UNITED STATES DISTRICT COURT
SOUTHERN DISTRICT OF NEW YORK

SECURITIES AND EXCHANGE COMMISSION, **Plaintiff,** **- against -** **RIPPLE LABS, INC., BRADLEY GARLINGHOUSE, and CHRISTIAN A. LARSEN,** **Defendants.**	:	**20 Civ. 10832 (AT) (SN)** **ECF Case**

DECLARATION OF MARK R. SYLVESTER IN SUPPORT OF PLAINTIFF SECURITIES AND EXCHANGE COMMISSION'S OPPOSITION TO DEFENDANTS' MOTION TO EXCLUDE THE TESTIMONY OF [redacted], PH.D.

I, Mark R. Sylvester, hereby declare under penalty of perjury, pursuant to 28 U.S.C. § 1746, that the following is true and correct:

1. I represent Plaintiff Securities and Exchange Commission (the "SEC") as counsel in this action.
2. I respectfully submit this declaration and the attachments hereto in support of Plaintiff Securities and Exchange Commission's Brief in Opposition to Defendants' Motion to Exclude the Testimony of [redacted], Ph.D., filed herewith.
3. Attached hereto as Exhibit A is a true and correct copy of Craig MacKinlay, "Event Studies in Economics and Finance," *Journal of Economic Literature*, Vol. XXXV, at 14-16 (March 1997).
4. Attached hereto as Exhibit B is a true and correct copy of Mohammad Hashemi Joo, Yuka Nishikawa, and Krishnan Dandapani, "Announcement effects in the cryptocurrency market," *Applied Economics* Vol. 52, No. 44 (2020).

5. Attached hereto as Exhibit C is a true and correct copy of Eugene Fama, "Efficient Capital Markets: II", *The Journal of Finance,* Vol. XLVI, No. 5 (December 1991).

6. Attached hereto as Exhibit D is a true and correct copy of David I. Tabak & Frederick C. Dunbar, "Materiality and Magnitude: Event Studies in the Courtroom," *Litigation Services Handbook* 19-2 (b) (3d ed. 2001).

7. Attached hereto as Exhibit E is a true and correct copy of David Tabak and Chudzoie Okongwu, "Inflation Methodologies in Securities Fraud Cases: Theory and Practice," *NERA Working Paper* (July 2002).

I declare under penalty of perjury under the laws of the United States of America that the foregoing is true and correct to the best of my knowledge, information, and belief.

Dated: New York, New York
August 9, 2022

/s/ Mark R. Sylvester
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EXHIBIT A

Journal of Economic Literature
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Event Studies in Economics and Finance

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1. *Introduction*

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

The event study has many applications. In accounting and finance research, event studies have 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 (see G. William Schwert 1981) and in legal liability cases event studies are used to assess damages (see Mark Mitchell and Jeffry Netter 1994). In the majority of 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 paper the methodology is discussed in terms of applications that use common equity. However, event studies can be applied using debt securities with little modification.

Event studies have a long history. Perhaps the first published study is James Dolley (1933). In this work, he examines the price effects of stock splits, studying nominal price changes at the time of the split. Using a sample of 95 splits from 1921 to 1931, he finds that the price in-

[1] The first three examples will be discussed later in the paper. Grant McQueen and Vance Roley (1993) provide an illustration of the fourth using macroeconomic news announcements.

creased in 57 of the cases and the price declined in only 26 instances. Over the decades from the early 1930s until the late 1960s the level of sophistication of event studies increased. John H. Myers and Archie Bakay (1948), C. Austin Barker (1956, 1957, 1958), and John Ashley (1962) are examples of studies during this time period. The improvements included removing general stock market price movements and separating out confounding events. In the late 1960s seminal studies by Ray Ball and Philip Brown (1968) and Eugene Fama et al. (1969) introduced the methodology that is essentially the same as that which is in use today. Ball and Brown considered the information content of earnings, and Fama et al. studied the effects of stock splits after removing the effects of simultaneous dividend increases.

In the years since these pioneering studies, a number of modifications have been developed. These modifications relate to complications arising from violations of the statistical assumptions used in the early work and relate to adjustments in the design to accommodate more specific hypotheses. Useful papers which deal with the practical importance of many of the complications and adjustments are the work by Stephen Brown and Jerold Warner published in 1980 and 1985. The 1980 paper considers implementation issues for data sampled at a monthly interval and the 1985 paper deals with issues for daily data.

In this paper, event study methods are reviewed and summarized. The paper begins with discussion of one possible procedure for conducting an event study in Section 2. Section 3 sets up a sample event study which will be used to illustrate the methodology. Central to an event study is the measurement of an abnormal stock return. Section 4 details the first step—measuring the normal performance—and Section 5 follows with the necessary tools for calculating an abnormal return, making statistical inferences about these returns, and aggregating over many event observations. The null hypothesis that the event has no impact on the distribution of returns is maintained in Sections 4 and 5. Section 6 discusses modifying this null hypothesis to focus only on the mean of the return distribution. Section 7 presents analysis of the power of an event study. Section 8 presents nonparametric approaches to event studies which eliminate 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. Section 9 presents a cross-sectional regression approach that is useful to investigate such hypotheses. Section 10 considers some further issues relating event study design and the paper closes with the concluding discussion in Section 11.

2. *Procedure for an Event Study*

At the outset it is useful to briefly discuss the structure of an event study. This will provide a basis for the discussion of details later. While there is no unique structure, there is a general flow of analysis. This flow is discussed in this section.

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 with daily data, the event will be the earnings announcement and the event window will include the one day of the announcement. It is customary to define the event window to be larger than the specific period of interest. This permits examination of periods surrounding the event. In practice, the period of interest is often expanded to multiple days, including at least the day of the announcement and the day after the announcement. This captures the price effects of announcements which occur after the stock market closes on the announcement day. The periods prior to and after the event may also be of interest. 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.

After identifying the event, 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 New York Stock Exchange or the American Stock Exchange or may involve restrictions such as membership in a specific industry. At this stage it is useful to summarize some sample characteristics (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.

Appraisal of the event's impact requires 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 expected return without conditioning on the event taking place. For firm i and event date τ the abnormal return is

$$AR_{i\tau} = R_{i\tau} - E(R_{i\tau}|X_\tau) \qquad (1)$$

where $AR_{i\tau}$, $R_{i\tau}$, and $E(R_{i\tau}|X_\tau)$ are the abnormal, actual, and normal returns respectively for time period τ. X_τ is the conditioning information for the normal return model. There are two common choices for modeling the normal return—the *constant mean return model* where X_τ is a constant, and the *market model* where X_τ 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.

Given the selection of a normal performance model, the estimation window needs to be defined. The most common choice, when feasible, is using 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.

With the parameter estimates for the normal performance model, the abnormal returns can be calculated. Next comes the design of the testing framework for the abnormal returns. Important considerations are defining the null hypothesis and determining the techniques for aggregating the individual firm abnormal returns.

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.

Ideally the empirical results will lead to insights relating to understanding the sources and causes of the effects (or lack of effects) of the event under study. Additional analysis may be included to distinguish between competing explanations. Concluding comments complete the study.

3. *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 the description of an example selected to illustrate event study methodology is presented. One particular type of disclosure—quarterly earnings announcements—is considered. The objective is to investigate the information content of these announcements. In other words, the goal is to see if the release of accounting information provides information to the marketplace. If so there should be a correlation between the observed change of the market value of the company and the information.

The example will focus on the quarterly earnings announcements for the 30 firms in the Dow Jones Industrial Index over the five-year period from January 1989 to December 1993. These announcements correspond to the quarterly earnings for the last quarter of 1988 through the third quarter of 1993. The five years of data for 30 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 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. For constructing such a measure, the mean quarterly earnings forecast reported by the Institutional Brokers Estimate System (I/B/E/S) is used 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.

To facilitate the examination of the impact of the earnings announcement on the value of the firm's equity, it is essential to posit the relation between the information release and the change in value of the equity. In this example the task is straightforward. If the earnings disclosures have information content, higher than expected earnings should be associated with increases in value of the equity and lower than expected earnings with decreases. To capture this association, each announcement is assigned to one of three categories: good news, no news, or bad news. Each announcement is categorized using the deviation of the actual earnings from the expected earnings. If the actual exceeds expected by more than 2.5 percent the announcement is designated as good news, and if the actual is more than 2.5 percent less than expected the announcement is designated as bad news. Those announcements where the actual earnings is in the 5 percent 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 parameters of the empirical design to analyze the equity return, i.e., the percent change in value of the equity. It is necessary to specify a length of observation interval, an event window, and an estimation window. For this example the interval is set to one day, thus daily stock returns are used. A 41-day event window is employed, comprised of 20 pre-event days, the event day, and 20 post-event days. For each announcement the 250 trading day period prior to the event window is used as the estimation window. After presenting the methodology of an event study, this example will be drawn upon to illustrate the execution of a study.

4. *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, the assumption that asset returns are jointly multivariate normal and independently and identically distributed through time is imposed. This distributional assumption is sufficient for the constant mean return model and the market model to be correctly specified. While this assumption is strong, in practice it generally does not lead to problems because the assumption is empirically reasonable and inferences using the normal return models tend to be robust to deviations from the assumption. Also one can easily modify the statistical framework so that the analysis of the abnormal returns is autocorrelation and heteroskedasticity consistent by using a generalized method-of-moments approach.

A. *Constant Mean Return Model*

Let μ_i be the mean return for asset i. Then the constant mean return model is

$$R_{it} = \mu_i + \zeta_{it} \tag{2}$$

$$E(\zeta_{it}) = 0 \qquad \text{var}(\zeta_{it}) = \sigma^2_{\zeta_i}.$$

where R_{it} is the period-t return on security i and ζ_{it} is the time period t disturbance term for security i with an expectation of zero and variance $\sigma^2_{\zeta_i}$.

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 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 risk free return generally measured using the U.S. Treasury Bill with one month to maturity) as well as nominal returns.

B. *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. For any security i the market model is

$$R_{it} = \alpha_i + \beta_i R_{mt} + \varepsilon_{it} \tag{3}$$

$$E(\varepsilon_{it} = 0) \qquad \text{var}(\varepsilon_{it}) = \sigma^2_{\varepsilon_i}$$

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 $\sigma^2_{\varepsilon_i}$ are the parameters of the market model. In applications a broad based stock index is used for the market portfolio, with the S&P 500 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 in turn 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.

C. *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 are motivated by the benefits 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. Other multifactor models include industry indexes in addition to the market. William Sharpe (1970) and Sharpe, Gordon Alexander, and Jeffery Bailey (1995, p. 303) provide discussion of 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 to unity. This procedure implicitly assumes that expected return is directly related to market value of equity.

Generally, the gains from employing multifactor models for event studies are limited. The reason for the limited gains is the empirical fact that the marginal explanatory power of additional factors 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.

The use of other models is dictated by data availability. An example of a normal performance return model implemented in situations with limited data is 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 zero and β_i constrained to be one. Because the model coefficients are prespecified, an estimation period is not required to obtain parameter estimates. An example of when such a model is used is in studies of the under pricing of initial public offerings. Jay Ritter (1991) presents such an example. A general recommendation is to only use such restricted models if necessary, and if necessary, consider the possibility of biases arising from the imposition of the restrictions.

D. *Economic Models*

Economic models can be cast as restrictions on the statistical models to provide more constrained normal return models. Two common economic models which provide restrictions are the Capital Asset Pricing Model (CAPM) and the Arbitrage Pricing Theory (APT). The CAPM due to Sharpe (1964) and John Lintner (1965) is an equilibrium theory where the expected return of a given asset is determined by its covariance with the market portfolio. The APT due to Stephen Ross (1976) is an asset pricing theory where the expected return of a given asset is a linear combination of multiple risk factors.

The use of the Capital Asset Pricing Model is common in event studies of the 1970s. However, deviations from the CAPM have been discovered, implying that the validity of the restrictions imposed by the CAPM on the market model is questionable.[2] This has introduced the possibility that the results of the studies may be sensitive to the specific CAPM restrictions. Because this potential for sensitivity can be avoided at little cost by using the market model, the use of the CAPM has almost ceased.

Similarly, other studies have employed multifactor normal performance models motivated by the Arbitrage Pricing Theory. A general finding is that with the APT the most important factor behaves like a market factor and additional factors add relatively little explanatory power. Thus the gains from using an APT motivated model versus the market model are small. See Stephen Brown and Mark Weinstein (1985) for further discussion. The main potential gain from using a model based on the arbitrage pricing theory is to eliminate the biases introduced by using the CAPM. However, because the statistically motivated models also eliminate these biases, for event studies such models dominate.

5. *Measuring and Analyzing Abnormal Returns*

In this section the problem of measuring and analyzing abnormal returns is considered. The framework is developed using the market model as the normal performance return model. The analysis is virtually identical for the constant mean return model.

Some notation is first defined to facilitate the measurement and analysis of abnormal returns. Returns will be indexed 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. Even if the event being considered is an announcement on given date it is typical to set the event window length to be larger than one. This facilitates the use of abnormal returns around the event day in the analysis. When applicable, the post-event window will be from $\tau = T_2 + 1$ to $\tau = T_3$ and of length $L_3 = T_3 - T_2$. The timing sequence is illustrated with a time line in Figure 1.

[2] Eugene Fama and Kenneth French (1996) provide discussion of these anomalies.

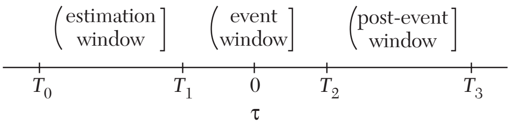

Figure 1. Time line for an event study.

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 returns around the event. 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 capture the event impact. This would be problematic because the methodology is built around the assumption that the event impact is captured by the abnormal returns. On occasion, the post event window data is included with the estimation window data to estimate the normal return model. The goal of this approach is to increase the robustness of the normal market return measure to gradual changes in its parameters. In Section 6 expanding the null hypothesis to accommodate changes in the risk of a firm around the event is considered. In this case an estimation framework which uses the event window returns will be required.

A. *Estimation of the Market Model*

Under general conditions ordinary least squares (OLS) is a consistent estimation procedure for the market model parameters. Further, given the assumptions of Section 4, OLS is efficient. For the i^{th} firm in event time, the OLS estimators of the market model parameters for an estimation window of observations are

$$\hat{\beta}_i = \frac{\sum_{\tau=T_0+1}^{T_1} (R_{i\tau} - \hat{\mu}_i)(R_{m\tau} - \hat{\mu}_m)}{\sum_{\tau=T_0+1}^{T_1} (R_{m\tau} - \hat{\mu}_m)^2} \tag{4}$$

$$\hat{\alpha}_i = \hat{\mu}_i - \hat{\beta}_i \hat{\mu}_m \tag{5}$$

$$\hat{\sigma}^2_{\varepsilon_i} = \frac{1}{L_1 - 2} \sum_{\tau=T_0+1}^{T_1} (R_{i\tau} - \hat{\alpha}_i - \hat{\beta}_i R_{m\tau})^2 \tag{6}$$

where

$$\hat{\mu}_i = \frac{1}{L_1} \sum_{\tau=T_0+1}^{T_1} R_{i\tau}$$

and

$$\hat{\mu}_m = \frac{1}{L_1} \sum_{\tau=T_0+1}^{T_1} R_{m\tau}.$$

$R_{i\tau}$ and $R_{m\tau}$ are the return in event period τ for security i and the market respectively. The use of the OLS estimators to measure abnormal returns and to develop their statistical properties is addressed next. First, the properties of a given security are presented followed by consideration of the properties of abnormal returns aggregated across securities.

B. *Statistical Properties of Abnormal Returns*

Given the market model parameter estimates, one can measure and analyze the abnormal returns. Let $\widehat{AR}_{i\tau}$, $\tau = T_1 + 1, \ldots, T_2$, be the sample of L_2 abnormal returns for firm i in the event window. Using the market model to measure the normal return, the sample abnormal return is

$$\widehat{AR}_{i\tau} = R_{i\tau} - \hat{\alpha}_i - \hat{\beta}_i R_{m\tau}. \tag{7}$$

The abnormal return is the disturbance term of the market model calculated on an out of sample basis. Under the null hypothesis, conditional on the event window market returns, the abnormal returns will be jointly normally distributed with a zero conditional mean and conditional variance $\sigma^2(\hat{AR}_{i\tau})$ where

$$\sigma^2(\hat{AR}_{i\tau}) = \sigma^2_{\varepsilon_i} + \frac{1}{L_1}\left[1 + \frac{(R_{m\tau} - \hat{\mu}_m)^2}{\hat{\sigma}^2_m}\right]. \qquad (8)$$

From (8), the conditional variance has two components. One component is the disturbance variance $\sigma^2_{\varepsilon_i}$ from (3) and a second component is additional variance due to the sampling error in α_i and β_i. This sampling error, which is common for all the event window observations, also leads 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 approaches zero as the sampling error of the parameters vanishes. The variance of the abnormal return will be $\sigma^2_{\varepsilon_i}$ and the abnormal return observations will become independent through time. In practice, the estimation window can usually be chosen to be large enough to make it reasonable to assume that the contribution of the second component to the variance of the abnormal return is zero.

Under the null hypothesis, H_0, that the event has no impact on the behavior of returns (mean or variance) the distributional properties of the abnormal returns can be used to draw inferences over any period within the event window. Under H_0 the distribution of the sample abnormal return of a given observation in the event window is

$$\hat{AR}_{i\tau} \sim N(0, \sigma^2(\hat{AR}_{i\tau})). \qquad (9)$$

Next (9) is built upon to consider the aggregation of the abnormal returns.

C. *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. The concept of a cumulative abnormal return is necessary to accommodate a multiple period event window. Define $\hat{CAR}_i(\tau_1,\tau_2)$ as the sample cumulative abnormal return (CAR) from τ_1 to τ_2 where $T_1 < \tau_1 \le \tau_2 \le T_2$. The CAR from τ_1 to τ_2 is the sum of the included abnormal returns,

$$\hat{CAR}_i(\tau_1,\tau_2) = \sum_{\tau=\tau_1}^{\tau_2} \hat{AR}_{i\tau}. \qquad (10)$$

Asymptotically (as L_1 increases) the variance of $\hat{CAR}_i$ is

$$\sigma^2_i(\tau_1,\tau_2) = (\tau_2 - \tau_1 + 1)\,\sigma^2_{\varepsilon_i}. \qquad (11)$$

This large sample estimator of the variance can be used for reasonable values of L_1. However, for small values of L_1 the variance of the cumulative abnormal return should be adjusted for the effects of the estimation error in the normal model parameters. This adjustment involves the second term of (8) and a further related adjustment for the serial covariance of the abnormal return.

The distribution of the cumulative abnormal return under H_0 is

$$\hat{CAR}_i(\tau_1,\tau_2) \sim N(0, \sigma^2_i(\tau_1,\tau_2)). \qquad (12)$$

Given the null distributions of the abnormal return and the cumulative abnormal return, tests of the null hypothesis can be conducted.

However, tests with one event observation are not likely to be useful so it is necessary to aggregate. The abnormal return observations must be aggregated for the event window and across observations of the event. For this aggregation,

TABLE 1

	Market Model					
	Good News		No News		Bad News	
Event Day	AR	CAR	AR	CAR	AR	CAR
−20	.093	.093	.080	.080	−.107	−.107
−19	−.177	−.084	.018	.098	−.180	−.286
−18	.088	.004	.012	.110	.029	−.258
−17	.024	.029	−.151	−.041	−.079	−.337
−16	−.018	.011	−.019	−.060	−.010	−.346
−15	−.040	−.029	.013	−.047	−.054	−.401
−14	.038	.008	.040	−.007	−.021	−.421
−13	.056	.064	−.057	−.065	.007	−.414
−12	.065	.129	.146	.081	−.090	−.504
−11	.069	.199	−.020	.061	−.088	−.592
−10	.028	.227	.025	.087	−.092	−.683
−9	.155	.382	.115	.202	−.040	−.724
−8	.057	.438	.070	.272	.072	−.652
−7	−.010	.428	−.106	.166	−.026	−.677
−6	.104	.532	.026	.192	−.013	−.690
−5	.085	.616	−.085	.107	.164	−.527
−4	.099	.715	.040	.147	−.139	−.666
−3	.117	.832	.036	.183	.098	−.568
−2	.006	.838	.226	.409	−.112	−.680
−1	.164	1.001	−.168	.241	−.180	−.860
0	.965	1.966	−.091	.150	−.679	−1.539
1	.251	2.217	−.008	.142	−.204	−1.743
2	−.014	2.203	.007	.148	.072	−1.672
3	−.164	2.039	.042	.190	.083	−1.589
4	−.014	2.024	.000	.190	.106	−1.483
5	.135	2.160	−.038	.152	.194	−1.289
6	−.052	2.107	−.302	−.150	.076	−1.213
7	.060	2.167	−.199	−.349	.120	−1.093
8	.155	2.323	−.108	−.457	−.041	−1.134
9	−.008	2.315	−.146	−.603	−.069	−1.203
10	.164	2.479	.082	−.521	.130	−1.073
11	−.081	2.398	.040	−.481	−.009	−1.082
12	−.058	2.341	.246	−.235	−.038	−1.119
13	−.165	2.176	.014	−.222	.071	−1.048
14	−.081	2.095	−.091	−.312	.019	−1.029
15	−.007	2.088	−.001	−.314	−.043	−1.072
16	.065	2.153	−.020	−.334	−.086	−1.159
17	.081	2.234	.017	−.317	−.050	−1.208
18	.172	2.406	.054	−.263	.066	−1.142
19	−.043	2.363	.119	−.144	−.088	−1.230
20	.013	2.377	.094	−.050	−.028	−1.258

TABLE 1 (*Cont.*)

Constant Mean Return Model					
Good News		No News		Bad News	
AR	CAR	AR	CAR	AR	CAR
.105	.105	.019	.019	–.077	–.077
–.235	–.129	–.048	–.029	–.142	–.219
.069	–.060	–.086	–.115	–.043	–.262
–.026	–.086	–.140	–.255	–.057	–.319
–.086	–.172	.039	–.216	–.075	–.394
–.183	–.355	.099	–.117	–.037	–.431
–.020	–.375	–.150	–.266	–.101	–.532
–.025	–.399	–.191	–.458	–.069	–.601
.101	–.298	.133	–.325	–.106	–.707
.126	–.172	.006	–.319	–.169	–.876
.134	–.038	.103	–.216	–.009	–.885
.210	.172	.022	–.194	.011	–.874
.106	.278	.163	.–031	.135	-.738
–.002	.277	.009	–.022	–.027	–.765
.011	.288	–.029	–.051	.030	–.735
.061	.349	–.068	–.120	.320	–.415
.031	.379	.089	–.031	–.205	–.620
.067	.447	.013	–.018	.085	–.536
.010	.456	.311	.294	–.256	–.791
.198	.654	–.170	.124	–.227	–1.018
1.034	1.688	–.164	–.040	–.643	–1.661
.357	2.045	–.170	–.210	–.212	–1.873
–.013	2.033	.054	–.156	.078	–1.795
.088	1.944	–.121	–.277	.146	–1.648
.041	1.985	.023	–.253	.149	-1.499
.248	2.233	–.003	–.256	.286	–1.214
–.035	2.198	–.319	–.575	.070	–1.143
.017	2.215	–.112	–.687	.102	–1.041
.112	2.326	–.187	–.874	.056	–.986
–.052	2.274	–.057	–.931	–.071	–1.056
.147	2.421	.203	–.728	.267	–.789
–.013	2.407	.045	–.683	.006	–.783
–.054	2.354	.299	–.384	.017	–.766
–.246	2.107	–.067	–.451	.114	–.652
–.011	2.096	–.024	–.475	.089	–.564
–.027	2.068	–.059	–.534	–.022	–.585
.103	2.171	–.046	–.580	–.084	–.670
.066	2.237	–.098	–.677	–.054	–.724
.110	2.347	.021	–.656	–.071	–.795
–.055	2.292	.088	–.568	.026	–.769
.019	2.311	.013	–.554	–.115	–.884

Abnormal returns for an event study of the information content of earnings announcements. The sample consists of a total of 600 quarterly announcements for the 30 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 return model. The announcements are categorized into three groups, good news, no news, and bad news. AR is the sample average abnormal return for the specified day in event time and CAR is the sample average cumulative abnormal return for day –20 to the specified day. Event time is days relative to the announcement date.

it is assumed that 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. Later inferences with clustering will be discussed.

The individual securities' abnormal returns can be aggregated using $\widehat{AR}_{i\tau}$ from (7) for each event period, $\tau = T_1 + 1, \ldots, T_2$. Given N events, the sample aggregated abnormal returns for period τ is

$$\overline{AR}_\tau = \frac{1}{N}\sum_{i=1}^{N} \widehat{AR}_{i\tau} \tag{13}$$

and for large L_1, its variance is

$$\mathrm{var}(\overline{AR}_\tau) = \frac{1}{N^2}\sum_{i=1}^{N} \sigma^2_{\varepsilon_i}. \tag{14}$$

Using these estimates, the abnormal returns for any event period can be analyzed.

The average abnormal returns can then be aggregated over the event window using the same approach as that used to calculate the cumulative abnormal return for each security i. For any interval in the event window

$$\overline{CAR}(\tau_1,\tau_2) = \sum_{\tau=\tau_1}^{\tau_2} \overline{AR}_\tau, \tag{15}$$

$$\mathrm{var}(\overline{CAR}(\tau_1,\tau_2)) = \sum_{\tau=\tau_1}^{\tau_2} \mathrm{var}\,(\overline{AR}_\tau). \tag{16}$$

Observe that equivalently one can form the CAR's security by security and then aggregate through time,

$$\overline{CAR}(\tau_1,\tau_2) = \frac{1}{N}\sum_{i=1}^{N} \widehat{CAR}_i\,(\tau_1,\tau_2) \tag{17}$$

$$\mathrm{var}(\overline{CAR}(\tau_1,\tau_2)) = \frac{1}{N^2}\sum_{i=1}^{N} \sigma_i^2(\tau_1,\tau_2). \tag{18}$$

For the variance estimators the assumption that the event windows of the N securities do not overlap is used to set the covariance terms to zero. Inferences about the cumulative abnormal returns can be drawn using

$$\overline{CAR}(\tau_1,\tau_2) \sim N[0, \mathrm{var}(\overline{CAR}\,(\tau_1, \tau_2))] \tag{19}$$

to test the null hypothesis that the abnormal returns are zero. In practice, because $\sigma^2_{\varepsilon_i}$ is unknown, an estimator must be used to calculate the variance of the abnormal returns as in (14). The usual sample variance measure of $\sigma^2_{\varepsilon_i}$ from the market model regression in the estimation window is an appropriate choice. Using this to calculate $\mathrm{var}(\overline{AR}_\tau)$ in (14), H_0 can be tested using

$$\theta_1 = \frac{\overline{CAR}(\tau_1,\tau_2)}{\mathrm{var}(\overline{CAR}\,(\tau_1,\tau_2))^{\frac{1}{2}}} \sim N(0,1). \tag{20}$$

This distributional result is asymptotic with respect to the number of securities N and the length of estimation window L_1.

Modifications to the basic approach presented above are possible. One common modification is to standardize each abnormal return using an estimator of its standard deviation. For certain alternatives, such standardization can lead to more powerful tests. James Patell (1976) presents tests based on standardization and Brown and Warner (1980, 1985) provide comparisons with the basic approach.

D. *CAR's for the Earnings Announcement Example*

The information content of earnings example previously described illustrates the use of sample abnormal residuals and sample cumulative abnormal returns. Table 1 presents the abnormal returns av-

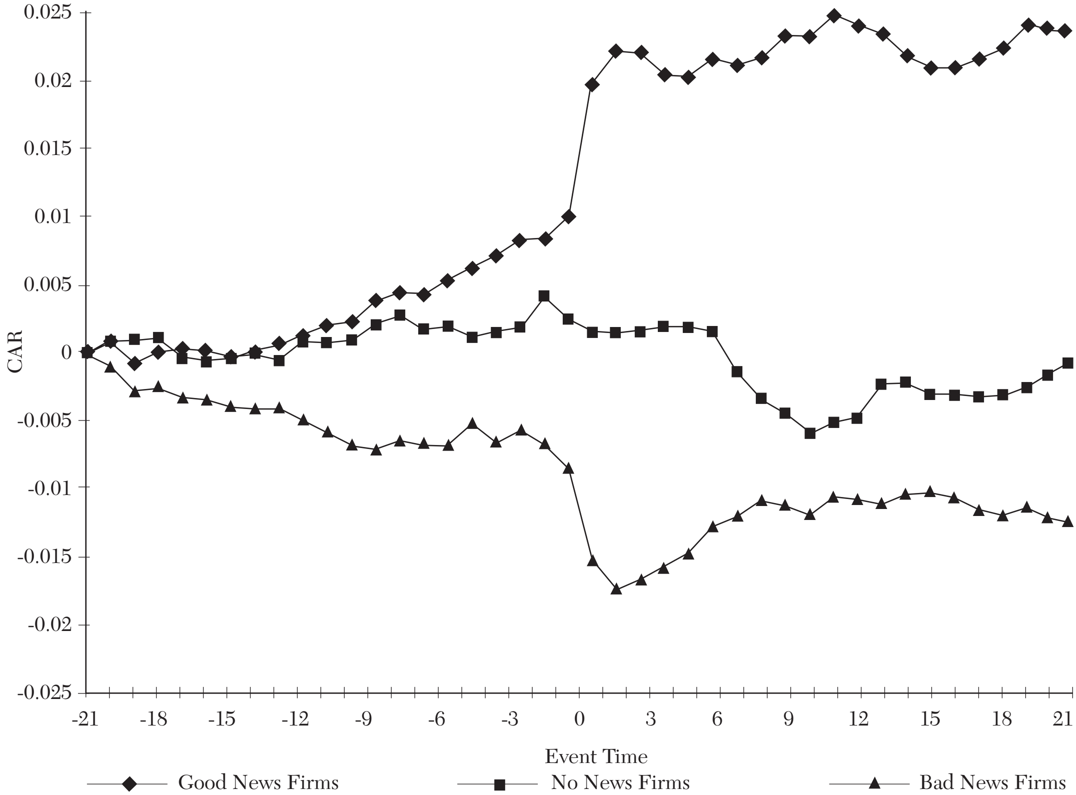

Figure 2a. Plot of cumulative abnormal return for earning announcements from event day -20 to event day 20. The abnormal return is calculated using the market model as the normal return measure.

eraged across the 600 event observations (30 firms, 20 announcements per firm) as well as the aggregated 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 CAR's from the market model in Figure 2a and the CAR's from the constant mean return model in Figure 2b.

The results of this example are largely consistent with the existing literature on the information content of earnings. The evidence strongly supports the hypothesis that earnings announcements do indeed convey information useful for the valuation of firms. Focusing on the announcement day (day 0) the sample average abnormal return for the good news firm using the market model is 0.965 percent. Given the standard error of the one day good news average abnormal return is 0.104 percent, the value of θ_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 percent, with a standard error of 0.098 percent, leading to θ_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 percent and

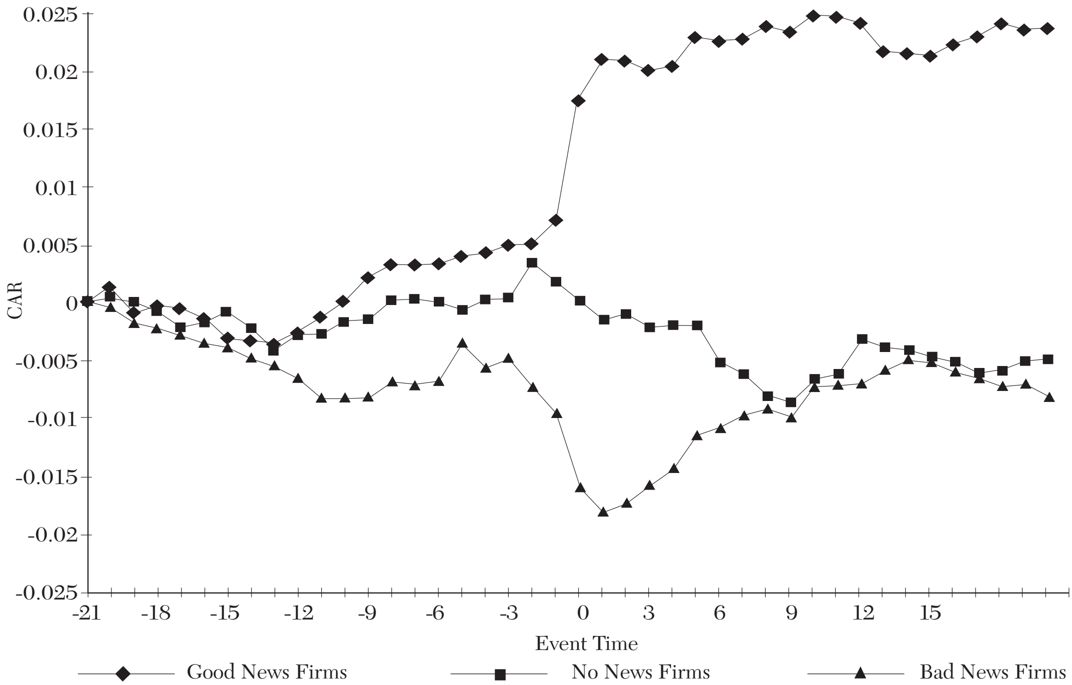

Figure 2b. Plot of cumulative abnormal return for earning announcements from event day -20 to event day 20. The abnormal return is calculated using the constant mean return model as the normal return

with a standard error of 0.098 percent is less than one standard error from zero. There is some evidence of the announcement effect on day one. The average abnormal return is 0.251 percent and –0.204 percent 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 return model are consistent with those from the market model. However, there is some loss of precision using the constant 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 percent to 0.130 percent for good news firms, from 0.098 percent to 0.124 percent for no news firms, and from 0.098 percent to 0.131 percent for bad news firms. These increases are to be expected when considering a sample of large firms such as those in the Dow Index because 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 with the bad news firms in days two through eight.

E. *Inferences with Clustering*

The analysis aggregating abnormal returns has assumed that the event windows of the included securities do not overlap in calendar time. This assumption allows us to calculate the variance of the aggregated sample cumulative abnormal returns without concern about the covariances across securities because they are zero. However, when the event windows do overlap and the covariances between the abnormal returns will not be zero, the distributional results presented for the aggregated abnormal returns are no longer applicable. Victor Bernard (1987) discusses some of the problems related to clustering.

Clustering can be accommodated in two ways. The abnormal returns can be aggregated into a portfolio dated using event time and the security level analysis of Section 5 can applied to the portfolio. This approach will allow for cross correlation of the abnormal returns.

A second method to handle clustering is to analyze the abnormal returns without aggregation. One can consider testing the null hypothesis of the event having no impact using unaggregated security by security data. This approach is applied most commonly when there is total clustering, that is, there is an event on the same day for a number of firms. The basic approach is an application of a multivariate regression model with dummy variables for the event date. This approach is developed in the papers of Katherine Schipper and Rex Thompson (1983, 1985) and Daniel Collins and Warren Dent (1984). The advantage of the approach is that, unlike the portfolio approach, an alternative hypothesis where some of the firms have positive abnormal returns and some of the firms have negative abnormal returns can be accommodated. However, in general the approach has two drawbacks—frequently the test statistic will have poor finite sample properties except in special cases and often the test will have little power against economically reasonable alternatives. The multivariate framework and its analysis is similar to the analysis of multivariate tests of asset pricing models. MacKinlay (1987) provides analysis in that context.

6. *Modifying the Null Hypothesis*

Thus far the focus has been on a single null hypothesis—that the given event has no impact on the behavior of the returns. With this null hypothesis either a mean effect or a variance effect will represent a violation. However, in some applications one may be interested in testing for a mean effect. In these cases, it is necessary to expand the null hypothesis to allow for changing (usually increasing) variances. To allow for changing variance as part of the null hypothesis, it is necessary to eliminate the reliance on the past returns to estimate the variance of the aggregated cumulative abnormal returns. This is accomplished by using the cross section of cumulative abnormal returns to form an estimator of the variance for testing the null hypothesis. Ekkehart Boehmer, Jim Musumeci, and Annette Poulsen (1991) discuss methodology to accommodate changing variance.

The cross sectional approach to estimating the variance can be applied to the average cumulative abnormal return ($\overline{CAR}(\tau_1,\tau_2)$). Using the cross-section to form an estimator of the variance gives

$$\mathrm{var}(\overline{CAR}(\tau_1,\tau_2)) = \frac{1}{N^2}\sum_{i=1}^{N}(\widehat{CAR}_i(\tau_1,\tau_2) - \overline{CAR}(\tau_1,\tau_2))^2. \quad (21)$$

For this estimator of the variance to be consistent, the abnormal returns need to be uncorrelated in the cross-section. An absence of clustering is sufficient for this requirement. Note that cross-sectional homoskedasticity is not required. Given this variance estimator, the null hypothesis that the cumulative abnormal returns are zero can then be tested using the usual theory.

