Event Studies and the Law: Part I: Technique and Corporate Litigation

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Event studies are among the most successful uses of econometrics in policy analysis. By providing an anchor for measuring the impact of events on investor wealth, the methodology offers a fruitful means for evaluating the welfare implications of private and government actions. This article is the first in a set of two that review the use and impact of the event study methodology in the legal domain. This article begins by briefly reviewing the event study methodology and its strengths and limitations for policy analysis. It then reviews in detail how event studies have been used to evaluate the wealth effects of corporate litigation: defendants experience economically meaningful and statistically significant wealth losses upon the filing of the suit, whereas plaintiff firms experience no significant wealth effects upon filing a lawsuit. Also, there is a significant wealth increase for defendant firms when they settle a suit with another *firm*, in contrast to other types of plaintiffs, and in contrast to the settling plaintiff firms. These findings suggest that, at a minimum, lawsuits are not a value-enhancing way for corporations to settle their disagreements with other corporations. In addition, the market appears to impose a higher sanction on firms than actual criminal sanctions, and reputational losses are of equal magnitude for civil fines as for criminal ones. The article concludes with some recommendations for researchers: the standards for conducting an event study are well established; researchers can increase the power of an event study by increasing the sample size,

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and by narrowing the public announcement period to as short a time frame as possible. The companion article reviews the use of event studies in corporate law and regulation.

1. Introduction

Event studies are among the most successful uses of econometrics in policy analysis. The methodology, which studies the movement of stock prices due to specific events (unexpected actions by managers or policy makers that are expected to affect firm values) was originally developed to test the hypothesis that the stock market was efficient—that publicly available information is impounded immediately into stock prices such that an investor cannot earn abnormal profits by trading on the information after its release. As evidence accumulated that the stock market was efficient, the methodology came to be used instead to value the event under study. It is through this latter usage that event studies have influenced policy analysis, particularly in corporate and securities law. This is no doubt because there is a natural fit between the methodology and those fields of law: the benchmark for evaluating the benefit of corporate and securities laws is whether they improve investor welfare, and this can be ascertained by what event studies measure, whether stock prices have been positively affected.

The event study methodology is well accepted and extensively used in finance. Event study results have been used in several hundred scholarly articles in leading academic finance journals to analyze corporate finance issues, such as stock repurchases and stock splits and the relation between stock prices and accounting information, by examining the impact of earnings releases. Its use in policy analysis in recent years has become more widespread, and it is the interaction between law and financial econometrics that is the focus of this review.

This is the first of a set of two articles. This one begins by briefly reviewing the event study methodology and its strengths and limitations for policy analysis. It then reviews in detail how event studies have been used to evaluate the wealth effects of one broad area of public policy: corporate litigation. The article concludes with a summary and our recommendations for use of event study results in policy analysis. The second

article, Bhagat and Romano (2002) focuses on the use of event studies in another broad area of public policy: corporate law and corporate governance.

2. A Guide to Event Studies

The price of a stock reflects the time- and risk-discounted present value of all future cash flows that are expected to accrue to the holder of that stock. According to the semistrong version of the efficient market hypothesis, all publicly available information is reflected completely and in an unbiased manner in the price of the stock, such that it is not possible to earn economic profits on the basis of this information.[1] Therefore, only an unanticipated event can change the price of a stock. This change should equal the expected changes in the future cash flows of the firm or the riskiness of these cash flows. Thus, an event is said to have an impact on the financial performance of a firm if it produces an abnormal movement in the price of the stock. Broad stock market movements are usually subtracted from the stock's price movement in estimating the abnormal return. Event studies apply conventional econometric techniques to measure the effect of specific events, such as actions by firms, legislatures, and government agencies, on the stock price of affected firms. Their special use for policy analysis is that they provide an anchor for determining value, which eliminates reliance on ad hoc judgments about the impact of specific events or policies on stock prices.

