taken to be recorded at time intervals of one length when in fact they are recorded at time intervals of other possibly irregular lengths. For example, the daily prices of securities usually employed in event studies are generally "closing" prices, prices at which the last transaction in each of those securities occurred during the trading day. These closing prices generally do not occur at the same time each day, but by calling them "daily" prices, we have implicitly and incorrectly assumed that they are equally spaced at 24-hour intervals. As we showed in Section 3.1 of Chapter 3, this nontrading effect induces biases in the moments and co-moments of returns.

The influence of the nontrading effect on the variances and covariances of individual stocks and portfolios naturally feeds into a bias for the market-model beta. Scholes and Williams (1977) present a consistent estimator of beta in the presence of nontrading based on the assumption that the true return process is uncorrelated through time. They also present some empirical evidence showing the nontrading-adjusted beta estimates of thinly traded securities to be approximately 10 to 20% larger than the unadjusted estimates. However, for actively traded securities, the adjustments are generally small and unimportant.

Jain (1986) considers the influence of thin trading on the distribution of the abnormal returns from the market model with the beta estimated using the Scholes-Williams approach. He compares the distribution of these abnormal returns to the distribution of the abnormal returns using the usual OLS betas and finds that the differences are minimal. This suggests that in general the adjustment for thin trading is not important.

The statistical analysis of Sections 4.3, 4.4, and 4.5 is based on the assumption that returns are jointly normal and temporally IID. Departures from this assumption can lead to biases. The normality assumption is important for the exact finite-sample results. Without assuming normality, all results would be asymptotic. However, this is generally not a problem for event studies since the test statistics converge to their asymptotic distributions rather quickly. Brown and Warner (1985) discuss this issue.

There can also be an upward bias in cumulative abnormal returns when these are calculated in the usual way. The bias arises from the observation-by-observation rebalancing to equal weights implicit in the calculation of the aggregate cumulative abnormal return combined with the use of transaction prices which can represent both the bid and the ask side of the market. Blume and Stambaugh (1983) analyze this bias and show that it can be important for studies using low-market-capitalization firms which have, in percentage terms, wide bid-ask spreads. In these cases the bias can be eliminated by considering cumulative abnormal returns that represent buy-and-hold strategies.

4.10 Conclusion

In closing, we briefly discuss examples of event-study successes and limitations. Perhaps the most successful applications have been in the area of corporate finance. Event studies dominate the empirical research in this area. Important examples include the wealth effects of mergers and acquisitions and the price effects of financing decisions by firms. Studies of these events typically focus on the abnormal return around the date of the first announcement.

In the 1960s there was a paucity of empirical evidence on the wealth effects of mergers and acquisitions. For example, Manne (1965) discusses the various arguments for and against mergers. At that time the debate centered on the extent to which mergers should be regulated in order to foster competition in the product markets. Manne argues that mergers represent a natural outcome in an efficiently operating market for corporate control and consequently provide protection for shareholders. He downplays the importance of the argument that mergers reduce competition. At the conclusion of his article Manne suggests that the two competing hypotheses for mergers could be separated by studying the price effects of the involved corporations. He hypothesizes that if mergers created market power one would observe price increases for both the target and acquirer. In contrast if the merger represented the acquiring corporation paying for control of the target, one would observe a price increase for the target only and not for the acquirer. However, at that time Manne concludes in reference to the price effects of mergers that "... no data are presently available on this subject."

Since that time an enormous body of empirical evidence on mergers and acquisitions has developed which is dominated by the use of event studies. The general result is that, given a successful takeover, the abnormal returns of the targets are large and positive and the abnormal returns of the acquirer are close to zero. Jarrell and Poulsen (1989) find that the average abnormal

return for target shareholders exceeds 20% for a sample of 663 successful takeovers from 1960 to 1985. In contrast the abnormal return for acquirers is close to zero at 1.14%, and even negative at −1.10% in the 1980's.