One may also be interested in the question of the impact of an event on the risk of a firm. The relevant measure of risk must be defined before this question can be addressed. One choice as a risk measure is the market model beta which is consistent with the Capital Asset Pricing Model being appropriate. Given this choice, the market model can be formulated to allow the beta to change over the event window and the stability of the risk can be examined. Edward Kane and Haluk Unal (1988) present an application of this idea.

7. *Analysis of Power*

An important consideration when setting up an event study is the ability to detect the presence of a non-zero abnormal return. The inability to distinguish between the null hypothesis and economically interesting alternatives would suggest the need for modification of the design. In this section the question of the likelihood of rejecting the null hypothesis for a specified level of abnormal return associated with an event is addressed. Formally, the power of the test is evaluated.

Consider a two-sided test of the null hypothesis using the cumulative abnormal return based statistic θ_1 from (20). It is assumed that the abnormal returns are uncorrelated across securities; thus the variance of $\overline{CAR}$ is $1/N^2 \sum_{i=1}^{N} \sigma_i^2(\tau_1,\tau_2)$ and N is the sample size. Because the null distribution of θ_1 is standard normal, for a two sided test of size α, the null hypothesis will be rejected if θ_1 is in the critical region, that is,

$$\theta_1 < c\left(\frac{\alpha}{2}\right) \text{ or } \theta_1 > c\left(1 - \frac{\alpha}{2}\right)$$

where $c(x) = \phi^{-1}(x)$. $\phi(\cdot)$ is the standard normal cumulative distribution function (CDF).

Given the specification of the alternative hypothesis H_A and the distribution of θ_1 for this alternative, the power of a test of size α can be tabulated using the power function,

$$P(\alpha,H_A) = pr\left(\theta_1 < c\left(\frac{\alpha}{2}\right) \mid H_A\right) + pr\left(\theta_1 > c\left(1 - \frac{\alpha}{2}\right) \mid H_A\right). \quad (22)$$

The distribution of θ_1 under the alternative hypothesis considered below will be normal. The mean will be equal to the true cumulative abnormal return divided by the standard deviation of $\overline{CAR}$ and the variance will be equal to one.

To tabulate the power one must posit economically plausible scenarios. The alternative hypotheses considered are four levels of abnormal returns, 0.5 percent, 1.0 percent, 1.5 percent, and 2.0 percent and two levels of the average variance for the cumulative abnormal return of a given security over the event period, 0.0004 and 0.0016. The

TABLE 2

Sample Size	Abnormal Return .005	.010	.015	.020	Abnormal Return .005	.010	.015	.020
	$\sigma = 0.02$				$\sigma = 0.04$			
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

Power of event study methodology for test of the null hypothesis that the abnormal return is zero. The power is reported for a two-sided test using θ_1 with a size of 5 percent. The sample size is the number of event observations included the study and σ is the square root of the average variance of the abnormal return across firms.

sample size, that is the number of securities for which the event occurs, is varied from one to 200. The power for a test with a size of 5 percent is documented. With $\alpha = 0.05$, the critical values calculated using $c(\alpha/2)$ and $c(1 - \alpha/2)$ are -1.96 and 1.96 respectively. Of course, in applications, the power of the test should be considered when selecting the size.

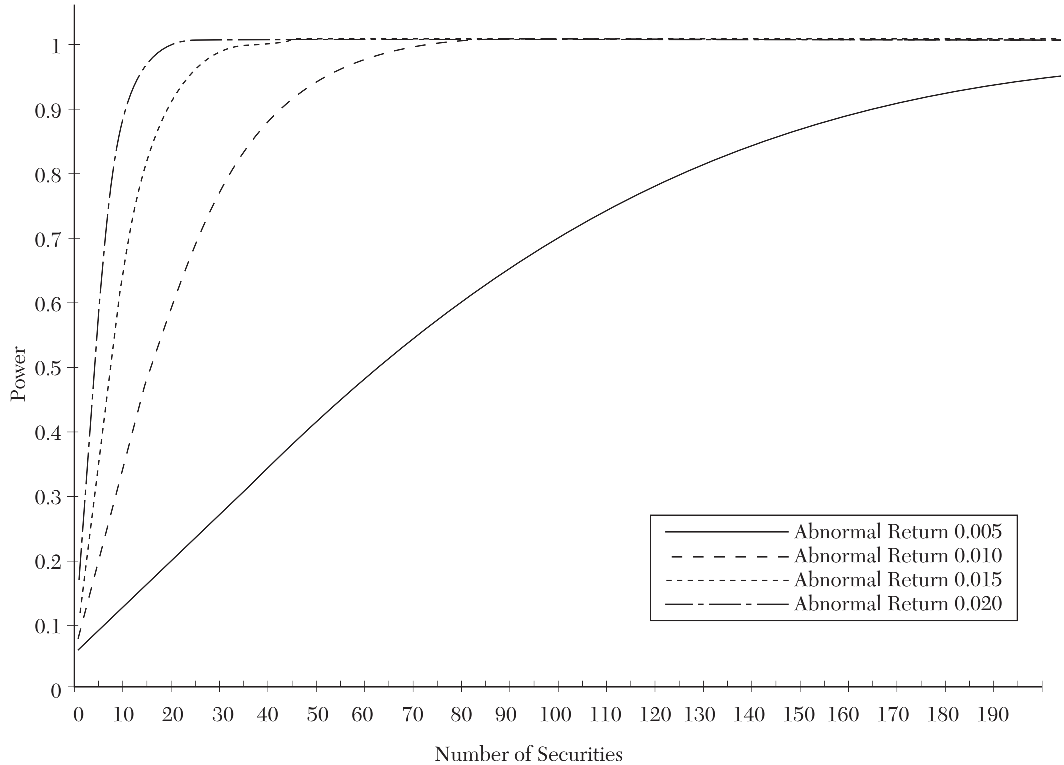

Figure 3a. Power of event study test statistic θ_1 to reject the null hypothesis that the abnormal return is zero, when the square root of the average variance of the abnormal return across firms is 2 percent.

The power results are presented in Table 2, and are plotted in Figures 3a and 3b. The results in the left panel of Table 2 and Figure 3a are for the case where the average variance is 0.0004. This corresponds to a cumulative abnormal return standard deviation of 2 percent and is an appropriate value for an event which does not lead to increased variance and can be examined using a one-day event window. In terms of having high power this is the best case scenario. The results illustrate that when the abnormal return is only 0.5 percent the power can be low. For example with a sample size of 20 the power of a 5 percent 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 percent the power of a 5 percent 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 percent 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 2 and in Figure 3b the power results are presented for the case where the average variance of the cumulative abnormal return is 0.0016. 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

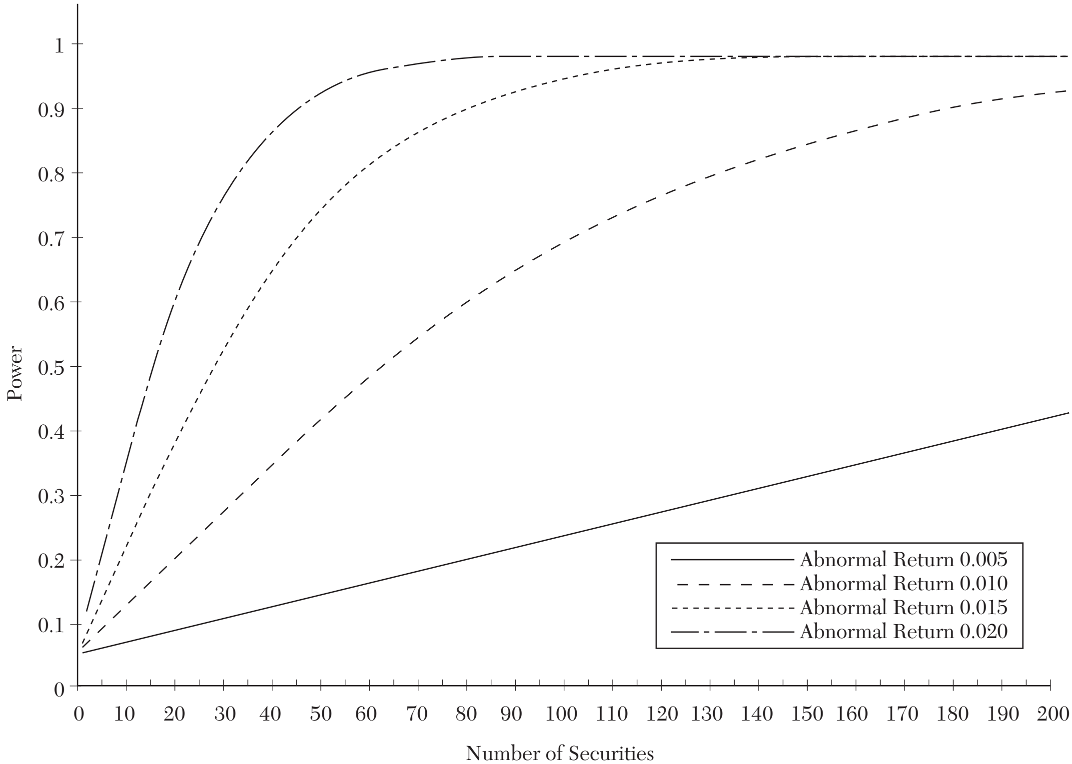

Figure 3b. Power of event study test statistic θ_1 to reject the null hypothesis that the abnormal return is zero, when the square root of the average variance of the abnormal return across firms is 4 percent.

which is accommodated as part of the null hypothesis. When the average variance of the CAR is increased from 0.0004 to 0.0016 there is a dramatic power decline for a 5 percent test. When the CAR is 0.5 percent the power is only 0.09 with 20 firms and is only 0.42 with a sample of 200 firms. This magnitude of abnormal return is difficult to detect with the larger variance. In contrast, when the CAR is as large as 1.5 percent or 2.0 percent the 5 percent test is still has reasonable power. For example, when the abnormal return is 1.5 percent and 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.

In the preceding analysis the power is considered analytically for the given distributional assumptions. If the distributional assumptions are inappropriate then the results may differ. However, Brown and Warner (1985) consider this possible difference and find that the analytical computations and the empirical power are very close.

It is difficult to make general conclusions concerning the adequacy of the ability of event study methodology to detect non-zero abnormal returns. When conducting an event study it is best 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 to test.

8. *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 approaches are available which are nonparametric in nature. These approaches are free of specific assump- tions concerning the distribution of returns. Common nonparametric tests for event studies are the sign test and the rank test. These tests are discussed next.

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 $H_A{:}\text{p} > 0.5$ 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 θ_2 be the test statistic,

$$\theta_2 = \left[\frac{N^+}{N} - 0.5\right]\frac{\sqrt{N}}{0.5} \sim N(0,1). \qquad (23)$$

This distributional result is asymptotic. For a test of size $(1 - \alpha)$, H_0 is rejected if $\theta_2 > \Phi^{-1}(\alpha)$.

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. In response to this possible shortcoming, Charles Corrado (1989) proposes a nonparametric rank test for abnormal performance in event studies. A brief description of his test of no abnormal return for event day zero follows. The framework can be easily altered for more general tests.

Drawing on notation previously introduced, consider a sample of L_2 abnormal returns for each of N securities. To implement the rank test, for each security it is necessary to rank the abnormal returns from one to L_2. Define $K_{i\tau}$ as the rank of the abnormal return of security i for event time period τ. Recall, 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 of the event day is $(L_2 + 1)/2$ under the null hypothesis. The test statistic for the null hypothesis of no abnormal return on event day zero is

$$\theta_3 = \frac{1}{N}\sum_{i=l}^{N}\left(K_{i0} - \frac{L_2 + 1}{2}\right) / s(K) \qquad (24)$$

where

$$s(K) = \sqrt{\frac{1}{L_2}\sum_{\tau=T_1+1}^{T_2}\left(\frac{1}{N}\sum_{i=1}^{N}\left(K_{i\tau} - \frac{L_2 + 1}{2}\right)\right)^2}. \qquad (25)$$

Tests of the null hypothesis can be implemented using the result that the asymptotic null distribution of θ_3 is standard normal. Corrado (1989) includes further discussion of details of this test.

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

9. *Cross-Sectional Models*

Theoretical insights can result from examining the association between the magnitude of the abnormal return and characteristics specific to the event observation. Often such an exercise can be helpful when multiple hypotheses exist for the source of the abnormal return. A cross-sectional regression model is an appropriate tool to investigate this association. The basic approach is to run a cross-sectional regression of the abnormal returns on the characteristics of interest.

Given a sample of N abnormal return observations and M characteristics, the regression model is:

$$AR_j = \delta_0 + \delta_1 x_{lj} + \cdots + + \delta_M x_{Mj} + \eta_j \qquad (26)$$

$$E(\eta_j) = 0 \qquad (27)$$

where AR_{j} is the j^{th} abnormal return observation, $x_{mj}, m = 1, \ldots, M$, are M characteristics for the j^{th} observation and η_j is the zero mean disturbance term that is uncorrelated with the x's. δ_m, $m = 0, \ldots, M$ are the regression coefficients. The regression model can be estimated using OLS. Assuming the η_j's are cross-sectionally uncorrelated and homoskedastic, inferences can be conducted using the usual OLS standard errors. Alternatively, without assuming homoskedasticity, heteroskedasticity-consistent t-statistics using standard errors can be derived using the approach of Halbert White (1980). The use of heteroskedasticity-consistent standard errors is advisable because there is no reason to expect the residuals of (26) to be homoskedastic.

Paul Asquith and David Mullins (1986) provide an example of this cross-sectional 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.

Issues concerning the interpretation of the results can arise with 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 the firm characteristics to forecast the likelihood of the event occurring. In these cases, a linear relation between the valuation effect of the event and the firm characteristic can be hidden. Paul Malatesta and Thompson (1985) and William Lanen and Thompson (1988) provide examples of this situation.

Technically, with 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 breaks down and the OLS estimators are inconsistent. Consistent estimators can be derived by explicitly incorporating the selection bias. Sankarshan Acharya (1988) and B. Espen Eckbo, Vojislav Maksimovic, and Joseph Williams (1990) provide examples of this approach. N. R. Prabhala (1995) provides a good discussion of this problem and the possible solutions. He argues that, despite an incorrect specification, under weak conditions, the OLS ap-

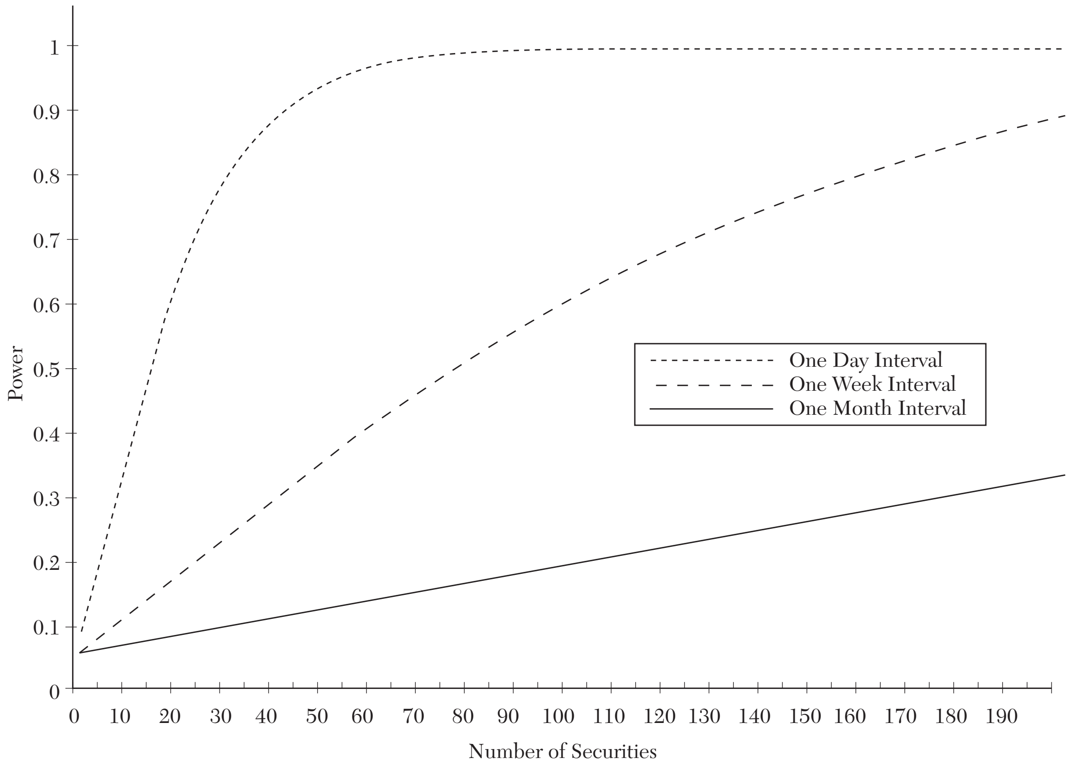

Figure 4. Power of event study test statistic θ_1 to reject the null hypothesis that the abnormal return is zero, for different sampling intervals, when the square root of the average variance of the abnormal return across firms is 4 percent for the daily interval. Size of test is 5 percent.

proach can be used for inferences and that the t-statistics can be interpreted as lower bounds on the true significance level of the estimates.

10. *Other Issues*

A number of further issues often arise when conducting an event study. These issues include the role of the sampling interval, event date uncertainty, robustness, and some additional biases.

A. *Role of Sampling Interval*

Stock return data is available at different sampling intervals, with daily and monthly intervals being the most common. Given the availability of various intervals, the question of the gains of using more frequent sampling arises. To address this question one needs to consider the power gains from shorter intervals. A comparison of daily versus monthly data is provided in Figure 4. The power of the test of no event effect is plotted against the alternative of an abnormal return of one percent for 1 to 200 securities. As one would expect given the analysis of Section 7, 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 percent 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 sampling interval. Dale Morse (1984) presents detailed analysis of the choice of daily versus monthly data and draws the same conclusion.

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. However, the net benefit of intervals less than one day is unclear as some complications are introduced. Discussion of using transaction data for event studies is included in the work of Michael Barclay and Robert Litzenberger (1988).

B. *Inferences with Event-Date Uncertainty*

Thus far it is 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 paper one can not be certain if the market was informed prior to 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 6 indicated that the power properties of two day event windows are still good suggesting that the costs are worth bearing rather than to take the risk of missing the event.

Clifford Ball and Walter Torous (1988) have investigated the 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 indicates that the informal procedure works well and there is little to gain from the more elaborate estimation framework.

C. *Robustness*

The statistical analysis of Sections 4, 5, and 6 is based on assumption that returns are jointly normal and temporally independently and identically distributed. In this section, discussion of the robustness of the results to departures from this assumption is presented. The normality assumption is important for the exact finite sample results to hold. Without assuming normality, all results would be asymptotic. However, this is generally not a problem for event studies because for the test statistics, convergence to the asymptotic distributions is rather quick. Brown and Warner (1985) provide discussion of this issue.

D. *Other Possible Biases*

A number of possible biases can arise in the context of conducting an event study. 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, one is implicitly and incorrectly assuming that they are equally spaced at 24-hour intervals. 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. Myron 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 which shows the nontrading-adjusted beta estimates of thinly traded securities to be approximately 10 to 20 percent larger than the unadjusted estimates. However, for actively traded securities, the adjustments are generally small and unimportant.

Prem 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. When comparing the distribution of these abnormal returns to the distribution of the abnormal returns using the usual OLS betas finds that the differences are minimal. This suggests that in general the adjustments for thin trading are not important.

The methodology used to compute the cumulative abnormal returns can induce an upward bias. 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 offer side of the market. Marshall Blume and Robert 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 offer spreads. In these cases the bias can be eliminated by considering cumulative abnormal returns which represent buy and hold strategies.

11. *Concluding Discussion*

In closing, examples of event study successes and limitations are presented. 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 first announcement.

In the 1960s there was a paucity of empirical evidence on the wealth effects of mergers and acquisitions. For example, Henry 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 argued that mergers represent a natural outcome in an efficiently operating market for corporate control and consequently provide protection for shareholders. He downplayed the importance of the argument that mergers reduce competition. At the conclusion of his article Manne suggested that the two competing hypotheses for mergers could be separated by studying the price effects of the involved corporations. He hypothesized 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, 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. Gregg Jarrell and Poulsen (1989) document that the average abnormal return for target shareholders exceeds 20 percent for a sample of 663 successful takeovers from 1960 to 1985. In contrast the abnormal returns for acquirers is close to zero. For the same sample, Jarrell and Poulsen find an average abnormal return of 1.14 percent for acquirers. In the 1980s they find the average abnormal return is negative at -1.10 percent. 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. Michael Jensen and Richard Ruback (1983) and Jarrell, James 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) find for a sample of 266 firms announcing an equity issue in the period 1963 to 1981 the two day average abnormal return is −2.7 percent and on a sample of 80 firms for the period 1972 to 1982 Wayne Mikkelson and Megan Partch (1986) find the two day average abnormal return is −3.56 percent. 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 percent for a sample of 171 issues. Findings such as these provide the fuel for the development of new theories. For example, in this case, the findings motivate the pecking order theory of capital structure developed by Stewart Myers and Nicholas 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. An important characteristic of a successful event study is the ability to identify precisely the date of the event. In cases where the event date is difficult to identify or the event date is partially anticipated, 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 generally will gradually be incorporated into the value of a corporation as the probability of the change being adopted increases.

Larry Dann and Christopher James (1982) discuss this issue in the context of the impact of deposit interest rate ceilings for thrift institutions. In their study of changes in rate ceilings, they decide not to consider a change in 1973 because it was due to legislative action. Schipper and Thompson (1983, 1985) also encounter this problem in a study of merger related regulations. They attempt to circumvent the problem of regulatory changes being anticipated by identifying dates when the probability of a regulatory change being passed changes. However, they find largely insignificant results leaving open the possibility the of absence of distinct event dates as the explanation of the lack of wealth effects.

Much has been learned from the body of research based on the use of event study methodology. In a general context, event studies have shown that, as would be expected in a rational marketplace, prices do respond to new information. As one moves forward, it is expected that event studies will continue to be a valuable and widely used tool in economics and finance.

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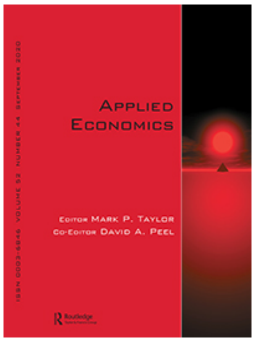
EXHIBIT B



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Announcement effects in the cryptocurrency market

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Announcement effects in the cryptocurrency market

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ABSTRACT
Cryptocurrencies have gained popularity as new economic investment assets globally in recent years. This study examines market reactions to major news events associated with cryptocurrencies. Abnormal returns as well as cumulative abnormal returns (CARs) around major news announcements, both positive and negative, are investigated for three primary cryptocurrencies: Bitcoin, Ethereum, and Ripple. High abnormal returns are observed on the event day (Day 0), and CARs typically diverge during event windows of (−3, 6) and (0, 6), indicating that the information is not fully reflected in prices immediately after the news events. The CARs that linger for six days after an event suggest that the information flow in the cryptocurrency market is visibly slow. The magnitudes of CARs are larger for negative events than for positive events, implying that the market reaction to negative events is stronger than to positive announcements. The findings of this study may have crucial implications for investors, arbitragers and practitioners as we document evidence of potential trading opportunities for investors who initiate a trading position even after announcements.

KEYWORDS
Cryptocurrency; event study; trading opportunities; abnormal returns

JEL CLASSIFICATION
E42; G14

I. Introduction

Cryptocurrencies, among which Bitcoin has been the largest by market capitalization, have become a mainstream investment asset in recent years. There are 2,424 different cryptocurrencies on the market as of February 2020 (coinmarketcap.com), and this number is still increasing. The aims of this study are twofold. First, we examine market reactions during major event announcement periods using event study methodology. Second, we further investigate if the information diffusion allows arbitragers to have an opportunity to make positive profits even after the event announcement. While the literature presents mixed views on the informational efficiency of the cryptocurrency market, this article attempts to find evidence of potential profitable trading opportunities even a few days after an event announcement. To the best of our knowledge, this study is the first to document trading opportunities for an investor to make a profit in the cryptocurrency market even when he/she places a trade after the news becomes public. As a result, this study has crucial implications for progressive investment strategies used by investors and practitioners who are already in the market as well as those who are willing to participate in the cryptocurrency market. These opportunities persist even after making robust adjustments incorporating trading costs.

Among several of their key findings, Ciaian, Rajcaniova, and Kancs (2016) present that the arrival of new information has a positive effect on Bitcoin price. In addition, numerous studies support speculative aspects of cryptocurrencies (for example, Dwyer 2015; Böhme et al. 2015). Some of these articles entertain the idea that cryptocurrency prices appear to be set mainly by market sentiments (Dowd 2014; Weber 2016). Motivated by these findings, we examine market reactions to major event announcements associated with cryptocurrencies by computing abnormal returns as well as cumulative abnormal returns (CARs) around such events. We focus on the three largest cryptocurrencies by market capitalization: Bitcoin, Ethereum, and Ripple. Ten positive and 10 negative major news announcements for each of the three currencies are collected from various online news outlets. There has been an unsettled debate on whether cryptocurrency should be categorized as a commodity,

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currency, or security with different regulators and countries applying different metrics. To move away from such a debate, and due to the unavailability of a reliable proxy for market returns such as a market index (S&P 500 for the stock market, for example) in the cryptocurrency market, we apply the mean-adjusted returns model. In this model, the mean return of the previous trading days is employed as the baseline-expected return, and abnormal returns are calculated as the difference between the actual daily return and the expected return. Cumulative abnormal returns are calculated by summing the abnormal returns during the event window. We use the event windows of Day −3 through Day 6 and Day 0 to Day 6 as baseline windows. Nonparametric test procedures proposed by Corrado (1989) as well as Kolari and Pynnonen (2011) are applied to obtain t-statistics as the sample is found not to be normally distributed. Our results show high abnormal returns on the event day (Day 0) and CARs that typically diverge during the event windows of (−3, 6) and (0, 6), indicating that the information is not fully reflected in prices immediately after the events. CARs that remain for six days after an event imply that investors may be able to take advantage of the slow information flow to make profits by entering the market even after the announcement of the events. The larger magnitudes of CARs observed for negative events than for positive events suggest that the market reaction to negative events is stronger than to positive events. As robustness tests, we utilize different time frames, 180 days and 60 days compared to 365 days used in the baseline analysis, to compute the expected return because of the extreme volatility experienced in the market from the end of 2017 to the beginning of 2018. Our baseline results are robust with expected returns computed based on mean values using different time frames. We further confirm the validity of our findings by presenting out-of-sample analysis in which the trading strategies utilizing our findings are found to be profitable during the period examined.

The rest of the article is organized as follows. Section 2 briefly reviews the literature on the cryptocurrency market and cryptocurrency prices. Section 3 presents the data and methodology. Section 4 discusses the main findings. Lastly, Section 5 offers concluding remarks.

II. Literature review

One of the most fundamental questions about a financial market is whether the market is efficient (Fama 1970, 1998). The first study on the informational efficiency of cryptocurrencies is done by Urquhart (2016) using Bitcoin data. The study concludes that the Bitcoin market is generally inefficient. Nadarajah and Chu (2017) test the market efficiency not on daily returns but on an odd integer power of daily returns without suffering from any loss of information and find that power transformed Bitcoin returns can satisfy the efficient market hypothesis. Bariviera (2017) studies the dynamics of long-range dependence properties of Bitcoin returns from 2011 to 2017 using the Hurst exponent and finds evidence of the time-varying behaviour of market efficiency. Tiwari et al. (2018) further test the informational efficiency of Bitcoin from July 2010 to June 2017 by employing a set of robust long-range dependence estimators and present evidence of Bitcoin market efficiency. Using high-frequency data, Zargar and Kumar (2019) provide evidence of informational inefficiency at higher frequency levels. In tests of weak-form market efficiency of intraday Bitcoin prices, Sensoy (2019) finds a trend of Bitcoin markets becoming more informationally efficient. Kristoufek (2018) examines Bitcoin market efficiency using data from two markets as to the US dollar and Chinese yuan and finds both markets being mostly inefficient from 2010 to 2017. Kristoufek and Vosvrda (2013) present strong evidence of inefficiency for most of the sample period of 2010 to 2017. Cheah et al. (2018) show moderate to high inefficiency in Bitcoin markets, suggesting the possibility for investors to capture speculative profits.

The market efficiency of Bitcoin has been thoroughly explored using different approaches, only resulting in inconclusive outcomes, necessitating the need for examination of the efficiency of other cryptocurrencies. Zhang et al. (2018b) examine nine different cryptocurrencies and find that all of these cryptocurrencies show market inefficiency. Greatly extending the sample, Wei (2018) tests 456 cryptocurrencies and concludes that the market efficiency of established cryptocurrencies is improving.

Some researchers bring psychological aspects to justify sharp movements in market prices. For example, DeBondt and Thaler (1985) find that investors in the stock market overreact to unexpected and dramatic news. They discover substantial weak form market inefficiencies in the stock market. In the cryptocurrency market, Chevapatrakul and Mascia (2019) find some evidence of overreaction in the Bitcoin market during days of sharp price declines and during weeks of market rallies.

Hodoshima and Otsuki (2019) assess Bitcoin performance using the Aumann and Serrano performance index and Sharpe ratio compared to the performance of other assets. Zhang et al. (2018a) study stylized facts (important statistical properties) of random variations in prices of eight different cryptocurrencies. Wang et al. (2019) investigate the predictive power of Bitcoin volatility forecasts of the ARJI, GARCH, EGARCH, and CGARCH models. Tiwari, Kumar, and Pathak (2019) test a number of GARCH and stochastic volatility models to find the best model to capture the dynamics of Bitcoin and Litecoin pricing. They also find evidence in the leverage effect that cryptocurrencies do not behave comparably to stock prices. Using an extreme-value-theory-based method, Feng, Wang, and Zhang (2018) analyse the extreme characteristics (tail risk) of seven cryptocurrencies. Among all, the study by Bouri et al. (2018) has the most practical implications for investors and fund managers. They examine return and volatility spillovers between Bitcoin and four asset classes in bear and bull market conditions up to October 2017 and find that the Bitcoin market is not completely isolated from other markets as its returns are correlated to returns of other assets, especially commodities. In addition, they find evidence that Bitcoin collects more volatility than it transmits to other markets.

Irrespective of the nature of cryptocurrencies, in an academic sense, we can use event study methodology to examine market reactions to announcements. Studies such as Brown and Warner (1985) report that the increase in variance may result in misspecification of traditional test statistics. The typical conclusion in event studies conducted on daily data is that, on average, stock prices seem to adjust within a day of event announcements. Although prices on average adjust quickly to firm-specific information, a common finding in event studies is that the dispersion of returns increases around information events.

Event study methodology is also applied by Park (2004), to multiple countries. The findings show that the use of the single country market model in a multi-country event study is likely to overestimate changes in firm value, demonstrating the need for a world market model. In a notable study, Kwok and Brooks (1990) apply event study methodology to the foreign exchange market with different conditions in the choice of foreign currency or numeraire, level of abnormal shock, sample size, length of estimation period, market return proxy, and time period examined. The results underscore some of the challenges when event study methodology is applied to the field of foreign exchanges.

Our article makes two primary contributions. First, we attempt to explore reactions of the cryptocurrency market to positive and negative events utilizing event study methodology. Second, we identify the possible profit-making opportunities based on the speed of information flow. This has significant implications for trading strategies that can be used by arbitragers, investors and practitioners. The objective of this article is to provide evidence of potential positive trading opportunities in the market. Our findings can be supported by the findings of the market inefficiency documented in several prior studies.

III. Data and methodology

Sample

In this study, we focus on the three largest cryptocurrencies by market capitalization: Bitcoin, Ethereum, and Ripple. These currencies have ample liquidity, are traded on multiple exchanges with substantial trading volume, and have a global market. As of February 2020, Bitcoin has a market cap of 175 USD billion representing about 64% of the total cryptocurrency market, followed by Ethereum with a market cap of 29 USD billion representing about 11% of the total market, and Ripple with a market cap of 12 USD billion representing approximately 4% of the total market. These three currencies represent approximately 80% of the market capitalization of the total cryptocurrency market, which has a market value of 275 USD billion. Historical daily pricing data on Bitcoin, Ethereum, and Ripple are obtained from the website finance.yahoo.com, which provides long histories of various cryptocurrency exchange rates against the U.S. Dollar (USD). To be consistent in our comparison, daily prices are taken at close in the British Standard Time.

Following the literature, the daily returns are calculated by taking the first difference in the logarithm of daily closing exchange rates:

$$R_t = \ln(P_t) - ln(P_{t-1})$$

Since no reliable proxy for the market such as a market index (S&P 500 for the stock market, for example) has been established in the cryptocurrency market, we use the mean-adjusted returns model in this study. Whereas we also considered an option of constructing a cryptocurrency market index of our own, it may not be feasible to come up with a practical index due to limited data availability. In addition, this approach may not work for Bitcoin since it dominates the market. Using the mean of the last year (365 trading days for exchange rates) as the expected return, the abnormal return is calculated as the difference between the actual daily return and the expected return. The CAR is calculated by summing the abnormal returns during the event window. We use the event windows of Day −3 through Day 6 and Day 0 to Day 6. In the case of analysing global markets, there may not be an immediate diffusion effect when changes are proposed (e.g. Park 2004). Time zone, trading zone, cultural as well as language differences, and liquidity issues in cryptocurrency markets may necessitate considering an event window beginning even before Day −1, a window typically used in finance literature when studies are conducted in one country.[1] The use of the event window ending in Day 6 is determined by the fact that the CARs continue to increase until Day 6. This is also to investigate if there is any trade opportunity for an investor who enters the market after the news comes out (i.e. becomes public information).[2]

The event day (Day 0) is defined as the day in which the news event occurs. Since we use the 365-day average, the events considered in this study are taken one year after the beginning of our sample period. With this, the events are chosen from the time period of 1 January 2015 to 31 October 2018 for Bitcoin; 5 August 2016, to 31 October 2018 for Ethereum; and 31 January 2016, to 31 October 2018 for Ripple.

Selection of major events

In this article, major news announcements are categorized as positive or negative events. Positive events are defined as those events that are predicted to bring an expansionary effect to the cryptocurrency market, and therefore we should expect a positive return from those events. Similarly, negative events are defined as those events that are expected to bring a contractionary effect to the cryptocurrency market, and therefore we should expect a negative return from those events. Below we summarize examples of positive and negative events based on their nature/types.

Positive Events:

- Regulation: Main regulation events are national regulation changes in one of the countries with a high volume of digital currency trading. Depending on the context of the regulation, a regulation event can have positive impacts. For example, the news of Japan's declaration of Bitcoin as a legal tender is considered a positive event as it has an expansionary effect bringing in more traders.
- Exchange: Exchange-related news can be a positive event. One of the main exchange news events is the launch of Bitcoin futures on the two major exchanges, the Chicago Mercantile Exchange (CME) and the Chicago Board Options Exchange (CBOE). News about a new exchange being established is also categorized as an exchange news event. One example of this is that Coinbase launched a U.S. licenced exchange on 26 January 2016. Exchange-related

[1]To validate this point, we also used event windows of (−2, 6) and (−1, 6) and found similar results. Such findings are not included in this paper for brevity.
[2]We also used shorter windows such as (−3, 3) and (0, 3) as robustness checks, and the CAR patterns still hold with similar results.

news is usually considered positive because it indicates the expansion and availability of enhanced trading possibilities.

- Split: News on currency splits or forks is normally a significant event in the cryptocurrency market as it enhances liquidity and enables the micro-structure needs of participants. One of the key examples of split news is the news on the first hard fork of Bitcoin into Bitcoin and Bitcoin Cash on 1 August 2017. The news came out several days earlier, on 22 July 2017. In most cases, split news can be considered positive because the reason for splits or forks is usually to upgrade a system.
- Partnership: Events under this category are news reports about new partnerships between large organizations. One example of this is the Ripple partnership announcement with MoneyGram on 11 January 2018. Partnership news is generally perceived as a positive event as it enhances the global platform of trading and brings in more players who can benchmark and leverage the existing technology for enhanced product offerings.