2.1. Mechanics of Event Studies

An event study has four components: defining the event and announcement day(s), measuring the stock's return during the announcement period,

1. The efficient-market hypothesis has been subjected to extensive empirical testing; perhaps the most intensive and extensive testing of any hypothesis in all of the social sciences. Most tests find evidence consistent with the efficient-market hypothesis. Some studies find that the stock price responds within minutes of a corporate announcement such as a stock offering (see Barclay and Litzenberger, 1988). Most finance scholars hold the view that the stock market in the United States is semistrong form efficient (Welch 2000). But controversy regarding the efficient-market hypothesis lingers. This controversy is based on issues regarding the definition and measurement of risk, and the relationship between risk and return. There is, however, agreement that these issues do not invalidate the event study methodology; see Brown and Warner (1985) and Fama (1990).

estimating the expected return of the stock during this announcement period in the absence of the announcement, and computing the abnormal return (actual return minus expected return) and measuring its statistical and economic significance.

In order to conduct an event study, the researcher first defines the event under investigation. Events are usually announcements of various corporate, legal, or regulatory action or proposed action. Examples of events that have been studied are takeovers, equity offerings, change in state of incorporation, adoption of antitakeover provisions, filing of lawsuits against corporations, deaths of corporate executives, and product recalls. After defining the event the researcher searches for the first public announcement of the event. Identification of the first public announcement of the event is critical since, under the semistrong form of the efficient-market hypothesis, the impact of the event on the value of the firm would occur on the announcement date. Historically, the *Wall Street Journal Index* has been a popular source for announcement dates. More recently, computer accessible databases such as *Lexis-Nexis* and the *Thompson Financial Securities Data* are being increasingly used.

Conceptually, the announcement date is straightforward: it is the "day" the public is first informed of the event.[2] However, identification of this date can sometimes be nontrivial. Consider the announcement of a tender offer. It is possible and probable that news of the tender offer may have leaked to some market participants before the first public announcement. If such is the case then some impact of the tender offer on the firm's share price would occur before the public announcement. Some researchers have attempted to address this issue by considering the period several weeks (or months) before and through the announcement day as the announcement period. However, this obvious solution has two problems, one conceptual and the other technical. Conceptually, it is unclear whether the leakage occurs over a few days, weeks, or months. Technically, as we increase the length of the announcement period, the noise-to-signal ratio increases,

2. Currently, most event studies consider daily returns; hence the announcement period is typically a day. However, historically, some event studies have considered monthly returns—where the announcement need be identified for only a particular month; see the classic study by Fama et al. (1969). More recently, announcements have been identified to the nearest minute, and returns have been computed over minute and trade intervals such that the event study is conducted with intraday data; see Barclay and Litzenberger (1988).

and it becomes increasingly difficult to measure the impact of the tender offer on share price with precision; we will discuss this later in the article. Aside from news-leakage issues, at the time the tender offer is announced there is uncertainty over whether it will be successful, and if successful, over the terms of the final offer. Sometimes the final resolution may not be known for months or even years.

Finally, some events may have several distinct event dates. For example, the enactment of a statute involves many different events, each of which may provide new information to investors regarding the likelihood of passage: when a bill is introduced, when a committee holds hearings on the bill, when one legislative chamber votes on the bill, when a conference committee approves a final bill, and when the executive signs the bill (if there is uncertainty over whether or not the bill will be vetoed). In this context, rather than treat the entire interval from bill introduction to executive signature as the event and run into the problems discussed, the researcher can adapt the methodology to permit each event date to be identified separately; however, in doing so the researcher's bias and priors on what is a significant or relevant event enter the analysis.

After defining the event and announcement period, one measures stock returns for this period. If daily data are being used, this is straightforward: the return is measured through closing prices. Often there is uncertainty if the announcement is made before or after the close of trade on the exchange. To address this, the returns from the next day are often included.

Calculation of the third component is more complicated. Although it is straightforward to measure the actual return for the announcement period, determination of the impact of the event itself on the share price is less so. To measure this impact, the *expected return* must be subtracted from the actual announcement-period return. This expected return is the return that would have accrued to the shareholders in the absence of this or any other unusual event. The finance literature has considered several models of expected returns. These models can broadly be classified as statistical models or economic models.

Statistical models. The constant expected returns model is

$$R_{it} = \mu_i + e_{it}, \tag{1}$$

where R_{it} is the return for stock i over time period t, μ_i is the expected return for stock i, and e_{it} is the usual statistical error term.

The market model is

$$R_{it} = a_i + b_i^* R_{mt} + e_{it}, \tag{2}$$

where, a_i and b_i are firm-specific parameters, and R_{mt} is the market return for the period t.