Eckbo (1983) explicitly addresses the role of increased market power in explaining merger-related abnormal returns. He separates mergers of competing firms from other mergers and finds no evidence that the wealth effects for competing firms are different. Further, he finds no evidence that rivals of firms merging horizontally experience negative abnormal returns. From this he concludes that reduced competition in the product market is not an important explanation for merger gains. This leaves competition for corporate control a more likely explanation. Much additional empirical work in the area of mergers and acquisitions has been conducted. Jensen and Ruback (1983) and Jarrell, Brickley, and Netter (1988) provide detailed surveys of this work.

A number of robust results have been developed from event studies of financing decisions by corporations. When a corporation announces that it will raise capital in external markets there is on average a negative abnormal return. The magnitude of the abnormal return depends on the source of external financing. Asquith and Mullins (1986) study a sample of 266 firms announcing an equity issue in the period 1963 to 1981 and find that the two-day average abnormal return is −2.7%, while on a sample of 80 firms for the period 1972 to 1982 Mikkelson and Partch (1986) find that the two-day average abnormal return is −3.56%. In contrast, when firms decide to use straight debt financing, the average abnormal return is closer to zero. Mikkelson and Partch (1986) find the average abnormal return for debt issues to be −0.23% for a sample of 171 issues. Findings such as these provide the fuel for the development of new theories. For example, these external financing results motivate the pecking order theory of capital structure developed by Myers and Majluf (1984).

A major success related to those in the corporate finance area is the implicit acceptance of event-study methodology by the U.S. Supreme Court for determining materiality in insider trading cases and for determining appropriate disgorgement amounts in cases of fraud. This implicit acceptance in the 1988 Basic, Incorporated v. Levinson case and its importance for securities law is discussed in Mitchell and Netter (1994).

There have also been less successful applications of event-study methodology. An important characteristic of a successful event study is the ability to identify precisely the date of the event. In cases where the date is difficult to identify or the event is partially anticipated, event studies have been less useful. For example, the wealth effects of regulatory changes for affected entities can be difficult to detect using event-study methodology. The problem is that regulatory changes are often debated in the political arena over time and any accompanying wealth effects will be incorporated gradually into the value of a corporation as the probability of the change being adopted increases.

Dann and James (1982) discuss this issue in their study of the impact of deposit interest rate ceilings on thrift institutions. They look at changes in rate ceilings, but decide not to consider a change in 1973 because it was due to legislative action and hence was likely to have been anticipated by the market. Schipper and Thompson (1983, 1985) also encounter this problem in a study of merger-related regulations. They attempt to circumvent the problem of anticipated regulatory changes by identifying dates when the probability of a regulatory change increases or decreases. However, they find largely insignificant results, leaving open the possibility that the absence of distinct event dates accounts for the lack of wealth effects.

Much has been learned from the body of research that uses event-study methodology. Most generally, event studies have shown that, as we would expect in a rational marketplace, prices do respond to new information. We expect that event studies will continue to be a valuable and widely used tool in economics and finance.

Problems—Chapter 4

4.1 Show that when using the market model to measure abnormal returns, the sample abnormal returns from equation (4.4.7) are asymptotically independent as the length of the estimation window (L_1) increases to infinity.

4.2 You are given the following information for an event. Abnormal returns are sampled at an interval of one day. The event-window length is three days. The mean abnormal return over the event window is 0.3% per day. You have a sample of 50 event observations. The abnormal returns are independent across the event observations as well as across event days for a given event observation. For 25 of the event observations the daily standard deviation of the abnormal return is 3% and for the remaining 25 observations the daily standard deviation is 6%. Given this information, what would be the power of the test for an event study using the cumulative abnormal return test statistic in equation (4.4.22)? What would be the power using the standardized cumulative abnormal return test statistic in equation (4.4.24)? For the power calculations, assume the standard deviation of the abnormal returns is known.

4.3 What would be the answers to question 4.2 if the mean abnormal return is 0.6% per day for the 25 firms with the larger standard deviation?