Negative Events:

- Hacking: There have been quite a few large, high-profile cryptocurrency hacks over the last few years. Millions of dollars are reported stolen every time a digital currency exchange gets hacked. While the underlying blockchain technology is fundamentally more secure than centralized database systems, the ecosystem is still new, and poor programming practices create various security vulnerabilities, especially with systems built around blockchains. News of hacking is perceived as a negative event since hacking of an exchange would directly affect the investors' confidence who trade at the hacked exchange with their assets directly exposed to the risk of being stolen. In addition, other potential investors in the market perceive it as riskiness of the whole digital currency system, since the vulnerability of the hacked exchange may be perceived as a proxy to represent the vulnerability of the market as a whole. Examples of high-profile hacking events include the hacking of the Bitfinex Exchange that took place on 2 August 2016. This hacking caused 120,000 units of Bitcoin, valued at 72 USD million at the time, to be stolen.
- Regulation: Regulation-related events constraining trading and investing can have negative impacts on the market depending on the context. For example, China's ban on cryptocurrencies is considered a negative event. While regulation news can be positive or negative, it is easy to distinguish based on the context of the regulation.
- Split: In some cases, news on splits or hard forks can have a negative impact on a specific cryptocurrency if it is a hard fork from the specific currency. Split news on one cryptocurrency can have a negative effect on other currencies that need an upgrade on the system but have not been upgraded for a while.
- Others: This category also includes news about comments made by significant market leaders in the financial or regulatory industry. While events in this category could have either a positive or a negative impact, it is typically apparent how the market will react to the news. For example, news on bad comments made by a well-known market-maker or regulatory authority should bring negative movement in the market.

We collect the major news events on Bitcoin, Ethereum, and Ripple from numerous online sources including but not limited to CNBC, Forbes, NY Times, Coindesk, CCN, and Cointelegraph. The dates of the news events are recorded in U.S. Eastern Time. For each digital currency, 10 news announcements we consider as the largest with a positive impact on the market and another 10 news announcements we consider as the largest with a negative impact are selected as major events. Table 1 lists all major events (10 positive and 10 negative) selected for Bitcoin, Ethereum, and Ripple. They are categorized as Positive/Negative indicating the expected direction of the impact on the market. The table also lists five different event types: Regulation, Exchange, Hacking, Split, and Others with brief descriptions of the events.

Normality and nonparametric tests

Most event studies have relied on parametric test statistics. Implicit in this parametric testing is the

Table 1. List of major events: Bitcoin, Ethereum, and Ripple.

Date	Positive/Negative	Type of News	Event
Bitcoin			
4 January 2015	Negative	HACK	Bitstamp hacked
26 January 2015	Positive	EXCHANGE	Coinbase launches US licenced exchange
22 October 2015	Positive	REGULATION	EU declares no VAT (value-added-tax) on Bitcoin trades
3 November 2015	Positive	PARTNERSHIP	Bitcoin Sign accepted into Unicode
14 January 2016	Negative	OTHERS	Mike Hearn, an ex-Google developer, declares Bitcoin had failed
2 August 2016	Negative	HACK	Bitfinex hacked
10 March 2017	Negative	REGULATION	SEC denies Winkelvoss ETF
1 April 2017	Positive	REGULATION	Japan declares Bitcoin as legal tender
22 July 2017	Positive	SPLIT	The first hard fork of Bitcoin happened on 1 August 2017, resulting in the creation of Bitcoin Cash
3 August 2017	Positive	EXCHANGE	CBOE plans to launch Bitcoin futures
3 September 2017	Negative	REGULATION	China bans companies from raising money through ICOs
31 October 2017	Positive	EXCHANGE	CME announces to launch Bitcoin futures
8 November 2017	Negative	SPLIT	SegWit2X cancelled
1 December 2017	Positive	EXCHANGE	CME, CBOE to begin Bitcoin futures trading
19 December 2017	Negative	EXCHANGE	Top cryptocurrency marketplace starts supporting rival bitcoin cash
29 January 2018	Negative	REGULATION	New cryptocurrency rules just came into effect in South Korea
7 March 2018	Negative	HACK	Binance detects unauthorized transactions
10 June 2018	Negative	HACK	Korea's Coinrail hacked
2 July 2018	Positive	EXCHANGE	Coinbase launches crypto custody service for institutional investors
15 October 2018	Positive	PARTNERSHIP	Fidelity launches trade execution and custody for cryptocurrencies
			(cont'd.)
Ethereum			
23 January 2017	Negative	OTHERS	Proposed 'Ethereum' investment vehicle sparks controversy
13 February 2017	Positive	PARTNERSHIP	JP Morgan, Santander said to join New Ethereum Blockchain Group
28 February 2017	Positive	PARTNERSHIP	Big Corporates (JP Morgan, Microsoft, BP and Wipro, etc.) unite for launch of Enterprise Ethereum Alliance
26 April 2017	Positive	PARTNERSHIP	Former Coinbase engineer launches Ethereum search engine
22 May 2017	Positive	PARTNERSHIP	Deloitte joins Blockchain Consortiums Ethereum Alliance and Hyperledger
5 June 2017	Positive	PARTNERSHIP	Vladimir Putin and Vitalik Buterin discuss Ethereum 'opportunities'
17 June2017	Negative	HACK	The DAO attacked: Code issue leads to $60 million Ether theft
25 June 2017	Negative	OTHERS	Fake news of a fatal car crash of Vitalik Buterin wiped out $4 billion in Ethereum's market value
3 August 2017	Positive	EXCHANGE	CBOE plans to launch Bitcoin futures
3 September 2017	Negative	REGULATION	China bans companies from raising money through ICOs
15 September 2017	Negative	REGULATION	China shuts down all Bitcoin and cryptocurrency exchanges
21 November 2017	Positive	OTHERS	Ethereum startup ConsenSys opens new London Office
11 December 2017	Positive	EXCHANGE	CBOE Bitcoin futures are launched
29 January 2018	Negative	REGULATION	New cryptocurrency rules come into effect in South Korea
8 February 2018	Negative	HACK	BitGrail hacked
14 March 2018	Negative	OTHERS	Google bans all cryptocurrency-related advertising
10 June 2018	Negative	HACK	Korea's Coinrail hacked
20 June 2018	Negative	HACK	Korea's Bithumb hacked
2 July 2018	Positive	EXCHANGE	Coinbase launches crypto custody service for institutional investors
15 October 2018	Positive	PARTNERSHIP	Fidelity launches trade execution and custody for cryptocurrencies
			(cont'd.)
Ripple			
13 June 2016	Positive	PARTNERSHIP	Ripple receives New York's first BitLicense for an institutional use case of digital assets
2 August 2016	Negative	HACK	Bitfinex hacked and $60 million stolen
23 September 2016	Positive	PARTNERSHIP	Announcement of Ripple's global payments steering group
1 November 2016	Positive	REGULATION	Japan rises to Blockchain challenge with new consortium
2 March 2017	Positive	PARTNERSHIP	47 banks complete DLT cloud pilot with Ripple Tech
10 March 2017	Negative	REGULATION	SEC denies Winkelvoss ETF
1 April 2017	Positive	REGULATION	Japan declares Bitcoin as legal tender
17 June 2017	Negative	HACK	The DAO attacked: code issue leads to $60 million Ether theft
3 August 2017	Positive	EXCHANGE	CBOE plans to launch Bitcoin futures
3 September 2017	Negative	REGULATION	China bans companies from raising money through ICOs
15 September 2017	Negative	REGULATION	China shuts down all Bitcoin and cryptocurrency exchanges
16 November 2017	Positive	PARTNERSHIP	American Express opens first Blockchain corridor with Ripple Tech
11 December 2017	Positive	EXCHANGE	CBOE Bitcoin futures are launched
26 January 2018	Negative	HACK	Japanese cryptocurrency exchange loses more than $500 million to hackers
2 February 2018	Negative	OTHERS	J.P. Morgan Chase, Bank of America and Citigroup announce decision not to allow customers to buy cryptocurrencies with the companies' credit card
7 March 2018	Negative	HACK	Binance detects unauthorized transactions
10 June 2018	Negative	HACK	Korea's Coinrail hacked
20 June 2018	Negative	HACK	Korea's Bithumb hacked
2 July 2018	Positive	EXCHANGE	Coinbase launches crypto custody service for institutional investors
15 October 2018	Positive	PARTNERSHIP	Fidelity launches trade execution and custody for cryptocurrencies

Table 1 presents the major news events for the three largest cryptocurrencies, Bitcoin, Ethereum, and Ripple, from January 2015 to 31 October2018. Panel A summarizes the major events for Bitcoin, Panel B summarizes the major events for Ethereum and Panel C for Ripple.

assumption of normality of the probability of distribution of returns. Corrado's early work in 1989 shows that in simulations with daily security-return data, the nonparametric rank test outperforms the parametric t-test. The rank test does not require symmetry in cross-sectional return distributions for the correct specification. In further study, Corrado and Zivney (1992) document that the performance of the sign test is dominated by the performance of the rank test. The correct specification of the sign test requires equal numbers of positive and negative abnormal returns. Kolari and Pynnonen (2011) advance the sign test further. They argue that when stock prices are not normally distributed, the power of nonparametric tests in event study analyses dominates the power of parametric tests. When applied to multiple day analyses, the proposed generalized rank (GRANK) test can better calculate both one single day and CARs. This statistic is robust, and the GRANK procedure outperforms the rank tests of CARs as well as is robust to abnormal return serial correlation and event-induced volatility. The GRANK procedure exhibits a superior power relative to popular parametric tests.

In this study, first we conduct a normality test to determine if our sample is normally distributed by using the Shapiro–Wilk test. This test is commonly found to have a strong power for a given significance (e.g. Razali and Wah 2011). Once non-normality of the sample is verified, nonparametric test procedures are applied to obtain statistics of our analysis. We use the classic rank test introduced by Corrado (1989) for single-day abnormal returns and the GRANK test proposed by Kolari and Pynnonen (2011) for multiple-day CARs. These procedures are selected as the Corrado rank test and the Corrado-Zivney rank test is found to be significantly less effective when extended for multiple-day tests (Kolari and Pynnonen 2011).

IV. Event analysis results

Baseline results

Panel A of Table 2 presents the summary statistics of daily abnormal returns for each of three digital currencies from the whole sample period based on the 365-day moving average. The mean abnormal return of Bitcoin is positive while that of Ethereum is negative and that of Ripple is almost zero. The maximum and minimum returns of Bitcoin and Ethereum are similar to each other (maxima of 24.5% and 25.4%, and minima of −28.9% and −27.2%, respectively) while those of Ripple are much larger in magnitude with a maximum of 102.3% and a minimum of −65.7%. Also, Ripple has the largest coefficient of variation with an absolute value of 3037.51, compared to Bitcoin's 329.75 and Ethereum's 22.84. Based on the maximum and minimum values as well as coefficient of variation, Ripple is the most volatile asset among the three currencies for our sample period.

Panel B of Table 2 illustrates the results of our normality test using the Shapiro-Wilk procedure. The null-hypothesis of the Shapiro–Wilk test is normality in the population. The test results show that in all of the three cases, we cannot reject the

Table 2. Summary statistics and normality – daily abnormal returns.

Panel A. Summary Statistics

	N	Mean	Median	Std Dev	Coeff. of Variation	Max	Min	Skewness	Excess Kurtosis
Bitcoin	1,400	0.012%	0.093%	3.975%	329.75	24.471%	−28.931%	−0.470	6.693
Ethereum	817	−0.261%	−0.544%	5.961%	−22.84	25.435%	−27.183%	0.348	3.059
Ripple	1,005	−0.003%	−0.576%	8.490%	−3037.51	102.268%	−65.655%	2.518	30.436

Panel B. Shapiro–Wilk Normality Test

	N	*W*	*V*	*z*	*Prob>z*
Bitcoin	1,400	0.897	88.549	11.253	0.000
Ethereum	817	0.943	29.792	8.336	0.000
Ripple	1,005	0.756	154.37	12.482	0.000

Panel A of Table 2 summarizes the statistics of abnormal returns of Bitcoin, Ethereum, and Ripple. The daily abnormal returns are calculated using the 365-day average as the expected return. The time period used for Bitcoin is from 1 January 2015 to 31 October 2018. The time period used for Ethereum is from 5 August 2016 to 31 October 2018. The time period used for Ripple is from 31 January 2016 to 31 October 2018. *N* is the number of observations, *Median* is the median value, *Mean* is the average, and *Std Dev* is the standard deviation of the observations. *Coeff. of Variation* is the coefficient of variation values computed as standard deviation divided by mean. *Min* and *Max* are the minimum and maximum values of observations for each cryptocurrency in this study, respectively. *Skewness* and *Excess Kurtosis* are the skewness and excess kurtosis of the observations for each cryptocurrency, respectively. Panel B of this table presents the results from the Shapiro-Wilk normality test for Bitcoin, Ethereum, and Ripple. *N* is the number of observations, *W* and *V are* the W and V values specific to the Shapiro–Wilk test, respectively, and *z* is the z-statistic.

null hypothesis, indicating that our sample populations of Bitcoin, Ethereum and Ripple are not normally distributed.[3] These results validate the use of nonparametric tests over parametric tests. Therefore, the t-statistics of the following arguments are obtained from nonparametric tests.

Table 3 presents the average abnormal returns (AR) from Day −3 to Day 6 of events. The average values are calculated for each 10 positive events and each 10 negative events separately for the three cryptocurrencies in this study. In general, the ARs on the event announcement day are larger than the ARs in other days within the event window, as expected. The only exceptions are observed for the positive events for Ethereum and the negative events for Ripple. The positive events for Ethereum exhibit the highest average AR on Day −2. This may be reflecting the cases where the diffusion of rumours about upcoming positive news start coming around a few days before the actual announcement of the news. As for the negative events for Ripple, the largest AR is observed on Day 3. It is not clear if this is due to the fact that the market reaction is slower to negative news related to Ripple and the market response to the news starts picking up a few days after the event or if this is from a market reaction to another main event not considered in our sample.

Panel A of Table 4 summarizes the average CARs from Day −3 to Day 6 of events for positive and negative events of Bitcoin, Ethereum, and Ripple. Panel A of Figure 1 plots the event days on the x-axis and CARs on the y-axis. In general, the cumulative returns continue to increase in magnitude from Day −3 to Day 6 with a slight pullback on Day 2 for most cases. Among positive events, Ethereum has the highest CARs at Day 6 with a value of 29.37%, whereas Ripple has the highest CARs in magnitude among negative events at Day 6 with a value of −45.12%. Bitcoin exhibits the smallest CARs in both positive and negative events (15.87% and −17.83%, respectively). The overall observation is that the CARs for negative events are larger in magnitude compared to the CARs for positive events.

Panel B of Table 4 summarizes the average CARs from Day 0 to Day 6 for positive and negative events of Bitcoin, Ethereum, and Ripple. Panel B of Figure 1 plots the event days on the x-axis and CARs on the y-axis. Again, this event window is used to investigate if an investor can make any profit by placing a position in the market after the news becomes public information. In general, the cumulative returns continue to increase in magnitude from Day 0 to Day 6 although the increase in magnitude is not as much in some cases. The information given by the news is not fully reflected in the price right after the news announcement. This could create a trade opportunity for the investor to make a profit by taking advantage of this unpredictability. If the investor is aware that the return continues to increase for several days after the positive event, then he/she could make a profit by buying the currency right after the event occurs and sell it three to six days after the announcement.

One observation to point out is that the CARs for negative events are in general larger in magnitude compared to the CARs for positive events. For positive events, Ripple shows the largest CARs from Day 0 to Day 6. This implies that the market reaction to positive news related to Ripple is slower than the market reaction to positive news related to the other two assets. For negative events, Ripple exhibits the largest CARs from Day 3 to Day 6. This implies that the market reaction to negative news related to Ripple is slower than the market reaction to negative news related to the other two assets. In addition, the magnitude of CARs for Ripple negative events is significantly larger than any other CARs in this study. This implies that the market reacts to negative news related to Ripple more than it does to any type of news related to the other two currencies.

Robustness tests

As robustness tests, we utilize different time frames, 180 days and 60 days, to compute the expected return motivated by high volatility experienced in the cryptocurrency market. The market experienced extreme volatility for the period spanning from the end of 2017 to the beginning of 2018, which is part of our sample period. Panels A and B of Figure 2 plot the event days on the x-axis and CARs on the y-axis for the event windows of (−3, 6)

[3]Using the Shapiro-Francia procedure as well as a normality test based on skewness and another based on kurtosis, we also confirm the results from the Shapiro-Wilk procedure. The results from these tests are excluded for brevity.

Table 3. Average abnormal returns (ARs) around major events.

Bitcoin

	Positive Events				Negative Events			
Day	Average AR	Corrado t-stat	Skewness	Excess Kurtosis	Average AR	Corrado t-stat	Skewness	Excess Kurtosis
−3	1.27%	1.478	2.530	7.363	0.12%	−0.575	0.889	1.346
−2	1.10%	2.256	−0.896	−0.439	−1.43%	−2.612	1.351	2.820
−1	1.63%	2.216	0.829	2.166	−3.54%	−2.922	−1.394	2.369
0	3.33%	2.656	0.026	−1.794	−4.58%	−3.545	1.418	1.087
1	1.46%	2.633	−0.238	−0.407	−3.09%	−2.596	−0.969	−0.150
2	−1.30%	−0.503	−0.432	−1.047	−1.51%	−2.625	−0.067	−0.688
3	1.38%	1.949	1.742	3.399	−0.82%	−1.764	−0.838	2.805
4	2.68%	3.443	−0.622	−1.263	−1.37%	−2.324	−0.405	−0.573
5	3.18%	2.716	0.636	0.773	1.13%	−0.437	−0.394	0.156
6	1.13%	1.246	1.975	4.974	−2.74%	−2.689	−0.730	2.268

Ethereum

	Positive Events				Negative Events			
Day	Average AR	Corrado t-stat	Skewness	Excess Kurtosis	Average AR	Corrado t-stat	Skewness	Excess Kurtosis
−3	2.29%	3.171	1.692	4.478	−2.54%	−1.280	−1.313	0.275
−2	7.96%	2.243	0.522	−1.141	−4.31%	−1.622	−1.053	0.006
−1	3.70%	3.083	1.157	1.675	0.73%	−0.890	0.344	−0.957
0	0.74%	1.491	−0.242	2.126	−7.60%	−3.980	−0.114	−0.948
1	7.23%	2.979	−0.564	−1.057	1.04%	−1.640	−0.446	1.286
2	1.43%	1.097	0.563	0.207	1.45%	0.583	−1.066	1.620
3	3.74%	2.508	−0.398	−0.626	−7.35%	−3.557	−0.682	1.331
4	0.49%	0.615	2.170	5.122	−2.99%	−2.910	0.852	1.484
5	0.22%	1.424	1.195	2.446	−2.93%	−1.908	−0.130	−1.154
6	1.56%	1.979	0.254	0.341	−1.86%	−1.037	−0.840	0.533

Ripple

	Positive Events				Negative Events			
Day	Average AR	Corrado t-stat	Skewness	Excess Kurtosis	Average AR	Corrado t-stat	Skewness	Excess Kurtosis
−3	1.96%	2.541	1.116	0.821	−6.95%	−2.380	−0.850	0.571
−2	0.64%	1.506	−0.768	0.785	−5.48%	−3.325	−0.660	−2.802
−1	1.56%	2.898	0.593	−0.721	−1.61%	−2.770	−1.036	0.483
0	13.87%	3.160	0.065	−2.086	−7.67%	−3.638	0.394	1.575
1	2.79%	1.492	−0.929	−0.794	−1.40%	−1.152	−1.467	2.320
2	0.21%	0.376	1.276	1.811	0.86%	−0.833	−1.837	3.609
3	3.39%	1.280	0.203	−2.496	−11.10%	−3.006	−0.351	−2.963
4	1.37%	0.938	−1.063	−0.618	−4.40%	−2.138	0.979	1.726
5	5.24%	1.929	−1.031	0.947	−1.71%	−3.089	0.045	−2.028
6	−3.29%	−0.928	−0.523	−0.864	−5.66%	−2.193	−0.366	−2.742

Table 3 presents the average abnormal returns (ARs) around the event time. First, the daily abnormal returns are calculated for the days in the event window of (−3, 6) and (0, 6) for each event using the 365-day average as the expected return. After ARs are computed for all events, the average of those values from all events are taken for each cryptocurrency. T-statistics are also computed for each average AR using the Corrado (1989) rank test. *Skewness* and *Excess Kurtosis* are the skewness and kurtosis of 10 observations for each group, respectively.

and (0, 6), respectively, using the 180-day moving average as the expected return in computing the abnormal return. Figure 3 presents the same results as Figure 2 using the 60-day moving average. As illustrated in these figures, our baseline results are robust with the expected returns computed based on the mean values using different time frames of 180 days and 60 days.[4]

Out-of-sample analysis

To further examine the validity of our findings, we present out-of-sample analysis by demonstrating trading strategies utilizing our findings. We construct six different investment strategies that trade on out-of-sample events (after 1 November 2018) and test if these strategies can make profits. In the strategies that trade on positive news, investors

[4]T-statistics based on the Kolari and Pynnonen (2011) GRANK test are utilized to evaluate the statistical significance of the results using the 180-day moving average as well as the 60-day moving average, in the same way, we tested the statistical significance of the baseline results presented in Table 4.

Table 4. Average cumulative abnormal returns (CARs) around major events.

Panel A. Average Cumulative Abnormal Returns (CARs) for Event Window (−3, 6)

Bitcoin

	Positive Events				Negative Events			
Event Window	Average CAR	GRANK t-stat	Skewness	Excess Kurtosis	Average CAR	GRANK t-stat	Skewness	Excess Kurtosis
(−3,-3)	1.27%	−0.190	2.530	7.363	0.12%	−1.875	0.889	1.346
(−3,-2)	2.38%	1.518	−0.850	3.095	−1.32%	−1.545	0.519	−0.344
(−3,-1)	4.01%	1.730	−0.191	2.346	−4.86%	−2.504	−0.875	2.465
(−3,0)	7.34%	2.292	0.170	−1.252	−9.44%	−3.275	0.517	−1.456
(−3,1)	8.80%	2.951	−0.022	−0.850	−12.53%	−3.752	−1.103	2.873
(−3,2)	7.49%	3.214	−1.171	2.214	−14.04%	−4.467	0.100	−0.062
(−3,3)	8.87%	3.523	−1.457	2.623	−14.86%	−4.289	−0.514	−0.975
(−3,4)	11.56%	3.578	−1.140	0.980	−16.23%	−4.252	−0.200	−1.385
(−3,5)	14.74%	3.474	−0.620	0.789	−15.09%	−3.863	−0.784	0.023
(−3,6)	15.87%	3.770	1.764	4.688	−17.83%	−3.663	−1.568	3.269

Ethereum

	Positive Events				Negative Events			
Event Window	Average CAR	GRANK t-stat	Skewness	Excess Kurtosis	Average CAR	GRANK t-stat	Skewness	Excess Kurtosis
(−3,-3)	2.29%	2.862	1.692	4.478	−2.54%	0.315	−1.313	0.275
(−3,-2)	10.25%	3.138	1.614	3.104	−6.85%	−0.505	−1.319	1.876
(−3,-1)	13.95%	3.822	0.489	−1.817	−6.12%	−0.920	0.645	−1.518
(−3,0)	14.69%	3.912	0.058	−1.739	−13.72%	−2.007	0.250	−2.373
(−3,1)	21.92%	4.171	0.274	−1.266	−12.68%	−3.939	0.036	−2.087
(−3,2)	23.35%	4.066	0.051	−1.415	−11.23%	−3.713	−1.921	3.453
(−3,3)	27.10%	3.958	0.642	−0.619	−18.59%	−4.298	−2.126	5.049
(−3,4)	27.59%	3.909	−0.039	−1.083	−21.57%	−4.357	−0.925	1.040
(−3,5)	27.81%	3.907	−0.195	−1.172	−24.50%	−4.269	−0.878	1.366
(−3,6)	29.37%	3.896	−0.088	−1.602	−26.37%	−4.208	−0.025	−0.112

Ripple

	Positive Events				Negative Events			
Event Window	Average CAR	GRANK t-stat	Skewness	Excess Kurtosis	Average CAR	GRANK t-stat	Skewness	Excess Kurtosis
(−3,-3)	1.96%	2.388	1.116	0.821	−6.95%	−0.787	−0.850	0.571
(−3,-2)	2.60%	1.700	1.101	0.093	−12.43%	−1.472	0.401	0.250
(−3,-1)	4.15%	3.348	−1.201	1.113	−14.04%	−2.591	−0.783	1.233
(−3,0)	18.02%	4.085	0.809	−1.018	−21.71%	−3.215	−0.731	−0.029
(−3,1)	20.81%	4.432	2.077	4.425	−23.11%	−2.902	−1.426	2.514
(−3,2)	21.01%	4.302	2.172	4.773	−22.25%	−2.460	−1.724	3.362
(−3,3)	24.41%	4.104	1.963	4.084	−33.35%	−2.728	−1.422	1.319
(−3,4)	25.78%	4.076	1.898	3.814	−37.75%	−2.750	−0.821	−1.023
(−3,5)	31.02%	4.038	1.823	3.616	−39.46%	−2.996	−1.676	2.474
(−3,6)	27.73%	3.754	2.114	4.508	−45.12%	−2.888	0.224	−3.010

Panel B. Average Cumulative Abnormal Returns (CARs) for Event Window (0, 6)

Bitcoin

	Positive Events				Negative Events			
Event Window	Average CAR	GRANK t-stat	Skewness	Excess Kurtosis	Average CAR	GRANK t-stat	Skewness	Excess Kurtosis
(0,0)	3.33%	1.685	0.026	−1.794	−4.58%	−1.951	1.418	1.087
(0,1)	4.79%	2.531	−0.327	−0.874	−7.67%	−2.058	0.033	0.767
(0,2)	3.48%	1.987	−1.175	1.247	−9.18%	−1.850	0.272	−0.991
(0,3)	4.87%	2.721	−0.778	0.071	−10.00%	−2.068	−0.271	−0.166
(0,4)	7.55%	2.506	−1.209	0.277	−11.37%	−2.038	0.445	−0.266
(0,5)	10.73%	2.795	−0.603	−0.192	−10.23%	−2.091	−0.613	−0.089
(0,6)	11.86%	2.909	1.101	2.552	−12.97%	−2.411	−1.290	1.659

Ethereum

	Positive Events				Negative Events			
Event Window	Average CAR	GRANK t-stat	Skewness	Excess Kurtosis	Average CAR	GRANK t-stat	Skewness	Excess Kurtosis
(0,0)	0.74%	0.269	−0.242	2.126	−7.60%	−2.623	−0.114	−0.948
(0,1)	7.97%	1.762	1.079	0.663	−6.56%	−4.149	−0.774	−0.283
(0,2)	9.41%	1.992	−0.019	−0.766	−5.11%	−2.798	−0.132	−0.396
(0,3)	13.15%	2.034	0.833	0.933	−12.47%	−2.999	−1.068	0.834
(0,4)	13.64%	2.614	−0.092	−0.348	−15.45%	−4.163	−0.148	−1.473

(*Continued*)

Table 4. (Continued).

Panel B. Average Cumulative Abnormal Returns (CARs) for Event Window (0, 6)

(0,5)	13.86%	3.175	0.118	−0.380	−18.38%	−5.395	−0.525	−0.938
(0,6)	15.43%	2.653	−0.229	0.472	−20.25%	−4.991	−0.624	−0.490

Ripple

	Positive Events				Negative Events			
Event Window	Average CAR	GRANK t-stat	Skewness	Excess Kurtosis	Average CAR	GRANK t-stat	Skewness	Excess Kurtosis
(0,0)	13.87%	1.773	0.065	−2.086	−7.67%	−2.305	0.394	1.575
(0,1)	16.65%	2.379	1.417	2.105	−9.07%	−2.208	−1.525	2.622
(0,2)	16.86%	1.990	1.905	3.846	−8.21%	−1.673	−0.560	−3.045
(0,3)	20.25%	2.131	2.150	4.719	−19.31%	−2.337	−0.590	−3.223
(0,4)	21.62%	2.317	2.181	4.791	−23.71%	−3.060	−0.796	0.522
(0,5)	26.86%	2.278	1.584	3.378	−25.43%	−3.245	−0.507	−2.339
(0,6)	23.57%	2.035	2.046	4.329	−31.09%	−3.642	−0.692	−0.722

Table 4 presents the average cumulative abnormal returns (CARs) around the event time. First, the daily abnormal returns are calculated for the days in the event window of (−3, 6) and (0, 6) for each event using the 365-day average as the expected return. Then, the cumulative abnormal returns are computed for each event window of each currency. After all CARs are computed for all events, the average of those values from all events are taken for each cryptocurrency, and t-statistics are also computed for each average CAR using the generalized rank (GRANK) test proposed by Kolari and Pynnonen (2011). *Skewness* and *Excess Kurtosis* are the skewness and kurtosis of 10 observations for each group, respectively. Panel A presents the summary of CARs for the event windows of (−3, 6), and Panel B presents the summary of CARs for the event windows of (0, 6).

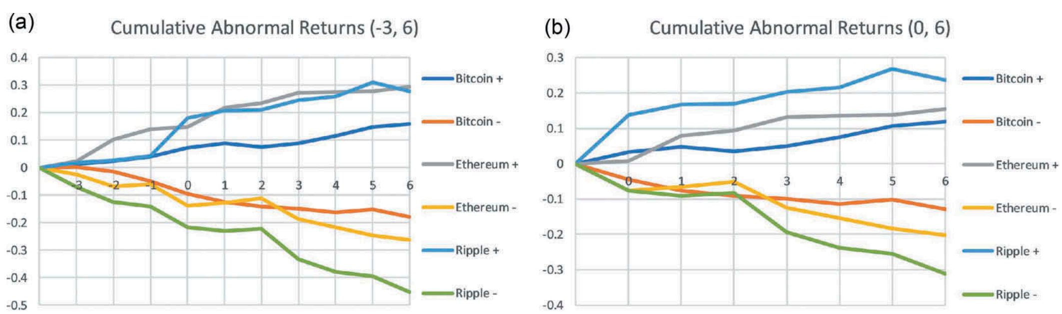

Figure 1. Average cumulative abnormal returns (CARs) around major events.

Figure 1 illustrates the movements of the average cumulative abnormal returns (CARs) around the event time for positive and negative events of each currency. Three hundred and sixty-five-day moving averages are used as the expected return in computing abnormal returns. Panel A exhibits the CAR movements for the event window of (−3, 6), and Panel B exhibits the CAR movements for the event window of (0, 6).Panel A. Movements of Average Cumulative Abnormal Returns (CARs) for Event Window (−3, 6). Panel B. Movements of Average Cumulative Abnormal Returns (CARs) for Event Window (0, 6).

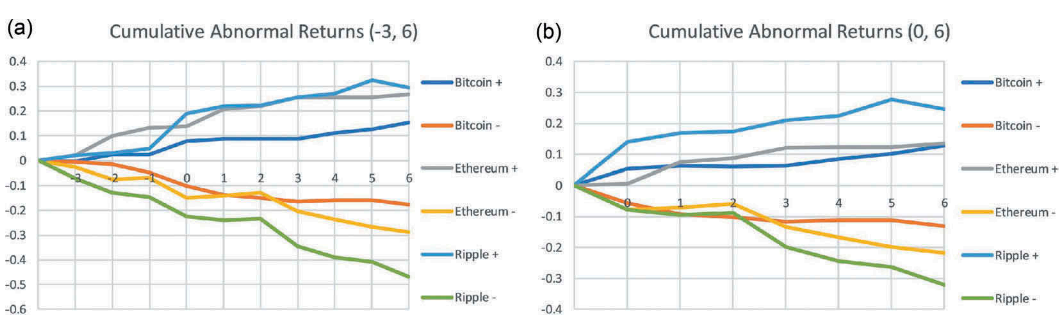

Figure 2. Average cumulative abnormal returns (CARs) around major events using 180-day average as expected return.

Figure 2 illustrates the movements of the average cumulative abnormal returns (CARs) around the event time for positive and negative events of each currency. One hundred and eighty-day moving averages are used as the expected return in computing abnormal returns. Panel A exhibits the CAR movements for the event window of (−3, 6), and Panel B exhibits the CAR movements for the event window of (0, 6).Panel A. Movements of Average Cumulative Abnormal Returns (CARs) for Event Window (−3, 6). Panel B. Movements of Average Cumulative Abnormal Returns (CARs) for Event Window (0, 6).

place a position in the market on Day 0 of the news event, hold the position for six days and close the position on Day 6. In the strategies that trade on negative news, investors short sell on Day 0 of the news event and buy back on Day 6. We assume 1.5% of each trading amount as the total

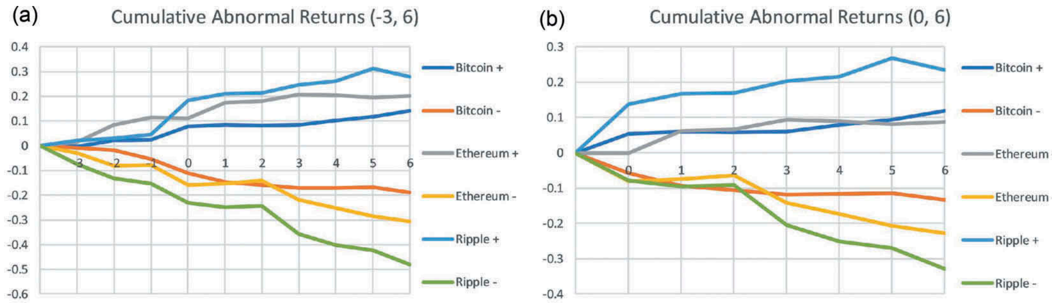

Figure 3. Average cumulative abnormal returns (CARs) around major events using 60-day average as expected return.

Figure 3 illustrates the movements of the average cumulative abnormal returns (CARs) around the event time for positive and negative events of each currency. Sixty-day moving averages are used as the expected return in computing abnormal returns. Panel A exhibits the CAR movements for the event window of (−3, 6), and Panel B exhibits the CAR movements for the event window of (0, 6).Panel A. Movements of Average Cumulative Abnormal Returns (CARs) for Event Window (−3, 6). Panel B. Movements of Average Cumulative Abnormal Returns (CARs) for Event Window (0, 6).

transaction cost accounting for exchange fees, network fees, and wallet fees. This value is based on the fees imposed by Coinbase.com, a digital currency exchange in the United States, in case funds are transferred from a U.S. bank or a Coinbase USD wallet. Potential transaction complications due to limited liquidity are ignored in this analysis as they have not been flagged as an issue recently at the actual exchanges.

Positive Event One: JP Morgan launches its own JPM Coin (14 February 2019)

Positive Event Two: Cryptocurrency wallet on WhatsApp set for release (25 March 2019)

Positive Event Three: Facebook announces Libra cryptocurrency (18 June 2019)

Negative Event One: Bitcoin Cash has a hard fork (13 November 2018)

Negative Event Two: Singaporean exchange Bitrue gets hacked (27 June 2019)

Negative Event Three: Federal Reserve Chairman Jerome Powell indicates concerns over Libra in his comments (10 July 2019)

Investor A: Trade 10,000 USD in Bitcoin based on positive news (buy, hold, and sell)

Investor B: Trade 10,000 USD in Ethereum based on positive news (buy, hold, and sell)

Investor C: Trade 10,000 USD in Ripple based on positive news (buy, hold, and sell)

Investor D: Trade 10,000 USD in Bitcoin based on negative news (short sell and buy back)

Investor E: Trade 10,000 USD in Ethereum based on negative news (short sell and buy back)

Investor F: Trade 10,000 USD in Ripple based on negative news (short sell and buy back)

The returns made by each of the investors are documented in Table 5. All investors in this analysis make positive profits by trading on the news events even after the news comes out, taking trading costs into consideration. This result confirms the validity of our baseline findings, and investors have opportunities to make positive returns by placing a position even after the information becomes publicly available.

One possible argument is that high returns made in these strategies can be due to the USD weakening against other currencies including cryptocurrencies, not due to the superior performance of each cryptocurrency. To inspect the legitimacy of this claim, we compare the performance of the USD measured as the reciprocal of the U.S. Dollar Index (DX) for the same holding periods as those in each trading strategy. As shown in Table 5, the effect of the USD performance on the three cryptocurrencies is minimal, and each trading strategy still makes positive returns even after taking into consideration the fluctuation of the USD.

V. Conclusion

In this article, we examine investors' reactions to the major news announcements associated with the three largest cryptocurrencies: Bitcoin, Ethereum, and Ripple. Nonparametric tests including the rank test proposed by Corrado (1989) and the GRANK test proposed by Kolari and Pynnonen (2011) are applied as non-normality is found in the returns of these cryptocurrencies. High abnormal returns are observed on the event day (Day 0), indicating that

Table 5. Out-of-sample analysis: investment strategies on news events.