Economic models. Capital Asset Pricing Model (CAPM) is

$$R_{it} = R_f + \beta_i^*(R_{mt} - R_f) + e_{it}, \tag{3}$$

where R_f is the risk-free rate and β_i is the beta or systematic risk of stock i.

Arbitrage Pricing Theory is

$$R_{it} = \delta_0 + \delta_{i1} F_{1t} + \delta_{i2} F_{2t} + \cdots + \delta_{in} F_{nt} + e_{it} \tag{4}$$

where $F_1, F_2, \ldots, F_n$ are the returns on the n factors that generate returns, and δ are the factor loadings.

The statistical models are simple models of price formation that are not grounded in a specific economic theory. The economic models are derived from specific economic theories of asset price formation. One can think of the economic models as placing certain restrictions on the statistical models (that is, on the slopes and intercepts being estimated).

The choice of a benchmark model can impact both the variance and mean of the abnormal returns. Simulations using actual returns suggest that abnormal returns estimated using statistical models as benchmark are better specified; see Brown and Warner (1985). In addition, since several studies have found evidence inconsistent with the economic models, in particular CAPM, the use of such restrictions is not appropriate. Hence, most researchers have begun to rely on the statistical models to estimate the expected returns during the announcement period. Researchers usually estimate these statistical models by using between 100 and 200 daily returns in the period preceding the announcement period. The unexpected announcement period return, also known as the *abnormal return*, is computed as the actual return minus the estimated expected return. This abnormal return is the estimated impact of the event on the share value.

The fourth and final step is to compute the statistical significance of this abnormal return. The standard error of the residuals from the estimated statistical model can be used as an estimate of the standard error

for the announcement-period abnormal return. However, since individual stock returns are quite volatile, this standard error can be quite high relative to the abnormal return. Event studies usually consider a sample of firms that have made or been the subject of the same type of announcement; each firm's announcement typically has been made on a different calendar day. Another benefit of this approach is that it increases the likelihood that no other information besides the event under study will be valued, since any additional unexpected information disclosed on one firm's announcement date will wash out with that on other firms' announcement days.

The abnormal returns of this sample of firms are averaged to obtain the *average abnormal return.* This average abnormal return is the estimated impact of the event on the share value. Next, the residuals from the estimated statistical model for these firms are averaged in *event time.* Usually the announcement day is defined as *event day 0.* Likewise, t days before (after) the announcement day is defined as *event day* $-t$ (*event day* $+t$). Finally, the standard error of these averaged residuals is used as an estimate of the standard error of the average abnormal return. Under the null hypothesis that the event under study has no impact on firm value, the expected average abnormal return is zero. Additionally, assuming that the announcement period returns for the sample firms are independently and identically distributed, then by the central limit theorem the average abnormal return is normally distributed with mean zero.

The above estimate of the standard error of the average abnormal return would be appropriate if the announcement period abnormal return had the same variance as the estimation period residuals. However, substantial evidence in the finance literature suggests that stock returns in the announcement period are typically more volatile. Brown and Warner (1985) have suggested the use of cross-sectional test statistics when there is an increase in return variance during the announcement period. The standard error of the announcement-period returns for the sample firms is used as an estimate of the standard error of the average abnormal return. Nonparametric tests, such as the Fisher sign test and the Wilcoxon signed rank test, are also conducted on the announcement-period returns; the usual null hypothesis is that the median announcement-period return is zero.

2.2. Statistical Power of Event Studies

If an event changes firm value by a specific amount, say, 1%, can the event study technique detect it with some statistical precision? Equally important, from a statistical, financial, and legal viewpoint is the following question: if an event has no impact on firm value, that is, the announcement-period abnormal return is zero, can the event study technique provide this inference with some statistical precision? These questions can be addressed by considering the statistical power of event studies.

The power of a test statistic is considered in the context of a null hypothesis and an alternate hypothesis. (Hopefully, the alternate hypothesis would be economically meaningful.) In the context of event studies, the usual null hypothesis is that the event has no impact on firm value. An interesting alternate hypothesis could be that the event increases firm value by 1%. Under the assumption that the alternate hypothesis is true, the power of the event study in this context is the probability of observing a statistically significant test statistic. Brown and Warner (1985) and MacKinlay (1997) have studied the power of test statistics typically used in event studies. These authors show that the power of the event study technique improves as the number of firms in the sample increase, as the number of days in the announcement window decrease, and as the alternative of a larger abnormal return is considered against the null hypothesis of zero abnormal return.

The following numerical examples from MacKinlay (1997, Table 2) illustrate the power of the event test methodology and how the power can be enhanced.

For a one-day announcement window, a sample size of 25 firms, and a two-sided test with a 5% significance level, the probabilities of detecting an abnormal return of 0.5%, 1%, and 2%, are 24%, 71% and 100%, respectively.

- If the sample size were increased to 50 firms, the probabilities of detecting an abnormal return of 0.5%, 1%, and 2%, are 42%, 94%, and 100%, respectively.
- If the sample size were increased to 100 firms, the probabilities of detecting an abnormal return of 0.5%, 1%, and 2%, are 71%, 100%, and 100%, respectively.

• For a two-days announcement window (or equivalently, doubling of the standard deviation of the event-day abnormal return), and a sample size of 25 firms, the probabilities of detecting an abnormal return of 0.5%, 1%, and 2% are 10%, 24% and 71%, respectively.

• For this two-days announcement window and a sample size of 50 firms, the probabilities of detecting an abnormal return of 0.5%, 1%, and 2% are 14%, 42%, and 94%, respectively.

• For this two-days announcement window and a sample size of 100 firms, the probabilities of detecting an abnormal return of 0.5%, 1%, and 2% are 24%, 71%, and 100%, respectively.

The above findings suggest that the power of the event study diminishes as the sample size decreases. An important question is, can an event study be conducted with just one firm, that is, is a sample size of one acceptable? This question is especially relevant in court cases or regulatory injunctions involving only one firm. Conceptually, a sample of one is a rather small sample, but this by itself does not invalidate the event study methodology. However, the statistical power with a sample of one is likely to be quite low. First, the variability of (abnormal) returns of a portfolio with just one stock in it is significantly higher than a portfolio with even a few, say five, stocks in it. Any standard finance or investment textbook will have a graph depicting the sharp drop in variance of portfolio returns as the number of stocks in the portfolio increases from one, to five, to ten; after about 50 stocks in the portfolio, the decrease in variance is quite small. Second, it is plausible that the announcement-period return of an announcing firm will be affected by other information unrelated to the event under study. If a sample of one is considered, it is quite difficult to determine the separate effects on firm value of the announcement and of the unrelated information item(s). If the sample has several firms, then the effect on firm value of such unrelated information is likely to cancel out. As the sample size increases, the effect on firm value of such unrelated information (goes to zero) becomes less and less significant.

The above findings also suggest that the power of the event study methodology diminishes substantially as the event period is increased from one to just two days. During the past decade an increasing number of finance studies have considered abnormal returns for long-horizon

windows of several *years*. Such studies have considered abnormal returns over *twelve to sixty months* after the announcements of various corporate events like mergers, share repurchases, initial public and seasoned equity offerings, spinoffs, stock splits, and dividends. Examples of such studies include Ikenberry, Lakonishok, and Vermaelen (1995); Loughran and Ritter (1995); Brav and Gompers (1997); McConnell, Ozbilgin, and Wahal (1999); Desai and Jain (1999).

There are two reasons for studying the long-horizon window of several years after an announcement. First, the market may be unable to fully understand and incorporate the impact of the announcement on the company's value. Over time the market gets the opportunity to fully understand and incorporate the impact of the announcement on the company's value. Under this explanation, no new information related to the first announcement is released in this postannouncement period; hence this reason presumes a semistrong form *inefficient* market. Second, new information pertinent to the initial announcement may become known to the market participants in the months or years subsequent to the announcement. For example, the initial announcement could be a takeover offer announcement. Before the offer is finalized and completed several events could occur that might change the likelihood of the success of the initial offer. Examples of such events include the arrival of a second bidder, litigation by target management, and regulatory objections (see Bhagat, Hirshleifer, and Noah, 2001). In this scenario, one way to estimate the full impact of the initial event would be to consider the period from the initial announcement through final resolution—a period that could extend several years in some cases.