Price	14 February 2019	20 February 2019	HPR	25 March 2019	31 March 2019	HPR	18 June 2019	24 June 2019	HPR	Total Return
Bitcoin (BTC)	3588.72	3974.05		3924.55	4112.69		9280.54	11740.34		
Ether (ETH)	120.85	149.23		133.96	142.40		269.01	316.53		
Ripple (XRP)	0.30	0.33		0.30	0.31		0.44	0.47		
Investor A	Buy ETH	Sell ETH	7.44%	Buy ETH	Sell ETH	1.67%	Buy ETH	Sell ETH	24.61%	36.12%
$ 10,000.00	$ 9,850.00	$ 10,907.62	$ 744.01	$ 10,582.85	$ 11,090.18	$ 179.82	$ 10,923.83	$ 13,819.18	$2,688.06	$ 3,611.89
Investor B	Buy ETH	Sell ETH	19.81%	Buy ETH	Sell ETH	3.14%	Buy ETH	Sell ETH	15.90%	43.21%
$ 10,000.00	$ 9,850.00	$ 12,163.14	$1,980.69	$ 11,800.98	$ 12,544.49	$ 375.63	$ 12,356.32	$ 14,539.04	$1,964.63	$ 4,320.95
Investor C	Buy ETH	Sell ETH	7.16%	Buy ETH	Sell ETH	−0.25%	Buy ETH	Sell ETH	5.35%	12.61%
$ 10,000.00	$ 9,850.00	$ 10,879.10	$ 715.92	$ 10,555.18	$ 10,851.87	$ (26.83)	$ 10,689.09	$ 11,432.96	$ 572.37	$ 1,261.46
1/DX	0.01031	0.01037	0.55%	0.01036	0.01029	−0.63%	0.01024	0.01042	1.73%	1.65%
	11/13/2018	11/19/2018	HPR	6/27/2019	7/3/2019	HPR	7/10/2019	7/16/2019	HPR	Total Return
Bitcoin (BTC)	6339.17	4809.62		12355.06	11156.52		11343.12	9679.22		
Ether (ETH)	206.42	148.22		309.37	283.10		268.56	211.17		
Ripple (XRP)	0.51	0.48		0.42	0.39		0.33	0.31		
Investor D	Short sell BTC	Buy back BTC	24.15%	Short sell BTC	Buy back BTC	9.72%	Short sell BTC	Buy back BTC	14.69%	56.22%
$ 10,000.00	$ 9,850.00	$ 7,473.34	$2,414.56	$ 12,228.34	$ 11,042.10	$1,206.83	$ 13,417.08	$ 11,448.95	$2,000.71	$ 5,622.11
Investor E	Short sell BTC	Buy back BTC	28.21%	Short sell BTC	Buy back BTC	8.51%	Short sell BTC	Buy back BTC	21.39%	68.88%
$ 10,000.00	$ 9,850.00	$ 7,072.80	$2,821.11	$ 12,628.79	$ 11,556.43	$1,091.34	$ 13,703.76	$ 10,775.33	$2,975.49	$ 6,887.93
Investor F	Short sell BTC	Buy back BTC	6.76%	Short sell BTC	Buy back BTC	8.70%	Short sell BTC	Buy back BTC	5.74%	22.70%
$ 10,000.00	$ 9,850.00	$ 9,186.41	$ 675.79	$ 10,515.66	$ 9,603.41	$ 928.34	$ 11,430.07	$ 10,776.72	$ 665.76	$ 2,269.89
1/DX	0.01028	0.01040	1.15%	0.01039	0.01033	−0.57%	0.01030	0.01027	−0.31%	0.28%

Table 5 presents the holding period returns (HPR) as well as the total returns of six investment strategies that trade on news events that took place out of the sample period used in our baseline analysis. Each investor (strategy) invests $10,000 in either Bitcoin, Ethereum, or Ripple on the day that the news comes out (Day 0), and closes the position on Day 6, and repeats the same trade pattern two more times (total of three). *1/DX* indicates the reciprocal of the U.S. Dollar Index (DX).

there is a market reaction to major news events. Even three days before the event (Day −3), abnormal returns are detected in the same direction as the news (positive or negative). This may imply that the market reacts to rumours of upcoming events. In general, cumulative abnormal returns (CAR) diverge during the event windows of (−3, 6) and (0, 6), suggesting that the information is not fully reflected in prices immediately after the events. This also indicates that there is a positive trade opportunity for an investor who begins trading even after the news comes out during the period examined. The magnitudes of CARs are larger for negative events than for positive events, indicating that the market reaction to negative events is stronger than that to positive events.

The findings of this study have crucial implications for arbitragers, investors and practitioners. Progressive investment strategies in which investors take a position in the market on the day of a news announcement are found to make profits. In other words, it is not too late to join the market and gain positive profits even after the event becomes public information. This is not possible in a market where the current prices reflect all publicly available information.

While we present significant results in this article, data limitation has been a concern in our study. For example, we limited our analysis only on the three largest cryptocurrencies partially due to inadequate time series lengths and data unavailability of other cryptocurrencies. Also, the cryptocurrency market experienced extreme volatility, potentially classified as a market bubble, from the end of 2017 to the beginning of 2018, and it has been perceived to be a more volatile market than many other markets. Regime switching models for capturing changes in stock and interest rate behaviour (e.g. Hamilton 1989; Pagan and Sossounov 2003; Sims and Zha 2006; Ang and Timmermann 2011) as well as a jump process proposed by Cox and Ross (1976) and a jump diffusion process introduced by Merton (1976) could be applied to further study the nature of cryptocurrency returns. Another limitation lies in the conclusion drawn by the outcomes of the mean-adjusted returns model. A couple of newly launched indices such as Bloomberg Galaxy Crypto Index and CMC Crypto 200 Index could serve as dependable cryptocurrency market benchmarks once data becomes available. The long-term persistence of our findings must be re-evaluated, and longitudinal analysis can be possible only as longer periods of data become available. Other potential macro-factors such as the global monetary policy and the role of global risk factors could be examined in greater depth as well.

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Disclosure statement

No potential conflict of interest was reported by the authors.

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EXHIBIT C

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Efficient Capital Markets: II

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SEQUELS ARE RARELY AS good as the originals, so I approach this review of the market efficiency literature with trepidation. The task is thornier than it was 20 years ago, when work on efficiency was rather new. The literature is now so large that a full review is impossible, and is not attempted here. Instead, I discuss the work that I find most interesting, and I offer my views on what we have learned from the research on market efficiency.

I. The Theme

I take the market efficiency hypothesis to be the simple statement that security prices fully reflect all available information. A precondition for this strong version of the hypothesis is that information and trading costs, the costs of getting prices to reflect information, are always 0 (Grossman and Stiglitz (1980)). A weaker and economically more sensible version of the efficiency hypothesis says that prices reflect information to the point where the marginal benefits of acting on information (the profits to be made) do not exceed the marginal costs (Jensen (1978)).

Since there are surely positive information and trading costs, the extreme version of the market efficiency hypothesis is surely false. Its advantage, however, is that it is a clean benchmark that allows me to sidestep the messy problem of deciding what are reasonable information and trading costs. I can focus instead on the more interesting task of laying out the evidence on the adjustment of prices to various kinds of information. Each reader is then free to judge the scenarios where market efficiency is a good approximation (that is, deviations from the extreme version of the efficiency hypothesis are within information and trading costs) and those where some other model is a better simplifying view of the world.

Ambiguity about information and trading costs is not, however, the main obstacle to inferences about market efficiency. The joint-hypothesis problem is more serious. Thus, market efficiency per se is not testable. It must be

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tested jointly with some model of equilibrium, an asset-pricing model. This point, the theme of the 1970 review (Fama (1970b)), says that we can only test whether information is properly reflected in prices in the context of a pricing model that defines the meaning of "properly." As a result, when we find anomalous evidence on the behavior of returns, the way it should be split between market inefficiency or a bad model of market equilibrium is ambiguous.

Does the fact that market efficiency must be tested jointly with an equilibrium-pricing model make empirical research on efficiency uninteresting? Does the joint-hypothesis problem make empirical work on asset-pricing models uninteresting? These are, after all, symmetric questions, with the same answer. My answer is an unequivocal no. The empirical literature on efficiency and asset-pricing models passes the acid test of scientific usefulness. It has changed our views about the behavior of returns, across securities and through time. Indeed, academics largely agree on the facts that emerge from the tests, even when they disagree about their implications for efficiency. The empirical work on market efficiency and asset-pricing models has also changed the views and practices of market professionals.

As these summary judgements imply, my view, and the theme of this paper, is that the market efficiency literature should be judged on how it improves our ability to describe the time-series and cross-section behavior of security returns. It is a disappointing fact that, because of the joint-hypothesis problem, precise inferences about the degree of market efficiency are likely to remain impossible. Nevertheless, judged on how it has improved our understanding of the behavior of security returns, the past research on market efficiency is among the most successful in empirical economics, with good prospects to remain so in the future.

II. The Main Areas of Research

The 1970 review divides work on market efficiency into three categories: (1) weak-form tests (How well do past returns predict future returns?), (2) semi-strong-form tests (How quickly do security prices reflect public information announcements?), and (3) strong-form tests (Do any investors have private information that is not fully reflected in market prices?) At the risk of damning a good thing, I change the categories in this paper.

Instead of weak-form tests, which are only concerned with the forecast power of past returns, the first category now covers the more general area of *tests for return predictability*, which also includes the burgeoning work on forecasting returns with variables like dividend yields and interest rates. Since market efficiency and equilibrium-pricing issues are inseparable, the discussion of predictability also considers the cross-sectional predictability of returns, that is, tests of asset-pricing models and the anomalies (like the size effect) discovered in the tests. Finally, the evidence that there are seasonals in returns (like the January effect), and the claim that security prices are too volatile are also considered, but only briefly, under the rubric of return predictability.

For the second and third categories, I propose changes in title, not coverage. Instead of semi-strong-form tests of the adjustment of prices to public announcements, I use the now common title, *event studies*. Instead of strong-form tests of whether specific investors have information not in market prices, I suggest the more descriptive title, *tests for private information*.

Return predictability is considered first, and in the most detail. The detail reflects my interest and the fact that the implications of the evidence on the predictability of returns through time are the most controversial. In brief, the new work says that returns are predictable from past returns, dividend yields, and various term-structure variables. The new tests thus reject the old market efficiency-constant expected returns model that seemed to do well in the early work. This means, however, that the new results run head-on into the joint-hypothesis problem: Does return predictability reflect rational variation through time in expected returns, irrational deviations of price from fundamental value, or some combination of the two? We should also acknowledge that the apparent predictability of returns may be spurious, the result of data-dredging and chance sample-specific conditions.

The evidence discussed below, that the variation through time in expected returns is common to corporate bonds and stocks and is related in plausible ways to business conditions, leans me toward the conclusion that it is real and rational. Rationality is not established by the existing tests, however, and the joint-hypothesis problem likely means that it cannot be established. Still, even if we disagree on the market efficiency implications of the new results on return predictability, I think we can agree that the tests enrich our knowledge of the behavior of returns, across securities and through time.

Event studies are discussed next, but briefly. Detailed reviews of event studies are already available, and the implications of this research for market efficiency are less controversial. Event studies have, however, been a growth industry during the last 20 years. Moreover, I argue that, because they come closest to allowing a break between market efficiency and equilibrium-pricing issues, event studies give the most direct evidence on efficiency. And the evidence is mostly supportive.

Finally, tests for private information are reviewed. The new results clarify earlier evidence that corporate insiders have private information that is not fully reflected in prices. The new evidence on whether professional investment managers (mutual fund and pension fund) have private information is, however, murky, clouded by the joint-hypothesis problem.

III. Return Predictability: Time-Varying Expected Returns

There is a resurgence of research on the time-series predictability of stock returns, that is, the variation (rational or irrational) of expected returns through time. Unlike the pre-1970 work, which focused on forecasting returns from past returns, recent tests also consider the forecast power of variables like dividend yields (D/P), earnings/price ratios (E/P), and term-structure variables. Moreover, the early work concentrated on the predictability of daily, weekly, and monthly returns, but the recent tests also examine the predictability of returns for longer horizons.

Among the more striking new results are estimates that the predictable component of returns is a small part of the variance of daily, weekly, and monthly returns, but it grows to as much as 40% of the variance of 2- to 10-year returns. These results have spurred a continuing debate on whether the predictability of long-horizon returns is the result of irrational bubbles in prices or large rational swings in expected returns.

I first consider the research on predicting returns from past returns. Next comes the evidence that other variables (D/P, E/P, and term-structure variables) forecast returns. The final step is to discuss the implications of this work for market efficiency.

A. Past Returns

A. 1. Short-Horizon Returns

In the pre-1970 literature, the common equilibrium-pricing model in tests of stock market efficiency is the hypothesis that expected returns are constant through time. Market efficiency then implies that returns are unpredictable from past returns or other past variables, and the best forecast of a return is its historical mean.

The early tests often find suggestive evidence that daily, weekly, and monthly returns are predictable from past returns. For example, Fama (1965) finds that the first-order autocorrelations of daily returns are positive for 23 of the 30 Dow Jones Industrials and more than 2 standard errors from 0 for 11 of the 30. Fisher's (1966) results suggest that the autocorrelations of monthly returns on diversified portfolios are positive and larger than those for individual stocks. The evidence for predictability in the early work often lacks statistical power, however, and the portion of the variance of returns explained by variation in expected returns is so small (less than 1% for individual stocks) that the hypothesis of market efficiency and constant expected returns is typically accepted as a good working model.

In recent work, daily data on NYSE and AMEX stocks back to 1962 [from the Center for Research in Security Prices (CRSP)] makes it possible to estimate precisely the autocorrelation in daily and weekly returns. For example, Lo and MacKinlay (1988) find that weekly returns on portfolios of NYSE stocks grouped according to size (stock price times shares outstanding) show reliable positive autocorrelation. The autocorrelation is stronger for portfolios of small stocks. This suggests, however, that the results are due in part to the nonsynchronous trading effect (Fisher 1966). Fisher emphasizes that spurious positive autocorrelation in portfolio returns, induced by nonsynchronous closing trades for securities in the portfolio, is likely to be more important for portfolios tilted toward small stocks.

To mitigate the nonsychronous trading problem, Conrad and Kaul (1988) examine the autocorrelation of Wednesday-to-Wednesday returns for size-grouped portfolios of stocks that trade on both Wednesdays. Like Lo and MacKinlay (1988), they find that weekly returns are positively autocorrelated, and more so for portfolios of small stocks. The first-order autocorrelation of weekly returns on the portfolio of the largest decile of NYSE stocks for 1962–1985 is only .09. For the portfolios that include the smallest 40% of NYSE stocks, however, first-order autocorrelations of weekly returns are around .3, and the autocorrelations of weekly returns are reliably positive out to 4 lags.

The results of Lo and MacKinlay (1988) and Conrad and Kaul (1988) show that, because of the variance reduction obtained from diversification, portfolios produce stronger indications of time variation in weekly expected returns than individual stocks. Their results also suggest that returns are more predictable for small-stock portfolios. The evidence is, however, clouded by the fact that the predictability of portfolio returns is in part due to nonsynchronous trading effects that, especially for small stocks, are not completely mitigated by using stocks that trade on successive Wednesdays.

An eye-opener among recent studies of short-horizon returns is French and Roll (1986). They establish an intriguing fact. Stock prices are more variable when the market is open. On an hourly basis, the variance of price changes is 72 times higher during trading hours than during weekend nontrading hours. Likewise, the hourly variance during trading hours is 13 times the overnight nontrading hourly variance during the trading week.

One of the explanations that French and Roll test is a market inefficiency hypothesis popular among academics; specifically, the higher variance of price changes during trading hours is partly transistory, the result of noise trading by uniformed investors (e.g., Black (1986)). Under this hypothesis, pricing errors due to noise trading are eventually reversed, and this induces negative autocorrelation in daily returns. French and Roll find that the first-order autocorrelations of daily returns on the individual stocks of larger (the top three quintiles of) NYSE firms are positive. Otherwise, the autocorrelations of daily returns on individual stocks are indeed negative, to 13 lags. Although reliably negative on a statistical basis, however, the autocorrelations are on average close to 0. Few are below $-.01$.

One possibility is that the transitory price variation induced by noise trading only dissipates over longer horizons. To test this hypothesis, French and Roll examine the ratios of variances of N-period returns on individual stocks to the variance of daily returns, for N from 2 days to 6 months. If there is no transitory price variation induced by noise trading (specifically, if price changes are i.i.d.), the N-period variance should grow like N, and the variance ratios (standardized by N) should be close to 1. On the other hand, with transitory price variation, the N-period variance should grow less than in proportion to N, and the variance ratios should be less than 1.

For horizons (N) beyond a week, the variance ratios are more than 2 standard errors below 1, except for the largest quintile of NYSE stocks. But the fractions of daily return variances due to transitory price variation are apparently small. French and Roll estimate that for the average NYSE stock, the upper bound on the transitory portion of the daily variance is 11.7%. Adjusted for the spurious negative autocorrelation of daily returns due to bid-ask effects (Roll (1984)), the estimate of the transitory portion drops to 4.1%. The smallest quintile of NYSE stocks produces the largest estimate of the transitory portion of price variation, an upper bound of 26.9%. After correction for bid-ask effects, however, the estimate drops to 4.7%—hardly a number on which to conclude that noise trading results in substantial market inefficiency. French and Roll (1986, p. 23) conclude, "pricing errors . . . have a trivial effect on the difference between trading and non-trading variances. We conclude that this difference is caused by differences in the flow of information during trading and non-trading hours."

In short, with the CRSP daily data back to 1962, recent research is able to show confidently that daily and weekly returns are predictable from past returns. The work thus rejects the old market efficiency-constant expected returns model on a statistical basis. The new results, however, tend to confirm the conclusion of the early work that, at least for individual stocks, variation in daily and weekly expected returns is a small part of the variance of returns. The more striking, but less powerful, recent evidence on the predictability of returns from past returns comes from long-horizon returns.

A. 2. Long-Horizon Returns

The early literature does not interpret the autocorrelation in daily and weekly returns as important evidence against the joint hypothesis of market efficiency and constant expected returns. The argument is that, even when the autocorrelations deviate reliably from 0 (as they do in the recent tests), they are close to 0 and thus economically insignificant.

The view that autocorrelations of short-horizon returns close to 0 imply economic insignificance is challenged by Shiller (1984) and Summers (1986). They present simple models in which stock prices take large slowly decaying swings away from fundamental values (fads, or irrational bubbles), but short-horizon returns have little autocorrelation. In the Shiller-Summers model, the market is highly inefficient, but in a way that is missed in tests on short-horizon returns.

To illustrate the point, suppose the fundamental value of a stock is constant and the unconditional mean of the stock price is its fundamental value. Suppose daily prices are a first-order autoregression (AR1) with slope less than but close to 1. All variation in the price then results from long mean-reverting swings away from the constant fundamental value. Over short horizons, however, an AR1 slope close to 1 means that the price looks like a random walk and returns have little autocorrelation. Thus in tests on short-horizon returns, all price changes seem to be permanent when fundamental value is in fact constant and all deviations of price from fundamental value are temporary.

In his comment on Summers (1986), Stambaugh (1986) points out that although the Shiller-Summers model can explain autocorrelations of short-horizon returns that are close to 0, the long swings away from fundamental value proposed in the model imply that long-horizon returns have strong negative autocorrelation. (In the example above, where the price is a stationary AR1, the autocorrelations of long-horizon returns approach -0.5.) Intuitively, since the swings away from fundamental value are temporary, over long horizons they tend to be reversed. Another implication of the negative autocorrelation induced by temporary price movements is that the variance of returns should grow less than in proportion to the return horizon.

The Shiller-Summers challenge spawned a series of papers on the predictability of long-horizon returns from past returns. The evidence at first seemed striking, but the tests turn out to be largely fruitless. Thus, Fama and French (1988a) find that the autocorrelations of returns on diversified portfolios of NYSE stocks for the 1926–1985 period have the pattern predicted by the Shiller-Summers model. The autocorrelations are close to 0 at short horizons, but they become strongly negative, around -0.25 to -0.4, for 3- to 5-year returns. Even with 60 years of data, however, the tests on long-horizon returns imply small sample sizes and low power. More telling, when Fama and French delete the 1926–1940 period from the tests, the evidence of strong negative autocorrelation in 3- to 5-year returns disappears.

Similarly, Poterba and Summers (1988) find that, for N from 2 to 8 years, the variance of N-year returns on diversified portfolios grows much less than in proportion to N. This is consistent with the hypothesis that there is negative autocorrelation in returns induced by temporary price swings. Even with 115 years (1871–1985) of data, however, the variance tests for long-horizon returns provide weak statistical evidence against the hypothesis that returns have no autocorrelation and prices are random walks.

Finally, Fama and French (1988a) emphasize that temporary swings in stock prices do not necessarily imply the irrational bubbles of the Shiller-Summers model. Suppose (1) rational pricing implies an expected return that is highly autocorrelated but mean-reverting, and (2) shocks to expected returns are uncorrelated with shocks to expected dividends. In this situation, expected-return shocks have no permanent effect on expected dividends, discount rates, or prices. A positive shock to expected returns generates a price decline (a discount rate effect) that is eventually erased by the temporarily higher expected returns. In short, a ubiquitous problem in time-series tests of market efficiency, with no clear solution, is that irrational bubbles in stock prices are indistinguishable from rational time-varying expected returns.

A. 3. The Contrarians

DeBondt and Thaler (1985, 1987) mount an aggressive empirical attack on market efficiency, directed at unmasking irrational bubbles. They find that the NYSE stocks identified as the most extreme losers over a 3- to 5-year period tend to have strong returns relative to the market during the following years, expecially in January of the following years. Conversely, the stocks identified as extreme winners tend to have weak returns relative to the market in subsequent years. They attribute these results to market overreaction to extreme bad or good news about firms.

Chan (1988) and Ball and Kothari (1989) argue that the winner-loser results are due to failure to risk-adjust returns. (DeBondt and Thaler (1987) disagree.) Zarowin (1989) finds no evidence for the DeBondt-Thaler hypothesis that the winner-loser results are due to overreaction to extreme changes in earnings. He argues that the winner-loser effect is related to the size effect of Banz (1981); that is, small stocks, often losers, have higher expected returns than large stocks. Another explanation, consistent with an efficient market, is that there is a risk factor associated with the relative economic performance of firms (a distressed-firm effect) that is compensated in a rational equilibrium-pricing model (Chan and Chen (1991)).

We may never be able to say which explanation of the return behavior of extreme winners and losers is correct, but the results of DeBondt and Thaler and their critics are nevertheless interesting. (See also Jagedeesh (1990), Lehmann (1990), and Lo and MacKinlay (1990), who find reversal behavior in the weekly and monthly returns of extreme winners and losers. Lehmann's weekly reversals seem to lack economic significance. When he accounts for spurious reversals due to bouncing between bid and ask prices, trading costs of 0.2% per turnaround transaction suffice to make the profits from his reversal trading rules close to 0. It is also worth noting that the short-term reversal evidence of Jegadeesh, Lehmann, and Lo and MacKinlay may to some extent be due to CRSP data errors, which would tend to show up as price reversals.)

B. Other Forecasting Variables

The univariate tests on long-horizon returns of Fama and French (1988a) and Poterba and Summers (1988) are a statistical power failure. Still, they provide suggestive material to spur the search for more powerful tests of the hypothesis that slowly decaying irrational bubbles, or rational time-varying expected returns, are important in the long-term variation of prices.

There is a simple way to see the power problem. An autocorrelation is the slope in a regression of the current return on a past return. Since variation through time in expected returns is only part of the variation in returns, tests based on autocorrelations lack power because past realized returns are noisy measures of expected returns. Power in tests for return predictability can be enhanced if one can identify forecasting variables that are less noisy proxies for expected returns that past returns.

B. 1. The Evidence

There is no lack of old evidence that short-horizon returns are predictable from other variables. A puzzle of the 1970's was to explain why monthly stock returns are negatively related to expected inflation (Bodie (1976), Nelson (1976), Jaffe and Mandelker (1976), Fama (1981)) and the level of short-term interest rates (Fama and Schwert (1977)). Like the autocorrelation tests, however, the early work on forecasts of short-horizon returns from expected inflation and interest rates suggests that the implied variation in expected returns is a small part of the variance of returns—less than 3% for monthly returns. The recent tests suggest, however, that for long-horizon returns, predictable variation is a larger part of return variances.

Thus, following evidence (Rozeff (1984), Shiller (1984)) that dividend yields (D/P) forecast short-horizon stock returns, Fama and French (1988b) use D/P to forecast returns on the value-weighted and equally weighted portfolios of NYSE stocks for horizons from 1 month to 5 years. As in earlier work, D/P explains small fractions of monthly and quarterly return variances. Fractions of variance explained grow with the return horizon, however, and are around 25% for 2- to 4-year returns. Campbell and Shiller (1988b) find that E/P ratios, especially when past earnings (E) are averaged over 10–30 years, have reliable forecast power that also increases with the return horizon. Unlike the long-horizon autocorrelations in Fama and French (1988a), the long-horizon forecast power of D/P and E/P is reliable for periods after 1940.

Fama and French (1988b) argue that dividend yields track highly autocorrelated variation in expected stock returns that becomes a larger fraction of return variation for longer return horizons. The increasing fraction of the variance of long-horizon returns explained by D/P is thus due in large part to the slow mean reversion of expected returns. Examining the forecast power of variables like D/P and E/P over a range of return horizons nevertheless gives striking perspective on the implications of slow-moving expected returns for the variation of returns.

B. 2. Market Efficiency

The predictability of stock returns from dividend yields (or E/P) is not in itself evidence for or against market efficiency. In an efficient market, the forecast power of D/P says that prices are high relative to dividends when discount rates and expected returns are low, and vice versa. On the other hand, in a world of irrational bubbles, low D/P signals irrationally high stock prices that will move predictably back toward fundamental values. To judge whether the forecast power of dividend yields is the result of rational variation in expected returns or irrational bubbles, other information must be used. As always, even with such information, the issue is ambiguous.

For example, Fama and French (1988b) show that low dividend yields imply low expected returns, but their regressions rarely forecast negative returns for the value- and equally weighted portfolios of NYSE stocks. In their data, return forecasts more than 2 standard errors below 0 are never observed, and more than 50% of the forecasts are more than 2 standard errors above 0. Thus there is no evidence that low D/P signals bursting bubbles, that is, negative expected stock returns. A bubbles fan can argue, however, that because the unconditional means of stock returns are high, a bursting bubble may well imply low but not negative expected returns. Conversely, if there were evidence of negative expected returns, an efficient-markets type could argue that asset-pricing models do not say that rational expected returns are always positive.

Fama and French (1989) suggest a different way to judge the implications of return predictability for market efficiency. They argue that if variation in expected returns is common to different securities, then it is probably a rational result of variation in tastes for current versus future consumption or in the investment opportunities of firms. They show that the dividend yield on the NYSE value-weighted portfolio indeed forecasts the returns on corporate bonds as well as common stocks. Moreover, two term-structure variables, (1) the default spread (the difference between the yields on lower-grade and Aaa long-term corporate bonds) and (2) the term spread (the difference between the long-term Aaa yield and the yield on 1-month Treasury bills), forecast returns on the value- and equally weighted portfolios of NYSE stocks as well as on portfolios of bonds in different (Moodys) rating groups.

Keim and Stambaugh (1986) and Campbell (1987) also find that stock and bond returns are predictable from a common set of stock market and term-structure variables. Harvey (1991) finds that the dividend yield on the S&P 500 portfolio and U.S. term-structure variables forecast the returns on portfolios of foreign common stocks, as well as the S&P return. Thus the variation in expected returns tracked by the U.S. dividend yield and term-structure variables is apparently international.

Ferson and Harvey (1991) formally test the common expected returns hypothesis. Using the asset-pricing models of Merton (1973) and Ross (1976), they try to link the time-series variation in expected returns, captured by dividend yields and term-structure variables, to the common factors in returns that determine the cross-section of expected returns. They estimate that the common variation in expected returns is about 80% of the predictable time-series variation in the returns on Government bonds, corporate bonds, and common-stock portfolios formed on industry and size. They can't reject the hypothesis that all the time-series variation in expected returns is common.

Fama and French (1989) push the common expected returns argument for market efficiency one step further. They argue that there are systematic patterns in the variation of expected returns through time that suggest that it is rational. They find that the variation in expected returns tracked by D/P and the default spread (the slopes in the regressions of returns on D/P or the default spread) increase from high-grade bonds to low-grade bonds, from bonds to stocks, and from large stocks to small stocks. This ordering corresponds to intuition about the risks of the securities. On the other hand, the variation in expected returns tracked by the term spread is similar for all long-term securities (bonds and stocks), which suggests that it reflects variation in a common premium for maturity risks.

Finally, Fama and French (1989) argue that the variation in the expected returns on bonds and stocks captured by their forecasting variables is consistent with modern intertemporal asset-pricing models (e.g., Lucas (1978), Breeden (1979)), as well as with the original consumption-smoothing stories of Friedman (1957) and Modigliani and Brumberg (1955). The general message of the Fama-French tests (confirmed in detail by Chen (1991)) is that

D/P and the default spread are high (expected returns on stocks and bonds are high) when times have been poor (growth rates of output have been persistently low). On the other hand, the term spread and expected returns are high when economic conditions are weak but anticipated to improve (future growth rates of output are high). Persistent poor times may signal low wealth and higher risks in security returns, both of which can increase expected returns. In addition, if poor times (and low incomes) are anticipated to be partly temporary, expected returns can be high because consumers attempt to smooth consumption from the future to the present.

For the diehard bubbles fan, these arguments that return predictability is rational are not convincing. Common variation in expected returns may just mean that irrational bubbles are correlated across assets and markets (domestic and international). The correlation between the common variation in expected returns and business conditions may just mean that the common bubbles in different markets are related to business conditions. On the other hand, if there were evidence of security-specific variation in expected returns, an efficient-markets type could argue that it is consistent with uncorrelated variation through time in the risks of individual securities. All of which shows that deciding whether return predictability is the result of rational variation in expected returns or irrational bubbles is never clearcut.

My view is that we should deepen the search for links between time-varying expected returns and business conditions, as well as for tests of whether the links conform to common sense and the predictions of asset-pricing models. Ideally, we would like to know how variation in expected returns relates to productivity shocks that affect the demand for capital goods, and to shocks to tastes for current versus future consumption that affect the supply of savings. At a minimum, we can surely expand the work in Chen (1991) on the relations between the financial market variables that track expected returns (D/P and the term-structure variables) and the behavior of output, investment, and saving. We can also extend the preliminary attempts of Balvers, Cosimano and McDonald (1990), Cechetti, Lam, and Mark (1990) and Kandel and Stambaugh (1990) to explain the variation through time in expected returns in the confines of standard asset-pricing models.

B. 3. A Caveat

The fact that variation in expected returns is common across securities and markets, and is related in plausible ways to business conditions, leans me toward the conclusion that, if it is real it is rational. But how much of it is real? The standard errors of the slopes for the forecasting variables in the return regressions are typically large and so leave much uncertainty about forecast power (Hodrick (1990), Nelson and Kim (1990)). Inference is also clouded by an industry-level data-dredging problem. With many clever researchers, on both sides of the efficiency fence, rummaging for forecasting variables, we are sure to find instances of "reliable" return predictability that are in fact spurious.

Moreover, the evidence that measured variation in expected returns is common across securities, and related to business conditions, does not necessarily mean that it is real. Suppose there is common randomness in stock and bond returns due to randomness in business conditions. Then measured variation in expected returns that is the spurious result of sample-specific conditions is likely to be common across securities and related to business conditions. In short, variation in expected returns with business conditions is plausible and consistent with asset-pricing theory. But evidence of predictability should always be met with a healthy dose of skepticism, and a diligent search for out-of-sample confirmation.

C. Volatility Tests and Seasonals in Returns

C. 1. Volatility Tests

Volatility tests of market efficiency, pioneered by LeRoy and Porter (1981) and Shiller (1979, 1981), have mushroomed into a large literature. Excellent reviews (West (1988), LeRoy (1989), Cochrane (1991)) are available, so here I briefly comment on why I concur with Merton (1987), Kleidon (1988), and Cochrane (1991) that the tests are not informative about market efficiency.

A central assumption in the early volatility tests is that expected returns are constant and the variation in stock prices is driven entirely by shocks to expected dividends. By the end of the 1970's, however, evidence that expected stock and bond returns vary with expected inflation rates, interest rates, and other term-structure variables was becoming commonplace (Bodie (1976), Jaffe and Mandelker (1976), Nelson (1976), Fama (1976a, b), Fama and Schwert (1977)). With all the more recent evidence on return predictability, it now seems clear that volatility tests are another useful way to show that expected returns vary through time.

The volatility tests, however, give no help on the central issue of whether the variation in expected returns is rational. For example, is it related in sensible ways to business conditions? Grossman and Shiller (1981) and Campbell and Shiller (1988a) attempt to move the volatility tests in this direction. Predictably, however, they run head-on into the joint hypothesis problem. They test market efficiency jointly with the hypothesis that their versions of the consumption-based asset-pricing model capture all rational variation in expected returns.

C. 2. Return Seasonality

The recent literature includes a spate of "anomalies" papers that document "seasonals" in stock returns. Monday returns are on average lower than returns on other days (Cross (1973), French (1980), Gibbons and Hess (1981)). Returns are on average higher the day before a holiday (Ariel 1990), and the last day of the month (Ariel (1987)). There also seems to be a seasonal in intraday returns, with most of the average daily return coming at the beginning and end of the day (Harris (1986)). The most mystifying seasonal is the January effect. Stock returns, especially returns on small stocks, are on average higher in January than in other months. Moreover, much of the higher January return on small stocks comes on the last trading day in December and the first 5 trading days in January (Keim (1983), Roll (1983)).

Keim (1988) reviews this literature. He argues that seasonals in returns are anomalies in the sense that asset-pricing models do not predict them, but they are not necessarily embarassments for market efficiency. For example, Monday, holiday, and end-of-month returns deviate from normal average daily returns by less than the bid-ask spread of the average stock (Lakonishok and Smidt (1988)). Turn-of-the-year abnormal returns for small stocks are larger, but they are not large relative to the bid-ask spreads of small stocks (Roll (1983)). There is thus some hope that these seasonals can be explained in terms of market microstructure, that is, seasonals in investor trading patterns that imply innocuous seasonals in the probabilities that measured prices are at ask or bid. The evidence in Lakonishok and Maberly (1990) on Monday trading patterns, and in Reinganum (1983), Ritter (1988), and Keim (1989) on turn-of-the-year trading are steps in that direction.

We should also keep in mind that the CRSP data, the common source of evidence on stock returns, are mined on a regular basis by many researchers. Spurious regularities are a sure consequence. Apparent anomalies in returns thus warrant out-of-sample tests before being accepted as regularities that are likely to be present in future returns. Lakonishok and Smidt (1988) find that the January, Monday, holiday, and end-of-month seasonals stand up to replication on data preceding the periods used in the original tests. The intramonth seasonal (most of the average return of any month comes in the first half) of Ariel (1987), however, seems to be specific to his sample period. Connolly (1989) finds that the Monday seasonal in NYSE returns is weaker after 1974.

Recent data on the premier seasonal, the January effect, tell an interesting story. Table I shows that for the 1941–1981 period, the average monthly January return on a value-weighted portfolio of the smallest quintile of CRSP stocks is 8.06% (!), versus 1.34% for the S&P 500. During the 1941–1981 period, there is only 1 year (1952) when the S&P January return is above the CRSP bottom-quintile return. Moreover, for 1941–1981, all of the advantage of the CRSP small-stock portfolio over the S&P comes in January; the February-to-December average monthly returns on the two portfolios differ by only 4 basis points (0.88% for CRSP Small versus 0.92% for the S&P).

For 1982–1991, however, the average January return on the CRSP small-stock portfolio, 5.32%, is closer to the January S&P return, 3.20%. More striking, the average January return on the DFA U.S. Small Company Portfolio, a passive mutual fund meant to roughly mimic the CRSP bottom quintile, is 3.58%, quite close to the January S&P return (3.20%) and much less than the January return for the CRSP small-stock portfolio (5.32%). The CRSP small-stock portfolio has a higher return than the DFA portfolio in every January of 1982–1991. But January is the exception; overall, the DFA portfolio earns about 3% per year more than the CRSP bottom quintile.

Table I

Comparison of Returns on the S&P 500, the Smallest Quintile of CRSP Stocks, and the DFA U.S. Small Company Portfolio: 1941–81 and 1982–91

The value-weighted CRSP small-stock portfolio (CRSP Small) contains the bottom qunitile of NYSE stocks, and the AMEX and NASDAQ stocks that fall below the size (price times shares) breakpoint for the bottom qunitile of NYSE stocks. The portfolio is formed at the end of each quarter and held for one quarter. Prior to June 1962, CRSP Small contains only the bottom quintile of NYSE stocks. AMEX stocks are added in July 1962 and NASDAQ stocks in January 1973. The DFA U.S. Small Company Portfolio (DFA Small) is a passive mutual fund meant to roughly mimic CRSP Small. DFA Small returns are only available for the 1982–1991 period.

Average Monthly Returns for January, February to December, and All Months

Portfolio	1941–1981 Jan	1941–1981 Feb–Dec	1941–1981 All	1982–1990 (91 for January) Jan	1982–1990 (91 for January) Feb–Dec	1982–1990 (91 for January) All
S&P 500	1.34	0.92	0.96	3.20	1.23	1.39
CRSP Small	8.06	0.88	1.48	5.32	0.17	0.60
DFA Small				3.58	0.66	0.90

Year-by-Year Comparison of January Returns for 1982–1991

Year	S&P	CRSP Small	DFA Small	CRSP-S&P	DFA-S&P
1982	−1.63	−1.53	−1.96	0.10	−0.33
1983	3.48	10.01	6.28	6.53	2.80
1984	−0.65	0.26	−0.08	0.91	0.57
1985	7.68	13.41	10.59	5.73	2.91
1986	0.44	3.82	1.12	3.38	0.68
1987	13.43	10.91	9.43	−2.52	−4.00
1988	4.27	7.58	5.56	3.31	1.29
1989	7.23	4.79	4.04	−2.44	−3.19
1990	−6.71	−6.38	−7.64	0.33	−0.93
1991	4.42	10.28	8.41	5.86	3.99

Why these differences between the returns on the CRSP small-stock portfolio and a mimicking passive mutual fund? DFA does not try to mimic exactly the CRSP bottom quintile. Concern with trading costs causes DFA to deviate from strict value weights and to avoid the very smallest stocks (that are, however, a small fraction of a value-weighted portfolio). Moreover, DFA does not sell stocks that do well until they hit the top of the third (smallest) decile. This means that their stocks are on average larger than the stocks in the CRSP bottom quintile (a strategy that paid off during the 1982–1991 period of an inverted size effect.)

The important point, however, is that small-stock returns, and the very existence of a January bias in favor of small stocks, are sensitive to small changes (imposed by rational trading) in the way small-stock portfolios are defined. This suggests that, until we know more about the pricing (and economic fundamentals) of small stocks, inferences should be cautious for the many anomalies where small stocks play a large role (e.g., the overreaction evidence of DeBondt and Thaler (1985, 1987) and Lehmann (1990), and (discussed below) the size effect of Banz (1981), the Value Line enigma of Stickel (1985), and the earnings-announcement anomaly of Bernard and Thomas (1989, 1990)).

Finally, given our fascination with anomalies that center on small stocks, it is well to put the relative importance of small stocks in perspective. At the end of 1990, there were 5135 NYSE, AMEX, and NASDAQ (NMS) stocks. Using NYSE stocks to define size breakpoints, the smallest quintile has 2631 stocks, 51.2% of the total. But the bottom quintile is only 1.5% of the combined value of NYSE, AMEX, and NASDAQ stocks. In contrast, the largest quintile has 389 stocks (7.6% of the total), but it is 77.2% of market wealth.

IV. Cross-Sectional Return Predictability

At the time of the 1970 review, the asset-pricing model of Sharpe (1964), Lintner (1965), and Black (1972) was just starting to take hold. Ross's (1976) arbitrage-pricing model and the intertemporal asset-pricing models of Merton (1973), Rubinstein (1976), Lucas (1978), Breeden (1979), and Cox, Ingersoll, and Ross (1985) did not exist. In the pre-1970 efficient markets literature, the common "models" of market equilibrium were the informal constant expected returns model (random-walk and martingale tests) and the market model (event studies, like Fama, Fisher, Jensen, and Roll (1969)).

This section considers the post-1970 empirical research on asset-pricing models. This work does not place itself in the realm of tests of market efficiency, but this just means that efficiency is a maintained hypothesis. Depending on the emphasis desired, one can say that efficiency must be tested conditional on an asset-pricing model or that asset-pricing models are tested conditional on efficiency. The point is that such tests are always joint evidence on efficiency and an asset-pricing model.

Moreover, many of the front-line empirical anomalies in finance (like the size effect) come out of tests directed at asset-pricing models. Given the joint hypothesis problem, one can't tell whether such anomalies result from misspecified asset-pricing models or market inefficiency. This ambiguity is sufficient justification to review tests of asset-pricing models here.

We first consider tests of the one-factor Sharpe-Lintner-Black (SLB) model. I argue that the SLB model does the job expected of a good model. In rejecting it, repeatedly, our understanding of asset-pricing is enhanced. Some of the most striking empirical regularities discovered in the last 20 years are "anomalies" from tests of the SLB model. These anomalies are now stylized facts to be explained by other asset-pricing models.

The next step is to review the evidence on the multifactor asset-pricing models of Merton (1973) and Ross (1976). These models are rich and more flexible than their competitors. Based on existing evidence, they show some promise to fill the empirical void left by the rejections of the SLB model.

The final step is to discuss tests of the consumption-based intertemporal asset-pricing model of Rubinstein (1976), Lucas (1978), Breeden (1979), and others. The elegant simplicity of this model gives it strong appeal, and much effort has been devoted to testing it. The effort is bearing fruit. Recent tests add to our understanding of the behavior of asset returns in ways that go beyond tests of other models (e.g., the equity-premium puzzle of Mehra and Prescott (1985)). On the other hand, the tests have not yet taken up the challenges (like the size effect) raised by rejections of the SLB model.

A. The Sharpe-Lintner-Black (SLB) Model

A. 1. Early Success

The early 1970's produce the first extensive tests of the SLB model (Black, Jensen, and Scholes (1972), Blume and Friend (1973), Fama and MacBeth (1973)). These early studies suggest that the special prediction of the Sharpe-Lintner version of the model, that portfolios uncorrelated with the market have expected returns equal to the risk-free rate of interest, does not fare well. (The average returns on such "zero-β" portfolios are higher than the risk-free rate.) Other predictions of the model seem to do better.

The most general implication of the SLB model is that equilibrium pricing implies that the market portfolio of invested wealth is ex ante mean-variance efficient in the sense of Markowitz (1959). Consistent with this hypothesis, the early studies suggest that (1) expected returns are a positive linear function of market β (the covariance of a security's return with the return on the market portfolio divided by the variance of the market return), and (2) β is the only measure of risk needed to explain the cross-section of expected returns. With this early support for the SLB model, there was a brief euphoric period in the 1970's when market efficiency and the SLB model seemed to be a sufficient description of the behavior of security returns.

We should have known better. The SLB model is just a model and so surely false. The first head-on attack is Roll's (1977) criticism that the early tests aren't much evidence for the SLB model because the proxies used for the market portfolio (like the equally weighted NYSE portfolio) do not come close to the portfolio of invested wealth called for by the model. Stambaugh's (1982) evidence that tests of the SLB model are not sensitive to the proxy used for the market suggests that Roll's criticism is too strong, but this issue can never be entirely resolved.

A. 2. Anomalies

The telling empirical attacks on the SLB model begin in the late 1970's with studies that identify variables that contradict the model's prediction that market β's suffice to describe the cross-section of expected returns. Basu (1977, 1983) shows that earnings/price ratios (E/P) have marginal explanatory power; controlling for β, expected returns are positively related to E/P. Banz (1981) shows that a stock's size (price times shares) helps explain expected returns; given their market β's, expected returns on small stocks are too high, and expected returns on large stocks are too low. Bhandari (1988) shows that leverage is positively related to expected stock returns in tests that also include market β's. Finally, Chan, Hamao, and Lakonishok (1991) and Fama and French (1991) find that book-to-market equity (the ratio of the book value of a common stock to its market value) has strong explanatory power; controlling for β, higher book-to-market ratios are associated with higher expected returns.

One argument says that the anomalies arise because estimates of market β's are noisy, and the anomalies variables are correlated with true β's. For example, Chan and Chen (1988) find that when portfolios are formed on size, the estimated β's of the portfolios are almost perfectly correlated (-0.988) with the average size of stocks in the portfolios. Thus, distinguishing between the roles of size and β in the expected returns on size portfolios is likely to be difficult. Likewise, theory predicts that, given a firm's business activities, the β of its stock increases with leverage. Thus leverage might proxy for true β's when β estimates are noisy.

Another approach uses the multifactor asset-pricing models of Merton (1973) and Ross (1976) to explain the SLB anomalies. For example, Ball (1978) argues that E/P is a catch-all proxy for omitted factors in asset-pricing tests. Thus, if two stocks have the same current earnings but different risks, the riskier stock has a higher expected return, and it is likely to have a lower price and higher E/P. E/P is then a general proxy for risk and expected returns, and one can expect it to have explanatory power when asset-pricing follows a multifactor model and all relevant factors are not included in asset-pricing tests.

Chan and Chen (1991) argue that the size effect is due to a distressed-firm factor in returns and expected returns. When size is defined by the market value of equity, small stocks include many marginal or depressed firms whose performance (and survival) is sensitive to business conditions. Chan and Chen argue that relative distress is an added risk factor in returns, not captured by market β, that is priced in expected returns. Fama and French (1991) argue that since leverage and book-to-market equity are also largely driven by the market value of equity, they also may proxy for risk factors in returns that are related to relative distress or, more generally, to market judgments about the relative prospects of firms.

Other work shows that there is indeed spillover among the SLB anomalies. Reinganum (1981) and Basu (1983) find that size and E/P are related; small stocks tend to have high E/P. Bhandari (1988) finds that small stocks include many firms that are highly levered, probably as result of financial distress. Chan, Hamao, and Lakonishok (1991) and Fama and French (1991) find that size and book-to-market equity are related; hard times and lower stock prices cause many stocks to become small, in terms of market equity, and so to have high book-to-market ratios. Fama and French (1991) find that leverage and book-to-market equity are highly correlated. Again, these links among the anomalies are hardly surprising, given that the common driving variable in E/P, leverage, size, and book-to-market equity is a stock's price.

How many of the SLB anomalies have separately distinguishable roles in expected returns? In tests aimed at this question, Fama and French (1991) find that for U.S. stocks, E/P, leverage, and book-to-market equity weaken but do not fully absorb the relation between size and expected returns. On the other hand, when size and book-to-market equity are used together, they leave no measurable role for E/P or leverage in the cross-section of average returns on NYSE, AMEX, and NASDAQ stocks. Chan, Hamao, and Lakonishok (1991) get similar results for Japan. The strong common result of Chan, Hamao, and Lakonishok (1991) and Fama and French (1991) is that for Japanese and U.S. stocks, book-to-market equity is the most powerful explanatory variable in the cross-section of average returns, with a weaker role for size. Thus, book-to-market equity seems to have displaced size as the premier SLB anomaly.

In truth, the premier SLB anomaly is not size or book-to-market equity but the weak role of market β in the cross-section of average returns on U.S. stocks. For example, Fama and French (1991) find that the relation between β and average returns on NYSE, AMEX, and NASDAQ stocks for 1963–1990 is feeble, even when β is the only explanatory variable. Their estimated premium per unit of β is 12 basis points per month (1.44% per year), and less than 0.5 standard errors from 0. Stambaugh (1982) and Lakonishok and Shapiro (1986) get similar results for NYSE stocks for 1953–1976 and 1962–1981.

Chan and Chen (1988) find that when the assets used in tests of the SLB model are common-stock portfolios formed on size, there is a strong relation between average returns and β in the 1954–1983 period. Fama and French (1991) show, however, that this result is due to the strong correlation between the β's of size portfolios and the average size of the stocks in the portfolios (-0.988 in Chan and Chen). Fama and French find that when portfolios are formed on size and β (as in Banz 1981), there is strong variation in β that is unrelated to size (the range of the β's just about doubles), and it causes the relation between β and average returns to all but disappear after 1950. In short, the rather strong positive relation between β and the average returns on U.S. stocks observed in the early tests of Black, Jensen, and Scholes (1972) and Fama and MacBeth (1973) does not seem to extend to later periods.

Finally, Stambaugh (1982) shows that when the assets in the SLB tests are extended to include bonds as well as stocks, there is a reliable positive relation between average returns and β in the post-1953 period. His results, along with those of Lakonishok and Shapiro (1986) and Fama and French (1991), suggest two conclusions. (1) As predicted by the SLB model, there is a positive relation between expected returns and β across security types (bonds and stocks). (2) On average, however, the relation between expected returns and β for common stocks is weak, even though stocks cover a wide range of β's.

A. 3. Market Efficiency

The relations between expected returns and book-to-market equity, size, E/P, and leverage are usually interpreted as embarrassments for the SLB model, or the way it is tested (faulty estimates of market β's), rather than as evidence of market inefficiency. The reason is that the expected-return effects persist. For example, small stocks have high expected returns long after they are classified as small. In truth, though, the existing tests can't tell whether the anomalies result from a deficient (SLB) asset-pricing model or persistent mispricing of securities.

One can imagine evidence that bears on the matter. If a past anomaly does not appear in future data, it might be a market inefficiency, erased with the knowledge of its existence. (Or, the historical evidence for the anomaly may be a result of the profession's dogged data-dredging.) On the other hand, if the anomaly is explained by other asset-pricing models, one is tempted to conclude that it is a rational asset-pricing phenomenon. (But one should be wary that the apparent explanation may be the result of model-dredging.) In any case, I judge the maturity of the tests of other asset-pricing models in part on how well they explain, or at least address, the anomalies discovered in tests of the SLB model.

A. 4. The Bottom Line

With the deck of existing anomalies in hand, we should not be surprised when new studies show that yet other variables contradict the central prediction of the SLB model, that market β's suffice to describe the cross-section of expected returns. It is important to note, however, that we discover the contradictions because we have the SLB model as a sharp benchmark against which to examine the cross-section of expected returns. Moreover, the SLB model does its job. It points to empirical regularities in expected returns (size, E/P, leverage, and book-to-market effects) that must be explained better by any challenger asset-pricing model.

The SLB model also passes the test of practical usefulness. Before it became a standard part of MBA investments courses, market professionals had only a vague understanding of risk and diversification. Markowitz' (1959) portfolio model did not have much impact on practice because its statistics are relatively complicated. The SLB model, however, gave a summary measure of risk, market β, interpreted as market sensitivity, that rang mental bells. Indeed, in spite of the evidence against the SLB model, market professionals (and academics) still think about risk in terms of market β. And, like academics, practitioners retain the market line (from the riskfree rate through the market portfolio) of the Sharpe-Lintner model as a representation of the tradeoff of expected return for risk available from passive portfolios.

B. Multifactor Models

In the SLB model, the cross-section of expected returns on securities and portfolios is described by their market β's, where β is the slope in the simple regression of a security's return on the market return. The multifactor asset-pricing models of Merton (1973) and Ross (1976) generalize this result. In these models, the return-generating process can involve multiple factors, and the cross-section of expected returns is constrained by the cross-sections of factor loadings (sensitivities). A security's factor loadings are the slopes in a multiple regression of its return on the factors.

The multifactor models are an empiricist's dream. They are off-the-shelf theories that can accommodate tests for cross-sectional relations between expected returns and the loadings of security returns on any set of factors that are correlated with returns. How have tests of the models fared?

One approach, suggested by Ross' (1976) arbitrage-pricing theory (APT), uses factor analysis to extract the common factors in returns and then tests whether expected returns are explained by the cross-sections of the loadings of security returns on the factors (Roll and Ross (1980), Chen (1983)). Lehmann and Modest (1988) test this approach in detail. Most interesting, using models with up to 15 factors, they test whether the multifactor model explains the size anomaly of the SLB model. They find that the multifactor model leaves an unexplained size effect much like the SLB model; that is, expected returns are too high, relative to the model, for small stocks and too low for large stocks.

The factor analysis approach to tests of the APT leads to unresolvable squabbles about the number of common factors in returns and expected returns (Dhrymes, Friend, and Gultekin (1984), Roll and Ross (1984), Dhrymes, Friend, Gultekin, and Gultekin (1984), Trzcinka (1986), Conway and Reinganum (1988)). The theory, of course, is no help. Shanken (1982) argues that the factor analysis approach to identifying the common factors in returns and expected returns is in any case doomed by fundamental inconsistencies.

I think the factor analysis approach is limited, but for a different reason. It can confirm that there is more than one common factor in returns and expected returns, which is useful. But it leaves one hungry for economic insights about how the factors relate to uncertainties about consumption and portfolio opportunities that are of concern to investors, that is, the hedging arguments for multifactor models of Fama (1970a) and Merton (1973).

Although more studies take the factor analysis approach, the most influential tests of the multifactor model are those of Chen, Roll, and Ross (1986). The alternative approach in Chen, Roll, and Ross is to look for economic variables that are correlated with stock returns and then to test whether the loadings of returns on these economic factors describe the cross-section of expected returns. This approach thus addresses the hunger for factors with an economic motivation, left unsatisfied in the factor analysis approach.

Chen, Roll, and Ross examine a range of business conditions variables that might be related to returns because they are related to shocks to expected future cash flows or discount rates. The most powerful variables are the growth rate of industrial production and the difference between the returns on long-term low-grade corporate bonds and long-term Government bonds. Of lesser significance are the unexpected inflation rate and the difference between the returns on long and short Government bonds. Chen, Roll, and Ross (1986) conclude that their business conditions variables are risk factors in returns, or they proxy for such factors, and the loadings on the variables are priced in the cross-section of expected returns.

Chen, Roll, and Ross confront the multifactor model with the SLB model. They find that including SLB market β's has little effect on the power of their economic factors to explain the cross-section of expected returns, but SLB market β's have no marginal explanatory power. They get similar results in tests of the multifactor model against the consumption-based model (see below). Moreover, Chan, Chen, and Hsieh (1985) argue that the business conditions variables in Chen, Roll, and Ross, especially the difference between low-grade corporate and Government bond returns, explain the size anomaly of the SLB model. These successes of the multifactor model are, however, tempered by Shanken and Weinstein (1990), who find that the power of the economic factors in Chen, Roll, and Ross is sensitive to the assets used in the tests and the way factor loadings are estimated.

The Chen, Roll, and Ross approach (identifying economic factors that are correlated with returns and testing whether the factor loadings explain the cross-section of expected returns) is probably the most fruitful way to use multifactor models to improve our understanding of asset-pricing. As in Ferson and Harvey (1991), the approach can be used to study the links between the common economic factors in the cross-section of returns and the financial (dividend-yield and term-structure) variables that track variation in expected returns through time. Since the approach looks for economic variables that are related to returns and expected returns, it can also be useful in the critical task of modelling the links between expected returns and the real economy (Chen (1991)). In the end, there is some hope with this approach that we can develop a unified story for the behavior of expected returns (cross-section and time-series) and the links between expected returns and the real economy.

There is an important caveat. The flexibility of the Chen, Roll, and Ross approach can be a trap. Since multifactor models offer at best vague predictions about the variables that are important in returns and expected returns, there is the danger that measured relations between returns and economic factors are spurious, the result of special features of a particular sample (factor dredging). Thus the Chen, Roll, and Ross tests, and future extensions, warrant extended robustness checks. For example, although the returns and economic factors used by Chen, Roll, and Ross are available for earlier and later periods, to my knowledge we have no evidence on how the factors perform outside their sample.

C. Consumption-Based Asset-Pricing Models

The consumption-based model of Rubinstein (1976), Lucas (1978), Breeden (1979), and others is the most elegant of the available intertemporal asset-pricing models. In Breeden's version, the interaction between optimal consumption and portfolio decisions leads to a positive linear relation between the expected returns on securities and their consumption β's. (A security's consumption β is the slope in the regression of its return on the growth rate of per capita consumption.) The model thus summarizes all the incentives to hedge shifts in consumption and portfolio opportunities that can appear in Merton's (1973) multifactor model with a one-factor relation between expected returns and consumption β's.

The simple elegance of the consumption model produces a sustained interest in empirical tests. The tests use versions of the model that make strong assumptions about tastes (time-additive utility for consumption and constant relative risk aversion (CRRA)) and often about the joint distribution of consumption growth and returns (multivariate normality). Because the model is then so highly specified, it produces a rich set of testable predictions about the time series and cross-section properties of returns.

The empirical work on the consumption model often jointly tests its time-series and cross-section predictions, using the pathbreaking approach in Hansen and Singleton (1982). Estimation is with Hansen's (1982) generalized method of moments. The test is based on a χ^2 statistic that summarizes, in one number, how the data conform to the model's many restrictions. The tests usually reject. This is not surprising since we know all models are false. The disappointment comes when the rejection is not pursued for additional descriptive information, obscure in the χ^2 test, about which restrictions of the model (time-series, cross-section, or both) are the problem. In short, tests of the consumption model sometimes fail the test of usefulness; they don't enhance our ability to describe the behavior of returns.

This is not a general criticism. Much interesting information comes out of the tests of the consumption model. For example, one result, from the so-called unconditional tests, that focus on the predictions of the model about the cross-section of expected returns, is the equity-premium puzzle (Mehra and Prescott (1985)). It says that the representative consumer, whose tastes characterize asset prices, must have high risk aversion to explain the large spread (about 6% per year) of the expected returns on stocks over low-risk securities like Treasury bills. In healthy scientific fashion, the puzzle leads to attempts to modify assumptions to accomodate a large equity premium. For example, Constantinides (1990) argues that a large premium is consistent with models in which utility depends on past consumption (habit formation).

The habit formation argument has a ring of truth, but I also think that a large equity premium is not necessarily a puzzle; high risk aversion (or low intertemporal elasticity of substitution for consumption) may be a fact. Roughly speaking, a large premium says that consumers are extremely averse to small negative consumption shocks. This is in line with the perception that consumers live in morbid fear of recessions (and economists devote enormous energy to studying them) even though, at least in the post-war period, recessions are associated with small changes in per capita consumption.

Moreover, the equity-premium puzzle is a special feature of unconditional tests that focus on the cross-section properties of expected returns. In these tests, estimates of the risk-aversion parameter are imprecise. Conditional tests, that also include the time-series predictions of the model, lead to reasonable estimates of the risk-aversion parameter of the representative consumer (Hansen and Singleton (1982, 1983)).

The central cross-section prediction of Breeden's (1979) version of the consumption model is that expected returns are a positive linear function of consumption β's. On this score, the model does fairly well. Breeden, Gibbons, and Litzenberger (1989) test for linearity on a set of assets that includes the NYSE value-weighted portfolio, 12 industry stock portfolios, and 4 bond portfolios. They argue that the expected returns on these assets are a positive linear function of their consumption β's. Wheatley (1988a) comes to a similar conclusion.

Wheatley (1988b) also cannot reject the hypothesis that the same linear relation between expected returns and consumption β's (with β's measured from U.S. consumption) holds for an opportunity set that includes portfolios of the common stocks of 17 international markets, as well as U.S. Government bonds, corporate bonds, and common stocks. Wheatley thus cannot reject the hypothesis that securities are priced as if the consumption-based model holds and capital markets are internationally integrated.

The plots in Breeden, Gibbons, and Litzenberger (1989) and Wheatley (1988a,b) suggest, however, that as in Stambaugh's (1982) tests of the SLB model, the evidence for a positive relation between expected returns and consumption β's comes largely from the spread between bonds (low β's and low average returns) and stocks (high β's and high average returns). The existence of a positive tradeoff among the stock portfolios is less evident in their plots, and they give no tests for stocks alone.

Breeden, Gibbons, and Litzenberger (1989) and Wheatley (1988a,b) bring the tests of the consumption model to about where tests of the SLB model were after the studies of Black, Jensen, and Scholes (1972), Blume and Friend (1973), and Fama and MacBeth (1973). In particular, a positive relation between expected returns and consumption β's is observed, but there is no confrontation between the consumption model and competing models.

Mankiw and Shapiro (1986) test the consumption model against the SLB model. They argue that in univariate tests, expected returns on NYSE stocks are positively related to their market β's and perhaps to their consumption β's. When the two β's are included in the same regression, the explanatory power of market β's remains, but consumption β's have no explanatory power. These results are, however, clouded by a survival bias. The sample of stocks used by Mankiw and Shapiro is limited to those continuously listed on the NYSE during the entire 1959–1982 period. Not allowing for delistings gives upward-biased average returns, and the bias is probably more severe for higher β (consumption or market) stocks.

Chen, Roll, and Ross (1986) include consumption β's with the β's for the economic variables used in their tests of multifactor models. Again, consumption β's have no marginal explanatory power. Thus Chen, Roll, and Ross reject the prediction of the consumption model that the explanatory power of other variables in the multifactor model is subsumed by consumption β's.

Finally, so far, the tests of the consumption model make no attempt to deal with the anomalies that have caused problems for the SLB model. It would be interesting to confront consumption β's with variables like size and book-to-market equity, that have caused problems for the market β's of the SLB model. Given that the consumption model does not seem to fare well in tests against the SLB model or the multifactor model, however, my guess is that the consumption model will do no better with the anomalies of the SLB model.

D. Where Do We Stand?

D. 1. The Bad News

Rejections of the SLB model are common. Variables like size, leverage, E/P, and book-to-market equity have explanatory power in tests that include market β's. Indeed, in recent tests, market β's have no explanatory power relative to the anomalies variables (Fama and French (1991)). The SLB model is also rejected in tests against multifactor models (Chen, Roll, and Ross (1986)).

If anything, the consumption-based model fares worse than the SLB model. It is rejected in combined (conditional) tests of its time-series and cross-section predictions (Hansen and Singleton (1982, 1983)). The equity-premium puzzle of Mehra and Prescott (1985) is ubiquitous in (unconditional) cross-section tests. And the model seems to fail miserably (consumption β's have no marginal explanatory power) in tests against the SLB model (Mankiw and Shapiro (1986)) and the multifactor model (Chen, Roll, and Ross (1986)).

The multifactor model seems to do better. It survives tests against the SLB and consumption-based models (Chen, Roll, and Ross (1986)). It helps explain the size anomaly of the SLB model (Chan, Chen, and Hsieh (1985), Chan and Chen (1991)). On the other hand, the evidence in Shanken and Weinstein (1990) that the results in Chen, Roll, and Ross and Chan, Chen, and Hsieh are sensitive to the assets used in the tests and the way the β's of economic factors are estimated is disturbing.

One can also argue that an open competition among the SLB, multifactor, and consumption models is biased in favor of the multifactor model. The expected-return variables of the SLB and consumption models (market and consumption β's) are clearly specified. In contast, the multifactor models are licenses to search the data for variables that, *ex post*, describe the cross-section of average returns. It is perhaps no surprise, then, that these variables do well in competitions on the data used to identify them.

D. 2. The Good News

Fortunately, rejections of the SLB model and the consumption model are never clean. For the SLB model, it is always possible that rejections are due to a bad proxy for the market portfolio and thus poor estimates of market β's.

With bad β's, other variables that are correlated with true β's (like size) can have explanatory power relative to estimated β's when in fact asset pricing is according to the SLB model.

Estimating consumption β's poses even more serious problems. Consumption is measured with error, and consumption flows from durables are difficult to impute. The model calls for instantaneous consumption, but the data are monthly, quarterly, and annual aggregates. Finally, Cornell (1981) argues that the elegance of the consumption model (all incentives to hedge uncertainty about consumption and investment opportunities are summarized in consumption β's) likely means that consumption β's are difficult to estimate because they vary through time.

In this quagmire, it is possible that estimates of market β's are better proxies for consumption β's than estimates of consumption β's, and, as a result, the consumption model is mistakenly rejected in favor of the SLB model. It is even less surprising that the consumption model is rejected in favor of the multifactor model. Since the multifactor model is an expansion of the consumption model (Constantinides (1989)), the estimated β's of the multifactor model may well be better proxies for consumption β's than poorly estimated consumption β's.

These arguments against dismissal of the SLB and consumption models would be uninteresting if the predictions of the models about the cross-section of expected returns are strongly rejected. This is not the case. At least in univariate tests that include both bonds and stocks, expected returns are positively related to market β's and consumption β's, and the relations are approximately linear. Although other predictions of the SLB and consumption models are rejected, the rough validity of their univariate predictions about the cross-section of expected returns, along with their powerful intuitive appeal, keeps them alive and well.

Finally, it is important to emphasize that the SLB model, the consumption model, and the multifactor models are not mutually exclusive. Following Constantinides (1989), one can view the models as different ways to formalize the asset-pricing implications of common general assumptions about tastes (risk aversion) and portfolio opportunities (multivariate normality). Thus, as long as the major predictions of the models about the cross-section of expected returns have some empirical content, and as long as we keep the empirical shortcomings of the models in mind, we have some freedom to lean on one model or another, to suit the purpose at hand.

V. Event Studies

The original event study (of stock splits) by Fama, Fisher, Jensen and Roll (1969) is a good example of serendipity. The paper was suggested by James Lorie. The purpose was to have a piece of work that made extensive use of the newly developed CRSP monthly NYSE file, to illustrate the usefulness of the file, to justify continued funding. We had no clue that event studies would become a research industry. And we can't take much credit for starting the industry. Powerful computers and the CRSP data made it inevitable.

Event studies are now an important part of finance, especially corporate finance. In 1970 there was little evidence on the central issues of corporate finance. Now we are overwhelmed with results, mostly from event studies. Using simple tools, this research documents interesting regularities in the response of stock prices to investment decisions, financing decisions, and changes in corporate control. The results stand up to replication and the empirical regularities, some rather surprising, are the impetus for theoretical work to explain them. In short, on all counts, the event-study literature passes the test of scientific usefulness.

Here I just give a flavor of the results from event studies in corporate finance. The reader who wants a more extensive introduction is well served by the reviews of research on financing decisions by Smith (1986) and corporate-control events by Jensen and Ruback (1983) and Jensen and Warner (1988). Moreover, I mostly ignore the extensive event-study literatures in accounting, industrial organization, and macroeconomics. (See the selective reviews of Ball (1990), Binder (1985), and Santomero (1991).) I dwell a bit more on the implications of the event-study work for market efficiency.

A. *Some of the Main Results*

One interesting finding is that unexpected changes in dividends are on average associated with stock-price changes of the same sign (Charest (1978), Ahrony and Swary (1980), Asquith and Mullins (1983)). The result is a surprise, given that the Miller-Modigliani (1961) theorem, and its refinements (Miller and Scholes (1978)), predict either that dividend policy is irrelevant or that dividends are bad news because (during the periods of the tests) dividends are taxed at a higher rate than capital gains. The evidence on the response of stock prices to dividend changes leads to signalling models (Miller and Rock (1985)) and free-cash-flow stories (Easterbrook (1984), Jensen (1986)) that attempt to explain why dividend increases are good news for stock prices.

Another surprising result is that new issues of common stock are bad news for stock prices (Asquith and Mullins (1986), Masulis and Korwar (1986)), and redemptions, through tenders or open-market purchases, are good news (Dann (1981), Vermaelen (1981)). One might have predicted the opposite, that is, stock issues are good news because they signal that the firm's investment prospects are strong. Again, the evidence is the impetus for theoretical models that explain it in terms of (1) asymmetric information [managers issue stock when it is overvalued (Myers and Majluf (1984))], (2) the information in a stock issue that cash flows are low (Miller and Rock (1985)), or (3) lower agency costs when free cash flows are used to redeem stock (Jensen (1986)).

Like financing decisions, corporate-control transactions have been examined in detail, largely through event studies. One result is that mergers and tender offers on average produce large gains for the stockholders of the target firms (Mandelker (1974), Dodd and Ruback (1977), Bradley (1980), Dodd (1980), Asquith (1983)). Proxy fights (Dodd and Warner (1983)), management buyouts (Kaplan (1989)), and other control events are also wealth-enhancing for target stockholders. The political pressure to restrict the market for corporate control is strong, but my guess is that without the barrage of evidence that control transactions benefit stockholders, the pressure would be overwhelming.

An aside. The research on corporate control is a good example of a more general blurring of the lines between finance and other areas of economics. Many of the corporate-control studies appear in finance journals, but the work goes to the heart of issues in industrial organization, law and economics, and labor economics. The research is widely known and has contributors from all these areas. Likewise, research on time-varying expected returns and asset-pricing models (especially the consumption-based model) is now important in macroeconomics and international economics as well as in finance. At this point, it is not clear who are the locals and who are the invaders, but the cross-breeding between finance and other areas of economics has resulted in a healthy burst of scientific growth.

The cursory review above highlights just a smattering of the rich results produced by event studies in corporate finance. My focus is more on what this literature tells us about market efficiency.

B. Market Efficiency

The CRSP files of daily returns on NYSE, AMEX, and NASDAQ stocks are a major boost for the precision of event studies. When the announcement of an event can be dated to the day, daily data allow precise measurement of the speed of the stock-price response—the central issue for market efficiency. Another powerful advantage of daily data is that they can attenuate or eliminate the joint-hypothesis problem, that market efficiency must be tested jointly with an asset-pricing model.

Thus, when the stock-price response to an event is large and concentrated in a few days, the way one estimates daily expected returns (normal returns) in calculating abnormal returns has little effect on inferences (Brown and Warner (1985)). For example, in mergers and tender offers, the average increase in the stock price of target firms in the 3 days around the announcement is more than 15%. Since the average daily return on stocks is only about 0.04% (10% per year divided by 250 trading days), different ways of measuring daily expected returns have little effect on the inference that target shares have large abnormal returns in the days around merger and tender announcements.

The typical result in event studies on daily data is that, on average, stock prices seem to adjust within a day to event announcements. The result is so common that this work now devotes little space to market efficiency. The fact that quick adjustment is consistent with efficiency is noted, and then the studies move on to other issues. In short, in the only empirical work where the joint hypothesis problem is relatively unimportant, the evidence typically says that, with respect to firm-specific events, the adjustment of stock prices to new information is efficient.

To be fair, and to illustrate that efficiency issues are never entirely resolved, I play the devil's advocate. (Attacks on efficiency belong, of course, in the camp of the devil.) Although prices on average adjust quickly to firm-specific information, a common finding in event studies (including the original Fama-Fisher-Jensen-Roll split study) is that the dispersion of returns (measured across firms, in event time) increases around information events. Is this a rational result of uncertainty about new fundamental values? Or is it irrational but random over and underreaction to information that washes out in average returns? In short, since event studies focus on the average adjustment of prices to information, they don't tell us how much of the residual variance, generated by the deviations from average, is rational.

Moreover, when part of the response of prices to information seems to occur slowly, event studies become subject to the joint-hypothesis problem. For example, the early merger work finds that the stock prices of acquiring firms hardly react to merger announcements, but thereafter they drift slowly down (Asquith (1983)). One possibility is that acquiring firms on average pay too much for target firms, but the market only realizes this slowly; the market is inefficient (Roll (1986)). Another possibility is that the post-announcement drift is due to bias in measured abnormal returns (Franks, Harris, and Titman (1991)). Still another possiblity is that the drift in the stock prices of acquiring firms in the early merger studies is sample-specific. Mitchell and Lehn (1990) find no evidence of post-announcement drift during the 1982–1986 period for a sample of about 400 acquiring firms.

Post-announcement drift in abnormal returns is also a common result in studies of the response of stock prices to earnings announcements (e.g., Ball and Brown (1968)). Predictably, there is a raging debate on the extent to which the drift can be attributed to problems in measuring abnormal returns (Bernard and Thomas (1989), Ball, Kothari, and Watta (1990)).

Bernard and Thomas (1990) identify a more direct challenge to market efficiency in the way stock prices adjust to earnings announcements. They argue that the market does not understand the autocorrelation of quarterly earnings. As a result, part of the 3-day stock-price response to this quarter's earnings announcement is predictable from earnings 1 to 4 quarters back. This result is especially puzzling, given that earnings are studied so closely by analysts and market participants. The key (if there is one) may be in the fact that the delayed stock-price responses are strongest for small firms that have had extreme changes in earnings.

In short, some event studies suggest that stock prices do not respond quickly to specific information. Given the event-study boom of the last 20 years, however, some anomalies, spurious and real, are inevitable. Moreover, it is important to emphasize the main point. Event studies are the cleanest evidence we have on efficiency (the least encumbered by the joint-hypothesis problem). With few exceptions, the evidence is supportive.

VI. Tests for Private Information

The 1970 review points to only two cases of market inefficiency due to the information advantages of individual agents. (1) Neiderhoffer and Osborne (1966) show that NYSE specialists use their monopolistic access to the book of limit orders to generate trading profits, and (2) Scholes (1972) and others show that corporate insiders have access to information not reflected in prices. That specialists and insiders have private information is not surprising. For efficiency buffs, it is comfortable evidence against (in the old terms) strong-form efficiency. Moreover, Jensen's (1968, 1969) early evidence suggests that private information is not common among professional (mutual-fund) investment managers.

What has happened since 1970 that warrants discussion here? (1) The profitability of insider trading is now established in detail. (2) There is evidence that some security analysts (e.g., Value Line) have information not reflected in stock prices. (3) There is also some evidence that professional investment managers have access to private information (Ippolito (1989)), but it is seems to be more than balanced by evidence that they do not (Brinson, Hood, and Beebower (1986), Elton, Gruber, Das, and Hklarka (1991)).

A. Insider Trading

In the 1970's, with the early evidence (Black, Jensen, and Scholes (1972), Fama and MacBeth (1973)) that the SLB model seemed to be a good approximation for expected returns on NYSE stocks, the thinking was that the model should be used routinely in tests of market efficiency, to replace informal models like the market model and the constant expected returns model. Jaffe's (1974) study of insider trading is one of the first in this mold.

Like earlier work, Jaffe finds, not surprisingly, that for insiders the stock market is not efficient; insiders have information that is not reflected in prices. His disturbing finding is that the market does not react quickly to public information about insider trading. Outsiders can profit from the knowledge that there has been heavy insider trading for up to 8 months after information about the trading becomes public—a startling contradiction of market efficiency.

Seyhun (1986) offers an explanation. He confirms that insiders profit from their trades, but he does not confirm Jaffe's finding that outsiders can profit from public information about insider trading. Seyhun argues that Jaffe's outsider profits arise because he uses the SLB model for expected returns. Seyhun shows that insider buying is relatively more important in small firms, whereas insider selling is more important in large firms. From Banz (1981) we know that relative to the SLB model, small stocks tend to have high average returns and large stocks tend to have low average returns. In short, the persistent strong outsider profits observed by Jaffee seem to be a result of the size effect.

There is a general message in Seyhun's results. Highly constrained asset-pricing models like the SLB model are surely false. They have systematic problems explaining the cross-section of expected returns that can look like market inefficiencies. In market-efficiency tests, one should avoid models that put strong restrictions on the cross-section of expected returns, if that is consistent with the purpose at hand. Concretely, one should use formal asset-pricing models when the phenomenon studied concerns the cross-section of expected returns (e.g., tests for size, leverage, and E/P effects). But when the phenomenon is firm-specific (most event studies), one can use firm-specific "models," like the market model or historical average returns, to abstract from normal expected returns without putting unnecessary constraints on the cross-section of expected returns.

B. Security Analysis

The *Value Line Investment Survey* publishes weekly rankings of 1700 common stocks into 5 groups. Group 1 has the best return prospects and group 5 the worst. There is evidence that, adjusted for risk and size, group 1 stocks have higher average returns than group 5 stocks for horizons out to 1 year (Black (1973), Copeland and Mayers (1982), and Huberman and Kandel (1987, 1990)).

Affleck-Graves and Mendenhall (1990) argue, however, that Value Line ranks firms largely on the basis of recent earnings surprises. As a result, the longer-term abnormal returns of the Value Line rankings are just another anomaly in disguise, the post-earnings-announcement drift identified by Ball and Brown (1968), Bernard and Thomas (1989), and others.

Stickel (1985) uses event-study methods to show that there is an announcement effect in rank changes that more clearly implies that Value Line has information not reflected in prices. He finds that the market takes up to 3 days to adjust to the information in changes in rankings, and the price changes are permanent. The strongest price changes, about 2.44% over 3 days, occur when stocks are upgraded from group 2 to group 1 (better to best). For most other ranking changes, the 3-day price changes are less than 1%.

The information in Value Line rank changes is also stronger for small stocks. For the smallest quintile of stocks, a change from group 2 to group 1 is associated with a 3-day return of 5.18%; for the largest quintile, it is 0.7%. Stickel argues that these results are consistent with models in which higher information costs for small stocks deter private information production. As a result, public information announcements (like Value Line rank changes) have larger effects on the prices of small stocks.

The announcement effects of Value Line rank changes are statistically reliable evidence against the hypothesis that information advantages do not exist. But except for small stocks upgraded from group 2 to 1 (or downgraded from 1 to 2), the price effects of rank changes (less than 1% over 3 days) are small. Moreover, Hulbert (1990) reports that the strong long-term performance of Value Line's group 1 stocks is weak after 1983. Over the 6.5 years from 1984 to mid-1990, group 1 stocks earned 16.9% per year compared with 15.2% for the Wilshire 5000 Index. During the same period, Value Line's

Centurion Fund, which specializes in group 1 stocks, earned 12.7% per year —live testimony to the fact that there can be large gaps between simulated profits from private information and what is available in practice.

Finally, Lloyd-Davies and Canes (1978), and Liu, Smith, and Syed (1990) find that the touts of the security analysts surveyed in the *Wall Street Journal's* "Heard on the Street" column result in price changes that average about 1.7% on the announcement day, an information effect similar to that for Value Line rank changes.

The evidence of Stickel (1985), Lloyd-Davies and Canes (1978), and Liu, Smith, and Syed (1990) is that Value Line and some security analysts have private information that, when revealed, results in small but statistically reliable price adjustments. These results are consistent with the "noisy rational expectations" model of competitive equilibrium of Grossman and Stiglitz (1980). In brief, because generating information has costs, informed investors are compensated for the costs they incur to ensure that prices adjust to information. The market is then less than fully efficient (there can be private information not fully reflected in prices), but in a way that is consistent with rational behavior by all investors.

C. Professional Portfolio Management

Jensen's (1968, 1969) early results were bad news for the mutual-fund industry. He finds that for the 1945–1964 period, returns to investors in funds (before load fees, but after management fees, and other expenses) are on average about 1% per year below the market line (from the riskfree rate through the S&P 500 market portfolio) of the Sharpe-Lintner model, and average returns on more than half of his funds are below the line. Only when all published expenses of the funds are added back do the average returns on the funds scatter randomly about the market line. Jensen concludes that mutual-fund managers do not have private information.

Recent studies do not always agree. In tests on 116 mutual funds for the February 1968 to June 1980 period, Henriksson (1984) finds that average returns to fund investors, before load fees but after other expenses, are trivially different (0.02% per month) from the Sharpe-Lintner market line. Chang and Lewellen (1984) get similar results for 1971–1979. This work suggests that on average, fund managers have access to enough private information to cover the expenses and management fees they charge investors.

Ippolito (1989) provides a more extensive analysis of the performance of mutual funds. He examines 143 funds for the 20-year post-Jensen period 1965–1984. He finds that fund returns, before load fees but after other expenses, are on average 0.83% per year above the Sharpe-Lintner market line (from the 1-year Treasury bill rate through the S&P 500 portfolio). He finds no evidence that the deviations of funds from the market line are related to management fees, other fund expenses, or turnover ratios. Ippolito concludes that his results are in the spirit of the "noisy rational expectations" model of Grossman and Stiglitz (1980), in which informed investors (mutual fund managers) are compensated for their information costs.

Ippolito's mutual-fund evidence is not confirmed by performance tests on pension plans and endowment funds. Brinson, Hood, and Beebower (1986) examine the returns on 91 large corporate pension plans for 1974–1983. The individual plans range in size from $100 million in 1974 to over $3 billion in 1983. Individual plans commonly have more than 10 outside managers, and large influential professional managers are likely to be well-represented in the sample. The plans on average earn 1.1% per year less than passive benchmark portfolios of bonds and stocks—a negative performance measure for recent data much like Jensen's early mutual fund results. Beebower and Bergstrom (1977), Munnell (1983), and Ippolito and Turner (1987) also come to negative conclusions about the investment performance of pension plans. Berkowitz, Finney, and Logue (1988) extend the negative evidence to endowment funds.

How can we reconcile the opposite recent results for mutual funds and pension funds? Performance evaluation is known to be sensitive to methodology (Grinblatt and Titman (1989)). Ippolito (1989) uses the Sharpe-Lintner model to estimate normal returns to mutual funds. Brinson, Hood, and Beebower (1986) use passive portfolios meant to match the bond and stock components of their pension funds. We know the Sharpe-Lintner model has systematic problems explaining expected returns (size, leverage, E/P, and book-to-market equity effects) that can affect estimates of abnormal returns.

Elton, Gruber, Das, and Hklarka (1991) test the importance of the SL methodology in Ippolito's results. They find that during Ippolito's 1965–1984 period, his benchmark combinations of Treasury bills with the S&P 500 portfolio produce strong positive estimates of "abnormal" returns for passive portfolios of non-S&P (smaller) stocks—strong confirmation that there is a problem with the Sharpe-Lintner benchmarks (also used by Jensen (1968, 1969), Henriksson (1984), and Chang and Lewellen (1984)).

Elton, Gruber, Das, and Hklarka then use a 3-factor model to evaluate the performance of mutual funds for 1965–1984. The 3 factors are the S&P 500, a portfolio tilted toward non-S&P stocks, and a proxy for the market portfolio of Government and corporate bonds. As in Brinson, Hood, and Beebower (1986), the goal of the Elton-Gruber-Das-Hklarka approach is to allow for the fact that mutual funds hold bonds and stocks that are not in the universe covered by the combinations of Treasury bills and the S&P 500 that Ippolito uses to evaluate performance. In simplest terms, the Elton-Gruber-Das-Hklarka benchmarks are the returns from passive combinations of Treasury bills with S&P stocks, non-S&P stocks, and bonds.

Elton-Gruber-Das-Hklarka find that for Ippolito's 1965–1984 period, their benchmarks produce an abnormal return on mutual funds of -1.1% per year, much like the negative performance measures for pension funds (Brinson, Hood, and Beebower (1986)) and endowments (Berkowitz, Finney, and Logue (1988)). Moreover, unlike Ippolito, but in line with earlier work (Sharpe (1966)), Elton, Gruber, Das, and Hklarka find that abnormal returns on mutual funds are negatively related to fund expenses (including management fees) and turnover. In short, if mutual, pension, and endowment fund managers are the informed investors of the Grossman-Stiglitz (1980) model, they are apparently negating their inframarginal rents by pushing research and trading beyond the point where marginal benefits equal marginal costs.

VII. Conclusions

The past 20 years have been a fruitful period for research on market efficiency and asset-pricing models. I conclude by reviewing briefly what we have learned from the work on efficiency, and where it might go in the future. (Section IV.D above provides a summary of tests of asset-pricing models.)

A. Event Studies

The cleanest evidence on market-efficiency comes from event studies, especially event studies on daily returns. When an information event can be dated precisely and the event has a large effect on prices, the way one abstracts from expected returns to measure abnormal daily returns is a second-order consideration. As a result, event studies can give a clear picture of the speed of adjustment of prices to information.

There is a large event-study literature on issues in corporate finance. The results indicate that on average stock prices adjust quickly to information about investment decisions, dividend changes, changes in capital structure, and corporate-control transactions. This evidence tilts me toward the conclusion that prices adjust efficiently to firm-specific information. More important, the research uncovers empirical regularities, many surprising, that enrich our understanding of investment, financing, and corporate-control events, and give rise to interesting theoretical work.

It would be presumptuous to suggest where event studies should go in the future. This is a mature industry, with skilled workers and time-tested methods. It continues to expand its base in accounting, macroeconomics, and industrial organization, with no sign of a letup in finance.

B. Private Information

There is less new research on whether individual agents have private information that is not in stock prices. We know that corporate insiders have private information that leads to abnormal returns (Jaffe (1974)), but outsiders cannot profit from public information about insider trading (Seyhun (1986)). We know that changes in Value Line's rankings of firms on average lead to permanent changes in stock prices. Except for small stocks, however, the average price changes are small (Stickel (1985)). The stock-price reactions to the private information of the analysts surveyed in the *Wall Street*

Journal's "Heard on the Street" column are likewise statistically reliable but small.

The investors studied in most detail for private information are pension fund and mutual fund managers. Unlike event studies, however, evaluating the access of investment managers to private information involves measuring abnormal returns over long periods. The tests thus run head-on into the joint-hypothesis problem: measured abnormal returns can result from market inefficiency, a bad model of market equilibrium, or problems in the way the model is implemented. It is perhaps no surprise, then, that Ippolito (1989), using the 1-factor benchmarks of the Sharpe-Lintner model, finds that mutual fund managers have private information that generates positive abnormal returns. In contrast, using 2- and 3-portfolio benchmarks that are consistent with multifactor asset-pricing models, Elton, Gruber, Das, and Hklarka (1991) and Brinson, Hood, and Beebower (1986) find that mutual funds and pension funds on average have negative abnormal returns.

The 1-factor Sharpe-Lintner model has many problems explaining the cross-section of expected stock returns (e.g., the size and book-to-market equity anomalies, and, worst of all, the weak relation between average returns and β for stocks). Multifactor models seem to do a better job on expected returns (Chen, Roll, and Ross (1986), Chan and Chen (1991), Fama and French (1991)). These results lean me toward the conclusion that the multifactor performance evaluation methods of Elton, Gruber, Das, and Hklarka (1991) and Brinson, Hood, and Beebower (1986), and their negative conclusions about the access of investment managers to private information, are more reliable than the positive results of Ippolito (1989) and others that are based on the Sharpe-Lintner model. In truth, though, the most defensible conclusion is that, because of the joint-hypothesis problem and the rather weak state of the evidence for different asset-pricing models, strong inferences about market efficiency for performance evaluation tests are not warranted.

Since we are reviewing studies of performance evaluation, it is well to point out here that the efficient-markets literature is a premier case where academic research has affected real-world practice. Before the work on efficiency, the presumption was that private information is plentiful among investment managers. The efficiency research put forth the challenge that private information is rare. One result is the rise of passive investment strategies that simply buy and hold diversified portfolios (e.g., the many S&P 500 funds). Professional managers who follow passive strategies (and charge low fees) were unheard of in 1960; they are now an important part of the investment-management industry.

The market-efficiency literature also produced a demand for performance evaluation. In 1960, investment managers were free to rest on their claims about performance. Now, performance measurement relative to passive benchmarks is the rule, and there are firms that specialize in evaluating professional managers (e.g., SEI, the data source for Brinson, Hood, and Beebower (1986)). The data generated by these firms are a resource for tests for private information that academics have hardly tapped.

C. Return Predictability

There is a resurgence of interesting research on the predictability of stock returns from past returns and other variables. Controversy about market efficiency centers largely on this work.

The new research produces precise evidence on the predictability of daily and weekly returns from past returns, but the results are similar to those in the early work, and somewhat lacking in drama. The suggestive evidence in Fama (1965) that first-order autocorrelations of daily returns on the stocks of large firms are positive (but about 0.03) becomes more precise in the longer samples in French and Roll (1986). They also show that the higher-order autocorrelations of daily returns on individual stocks are reliably negative, but reliably small. The evidence in Fisher (1966) that autocorrelations of short-horizon returns on diversified portfolios are positive, larger than for individual stocks, and larger for portfolios tilted toward small firms is confirmed by the more precise results in Lo and MacKinlay (1988) and Conrad and Kaul (1988). This latter work, however, does not entirely allay Fisher's fear that the higher autocorrelation of portfolio returns is in part the spurious result of nonsynchronous trading.

In contrast to the work on short-horizon returns, the new research on the predictability of long-horizon stock returns from past returns is high on drama but short on precision. The new tests raise the intriguing suggestion that there is strong negative autocorrelation in 2- to 10-year returns due to large, slowly decaying, temporary (stationary) components of prices (Fama and French (1988a), Poterba and Summers (1988)). The suggestion is, however, clouded by low statistical power; the data do not yield many observations on long-horizon returns. More telling, the strong negative autocorrelation in long-horizon returns seems to be due largely to the Great Depression.

The recent evidence on the predictability of returns from other variables seems to give a more reliable picture of the variation through time of expected returns. Returns for short and long horizons are predictable from dividend yields, E/P ratios, and default spreads of low- over high-grade bond yields (Keim and Stambaugh (1986), Campbell and Shiller (1988b), Fama and French (1988b, 1989)). Term spreads (long-term minus short-term interest rates) and the level of short rates also forecast returns out to about a year (Campbell (1987), Fama and French (1989), Chen (1991)). In contrast to the autocorrelation tests on long-horizon returns, the forecast power of D/P, E/P, and the term-structure variables is reliable for periods after the Great Depression.

D/P, E/P, and the default spread track autocorrelated variation in expected returns that becomes a larger fraction of the variance of returns for longer return horizons. These variables typically account for less than 5% of the variance of monthly returns but around 25–30% of the variances of 2- to 5-year returns. In short, the recent work suggests that expected returns take large, slowly decaying swings away from their unconditional means.

Rational variation in expected returns is caused either by shocks to tastes for current versus future consumption or by technology shocks. We may never be able to develop and test a full model that isolates taste and technology shocks and their effects on saving, consumption, investment, and expected returns. We can, however, hope to know more about the links between expected returns and the macro-variables. The task has at least two parts.

1. If the variation in expected returns traces to shocks to tastes or technology, then the variation in expected returns should be common across different securities and markets. We can profit from more work, like that in Keim and Stambaugh (1986), Campbell (1987), and Fama and French (1989), on the common variation in expected returns across bonds and stocks. We can also profit from more work like that in Harvey (1991) on the extent to which the variation in expected returns is common across international markets. Most important, closure demands a coherent story that relates the variation through time in expected returns to models for the cross-section of expected returns. Thus we can profit from more work like that in Ferson and Harvey (1991) on how the variation through time in expected returns is related to the common factors in returns that determine the cross-section of expected returns.
2. The second interesting task is to dig deeper and establish (or show the absence of) links between expected returns and business conditions. If the variation through time in expected returns is rational, driven by shocks to tastes or technology, then the variation in expected returns should be related to variation in consumption, investment, and savings. Fama and French (1989) argue that the variation in expected returns on corporate bonds and common stocks tracked by their dividend yield, default spread, and term spread variables is related to business conditions. Chen (1991) shows more formally that these expected-return variables are related to growth rates of output in ways that are consistent with intertemporal asset-pricing models. Output is an important variable, and Chen's work is a good start, but we can reasonably hope for a more complete story about the relations between variation in expected returns and consumption, investment, and saving.

In the end, I think we can hope for a coherent story that (1) relates the cross-section properties of expected returns to the variation of expected returns through time, and (2) relates the behavior of expected returns to the real economy in a rather detailed way. Or we can hope to convince ourselves that no such story is possible.

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The Role of the Financial Expert

Third Edition

Edited by

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MICHAEL J. WAGNER

PETER B. FRANK

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CHAPTER 19

MATERIALITY AND MAGNITUDE: EVENT STUDIES IN THE COURTROOM

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CONTENTS

19.1 INTRODUCTION AND BACKGROUND. The Supreme Court's *Daubert* ruling[1] has led to increased scrutiny of expert testimony in the courtroom. This scrutiny has generated a need for analyses that, to the extent possible, are testable, supported by published literature, have a "known or potential rate of error," and

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follow procedures derived from objective standards, rather than from an expert's own potentially subjective opinions or beliefs.

Courts can screen an event study of a security's price, typically the measurement of a stock price's movement in response to a specific event or announcement, for admissibility with straightforward application of the *Daubert* factors. Courts have admitted testimony based on correctly done event studies but excluded testimony based on an infirm event study.[2] Although commonly used in securities litigation, their use in other commercial litigation is less common, but increasing.[3]

Here, we argue that a properly conducted event study can help in litigation outside the field of securities law, and that event studies are often applied in a crude or unscientific manner within securities litigation.[4] This chapter discusses how experts can use event studies to measure the impact of two different types of events. First, we look at revelations of securities fraud, where event studies are already common, though often nonrigorous. Second, we examine the measurement of the effect of offending actions on a plaintiff's future profits, an area in which the use of event studies is less common.

We also compare the event study to other methodologies for measuring the importance and size of an outside event on a company, and examine the conditions under which properly conducted event studies provide more objective and accurate measurements of the effects of these events on the company. We describe the event study technique and the two items that stock price changes let us measure, materiality and magnitude, as well as their relevance to the determinations of liability and damages in a litigation context.

(a) Overview of the Event Study Technique. Event studies of the type used in litigation rely on two well-accepted principles: first, the semi-strong version of the Efficient Market Hypothesis, which states that stock prices in an actively traded security reflect all publicly available information and respond quickly to new information;[5] second, the price of an efficiently traded stock is equal to the present value of the discounted future stream of free cash flow.[6] Consequently, the stock price impacts of an event can reveal the effects of the event on future cash flows if the following four conditions are present:

1. The event is a well-defined news item or series of items.
2. The times that the news reaches the market are known.
3. There is no reason to believe that the market anticipated the news.
4. It is possible to isolate the effect of the news from market, industry, and other firm-specific factors simultaneously affecting the firm's stock price.

The procedure for performing an event study has several well-defined steps.

First, one estimates a predicted stock price return, or percentage change, from the day before the news reaches the market to the day the stock price assimilates the news. In doing this estimation, one uses a model that takes into account market and industry effects on stock price returns. One can do this for several dates, not necessarily consecutive.

Next, the analyst subtracts the predicted return from the actual return to compute the so-called *abnormal return*. If the abnormal return is calculated as the sum of individual abnormal returns over a number of periods (usually individual trad-

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ing days), the difference between the actual and predicted returns summed over all these periods is called the *cumulative abnormal return* (or *CAR*).

Typically, the predicted return does not exactly equal the actual return even when no event has occurred. To decide whether the difference between the actual and the predicted return (the CAR) results merely from chance, one tests the CAR for statistical significance, as described in section 19.4 (a).

The final step, if necessary, involves computing the relevant magnitude of the event. To do this, one calculates the change in stock price or capitalized value of the firm implied by the estimated CAR and thus attributable to the event in question.

Because of its wide acceptance, the existence of standards governing its operation, the known rate of error and the ability to test hypotheses, the event study technique provides a good example of scientific evidence. Furthermore, these same factors mean that a court can screen any particular event study under the *Daubert* guidelines to determine its admissibility as the basis for expert testimony.

(b) Materiality. An event study can help measure the materiality of the event under consideration. While all can agree that an event is material if it is important, this begs the question of how to measure importance. We can consider several measures suggested by Mitchell and Netter in their examination of the role of financial economics in litigation. They note three such measures: reasonable investor, probability/magnitude, and market impact.[7] Unfortunately, these imprecise standards require subjective determinations that vary from case to case. For example, how should a trier of fact determine what a reasonable investor would consider material? One could ask a long-time investor to serve as an expert on materiality, and while this does provide useful insight, the results are necessarily subjective and could vary from case to case.[8] Standardization over different cases could come only from a careful reading of the case law and would be followed by disputes about the similarity or difference between the case at bar and cited precedents. Instead, using the tools of financial economics, one can measure materiality as the probability that a stock price movement resulted from chance and not from the news about a particular event.[9] One can quantify materiality with an event study in a manner comparable across cases and events.

(c) Magnitude. Event studies can also measure the size of a stock price movement as the basis for a damages calculation. For example, in cases of securities fraud, experts commonly measure changes in the alleged inflation in a stock price by the movement in that stock price in the wake of a corrective disclosure, after controlling for market, industry, and other company-specific influences.[10] This results from the disclosure's removing the inflation, and an event study measures the change in inflation in the stock at the time of the disclosure. Often, courts find that this is the best estimate of the inflation per share if the defendant had a duty to disclose the same information that the corrective disclosure revealed. As a result, an event study is a common method that serves as the basis for quantifying damages in securities fraud cases.[11]

Consider a different litigation setting where the plaintiff is a firm suing for lost profits due the company.[12] According to economic theory, there are circumstances in which damages can be measured by the change in the stock price caused by the defendant's conduct multiplied by the number of shares outstanding.[13] This is true because stock prices are the market's estimate of the present value of future cash flows.[14] Equivalently, stock prices are the market's estimate of a company's net liquid assets plus the present value of future profits from the operating assets.[15] Consequently, when there is an unexpected change in assets, liabilities, or expected future profits, this will show up as a change in the stock price. To the extent that the defendant's actions giving rise to liability negatively impact the company's financial well-being, the stock price will decline by the market's estimation of the present value of the harm that the company has suffered.

Though not common in litigation, case law supports this proposition. For example, consider a situation where a business files a suit claiming that another party's illegal actions have damaged it and reduced the company's value or worth. Courts have supported the use of market value to determine the value of a company.[16] Thus, it naturally follows that the portion of the change in the enterprise's market value that can be attributed to the defendant, as measured by a careful event study, is a proper measure of the change in corporate value, that is, damages, in this case.

19.2 PERFORMING THE BASIC EVENT STUDY

(a) Identifying the Event. Many texts discuss how to perform an event study.[17] While there are some differences in exposition, authors agree on the necessary steps and general procedures. First, one must identify the event or events to be studied. In securities fraud cases, the events of interest usually include all the alleged disclosures of fraud, the dates of the fraudulent statements, or both. When one is measuring lost profits, the relevant dates would be those dates on which the public received information about the alleged wrongful act.

(b) The Event Window. Next, we establish the event windows. *Event windows* are the periods over which stock price movements are calculated. Generally, these windows begin immediately before an announcement and conclude shortly thereafter. When it is unlikely that the news of an announcement was leaked beforehand, one typically would start the event window at the end of the trading day before the announcement was made. When there is a reasonable possibility that the information reached the market before a formal announcement, the event window may be extended back to include the potential leakage.[18] The end of the event window is somewhat more arbitrary. In securities fraud cases, many experts have adopted the convention of looking at one-day, two-day, or five-day periods following an announcement. The most recent academic pronouncement expresses support for the shorter one-day or two-day window, though it recognizes that in practice, analysts often use longer windows.[19] Occasionally, there is another news announcement, or confounding event, within the event window. When this occurs, the event window is often cut short so that it does not include the effects of the confounding event.

The longer the event window, the more likely it incorporates all of the prior leakage and the market's ongoing adjustment to the news, but also the more likely it picks up other effects unrelated to the event under consideration. Deciding on the length of the event window is thus one of the most important considerations in performing an event study.[20]

(c) Controlling for Market and Industry Effects. Once the event windows have been established, the analyst next calculates the relations between the company's stock and an index or indices that proxy for outside forces such as market and industry effects. These relations will later be used to remove those market and industry effects from the price movement observed in the event window.[21] One finds these relations by running a regression of the company's stock price on a market or industry index, or both, over a period of time labeled an estimation window.[22] Here, the analyst must make two additional decisions. First, over what period should the regression be run? This period is called the *estimation window.* And, second, which market or industry indices should be used to control for outside influences on the company's stock price?

In regard to the first question, one would typically like to use an estimation window close to the event because the relation between the company's stock and an index changes over time. Therefore, the closer the estimation window is to the event, the more relevant the estimated relation will be. Three general choices for the placement of an estimation window are before the event window, surrounding the event window, and after the event window. The most common choice places the estimation window before the event.[23] Analysts sometimes place the estimation window after the event window or split the estimation window to cover periods before and after the event window. When the analysis studies multiple events, the estimation window may cover the periods around the event windows, including the period(s) between event windows. The estimation window is often placed at one of these locations rather than before the event window because of a lack of relevant prior trading history (for example, because the event window comes shortly after an IPO or change in regulatory environment).[24]

In addition to determining the placement of the estimation window, the analyst must also determine the length of that window. Again a tradeoff applies: the longer the estimation window is, the more data there will be, implying a more accurate regression. On the other hand, the farther the estimation window stretches from the event window, the less the estimated relation between the stock price and the market index is likely to represent the underlying relation during the event window.

A second decision the analyst must make is which market and/or industry indices to use to control for outside influences on the company's stock price. When deciding which indices to use, the analyst should consider both the source of the index and the relation between movements of the company's stock price and the index during the estimation window.

A good index can be

- a standard index (say one developed by Standard & Poor's), or
- one that was constructed based on comparable companies listed in analyst reports or public filings, or
- one based on selecting all companies that meet certain objective criteria (e.g., market capitalization within 10 percent of the pre-event market capitalization of the company being studied).

On the other hand, an index is suspect when the expert chooses the companies in the index without objective criteria.

The second consideration in selecting an index relates to how the company's stock price movements relate to those of the index during the estimation window.

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When the estimation regression is run, one of the statistics generated is the adjusted R-squared.[25] This statistic measures the strength of the fraction of the variability of the variable being explained by the combined set of independent variables (the market and industry indices) used to do the explaining. The higher this statistic, the larger the portion of the variability explained. Another relevant statistic is the t-statistic associated with each independent variable; this statistic measures the strength of the individual independent variable's correlation with the company's stock price. The farther the t-statistic is from zero, the stronger the relation. While one should not use either the adjusted R-squared or t-statistics as a blind measure for the comparison of the explanatory power of different indices, an expert should be prepared to provide these statistics.[26] Moreover, if the expert chooses one index with less statistical explanatory power than a second index, he or she should be prepared to defend this choice.[27]

(d) Estimating the Effects of the Event. The choice of the event window and indices used to predict the stock price over the estimation window provide the basic ingredients for the analytical steps of the event study. The estimated relations from the regression during the estimation window are applied to control for market and industry movements in the event window. The predicted return is then compared to the actual return in the event window, with the difference representing the abnormal or excess return. This return, multiplied by the company stock price, provides an estimate of the per-share dollar effect of the event being studied.[28]

Finally, note that the abnormal return would include the effects of the event being studied as well as any other company-specific news or events (if any) that occur in the event window. Whenever possible, the analysis should disentangle the effects of these events. The procedure for doing so depends on the available data and the nature of the other event(s) in the window. For example, if the event coincided with an earnings announcement, the effects of the latter could be removed by estimating the stock price's response to earnings surprises and applying the measured relation to the announcement within the event window. Though this would not perfectly remove the effects of the earnings announcement, the remaining abnormal return would be a much better estimate of the effect of the event for which the window was constructed. After removing the effects of these other events, materiality tests have to be adjusted to account for both the magnitudes of these events and the uncertainty surrounding the estimates of those magnitudes.[29]

19.3 ARE EVENT STUDIES REALLY SCIENCE UNDER *DAUBERT*?

(a) The *Daubert* Decision. In 1993, the Supreme Court reviewed a standard on the admissibility of expert testimony stated in *Frye v. United States* (1923). *Frye* set a standard that an expert's methodology must be "generally accepted" in the scientific community to be admissible. In its 1993 ruling in *Daubert v. Merrell Dow Pharmaceuticals,* the Supreme Court expanded the admissibility standard set forth in *Frye* to allow potentially new though reliable techniques that had not yet achieved peer review. In its 1999 ruling in *Kumho Tire Co. v. Carmichael,* the Supreme Court clarified that the *Daubert* criteria apply to "all expert testimony."

The plaintiffs in the *Daubert* case, a products-liability action, sought to establish a causal link between the ingestion of a prescription drug during pregnancy and

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the subsequent delivery of children with birth defects. Consequently, the Court's decision focused on scientific rather than economic testimony.

The Court found that the basic rule is that "all relevant evidence is admissible."[30] According to the Court, relevant evidence must "assist the trier of fact to understand the evidence or determine a fact in issue."[31]

The Supreme Court also limited the *Daubert* analysis to "scientific" knowledge, which it defined as based on the scientific method. The evidence must meet the same standards of all evidence by being "not only relevant, but reliable." In the scientific context, reliable evidence is "based upon scientific validity."[32] A federal district court in the Sixth Circuit has expanded the *Daubert* framework beyond scientific testimony, to include technical and other specialized knowledge.[33]

The *Daubert* Court admonished against proffering testimony that was based on "unsupported assertion" or "subjective belief" and provided guidance by noting four factors that should be considered to assist in this inquiry:

1. whether the theory or technique can be (and has been) tested;
2. whether the technique or theory has been subjected to peer review and publication;
3. the known or potential rate of error and standards controlling the technique's operations; and
4. whether the theory or technique has been generally accepted by the scientific community.[34]

After the Supreme Court sent the case back to the Court of Appeals for the Ninth Circuit, that court suggested a fifth factor: whether the methodology was created solely for purposes of litigation.[35] The Sixth Circuit apparently also considers this factor important.[36]

(b) Are Event Studies Objective? We now take a moment to consider the effects of the choices discussed in the previous section to see if they would withstand being called "subjective belief" or "unsupported assertion" under *Daubert*. If the calculation is to take into consideration the specifics of a particular case, some common sense must be added to the science because several considerations determine the proper methodology for running an event study.[37]

The foregoing does not imply that one cannot use a standard procedure. In fact, it is often useful to do so when one is running a number of event studies for different firms (for example, if one wanted to look at how the average firm responded to a certain type of announcement). In that situation, it is common to establish a standard price reaction methodology both for ease of analysis and to prevent the possibility that choosing different methodologies for different events biased, or even determined, the overall result. When combining multiple event studies to determine how stock prices respond to events, it is not necessary to find a procedure that provides the best estimate for each firm or event individually; instead, with a large number of events being combined, errors in one event will, at least to some degree, cancel those in another.

When looking at the particular firm and events at issue in a lawsuit, however, it may be preferable to tailor the event study to the special circumstances at hand. As discussed above, in performing an event study there are at least three choices that

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feature prominently in the analyst's mind: (1) the time frame for the measurement of the price reaction; (2) the time frame for estimating the relation between the stock and the market, both in terms of length of period and when the period should be relative to the event under consideration; and (3) which index or indices should be used to control for market and/or industry effects.

The effects of the different choices will naturally vary from case to case. Generally, though, the choice of index and estimation window will likely have a relatively minor impact.[38] On the other hand, the choice of the time frame for measuring the price reaction (the event window) would be expected to lead to greater variability.

Even so, once the analyst makes these choices, the analysis is completely objective, in that another expert would be able to replicate it. That is, if one specified an index, an estimation window, and an event window, any two experts would come up with the same measurement for the price reaction.[39] This means that if one expert presents a result based on an event study, an opposing expert can check for errors in the underlying calculations. The opposing expert can also test what would happen if the first expert changed any of the assumptions. This allows the opposing expert to discover which assumptions, if any, are innocuous because changing them has no significant effect on the results. (For example, adjusting for market movements with the S&P 500 and with the Nasdaq Composite Index is likely to produce similar results if the indices moved similarly in the event window.) Conversely, the opposing expert can identify which assumptions, if any, drive the first expert's result and focus the debate on those points. (For example, if the price reaction is large after two days but the price returns to its original level after five days, the debate can focus on how long it took for the market to absorb all of the effects of the relevant event.)

In most cases, objective measures can aid in evaluating the choices. For example, as discussed above, comparing the adjusted R-squared from one estimating regression to the next provides information that can help in deciding which better explains the stock price's movements.[40] In deciding the proper price reaction period, one could go with accepted standards in the literature or in litigation. Alternatively, one could use a proxy for materiality, such as trading volume or number of news stories relating to the event, in deciding over what period of time the stock was still responding to an event.

(c) Applying the *Daubert* Factors. The event study technique satisfies the four factors to be used in determining the admissibility of expert testimony as described by the Court.

1. Can the theory or technique be tested? The choices available to the analyst involving the index and estimation window are testable using statistics that are computed when the analyst performs a regression analysis of the firm's stock price return on the return of an index. The choice of event window may be tested using approaches mentioned above, although it may instead be based on convention (that is, the analyst can use a one- or two-day window supported by the literature rather than doing a separate analysis for each event study to determine *de novo* the length of the window most appropriate for the individual event under consideration).

2. Has the technique or theory been subjected to peer review and publication? By now, hundreds of peer-reviewed articles have applied the event study methodology, including many that focus specifically on methodological considerations.
3. Does the technique have a known or potential rate of error and standards for controlling its operations? The error associated with either a test of materiality or the measurement of the size of the event is a statistic that can be estimated with each application; moreover, the academic literature provides guidance on proper application of the technique.
4. Has the scientific community generally accepted the theory or technique? The scale of publications alone shows that the technique has gained general acceptance.

19.4 DECIDING ON MATERIALITY

(a) Standard for Materiality. In determining materiality, statistical analysis can provide information on the likelihood that the price movement was due solely to chance. Formally, a materiality test provides a statistical answer to the question: How likely is it that the observed stock price movement in the event window could have occurred if there were no event influencing stock prices in that window? For example, if an event is material at the 5 percent level, this means that there is only a 5 percent likelihood that the abnormal return (or the stock price movement once one controls for market, industry, and other effects) could have been caused by the stock's normal random price fluctuations. Alternatively, we can say that we are 95 percent confident that the abnormal return is greater than what would be expected based on the stock's normal random price fluctuations.

It is not clear what level of statistical significance corresponds to a legal definition of materiality. As Mitchell and Netter point out, the 95 percent confidence level is commonly used, while the 90 percent and 99 percent levels are also options. There is no definitive case law on how statistical confidence levels relate to burden of proof in civil (or criminal) litigation. With an event study, however, courts can quantify the level of materiality, compare it across cases, and assess it using professional standards from the economics literature.

(b) Other Price Reaction Methodologies

(i) Simple Price Reaction. Occasionally, expert reports will contain a conclusion on materiality based on the observation that the stock price reacted to an event and, relying solely on the expert's background and judgment, this price reaction was material. Absent more, such an opinion would fail the Court's admonishment to avoid "unsupported assertion" and "subjective belief." Besides obviously not satisfying the factors laid down for scientific evidence (including testability, known rate of error, standards for operation of the technique, and general acceptability), the approach fails to consider the other potential influences on the stock price over the time period the price was observed to be falling. This alone would make the proffered opinion run afoul of *Executive Telecard.*

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(ii) Net-of-Market Price Reaction. A somewhat more sophisticated, and common, methodology designed to take account of other influences on price reactions is to simply measure so-called net-of-market movements. This is done, for example, by first computing the average price of a security over the five days before an event and the average price over the five days after the event. The percentage change in these two averages is then compared to the percentage change of a market or industry index over the same period of time.

This methodology has at least two flaws. First, this form of price reaction implicitly assumes that the stock moves one-for-one with the market or the chosen industry index. One can, and should, test this assumption with, say, a regression analysis to measure the relation between the security and the index. The regression would supply a beta or a coefficient showing how much the stock moved with the index, and a *t*-statistic on that coefficient, showing the statistical significance of that relation. If the beta is statistically different from one, it would be difficult to see why one would throw out the empirical results in favor of the generic alternative.[41] A second problem with the net-of-market methodology is that it does not allow for the most accurate determination of materiality. Because the assumed one-for-one relation between the stock and the market or industry index is generally not statistically the best estimate, the estimated abnormal returns with this approach may also not be a best estimate statistically, and a materiality test is not as powerful as it otherwise could be.[42]

When used by itself, the net-of-market adjustment would appear to fail some of the *Daubert* criteria. First, a simple net-of-market calculation has no known rate of error (in part because the analyst never computes the standard deviation of the average five-day return). Second, its support in the academic literature is generally limited to studies that focus on many firms, where running multiple market model regressions may be cumbersome. Finally, because a net-of-market model provides no test for the goodness-of-fit of the market or industry index (i.e., an adjusted *R*-squared or any other residual analysis is never computed), the choice of index is more subjective than when such a statistic is used to evaluate the appropriateness of different indices. None of the above is meant to say that net-of-market models are useless or necessarily wrong; rather, they are dominated by regression analysis and should not be used unless other choices add error or are infeasible. For example, if a company goes public and then stops trading all within an extremely short time frame, the lack of trading data may make the regression results unreliable.

(c) Changing Levels of Materiality. A final issue pertaining to materiality arises when the cumulative price reaction moves in and out of materiality as time passes. For example, if a stock drops by a large amount on the day of an announcement, the one-day reaction may be significant. However, a rebound on the next day may cause the two-day price reaction to be not material, while another drop on the third day may cause the three-day price reaction to regain its status as a material event. In general, one needs to look at why the level of materiality changes over the price reaction window. If new information comes into the market that is not relevant to the instant case, then the analysis should remove the effects of this new information in considering the materiality of the event under examination. In addition, one would want to see whether the changes in materiality result from the market's reevaluating the importance of the initial event or information, something that one can often deduce from contemporaneous news stories or analyst reports.

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As a general matter, the potential for stock market overreaction is now generally accepted as a factor in stock market behavior.[43] Though there remains some dispute over long-term overreaction, short-term price reversal after unconditional price declines has been detected in large samples of stock prices. This means that if the price initially declines after an event and if, say on the second day, the price returns to a level that makes the event not material with no intervening news event, then there is justification for assuming short-term overreaction. The analyst would be hard pressed to make a finding that the event was material under this fact pattern.

19.5 MEASURING LOST PROFITS. This section examines how an event study compares to other methodologies for measuring lost profits.[44]

(a) Lost Profits Calculations Based on Projections. Experts often calculate lost profits by measuring changes in future profit estimates using data from before and after some specific action by the defendant that harmed the plaintiff. For example, suppose that before an allegedly tortious event occurred, analysts projected that Prospects, Ltd. would have profits of $5 million in each of the next five years. Further suppose that following the event, analysts projected that Prospects' profits would be $1 million for the next three years and $2 million for the following two years. Prospects has therefore been harmed, in the analysts' view, by the present value of $4 million for each of the next three years plus $3 million for each of the two years after that. Prospects' harm would also include effects that would be measured by the changes in projections that could have been made for periods more than five years in the future.

Note that this form of measurement does not depend on the actual realization of profits. Instead, it concerns changes in the expectation of future profits at the time of an event. In that sense, it is quite similar to an event study, in which stock prices before and after the event are the market's projections of future profits or cash flows.

Thus, the principal question that arises here is which set of projections to use, those assumed by the market in setting stock prices, or some other set of projections from a different source. In deciding this issue, one criterion is the degree of objectivity in the two measures. The event study is based primarily on market conditions, or on values set by investors only concerned with obtaining the proper value for their purchases and sales, and not by parties interested in the outcome of the litigation. Investors have incentives to set the price correctly because they invest their own money. If the market believes that a stock is underpriced relative to the company's value, investors will place orders to buy the stock, driving its price up; similarly, if the market consensus is that a company's stock is overpriced, sell orders will drive the stock price down.

In an examination of expected lost profits based on the change in analyst or expert projections surrounding the allegedly tortious act, the results will naturally depend on the projections used. Often, there is a large range of projections for profits from the company and analysts for the short term. If one goes out more than a few years, there are often no projections or only internal company projections. And at some point, there are generally not even company projections. Thus, for events likely to have a long-term impact on profits, the expert must create projections in the litigation. Even if the expert attempts to be completely objective, this often involves a large degree of subjectivity.[45] Moreover, even when projections are available, the expert must decide which one or ones to use.

If done carefully, the use of analyst projections to calculate lost profits is likely to satisfy the *Daubert* criteria, though perhaps not as well as an event study would. In an event study, one uses stock prices, figures that are accepted by all to be what they represent: the market's current valuation of a company's equity. In looking at projections, the question arises as to which set of projections to use. If one uses projections from the same disinterested analysts both before and after the tortious event, there is likely to be little objection about subjectivity (provided, of course, that one does not select only those analysts who viewed the event as especially large or small). If the expert uses projections from one of the interested parties, such as the plaintiff company, or makes his or her own estimates of what projections should have been before and after the tortious event, then subjectivity becomes a serious concern.

(b) Lost Profits Calculations Based on Future Events. Lost profits are also often calculated by comparing actual results to projections made at or immediately before the alleged business interference. This section compares the calculation of lost profits using this methodology to the calculation of lost profits through the use of an event study.

Let us return to the example of Prospects, Ltd., which was expected to have $5 million in profits in each of the five years immediately following the tortious event. Suppose that its actual profits were $1 million in the first year and $3 million in each of the next four years. Prospects' damages claim would then include $4 million in lost profits from the first year and $2 million in lost profits from each of the subsequent years. These values would then be expressed in present value terms, or adjusted for prejudgment interest. In addition, the company would still have a claim for any lost profits occurring more than five years from the time of the wrong.

The most important difference between the methodologies is that an event study (or a comparison of changes in projections as discussed in the previous section) is an ex ante analysis, while an examination of actual results is an ex post analysis.

19.6 RECENT LITERATURE AND CASE LAW. The question of whether experts can use the stock (and debt) market value of a firm to value the underlying asset has been answered affirmatively by both appraisers and the courts. In the legal context, the so-called stock and debt approach to valuation has been advocated primarily for railroad and utility properties, but applies to firms in other industries.[46] Indeed, a textbook on corporate valuation devotes an entire chapter to the approach without limitation to type of firm or industry.[47]

Adherents to the approach make the claim that "[w]here data to make possible a stock and debt valuation are available, it is best to go no further."[48] With regard to the objectivity of the approach, "[t]he stock and debt method avoids overreliance on the judgment or expectations of a single individual (the appraiser) about the future prospects of the firm, substituting instead the consensus view of many market participants—all of whom, as we have said, have a strong interest in making accurate forecasts."[49]

The stock and debt approach to appraisal has been accepted by both regulatory bodies and courts.[50] A circuit court decision, *Mills v. Electric Auto-Lite Co.*, used language that virtually mirrored the professional literature.[51] The court held that to determine the worth of a company "when market value is available and reliable, other factors should not be utilized. . . . Although criteria such as earnings and book value are an indication of actual worth, they are only secondary indicia. In a market economy, market value will always be the primary gauge of an enterprise's worth."

Furthermore, another court recognized the distinction between the projections made by analysts and those implicitly made by the market: "self-interest concentrates the mind, and people who must back their beliefs with their purses are more likely to assess the value of the judgment accurately than are people who simply seek to make an argument. Astute investors survive in competition; those who do not understand the value of assets are pushed aside. There is no similar process of natural selection among expert witnesses and bankruptcy judges."[52]

It follows from these citations that the change in capitalization of the company accurately measures a change in the worth of a company. Such change in worth, of course, can come from the present discounted value of the future stream of cash flows lost by the actions of a defendant. As such, the appraisal and valuation methods that support determining the value of a company by using the market value of stock and debt would also support determining the value of a company before and after the wrongful act of a defendant. The event study method measures this change in valuation.

19.7 DO EVENT STUDIES ACCURATELY MEASURE LOSS TO THE CORPORATION?

We next ask whether the event study is a reliable measure of damages. This includes a discussion of the issue of whether the technique really measures the loss to the corporation instead of, for example, the loss to shareholders.

(a) Stock Market Anomalies. A violation of the efficient market hypothesis means that stock prices may not reflect fundamental values at every moment, which, in turn, means that the prices do not always equate to the present discounted value of future dividends. Over the years following the stock market crash of 1987, there developed an academic literature that found a variety of anomalies in stock price behavior and that, when taken as a whole, has probably led economists to have less faith in the efficient market hypothesis than they had in the 1970s.[53]

We do not wish to overturn the presumption accorded the efficient market hypothesis in *Basic v. Levinson*.[54] Rather, we should view the efficient market hypothesis as a presumption that can be disproved for a particular security in a particular time frame. The same literature that has focused on stock market anomalies has also provided analysts with the tools to diagnose the patterns of a stock price to determine whether its behavior is anomalous.

Although it is not the purpose of this chapter to review either the stock market efficiency literature or the adjustments to the event study technique that might be called for if an anomaly exists, we mention a few issues that might arise that could affect the event analysis.[55]

(i) Volatility. Financial economists studied stock market volatility well before the late 1990s.[56] Their principal finding was that the volatility of stock prices likely exceeds

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that justified by the variance of dividends. This means that there is no guarantee that stock prices will reflect fundamental value. This being said, these findings by themselves did not lead to professional rejection of the efficient market hypothesis—it was still treated as a presumption for any individual stock or stock market index.

By itself, volatility does not mean that the event study technique is worse than other methods of valuation. The reason for this is that it can be shown that virtually any asset returning a cash flow is likely to be volatile. For example, Paul Samuelson has shown that stock prices following what appears to be a random walk could be based on fundamental values and, in a later article, that the price of land could be a stochastic process much as stock prices appear to be.[57] In both cases, the source of the variation in prices is similar: for stock prices, dividends are a stochastic process because earnings themselves contain a stochastic component; for land prices, rents may also contain a stochastic component. This means, of course, that lost profits (damages) to the underlying asset will themselves be volatile. We should not be surprised, then, that event studies (measuring, as they do, the present discounted value of the lost profits) have some statistical error associated with them though they are generally unbiased. The test statistics typically computed when performing an event study help in assessing the dimensions of this error.

(ii) Speculative Bubbles. In light of the behavior of both the market and individual stocks since 1987, there has grown theoretical literature to show how speculative bubbles can form.[58] These theories show that in speculative markets where there are both informed and uninformed traders, it may be rational for the informed traders to follow the uninformed in a price trend away from fundamental value. If the theory is true, there is no mechanism that, in the short term, causes stock prices to equal the value of their underlying assets. Such an overpriced stock has the unfortunate tendency to crash when the bubble bursts. The bursting of the bubble can occur at the same time as, indeed be precipitated by, the event being analyzed to compute damages. Consequently, the price drop unadjusted for the speculative bubble likely mismeasures damages. The effect of this condition has been noted in the legal literature with reference to shareholder class actions.[59] Fortunately, there are diagnostics that can be used to ascertain whether there appears to be a speculative bubble and, if so, whether other techniques are available to measure the lost profits.[60]

(b) Bias from Litigation Expectations. Event studies are biased toward finding a price drop that is too small because of the market's expectation of a possible recovery through the legal system. To see this, suppose a company lost $1 million in future profits and was expected to sue and recover the million dollars and appropriate interest, but at the expense of $300,000 of legal fees. Then the price reaction observed in the market would reflect only the $300,000 net loss. If this were successfully used as the basis for a damages calculation at trial, the company would receive only the $300,000 of legal fees but not compensation for its actual loss of $1 million in future profits. At the extreme, if the market expected the company to recover lost profits plus punitive damages, or treble damages, its stock price could go up as a result of the malfeasance. Consequently, interpreting the event study re-

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sults requires care. Still, because this bias serves to make price reactions show a smaller drop than that due to the defendant's act alone, an event study can still serve to show the minimum damages caused by that act.

(c) Whose Loss Does an Event Study Measure? One of the potential objections one may make to an event study is that it is measuring the wrong damages. Because an event study looks at the value of a corporation's shares, some may argue that one is measuring the loss suffered by shareholders and not by the corporation itself. This leads to the question of whether the event study is measuring the proper damages for use in corporate litigation.

The first answer is that this is a fair criticism of any measure of damages to a corporation. Suppose that Prospects' factory burns down in an apparently accidental fire on February 1, and that Prospects has no insurance to cover the loss. Further suppose that on March 1 it is suddenly revealed that the fire was not an accident but instead was set by agents of a competitor, Ruthless Corp. Last, suppose that on March 1, Prospects sues Ruthless and that everyone believes that Prospects will recover the cost of rebuilding the factory plus any lost profits, however measured, as a result of the arson. We now ask: Who wins and who loses?

In theory, if the damages payment is truly comprehensive, covering all manner of costs and legal fees, loss of competitive position, and so forth, and assuming no punitive damages are awarded, Prospects will be left exactly as well off as if there had been no fire. On March 1, its stock price would therefore recover to where it was on February 1, once one adjusts for market and other forces in the interim. Shareholders on February 1 who held through March 1 have seen a temporary drop in the value of their holdings but are unaffected at the end of the day. February 1 shareholders who sold before March 1 are worse off, because they sold their shares at a time when the price was unduly low. Conversely, investors who purchased between February 1 and March 1 benefit when their shares of Prospects appreciate in value on March 1. But note that this is true no matter how the damages to Prospects are measured, whether it is by the change in its share price or a discounted cash flow model of lost profits. Simply put, under the current legal system, investors who hold shares at the time of a bad act are damaged, while those who hold at the time of an unexpected recovery are benefited.[61] As such, because the change in stock price is simply a *measure* of the damages to Prospects, in the same way that a discounted valuation of lost profits is such a measure, concerns about winners and losers are not specific to the event study methodology.

(d) Do Event Studies Capture All Components of a Loss? Another argument against event studies is that by focusing on a small period of time, an event study does not provide a complete characterization of the effects of a wrongful act. To answer this, let's create an example where the case at bar involves some defamatory statements made by Ruthless against Prospects. Also, suppose that Prospects' stock price falls at the time that the statements are made. One could then ask whether changes in the public's views of the credibility of those statements shouldn't change the damages estimate from the libel. This possibility can be addressed in an event study by looking for changes in the perception of the libelous statements and measuring the effects that those changes in perceptions had on the stock price. For example, if there were a public retraction by Ruthless, one would want to offset the drop in

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Prospects' stock price in the event window corresponding to the original libel by the rebound, if any, in an event window corresponding to the retraction.

Again, however, this criticism does not apply solely to event studies. Suppose that an expert was measuring damages by looking at the decline in expected future income. Suppose further that the original libel caused a permanent 30 percent drop in Prospects' sales, perhaps because consumers were misled into believing that Prospects was marketing an unsafe product. If the retraction caused sales to rebound to within 10 percent of their previous level, this information would also have to be incorporated into measures of discounted lost cash flows.[62] Therefore, to the extent that new information affects continuing results, *any* measure of lost profits that does not purport to measure expected lost profits solely at the time of the original bad act must take this new information into account.

(e) Tax Effects. The discounted cash flows measured by stock prices reflect free cash flow available to stockholders that, of course, are after tax. This creates the need for an adjustment to the event study measure of damages. Because damages awards are usually taxable, the convention has arisen that lost profit damages are awarded on a pretax basis. Fortunately, the adjustment to the event study magnitude to remove tax effects is rather simple; in most instances, it can be accomplished by dividing the event study result by one minus the marginal income tax rate of the corporation.

19.8 CONCLUSION.

We have seen that event studies can be useful in quantifying damages in cases ranging from securities fraud to other commercial litigation requiring the calculation of lost profits. In some areas, such as securities fraud, stock price reactions are already a standard method for quantifying damages. In such cases, the overarching question is how to perform the most accurate price reaction. This entails developing a model that accounts for market and industry effects. It also entails explicitly testing for the materiality of stock price movements. When this is done, we have a damages calculation that is based on economic literature and that, given the results of the materiality test, has a known rate of error. In this manner, one can perform a damages calculation that meets the *Daubert* criteria for admission as expert testimony. A failure to perform these analyses when possible would mean that the analysis is not in accordance with the literature and has an unknown rate of error.[63]

In comparison to many other methods of calculating lost profits, the measurement of stock price reactions has the benefit of being based on numbers that, being determined by the collective decisions of all investors in the market, are both objective and present a consensus, rather than an idiosyncratic, viewpoint. While the measurement of stock price reactions will inevitably incorporate some degree of choice on the part of the analyst, the degree of subjectivity in these choices is usually low.[64] This contrasts with the situation where an analyst has to choose some set of projections and then decide how to discount those projections back in time, to say nothing of the subjectivity involved in making new projections for the purposes of litigation.

When using stock price movements to measure lost profits, one employs a methodology that is supported by the academic literature, is completely replicable, has a measurable rate of error, and uses a minimal number of variables. By con-

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trast, an analyst creating projections of future profits is engaging in a process that may not be replicable by others; while other experts can create their own projections, there is often no reason to believe that they would match those of the original analyst. When several independent sources of profits are available, a study of lost profits using projections requires deciding which projection(s) to use and how to discount the cash flows envisioned in those projections.

None of the foregoing discussion is meant to say that other analyses are not useful, or even necessary at times. When a company is not publicly traded, there would be no stock price data that one can use for an event study, and other methodologies often have to be employed.[65] In addition, other assumptions underlying the appropriateness of the technique, such as the efficient market hypothesis, may not be valid in any individual application requiring either adjustments to the results or abandonment of the method altogether.

Moreover, an event study and another methodology such as a discounted cash flow analysis can be used in conjunction as a test of the robustness of the damages calculation. If the two yield similar results, one can feel more confident in the final figure. If the results differ materially, then the expert should look for errors in both studies by considering the reliability of the data underlying each, the uncertainty surrounding any assumptions made in each analysis, and sources of error such as those discussed in this chapter. If both methodologies still seem reasonable, the expert can use the two results to establish a likely range for alleged damages.

An event study provides an objective methodology for calculating the magnitude of damages and the materiality of the event that may have caused damages. In general, other methodologies for calculating damages do not provide a measure of materiality, other than the simple observation that calculated damages are large, small, or zero. By using the statistical tools that are the basis for event studies, an expert can provide not only a measure of damages based on objective data and calculations but also a statistically accepted means of testing the materiality of this measurement.

NOTES

1. *Daubert v. Merrell Dow Pharmaceuticals*, 509 U.S. 579, 113 S. Ct. 2786 (1993).

2. *In re Executive Telecard, Ltd. Securities Litigation*, 94 Civ. 7846 (CLB) (S.D.N.Y. 1997). See also *In re Seagate Technology II Securities Litigation*, C-89-2498(A)-VRW (N.D. Cal.), in which the court accepted some of the defendants' event studies and dismissed certain claims on that basis, but ruled that the defendants' other event studies were inadequate and denied their request for summary judgment with regard to those issues. The court also found the plaintiffs' event studies lacking and therefore denied a cross-motion for summary judgment. Also, see *Goldkrantz v. Griffin*, QBS: 02760800 (S.D.N.Y. 1999), in which the court granted summary judgment based on the plaintiffs' failure to contest the defendants' event study analysis.

3. With regard to securities litigation, see, for example, Janet C. Alexander, "The Value of Bad News," *UCLA Law Review*, Vol. 41, No. 6, 1994, pp. 1421–69; Daniel R. Fischel, "Use of Modern Finance Theory in Securities Fraud Cases Involving Actively Traded Securities," *The Business Lawyer*, Vol. 38, November 1982, pp. 1–20; Jonathan R. Macey, Geoffrey P. Miller, Mark L. Mitchell, and Jeffry M. Netter, "Lessons from Financial Economics: Materiality, Reliance, and Extending the Reach of *Basic v. Levinson*," 77 *Virginia Law Review Association* 1017 (1991), pp. 1021–28; A. Craig MacKinlay, "Event Studies in Economics and Finance," *Journal of Economic Literature*, Vol. 35, No. 1, March 1997, pp. 13–39; and Mark L. Mitchell and Jeffry

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M. Netter, "The Role of Financial Economics in Securities Fraud Cases: Applications at the Securities and Exchange Commission," *The Business Lawyer*, Vol. 49, February 1994, pp. 545–90.

4. The use of an event study to measure the magnitude of an event is certainly not new. See, for example, Mark P. Kritzman, "What Practitioners Need to Know About Event Studies," *Financial Analysts Journal*, November–December 1994. ("Aside from tests of market efficiency, event studies are valuable in gauging the magnitude of an event's impact.") For a recent application, see Jay Dial and Kevin J. Murphy, "Incentives, Downsizing, and Value Creation at General Dynamics," *Journal of Financial Economics*, March 1995.

5. Richard A. Brealey and Stewart C. Myers, *Fundamentals of Corporate Finance*, New York: McGraw-Hill, 1995, p. 306.

6. Brealey and Myers, 1995, Chapter 4.

7. Arnold S. Jacobs, "Litigation and Practice Under Rule 10b-5," cited in Mark L. Mitchell and Jeffry M. Netter, *op. cit.*

8. Recognizing this problem, the court in *Feit v. Leasco Data Processing Equipment Corp.*, 332 F. Supp. 544, 586 (E.D.N.Y. 1971), confronted with determining the materiality of an omission from a proxy statement, suggested drawing a scientific sample of investors to determine how they might have voted if the truth had been known. To our knowledge, this approach to materiality has never been attempted.

9. Intuitively, if an event is material to investors, it should move stock prices, and if it is not material, it should not affect stock prices. By examining whether the stock price change is different from random movements that occur on days when there is no news, we can determine whether investors felt that the event under consideration was material. As an example, Mitchell and Netter state that "[t]he SEC recently began to use stock price evidence to show materiality in securities fraud cases, especially insider trading cases." Also, from the same paper, "Statistical tests of significance are useful both in establishing materiality and in calculating disgorgement. A finding that a stock return associated with the release of information is large enough that it is unlikely that the return occurred by chance is strong evidence that the information was important." Of course, this is relevant only for potentially large events. An announcement that someone had stolen a $20 bill from a Sears cash register would likely not have any material effect on Sears' stock price.

10. See, for examples, the Alexander and MacKinlay articles cited in note 3 above. Note that in the vocabulary of securities litigation, the inflation in a stock price is the difference between the market price of the stock and the price it would have traded at had there been no misrepresentation or omission of public information. This use of inflation is not to be confused with its traditional economics usage referring to overall price level rate of change.

11. It is, however, not the only way to compute damages. Sometimes a fundamental analysis is appropriate. Also, the expected change in the stock price based on a sample of stock price changes in response to similar events may be used. Which approach is best will depend on circumstances relating to the allegations in the complaint and the reliability of the various types of estimates given the available data.

12. The distinction between a firm and its shareholders is a legal artifact and ignores certain economic ambiguities. For example, in economics it is theoretically possible for the current shareholders to be the firm, whereas under the law the firm is a distinct person. Presumably, this legal distinction is necessary to allow the firm to have access to the courts on behalf of the shareholders, thereby reducing the inefficiencies that would occur if the shareholders themselves had to perform the legal duties of the firm.

13. This is strictly true only if common equity is the sole source of financing. When the company has also issued debt and/or other forms of equity, lost profits would be measured by summing the changes in the market value of all of the outstanding financing sources (e.g., number of shares times the share price movement plus the number of bonds times the bond price movement). When the company does not face any serious threat of default on its senior obligations, the change in the market value of its common stock should serve as a good proxy for the change in the total capitalized value of the firm.

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14. But note an exception due to the bias from the recovery from future litigation discussed in section 19.7(b).

15. The stock market's assessment would be on an after-tax basis taking account of litigation expenses and contingent claims. The effects of these issues on damages assessment using stock prices are discussed in more detail in section 19.7.

16. See section 19.6.

17. See, for example, MacKinley, *op. cit.* for a description.

18. It is also possible to look at intraday trading to get a tighter event window. This is especially useful if before the news announcement there was a large change in the stock price that one believes was caused by other events. Use of intraday prices, however, entails several difficulties. Among these are calculating movements of the market or industry index over the same time period and adjusting materiality tests to account for the nonstandard event window. A suggested approach for determining the length of an intraday event window is in S. C. Hilmer and P. L. Yu, "The Market Speed of Adjustment to New Information," *Journal of Financial Economics*, Dec. 1979. See also S. J. Chang and Son Nan Chen, "Stock Price Adjustment to Earnings and Dividend Surprises," *Quarterly Review of Economics and Business*, Spring 1989.

19. See MacKinlay, note 3.

20. As with other expert decisions, it is helpful to have some rationale for the length of the event window chosen. For example, one can employ a standard period over different cases, cutting short the window when new information reaches the public. Alternatively, one can look at some other indicator of materiality, such as trading volume or the quantity of news coverage, to decide the period in which the market was reacting to the new information.

21. While some analysts perform crude event studies without adjusting for market effects, the literature nearly uniformly argues that a market adjustment is desirable. Moreover, relevant case law, such as *In re Executive Telecard Ltd. Securities Litigation*, states that in measuring stock price declines, one must eliminate "that portion of the price decline that is the result of forces unrelated to the wrong."

22. A regression is a statistical tool used to estimate the relation between one or more variables (here, the market and/or industry index) and another variable (the stock price of a particular company). An early, but still useful, discussion is provided in Franklin M. Fisher, "Multiple Regression in Legal Proceedings," *Columbia Law Review*, May 1980.

23. In securities fraud cases, estimation windows are often placed before the beginning of the alleged class period, even if the only event measured is at the end of the period. This is likely done so that the estimation window would cover a "clean" period that could not have been tainted by any alleged stock price inflation. There is often no theoretical basis for doing so, because the concern about a "clean" period actually relates to the possibility that the estimation of the relation between the stock and the index is contaminated by the effects of the event being studied. That is, one does not want any overlap between the estimation window and the event window. Depending on the nature of the alleged stock price manipulation, there may be no statistical basis for excluding prices during the period of alleged manipulation from the estimation window.

24. Michael Salinger ("Value Event Studies," *Boston University School of Management Working Paper*, 1991) provides a theoretical discussion on whether to place the estimation window before or after the event window. However, see note 38 on his conclusion that such methodological choices are generally irrelevant for short event windows.

25. The adjusted R-squared should not be confused with the [unadjusted] R-squared. The R-squared is a simple measurement of the explanatory power of the independent variables. The adjusted R-squared penalizes the use of additional independent variables to account for the possibility that any additional explanatory power that these variables bring to the overall regression is due solely to chance.

26. Sometimes, the expert will use a multifactor model to predict the stock price. This is a model that has more than one index in it, such as the S&P 500 and an industry index; see MacKinlay, *op. cit.* If the analyst tries a number of multifactor models and ultimately chooses

a model with only one index, then the expert may have to support this decision with the relevant statistical test.

27. There are various reasons for not simply choosing the index with the strongest statistical properties. One would be if it were known that there was a change in the operating characteristics or competitive environment facing members of one index between the estimation and event windows. Also, there are other considerations involved in comparing statistics from different estimation windows, most notably that a good index from an estimation period near the event window may be preferable to an index with more explanatory power in an estimation period further from the event window.

28. There is a question about whether to apply the abnormal return to the stock price at the beginning or end of the event window. This question essentially turns on whether the event occurs first, followed by market effects, or whether market effects come first, followed by the event. As an example, suppose that a stock price drops from $20 to $9 during an event window in which the predicted return was −10%. If we apply the predicted return first, the stock would have been expected to drop to $18, and then it fell by an additional $9 as a result of the event. Alternatively, one could say that the stock fell by $10 as a result of the event, reaching a level of $10, and then fell an additional 10% due to market forces, to reach its final level of $9. The difference is generally not important when market movements are small. In general, one would want to consider when in the event window the effects of the event were more likely to have been felt.

29. See, for example, Katherine Schipper, Rex Thompson, and Roman L. Weil, "Disentangling Interrelated Effects of Regulatory Changes on Shareholder Wealth: The Case of Motor Carrier Deregulation," *Journal of Law and Economics,* Vol. 30, April 1987, pp. 67–100.

30. Fed. R. Evid. 402, as cited in *Ohio ex rel. Montgomery v. Louis Trauth Dairy, Inc.,* 925 F. Supp. 1247, 1251 (S.D. Ohio 1996).

31. Fed. R. Evid. 702, as cited in *Ohio ex rel. Montgomery,* 925 F. Supp. at 1251.

32. *Daubert,* 113 S. Ct. at 2795.

33. *Ohio ex rel. Montgomery,* 925 F. Supp. at 1251.

34. *Daubert,* 113 S. Ct. at 2795.

35. *Daubert v. Merrell Dow Pharmaceuticals, Inc.,* 43 F.3d 1311, 1317 (9th Cir. 1995).

36. *Smelser v. Norfolk Southern Railway Co.,* 105 F.3d 299, 303 (6th Cir. 1997).

37. Still, at least one court has ruled that in examining how events affected stock prices, "available techniques of proof such as econometric modeling are sufficiently demanding of internal consistency as to reduce the opportunity for such manipulation of data." *In re LTV Securities Litigation* (88 F.R.D. 134). Such a statement certainly could not be made about analyses where the analyst has the freedom to essentially make up one or more inputs to the calculation based on nothing more than a claim that those inputs are reasonable.

38. In a seminal article ("Measuring Security Price Performance," *Journal of Financial Economics,* 1980), Stephen J. Brown and Jerold B. Warner state that "a simple methodology based on the market model performs well under a wide variety of conditions." In discussing this and a later paper by the same authors, Michael Salinger, *op. cit.,* states that the previous authors' "results tended to be robust to the methodological alternatives." Salinger further states, "There is a schizophrenia in the event study literature between a very close attention to methodology and the view that for events of any importance, methodology is unlikely to matter a great deal. The latter view is probably quite appropriate when news is revealed over a brief, identifiable interval." "Measuring Security Price Performance," *Journal of Financial Economics* 8 (1980), pp. 205–58.

39. To be entirely correct, one would also have to specify several other minor choices, such as whether to use logarithmic or percentage returns and whether returns are measured daily or over some other period of time.

40. Of course, one must still use common sense in interpreting these results. For instance, if a company moves from an environment characterized by a high degree of government regulation to one of low regulation, there would be reasons to potentially challenge the use of a regression from one period to account for market movements in the other.

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41. The net-of-market methodology also assumes that the constant term from the regression, or alpha, equals zero. References to an assumed beta of one should be considered to include this second assumption as well. We do not independently criticize the assumption that alpha is set equal to zero over the event window. Given an accurate estimate of beta, it is possible for reasonable analysts to perform the analysis with different estimates of alpha than what would be produced from a market model regression. One assumption, for example, is to compute an estimate based on the formula given by the Capital Asset Pricing Model; this formula would produce a value of alpha equal to zero whenever beta equals one. In practice, different reasonable estimates of the value of alpha generally do not make a noticeable difference in the expert's findings.

42. If betas are symmetrically distributed around one, the materiality test would be unbiased but not efficient. This means (1) that excessively positive and excessively negative returns would roughly balance over numerous observations but (2) that other tests were more likely to give the correct answer on any one individual case.

43. See Werner F. M. DeBondt, "Stock Price Reversals and Overreaction to New Events: A Survey of Theory and Evidence," 1989; Rui M. C. Guimaraes, Brian G. Kingsman and Stephen J. Taylor, eds., *A Reappraisal of the Efficiency of Financial Markets*, Berlin: Springer-Verlag. Much of the evidence is from the research of Paul Zarowin, including his 1989 article "Short-Run Market Overreaction: Size and Seasonality Effects," *The Journal of Portfolio Management*, Spring, pp. 26–29. See also M. Bremer and Richard Sweeney, "The Information Content of Extreme Daily Rates of Return," *Claremont McKenna College Working Paper*, 1987, later published in the *Journal of Finance*.

44. For more detail on some of the alternative methods, see, for example, Carroll B. Foster, Robert R. Trout, and Patrick A. Gaughan, "Losses in Commercial Litigation," *Journal of Forensic Economics* Vol. 6, No. 3, 1993, pp. 179–96. This paper discusses some of the means of measuring lost profits based on projections and accounting statements. Interestingly, while the authors say that the methodology they describe "is conceptually similar to the situation where a plaintiff suffered a loss of a passive investment, such as a securities fraud case," they do not appear to consider whether the event study methodologies of securities fraud cases could be applied to measuring lost profits in a commercial setting.

45. For a series of simple examples on how the same data can lead to extreme variations in lost profits, see Robert L. Dunn, *Recovery of Damages for Lost Profits*, 1992, pp. 459–70. ("The point made is that, depending on the approach taken, great variations in projections will result.")

46. See, for example, Richard R. Simonds, "Public Utility Valuation Methods: Theoretically Equivalent But Not Redundant," *Property Tax Journal*, September 1992, pp. 289–300.

47. Bradford Cornell, *Corporate Valuation: Tools for Effective Appraisal and Decision Making*, Irwin Prof. Publishing, 1993.

48. Steven H. Hanke and Stephen J. K. Walters, "Recent Controversies in the Valuation of Utility Properties," *Public Utilities Fortnightly*, July 21, 1988, p. 24.

49. *Id.* at 23.

50. "Court Declines Review in Five Cases, Including Three Involving Rail Property," *BNA Washington Insider*, June 6, 1995.

51. *Elmer E. Mills and Louis Susman v. The Electric Auto-Lite Co. et al.*, 552 F.2d 1239, 1247 (1997).

52. *In re Central Ice Cream Co.*, 836 F.2d 1068, 1072 (7th Cir. 1987).

53. Recent financial press commentaries on the volatility, previously high values, and rapid collapse of many Internet stocks represent another event that should lead to investigation of stock market anomalies.

54. *Basic v. Levinson*, 485 U.S. 224, 247 (1988).

55. We omit here mention of stock price overreaction, which was discussed in section 19.4.

56. For a review, see Stephen F. LeRoy, "Efficient Capital Markets and Martingales," *Journal of Economic Literature*, Vol. XXVII, 1989, pp. 1583–621.

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57. Paul A. Samuelson, "Proof That Properly Discounted Present Values of Assets Vibrate Randomly," *The Bell Journal of Economics and Management Science,* Vol. 4, No. 2, 1973, pp. 369–74; and "Stochastic Land Valuation: Total Return as Martingale Implying Price Changes a Negatively Correlated Walk," *Paul A. Samuelson's Collected Scientific Papers,* Vol. 5, Cambridge: MIT Press, 1986.

58. See, for example, J. Bradford DeLong, Andrei Shleifer, Lawrence H. Summers, and Robert J. Waldmann, "Positive Feedback Investment Strategies and Destabilizing Rational Speculation," *Journal of Finance,* Vol. 45, No. 2, 1990, pp. 379–95.

59. Baruch Lev and Meiring Devilliers, "Stock Price Crashes and 10b-5 Damages: A Legal, Economic, and Policy Analysis," *Stanford Law Review,* November 1994.

60. *Ibid* at p. 35.

61. In some ways this is clearly unfair, as there has been a transfer of wealth from one set of shareholders to another as a result of an illegal act in which neither group knowingly participated. An alternative view is that when investors buy and sell shares, they are trading in the company's fortunes, including unexpected gains and losses from legal actions and certain illegal acts affecting the company's value.

62. One advantage of event studies here is that if the changes in the perception of the libel occur at discrete times, these effects can be captured by using readily available stock market data. Directly measuring the changes in expected income at various points in time would require a large set of contemporaneous projections.

63. While the actual rate of error is not known without the materiality test, if the stock is simply assumed by the expert to move one-for-one with the market, we can be sure that the rate of error is higher than if the relation between market and stock movements used in the damages calculation is based on statistical analysis.

64. The general attitude toward event studies may be best summed up by Glenn V. Henderson, Jr., "Problems and Solutions in Conducting Event Studies," *Journal of Risk and Insurance* 57(2), June 1990: "The event study is a classic design. Classic designs are simple and elegant, and above all else, functional. The event study has become a classic because it works. It can be used under less than perfect conditions and still produce reliable results."

65. In a case where a private company went through with an initial public offering after suffering some harm, data on the actual offering price can be compared to a previously expected offering price to perform a basic event study. Appropriate market and industry adjustments to the expected offering price can be made based upon the stock's post-offering behavior.

LIST OF CASES

Basic Inc. v. Levinson, 485 U.S. 224, 247 (1988)
In re Central Ice Cream Co., 836 F. 2d 1068 (7th Cir. 1987)
Daubert v. Merrell Dow Pharmaceuticals, 509 U.S. 579, 113 S. Ct. 2786 (1993)
In re Executive Telecard, Ltd. Securities Litigation, 94 Cir. 7846 (CLB) (S.D.N.Y. 1997)
Feit v. Leasco Data Processing Equipment Corp., 332 F. Supp. 544, 586 (E.D.N.Y. 1971)
Frye v. United States, 293 F. 1013 (D.C. Cir. 1923)
Goldkrantz v. Griffin (S.D.N.Y. 1999)
Kumho Tire Co. v. Carmichael, 526 U.S. 137, 119 S. Ct. 1167 (1999)
In re LTV Securities Litigation, 88 F.R.D. 134, 149 (1980)
Mills v. Electric Auto-Lite Co., 552 F. 2d 1239, 1247 (1977)
Ohio ex rel. Montgomery v. Louis Trauth Dairy, Inc., 925 F. Supp. 1247, 1251 (S.D. Ohio 1996)
In re Seagate Technology II Securities Litigation, C-89-2498(A)-VRW (N.D. Cal.)
Smelser v. Norfolk Southern Railway Co., 105 F. 3d 299, 303 (6th Cir. 1997)

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EXHIBIT E

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Inflation Methodologies in Securities Fraud Cases: Theory and Practice

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Inflation Methodologies in Securities Fraud Cases: Theory and Practice

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There are several basic methodologies for measuring the true value in a stock before a corrective disclosure of previously omitted or misstated information. Among the most common are the constant dollar inflation, constant percentage inflation, and constant true value methodologies. In this paper, we consider the theoretical justifications for each methodology given different types of allegations. We further examine the interaction of the choice of inflation methodology with the measurement of damages given the loss causation requirements of the securities laws. Finally, we examine settlements of shareholder class actions and document that an extremely large (and likely unreasonable) share of those settlements use the constant true value methodology.

I. Introduction

One of the key tasks in assessing damages in a securities fraud case is the determination of what portion of the traded stock price is real (the true value) versus the part that is due to alleged misstatements or omissions (the inflation). If liability is established, then the calculation of the inflation in the stock price serves as the basis for all damage claims. As such, it might be expected that there is a large literature on how the inflation is to be measured. In fact, while there are some papers that do discuss how to measure inflation, there is not a large literature on which methodology is appropriate,[1] and we are unaware of any literature on the differing bases for deciding between a constant dollar and constant percentage inflation. At best, most papers discuss how to measure the effects of a corrective disclosure, generally at the end of a class period, and then assert how this information is to be used to calculate the inflation in the stock price at earlier points in time.

Section II of this paper attempts to provide a framework for thinking about which inflation measure may apply in different situations. Section III then discusses how these

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[1] One partial exception is Bradford Cornell and R. Gregory Morgan, "Using Finance Theory to Measure Damages in Fraud on the Market Cases," *UCLA Law R., 1990*, which discusses the differences between the constant percentage inflation and the index method/constant true value.

different inflation measures interact with the loss causation requirements of the securities laws. Section IV examines a year's worth of settlement plans of allocation to estimate the frequency with which different inflation measures are used in practice. Sections V and VI review the results and conclude.

II. Theoretical Bases for Different Inflation Measures

The implicit basis for most measurements of inflation is that a stock price represents a share of the sum of the current net assets plus the net present value (NPV) of a company's future free cash flows.[2] Consequently, if a company provides misleading information about its operations or plans, or fails to provide required information, the market will misvalue the company's cash flows and the resulting stock price will be incorrect. When there is a corrective disclosure, the market then reassesses the company's current net assets and/or future cash flows and sets a new stock price. For example, if a company's stock price falls from $10 to $9 after a corrective disclosure (after adjusting for concurrent market and/or industry effects and any non-disclosure company-related news), then the effect of the disclosure was $1 *at the time of the disclosure*. The question then becomes: what would the effect of that disclosure have been at an earlier time?

There are at least three commonly utilized answers to this question. One may assume that the disclosure at any time in the past would also have resulted in a stock price decline of $1. This is *the constant dollar inflation* method. Alternately, it can be assumed that an earlier disclosure would have also produced a 10% stock price drop, regardless of the initial price. This is the *constant percentage* method of calculating inflation. Finally, we may assume that an earlier disclosure would have resulted in a subsequent stock price of the stock's "true" value, or $9. This is the *constant true value* method of calculating inflation. As discussed below, each of these methods has profound implications for the assumed effect of the fraud on future cash flows and for the calculation of damages.

[2] The Present Value (PV) of a stream of future cash flows is the amount that must be invested today in a project of an equal risk level to produce those cash flows in the future. Barring arbitrage, therefore, it is the value today of those cash flows.

A. Theoretical Underpinnings

Basic corporate finance, in the form of the dividend discount model and its variants teaches that the value of a company's stock is the present value of the expected cash flows that will accrue to that stock. This result has implications for calculating damages due to a misrepresentation of a company's current and/or future business. In essence, the nature of the misrepresentation – its effect on expected future cash flows and hence the value of the firm – will determine the proper method to use when estimating share price inflation.

We begin by examining four basic types of disclosures about future earnings in order to determine the implications for the price of a company's stock price, and hence to draw some conclusions about the appropriate model of share price inflation. The first type (Type I) of disclosure is a one-off shortfall. A firm announces that future earnings will decline by a one-time amount (say an unexpected expense) of $100 next period.[3] The effect of such an announcement is to decrease the value of the company's equity by the present value of $100 now. Alternatively (Type II), the firm may have announced that earnings in every future period would be $100 less than originally anticipated. In such a case, the company's equity will decrease in value by the PV of $100 received in perpetuity.[4] Two more cases are of interest. In the first (Type III), a company may announce that instead of growing by 10 percent a year from a base of $100 in the next period, earnings will instead grow at 5 percent a year. It is apparent that the dollar difference in the two earnings streams will diverge over time; however, the divergence will be a simple function of the discount rate, and the difference in expected growth rates, and is captured by the present value formula adjusted for the differing growth rates. The effect on the company's equity is a decrease in value equal to the present value of $100 in perpetuity adjusted for the difference in the two assumed growth rates. Finally (Type IV), a company may announce that earnings in each period of the future will be a fraction - say 90 percent - of what was originally anticipated. In this case, the PV of expected future earnings after the disclosure – and by extension, the company's new stock price – are the same

[3] Unless stated otherwise, these figures are assumed to be after-tax values. These changes are also assumed to come from events that will impact cash flows and not from events with no true economic significance, such as a change in accounting policy.

[4] The value of $1 in perpetuity is $1/r, where r is the appropriate discount rate.

fraction of the PV of the originally expected cash flows.[5] The fall in the company stock price as a result of this disclosure will be the PV of $100 in perpetuity multiplied by one less 0.9. Interested readers can see Appendix 1 for mathematical representations of these inflation measurements.

The determination of the appropriate method to estimate share price inflation is aided by a simple observation derived from the theory of efficient markets: the change in the company's stock price should only reflect new information; as such, the choice between a proportional or absolute inflation measure is equivalent to the question of which measure only depends on new information.[6] Referring to our taxonomy of omitted or misstated information about earnings and to Appendix 1, the *difference* in the pre and post-disclosure prices can be expressed exclusively as a function of new information for Type I and Type II disclosures while for Type III and Type IV the *ratio* of the two prices reflects only new information. (The boxed calculations in Appendix 1 show the inflation measurement that does not depend on the original earnings level, X, but instead depends on the new information provided in the disclosure.) This shows that a constant dollar inflation measure is appropriate for Types I and II while a constant percentage inflation measure should be used for types III and IV.

There is intuitive support for this result. Type I and II disclosures are news about a fixed or constant change in future cash flows. As such, their immediate effect – the difference in price before and after the disclosure - can be expressed exclusively as a function of those (newly revealed) fixed amounts. Type III and Type IV disclosures are news about relative changes in future cash flows. Thus their immediate effect – the difference in price before and after the disclosure – must reflect both the old and new information.[7] The proportion of pre and post-disclosure prices, however, will fully reflect this information.

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[5] We thank Dmitry Krivin for pointing out that this is only true if the company has no debt. In fact, there is a constant percentage change in the total enterprise value of the company, meaning its debt plus equity. However, because of greater sensitivity of equity to changes in cash flows, the effect on equity is not a constant fraction. Interested readers can see an example of this phenomenon in Appendix 2.

[6] Speaking strictly, we want the inflation measure to reflect the PV of new information. Therefore it will also be a function of the discount rate. If we assume that present value equals future value, a discount rate of 1, then the appropriate inflation measure will – strictly speaking – contain only new information.

[7] This is because a proportion is meaningless in absolute terms without a standard of reference.

The constant true value inflation measure is not appropriate for any of the cases above. As discussed in more detail below, the principal assumption of the method, that no news other than the curative disclosure affects the stock price, is so extreme that it seems likely to be justified in only one type of real world case. This is the case of a completely fraudulent company that convinced the market that it had a positive value. Its constant true value, however, was zero.

Our taxonomy of curative disclosures is by no means exhaustive. Our aim has been to address the most common types of disclosures and provide a framework for systematically selecting an inflation measure. One can imagine variants and combinations of the four above, as well as disclosures of a completely different nature. The framework discussed above is a guide to both avoiding the use of a clearly incorrect measure and developing an appropriate one.

B. Examples

We can now consider how these disclosure types apply to certain examples. Suppose first that the company in question is a holding company with no liabilities and only one asset, a 50% ownership stake in a second company with actual operations. Now suppose that one day the company announces that it never held a 50% ownership stake in the second company, but instead only held a 45% stake. It should be apparent that in this example the original stock price would have been 10% (the ratio of 5% to 50%) lower had the market known that the holding company only held 45%, instead of 50%, of the operating company. In fact, one would also expect that in an efficient market, if only the future cash flows from the operating company matter, then the stock price reaction to the corrective disclosure would be a 10% decline.[8] This type of reasoning applies generally where the allegation has a multiplicative effect on a component of the net present value calculation, whether it be a percentage change in cash flows (as above), effective tax rates or margins (both affecting cash flows in a

[8] Unfortunately, life is not always so easy. In addition to revaluing the future cash flows, the market would likely consider additional effects such as whether the company is now due a lump-sum tax refund if it overpaid previous taxes as well as additional litigation costs that the holding company many now incur if it is sued for securities fraud. To the extent that these are one-time effects, such as the tax refund, then this would not represent a pure percentage inflation.

multiplicative fashion), or the company's growth rate (which, with a constant discount rate, would have a multiplicative effect on the present value of future cash flows). A number of these cases are illustrated in the mathematical appendix.

As another example, consider a company with various operations that announces that it has received a one-time after-tax million dollar cash payment for some event that will never happen again. This million dollars would then be incorporated into the market's views of the company's current net assets and therefore raise the company's stock price by a million dollars divided by the number of shares outstanding. Continuing our example, if the company had a million shares outstanding the effect would be a dollar per share. Consequently, if the company later announces that it never received the million dollars, or perhaps that the million dollars is uncollectable, then the direct effect on the stock price will be a decline of a dollar per share.[9] The effect of the announcement would be independent of the state of the company and would be best represented by a constant dollar inflation, meaning that the inflation would have been a dollar at earlier points in the class period. This reasoning then applies to any disclosure of a one-time event.

Unfortunately, it is not always clear whether a certain disclosure is better modeled as a constant percentage or constant dollar inflation. For example, consider a conglomerate with ten different factories each producing a different product. Suppose that at some point the conglomerate announces that one of the factories was completely fictitious and never existed. One possibility is that this represents a relatively constant percentage of the company's stock price equal to the percentage of cash flows that were expected from the fictitious factory. This presumption would be supported if most of the movements in the company's stock price were due to events that affected the company across the board, such as changes in tax and interest rates in the economy. Another possibility is that the market viewed the fictitious factory as a single asset in the company's portfolio - in other words, that the value of the company was simply the sum of the values of the different factories, and those values moved independently. This presumption would be supported if most of the movements in the company's stock price

[9] As discussed above, there may be other effects such as tax implications and the costs of expected litigation. To the extent that these are also perceived to be one-time events, then the qualitative discussion in the text above is unchanged.

were tied to favorable or adverse news about particular factories or product lines. In that case, stock price movements due to news about a different factory should not affect the inflation in the stock price, which would then be best represented as a constant dollar inflation, with the dollar amount changing when there was news about the fictitious factory. The distinction between these two paradigms is therefore not as clear as one might hope.

Next, consider an example where the only operating asset is the fictitious factory, and that news releases about the non-existent factory appeared to move the stock price over the course of the class period. It should be clear that the true stock price would be zero, or at best near zero if there was a chance that the factory could be built. In this case, one could model the true value as being a constant, or nearly so, at the value the stock price reached after the corrective disclosure. Note, however, that it is crucial for this scenario that the factory truly be fictitious. If it were a real factory that was producing cash flows, even if those cash flows were overstated, then there is no *ex ante* reason to believe that the value of that factory would have been a constant, or even relatively constant, over the class period.

Finally, it should be reemphasized that all three of the examples discussed above – constant percentage inflation, constant dollar inflation, and constant true value – are simply idealized representations used to model inflation. Many examples of fraud will have some characteristics of one or more type, and one goal should be to see which paradigm is the most reasonable, while recognizing that none will perfectly measure the inflation in the stock price[10]On the other hand, this does not mean that a careful statistical analysis that can provide a better model should be eschewed in favor of a simple paradigm. If an objective analysis can provide a more accurate inflation measurement, then it would make sense to use that inflation measure instead.

III. The Interrelation Between Inflation and Loss Causation

One of the elements in proving damages in a securities fraud case is to show that plaintiffs' losses were caused by the alleged fraud. This analysis is often thought of either as

[10] It is also the case that some patterns of inflation can exhibit inconstancy of the dollar amount or percentage of inflation as in the example given in Judge Sneed's concurring opinion in *Green v. Occidental Petroleum*. However, the framework presented in this paper is useful even in such cases.

separate from the calculation of inflation, or else is considered to be accomplished by calculating damages as the difference between inflation at the time of purchase and at the time of sale (or simply inflation at the time of purchase if a share is held to the end of a class period.) For example, in *Blackie v. Barrack* 524 F.2d 891 (1975) at 906, the court opined that "[m]ateriality circumstantially establishes the reliance of some market traders and hence the inflation in the stock price – when the purchase is made the causal chain between defendant's conduct and plaintiff's loss is sufficiently established to make out a prima facie case," thereby tying the inflation at the time of purchase to the loss causation requirement.

Consider, however, the following example. Suppose that an investor purchases a stock at a price of $20. The stock then falls in value to $10 due to non-fraudulent reasons. There is then a corrective disclosure that lowers the stock price to $9. We then ask what the inflation is and what the investor's allowable damage claim is.

A. Constant Dollar Inflation

Under a constant dollar inflation, there is a $1 per share inflation, measured at the time of the corrective disclosure. This $1 inflation is then the investor's damage claim. This is both the amount that she overpaid upon purchasing the stock and the amount that she actually lost at the time of the corrective disclosure.

B. Constant Percentage Inflation

Suppose instead we used a constant percentage inflation measure. The measured price decline of 10% at the time of the disclosure would then translate into a $2 inflation at the time of purchase. If the investor is allowed to recover the amount she overpaid at the time of purchase, this would be her claim. On the other hand, if her damages are limited to the decline in the value of her investment *due to* the disclosure, then she is only entitled to a claim of $1.[11] In this case, the constant percentage inflation methodology would result in a damage claim that is too large – i.e., it allows the investor to recover losses that were unrelated to the fraud. Note

[11] See, for example, *The Ambassador Hotel Company, Ltd. v. Wei-Chuan Investment*, 189 F.3d 1017, 1999: "In fact, some securities fraud cases do state that if the plaintiff would have lost its investment despite any misrepresentation by the defendant, plaintiff has failed to prove loss causation."

that if the stock price had been low at the beginning of the class period, say $5 per share, then the inflation at the time of purchase would be $0.50, or ten percent of the stock price. This figure would then equal the maximum damage under the out-of-pocket measure. Therefore, if damages are limited to the *minimum* of inflation on purchase and the loss caused by a disclosure, the constant percentage methodology (or, in fact, any methodology other than the constant dollar inflation) will either give the correct damage claim or else overstate the damage claim.

The use of a constant percentage methodology may also lead to concerns about some investors receiving a damage claim even though they did not hold through a disclosure. Consider, for example, an investor who buys a stock at $20 and sells it at $10 in the above example, where the $10 price decline was not due to the fraud or its revelation. If there is a constant 10% price inflation, then the investor overpaid by $2 on purchase but only received back $1 in inflated proceeds. Should this investor therefore have a claim of $1 based on the difference between her purchase and sale inflation? One view is that because the fraud interacted with the stock price, the investor is indeed entitled to claim the $1 in damages. Another view is that because the price decline is independent of the fraud, the investor's loss was not caused by any fraudulent action or disclosure, and therefore she has no damage claim. If proving loss causation means that the latter view is correct, then the straightforward application of a percentage inflation will give many in-and-out traders an improper damage claim. Some experts have attempted to circumvent this problem by only giving a damage claim to in-and-out traders who held past a disclosure. Yet, even this attempted solution creates its own problems. Consider first an investor who bought at $20 and retained her share until the end of the class period. In the example given above, she has a $2 claim. Now consider what her claim would be if at some point when the stock was trading at $10 she sold her share and then bought it back again for $10 a second later. The first share, the in-and-out, would have no claim because it was not held over a disclosure; the second share, the retention share bought at $10, would have a $1 claim. Therefore, adding in an economically meaningless set of transactions, a virtually instantaneous sale and purchase at the same price, significantly changes the damage claim. While one might argue that such a result is legally correct, it is clearly economically nonsensical.

C. Constant True Value

Finally, consider what one would calculate using a constant true value methodology. Since the stock price is $9 after the curative disclosure, this is also the true value at the time of purchase, making the damage claim $11. However, under the assumptions of the hypothetical case that we present, this significantly overestimates the investor's loss due to the fraud. The problem, of course, stems from the failure to determine what portion of the $11 decline from the $20 purchase price to the $9 value at the end of the class period is due to the fraud. A constant true value methodology would only makes sense if there were no material changes in the company's stock price as a result of non-fraudulent factors.[12] Unless a company were a total fraud, having no true component to its value, it is unlikely that all or nearly all of the price movements over a class period could be attributed to fraud.

In fact, many courts have determined that it is necessary to use an event study to distinguish between stock-price movements due to fraud and movements due to other factors.[13] To use the event study methodology, an expert has to first gather news stories related to the company over the class period and to determine which of those stories represent a possible misstatement or curative disclosure. To move from this data collection to an event study, the expert must then perform statistical analyses to determine the effects of that news, generally after controlling for contemporaneous market and/or industry influences.[14,15] The calculations

[12] A refinement on the constant true value methodology is the index method, in which the true value is determined at the end of the class period and then, rather than taking this value as constant throughout the class period, the value is pegged to a market or industry index which is then backcast to the beginning of the class period. This methodology has the advantage of incorporating market and/or industry influences on the stock. On the other hand, it can greatly inflate in-and-out damages because the stock and index returns are not equal, which leads to a varying inflation over the class period.

[13] See, for example, *In re Seagate Technology II Securities Litigation*, 1994 WL– 41834 (N.D. Cal.), in which the court accepted some of defendants' event studies and dismissed certain claims on that basis, but ruled that defendants' other event studies were inadequate and denied their request for summary judgment with regard to those issues. The court also found plaintiffs' event studies lacking and therefore denied a cross-motion for summary judgment. See also, *In re Executive Telecard , Ltd. Securities Litigation*, 94 Civ. 7846(CLB), (S.D. New York 1997) and see *Goldkrantz v. Griffin*, 97-CV-9075 (U.S.D.C. SDNY), in which the court granted summary judgment based on plaintiffs' failure to contest defendants' event study analysis.

[14] See, for example, *In re Seagate Technology II Securities Litigation*, 1994 WL– 41834 (N.D. Cal.) ("Decoding how much of the price behavior of a security is attributable to alleged market manipulation requires statistical analysis.") Some academic papers that use the event study methodology include:

a. Daniel R. Fischel, "Use of Modern Finance Theory in Securities Fraud Cases Involving Actively Traded Securities," 38 *Bus. Law.* 1 (1982).

in the event study allow for an objective quantification of the statistical significance (or materiality) of the effects of the news, a feature that distinguishes it from a mere listing of news stories and contemporaneous stock prices that requires subjective interpretation. Because the constant true value methodology does none of the above, it often misestimates the inflation in a stock at the time of purchase. And, like the constant percentage inflation method, if damages are limited to the lower of inflation on purchase and actual loss on disclosure, the constant true value methodology can overstate damages though it would never understate them.

IV. Emprical Analysis of the Use of Different Inflation Measures

A. Case Study: *Cendant Securities Litigation*

It is clear that if there is only one curative disclosure of a misstatement, the methods of calculating share price inflation have very different implications not only for retained shares, but for estimating damages to those who bought and sold shares during the class period, the so called in-and-outs. In the case of constant dollar inflation, in-and-outs would receive no damages as the stock price was inflated by a constant amount during the class period.[16] For constant percentage inflation, unless the expert makes an explicity adjustment, as discussed above, the model will generally award damages to some in-and-out shares since the dollar amount of inflation changes day to day, though the percentage does not. Similarly, a constant

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b. Jon Koslow, Note, "Estimating Aggregate Damages in Class Action Litigation Under Rule 10b-5 for Purposes of Settlement," 59 *Fordham L. Rev.* 811, 826-42 (1991).
c. Philip J. Leas, Note, "The Measure of Damages in Rule 10b-5 Cases Involving Actively Traded Securities," 26 *Stan. L. Rev.* 371 385-96 (1974).
d. Jonathan R. Macey, Geoffrey P. Miller, Mark L. Mitchell and Jeffrey M. Netter, "Lessons from Financial Economics: Materiality, Reliance, and Extending the Reach of Basic v. Levinson," 77 *Va. L. Rev*. 1017, 1021-28 (1991).
e. A. Craig MacKinlay, "Event Studies in Economics and Finance," 35 *Journal of Economic Literature* (March 1997), pp. 13-39.
f. Mark L. Mitchell and Jeffrey M. Netter, "The Role of Financial Economics in Securities Fraud Cases: Applications at the Securities and Exchange Committee," 49 *Bus. Law*. (1994), pp. 545-590.
g. David Tabak and Frederick Dunbar, "Materiality and Magnitude: Event Studies in the Courtroom," in *Litigation Services Handbook: The Role of the Financial Expert, Third Edition* (2001).

[15] In cases where there is both fraud-related and non-fraud-related information released at the same time, it is then further necessary to separate out the effects of those two sources of news.

[16] Here we assume that there is only one alleged disclosure at the end of the class period. Obviously if there are partial disclosures, then in-and-out traders can receive a damage claim.

true value methodology will yield damages for some in-and-outs, as the inflation will tend to change daily.

As Judge Walker observed in *Ravens et al v. Iftikar et al.*, (174 F.R.D. 651) "Because actual price behavior is given, the parties can only dispute what the price of the security would have been in the absence of fraud." Given that large differences can result in the estimated damages to retained shares solely as a result of the choice of the inflation model and the fact that the presence or absence of damages to in-and-outs may also hinge on this choice, one would hope that there is a sound basis for which inflation methodology has been used in different cases.

Unsurprisingly, however, the question of the appropriate method to measure stock price inflation has been the basis of some controversy in the case law. For example, in the *Cendant Corporation Securities Litigation* (109 F, Supp.2d 235) Judge Walls approved a settlement based on an increasing constant percentage model of inflation developed by Plaintiffs' expert Frank C. Dorkey. This approach estimated the inflation due to multiple misstatements by assuming that the stock price equaled its true value at the end of the class period but that the inflation percentage had increased over the class period due to the successive misstatements during earnings announcements. Class members Janice and Robert Davidson objected to the plan on a number of grounds, among them that "the fraudulent inflation…increased constantly throughout the Class period. It therefore assumes in and out purchasers and sellers assumed no damages." Lead Plaintiffs countered that the plan "expressly rejects such damages…because…those who purchased then sold Cendant stock while it was still inflated…benefited from the company's ongoing fraud and suffered no damage." Though the plan was approved, we observe that the the model would in fact not rule out damages for in-and-outs, because those who traded between earnings announcements had at least a theoretical case for damages if their purchase inflation exceeded their sale inflation. Interestingly enough, Plaintiffs claimed that Mr. Dorkey's model was based on the model used by David J. Ross and accepted by Judge Walker in the *California Micro Devices Securities Litigation* (965 F. Supp. 1327). Mr. Ross' model however assumed a *constant dollar* amount of inflation that grew with successive misstatements, as opposed to the *constant percentage* inflation used in the Cendant case. The constant dollar formulation would indeed rule out-in-and out damages prior to a curative disclosure.

B. Data on 2001 Settlements

There appears to be a large distribution of inflation scenarios used in practice. Janet Cooper Alexander has claimed that following an event study to determine the effect of a disclosure, the "simplest (and most common) method [of determining the inflation before the disclosure] is to assume that the value of the information remains constant throughout the class period,"[17] referring to the constant dollar inflation methodology. Professor Alexander then cites as an "alternative method," the assumption that the inflation is a constant percentage of the stock price. Interestingly, Professor Alexander states that the plaintiffs' expert in a case she examines presented the constant dollar inflation as the basis of his calculations, but also "testified that it would have been *equally plausible* to assume that the value of the information was not a constant dollar value of $3.25, but 10% of the stock price."[18] (Emphasis added.) For the constant dollar and constant percentage inflation measures to be considered equally plausible would either have to be an extreme coincidence, or else be an admission of an inability to analyze which inflation methodology is more appropriate. Returning to Professor Alexander's taxonomy, she ends by noting a "third method … [that] would attempt to determine the actual value of the information on each day of the class period. Applying this method would likely require some heroic assumptions …" As discussed above, such methods would indeed either require assumptions that are either unrealistic (e.g., a constant true value, which assumes that there was no major news or market influences throughout the class period) or would require careful analyses in order to quantify effects that change over time.

To test Professor Alexander's assertion that the most common method for estimating inflation throughout a class period is the constant dollar method, we examined all of the settlement plans of allocation reported in *Securities Class Action Alert* in 2001. These settlement plans are, of course, not necessarily the inflation calculations by either plaintiffs' or

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[17] Janet Cooper Alexander, "The Value of Bad News in Securities Class Actions," *UCLA Law R.*, August 1994, p. 1433.

[18] Alexander, *op. cit*,. p. 1457.

defendants' experts, but instead reflect some view of the amount that plaintiffs could be expected to recover if they went to trial. For our analysis, we classified the plan of allocation for each category of securities and determined whether the allowable loss was based on a constant dollar amount (where the amount could be variable throughout the class period), a constant percentage, or a constant true value. Plans not classifiable into one of the above methodologies were counted in a residual category for allocations that were either unknown or appeared to follow some other scheme. We also classified the plans by whether the allowable loss was based on a single methodology, or whether there were multiple categories involved (e.g., the allowable loss could be the minimum of a constant dollar inflation and the difference between the purchase price and a constant true value.) We also excluded from our analysis any provisions that limited an allowable claim to a plaintiff's purchase price less sales price, because this is often used as a means of ensuring that no plaintiff recovers more than her actual loss.

It should be noted that there are several issues that make some of the classifications either difficult or somewhat subjective. First, it is not always possible to distinguish a change in a constant percentage inflation from a change in the merits of plaintiffs' claims at different points in time. For example, consider a settlement that provides purchasers during one part of the class period with a claim of 90% of their purchase price and provides purchasers during another portion of the class period with a claim of 75% of their purchase price. It could be the case that there was a constant percentage inflation, equal to 90% of the stock price at one point and 75% of the stock price at another. However, it could also be the case that purchasers during one portion of the class period had a stronger case on liability and therefore merited a settlement that was a larger percentage of their purchase price.

A second issue confounding the classification is the bounce-back provision of the 1995 Private Securities Litigation Reform Act (the "PSLRA"). This provision limited damage claims for investors who held shares for at least ninety days past a disclosure to the difference between their purchase price and the average price over the ninety days following a disclosure. Investors who sold within the ninety days after a disclosure had their damage claim limited to their purchase price less the average price between the disclosure and their sale. If this provision is implemented in a settlement, and if sales within the ninety day post-disclosure period are not accounted for, then the settlement has a provision that limits damages to purchase price less some other price, or exactly the same calculation as is seen under a constant true value analysis. In fact, some settlements did include this cap and referred to the ninety-day post-class period stock price (though without making the appropriate statutory provisions for plaintiffs who sold within the ninety-day period!) Other settlements did not even mention this cap, which, depending on the stock price over the class period, could have left some investors with an allowable claim larger than the amount permitted under the PSLRA.[19]

A third confounding factor in the classification of settlements is that if there is a constant percentage of inflation over the entire class period, this percentage can become irrelevant in the allocation of a fixed settlement pool. For example, if Plaintiff 1 purchased her shares for $1,000 and Plaintiff 2 purchased her shares for $500, then if the allowable claim is simply a plaintiff's purchase price, Plaintiff 1 has an allowable claim twice that of Plaintiff 2, and would be entitled to two-thirds of the settlement fund if they were the only plaintiffs. Suppose, however, that it was determined that 38% of the stock price was due to inflation over the whole class period. Plaintiff 1 would then have a claim of $380 and Plaintiff 2 would have a claim of $190, again entitling Plaintiff 1 to two-thirds of the settlement fund. In this case, which covers both retention and in-and-out plaintiffs, the inflation percentage becomes irrelevant, and it may not be included in the plan of allocation. However, it then is unclear whether damages are really based on a constant percentage inflation methodology, or instead are simply based on plaintiffs' actual losses without regard to any inflation methodology.

With these caveats in mind, we then turn to the results of our empirical investigation. Within those classes of securities for which only one inflation methodology was used in the settlement, we find that 21, or 18.4%, of cases involved a constant dollar approach. Seven

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[19] The conflating of the bounce-back limitation with the actual damage measure may not be limited to practitioners. Philip H. Dybvig, Ning Gong, and Rachel Schwartz in "Bias of Damage Awards and Free Options in Securities Litigation," *Journal of Financial Intermediation*, 2000 note first that the PSLRA caps damages, yet their paper later relies on "basing compensation for damages on the difference between the purchase price and the price on the day the misrepresentation is corrected," and generally ignores any interaction between the bounce-back cap the standard inflation analyses. Perhaps similarly, Edward A. Dyl, "Estimating Economic Damages in Class Action Securties Fraud Litigation," *Journal of Forensic Economics*, 1999, notes that the bounce-back measurement is a cap on allowable claims, but still refers to it as an "approach for estimating damages." Dyl also argues for use of a constant true value methodology, particularly in cases where there is no significant relationship of the company's stock price with a market index following the corrective disclosure(s).

cases, or 6.1%, involved a constant percentage methodology. A hefty 55 cases, or 48.2%, were based on a constant true value, and 31 cases, or 27.2%, were either based on some other methodology or else the methodology used was not described in sufficient detail for classification.

A slightly different pattern is displayed in those cases where a combination of inflation methodologies was used. Here we count how many times each methodology showed up in the plan of allocation for a class of securities. There were 33 usages of the constant dollar method, representing 25.4% of the usages in those settlements with more than one inflation methodology. A constant percentage inflation was employed 36 times, or in 27.7% of all usages. There were 55 usages, or 42.3%, of the constant true value, and 6 usages, or 4.6%, of some other methodology.

V. Discussion of Results

The results presented above indicate that the constant dollar inflation is slightly more popular than the constant inflation methodology. Also, this difference is essentially entirely based on settlements that involve only a single inflation methodology, with the two being used roughly the same number of times in settlements using more than one methodology.

Another result that jumps out of the data is the high usage of settlements based on constant true values, particularly in the case where only one inflation methodology is used. This result is potentially disturbing. As noted above, because a constant true value rule only makes sense if the stock price would have stayed the same throughout the class period, these should be cases where the issuer had absolutely no truthful material announcements that either positively or negatively affected the stock price throughout the entire class period. Given that many of these constant true values were not at trivial stock prices, this says that many settlements are based on the implicit assumption that many companies that had legitimate businesses never suffered any meaningful changes in the true value of their business over periods of months or years, a result that appears highly implausible.

By implicitly assuming that all stock price declines during the class period are due to fraud, these settlements do not appear to be following the general rule of testing for materiality and then accounting for loss causation through an event study or other methodology to separate out fraudulent from non-fraudulent components of stock price movement. This means that because the allowable claims are not tied to plaintiffs' actual damages, some plaintiffs will get an overly large share of the settlement fund while others will receive an inadequate share. While one cannot insist on exact precision in having plaintiffs' recoveries be proportional to their losses, the use of a constant true value methodology can result in extreme deviations from that goal. For example, suppose one plaintiff purchases a stock at $50 and then the company suffers a legitimate loss that lowers its stock price to $40. A second plaintiff who purchases at the lower price will have a damage claim only $10 less than the first. Therefore, the first plaintiff will be able to claim her entire $10 non-fraud related loss as a damage claim and have a portion of that loss recovered at the expense of other plaintiffs who may have legitimate damage claims.[20] This result suggests that there are many settlements where some plaintiffs with legitimate fraud-related losses, assuming that defendants are indeed liable in the matter, are receiving an inadequate settlement and in fact subsidizing other plaintiffs whose losses are not wholly related to the alleged fraud. Such a result is not wholly unexpected, given that plaintiffs' counsel are generally compensated based on the total recovery of the class, and not whether the recovery is allocated appropriately. However, unless one is willing to assume that these cases represent situations where there was no material non-fraud related announcement affecting a company's stock price during the class period, it does suggest that many recent settlements are not treating all plaintiffs equitably.

VI. Conclusion

This paper presents an analysis of the theoretical justifications for different inflation methodologies. We conclude that the constant dollar and constant percentage inflation methodologies serve as useful idealized paradigms for modeling various types of different allegations. The constant true value methodology is generally too restrictive except when that

[20] Denise N. Martin, Vinita M. Juneja, Todd S. Foster, and Frederick C. Dunbar in "Recent Trends IV: What Explains Filings and Settlements in Shareholder Class Actions?" (November 1996) report that actual settlements only rise between 0.52 and 0.57 log points for each log point increase in plaintiffs' claimed damages, with the difference in measurements being due to use of different control variables in their regression analysis. (See Table 19.) This finding implies that if one plaintiff is able to marginally increase her claim illegitimately, defendants pay for approximately 52 to 57% of her extra recovery while the remainder is borne by other plaintiffs who see their recoveries shrink.

true value is at or near zero. When considering the interaction between inflation and loss causation, there is no difficulty with the constant dollar inflation, while other inflation methodologies may overstate the proper damage claim. In practice, however, the use of a constant true value is highly popular in settlement plans of allocation, including cases where that value is not close to zero. After that, the constant dollar inflation appears somewhat more popular than the constant percentage inflation. Whether these usages are indeed justified in the individual cases in which they were applied is a question we do not address; however, given the general lack of previous discussion about the proper inflation methodology and the interaction between inflation and loss causation, one must at least question whether all plaintiffs have been treated equitably in these settlements.

Appendix 1

This appendix shows the pre-disclosure (P_1) and post-disclosure (P_2) valuations of a theoretical company. The constant percentage (P_1/P_2-1) and constant dollar (P_1-P_2) inflations are then calculated. (The constant percentage inflation is shown as simply the ratio of prices, P_1/P_2, for ease of presentation.)

1) Disclosure: Earnings will experience a one-time decrease of k at the end of the next period:

$$P_1 = X/r \quad ; \quad P_2 = X/r - k/(1+r)$$

$$\Rightarrow \frac{P_1}{P_2} = \frac{X}{X - \frac{rk}{1+r}} \quad ; \quad \boxed{P_1 - P_2 = -k/(1+r)}$$

2) Disclosure: Earnings will experience a shortfall of k in every period beginning the end of next period:

$$P_1 = X/r \quad ; \quad P_2 = \frac{X-k}{r}$$

$$\Rightarrow \frac{P_1}{P_2} = \frac{X}{X-k} \quad ; \quad \boxed{P_1 - P_2 = -k/r}$$

3) Disclosure: Earnings will grow at a rate of $g_2 = ag_1$ instead of g_1 (a<1), from a base of X, after next period:

$$P_1 = X/(r-g_1) \quad ; \quad P_2 = X/(r-g_2)$$

$$\Rightarrow \boxed{\frac{P_1}{P_2} = \frac{r-g_2}{r - g_2/a}} \quad ; \quad P_1 - P_2 = X\left(\frac{1}{r - g_2/a} - \frac{1}{r-g_2}\right)$$

4) Disclosure: Instead of X in every period hereafter, earnings will be aX (a<1):

$$P_1 = X/r \quad ; \quad P_2 = aX/r$$

$$\Rightarrow \boxed{\frac{P_1}{P_2} = \frac{1}{a}} \quad ; \quad P_1 - P_2 = X/r\,(1-a)$$

Appendix 2

This appendix shows why the constant percentage inflation method is generally not appropriate for companies with positive amounts of debt.

Consider a company with a present discounted value of future cash flows equal to $300. Suppose further that the company has $100 in debt. Its equity would then be worth $200. Finally, assume that, unknown to the market, 50% of the company's assets (and their expected cash flows) simply do not exist. The market should then value the company at $150, with $100 going to debt and $50 to equity. The inflation in the stock, $150, equals 75% of the $200 stock price.

Now suppose that the company is legitimately able to double its profit margin on all operations. The market then believes that the present value of future cash flows is now $600, and allocates $100 to debt and $500 to equity. However, in truth, the company is only worth $300, of which $100 should be allocated to debt and $200 to equity. Thus, $300 of the $500 that the market believes the equity to be worth is actually fraudulent. Therefore 60% of the stock price is attributable to the fraud.

This example is also illustrated in the table below.

	Case 1			Case 2	
	Market View	Truth		Market View	Truth
Debt	100	100		100	100
Equity	200	50		500	200
Total Enterprise Value	300	150		600	300

Inflation = 150/200 = 75%

Inflation = 300/500 = 60%