Kothari and Warner (1997), Barber and Lyon (1997), and Lyon, Barber, and Tsai (1999) have raised serious concerns about the specification and power of the event study methodology when long-horizon windows of several years are considered. Kothari and Warner find that the event study test-statistics used in the above-mentioned studies are generally misspecified in the sense that they reject the null hypothesis of normal performance when there is no abnormal performance too frequently, given the significance level. Lyon, Barber, and Tsai suggest ways to construct properly specified test-statistics. However, these authors caution that though these test statistics appear to be well specified for random samples, they are not well specified for nonrandom samples. Given that tests of most interest-

ing finance and legal hypotheses are likely to lead to the construction of nonrandom samples, the concern with the misspecification of the long-run test-statistics remains. Finally, Lyon, Barber, and Tsai document the power of the long-horizon test statistic to detect abnormal performance when it is actually present. Using state-of-the-art techniques, for a twelve-month buy-and-hold abnormal return,[3] a sample size of 200 firms, and a one-sided test with a 5% significance level, the probabilities of detecting an abnormal return of 5%, 10%, and 20%, are 20%, 55% and 100%, respectively. As the horizon increases beyond twelve months, and the sample size decreases, the power of the technique would further diminish. For these reasons, these authors conclude that "the analysis of long-run abnormal returns is treacherous."

2.3. Cross-Sectional Determinants of the Stock Market's Reaction

Some researchers have sought to provide insight into the cross-sectional determinants of the stock market's reaction to the announcement of an event by examining the relation between the size of the abnormal return (AR) identified in an event study and characteristics specific to the event observations, that is, cross-sectional differences in the firms in the study. This approach can be used, for instance, where there are multiple hypotheses for the source of a wealth effect. The AR is the dependent variable in an ordinary least squares regression on the firm characteristics of interest:

$$AR_j = d_0 + d_1 x_{1j} + \cdots + d_M x_{Mj} + e_j, \tag{5}$$

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 e_j is the zero mean disturbance term that is uncorrelated with the x's. Additionally, d_m, $m = 0, \ldots, M$ are the regression coefficients.

3. Buy and hold returns mimic the returns of an investor that buys and holds the portfolio for the entire period under study; usually an equal amount is assumed to be invested in each of the stocks in the portfolio and no rebalancing of the portfolio is done. Cumulative abnormal returns (abnormal returns summed or cumulated over the days under study) assume that the portfolio is rebalanced daily such that an equal amount is assumed to be invested in each of the stocks in the portfolio at the start of each day. Mathematically, buy and hold returns are computed as products rather than sums.

This approach has been used in a variety of contexts. We note here an illustration from the methodology's application to assessing the wealth effects of corporate litigation discussed in section 3. Bhagat, Brickley, and Coles (1994) provide an example of its use in determining the source of the significant negative wealth effects experienced by corporate defendants. They find that the negative abnormal returns from litigation are significantly related to variables proxying for the defendant's proximity to financial distress.

An interpretational concern involving cross-sectional models is whether the abnormal return is related to the firm characteristics not only through the wealth effect identified in the event study but also through investors' anticipation of the event. Namely, investors may expect that firms with the specified characteristics will be subject to the event under study. In this case the linear specification will not uncover a relation between the variables. Moreover, the greater the connection between the specified characteristics and the occurrence of the event—that is, the more highly the event is anticipated—the less likely a relation will be found in the cross section because the information effect (the AR) will be that much smaller (Bhagat and Jefferis, 1991; Prabhala, 1997). MacKinlay (1997) provides an overview and further references. The issue also implicates event studies in general, for if the anticipation is sufficiently great, there will be no announcement effect; given this possibility, some researchers have proposed the use of a conditional approach instead of the conventional approach that we have discussed (for example, Acharya 1988). However, Prabhala (1997) shows that the significance test for the existence of an information effect in the traditional methodology is, in fact, well specified. He also shows the circumstances under which the regression coefficients on firm characteristics in traditional cross-sectional models are proportional to the true cross-sectional parameters, and hence the associated t-statistics may be interpreted as a conservative (lower bound) estimate of the parameters' true statistical significance. We therefore conclude that the principal use of cross-sectional models will continue to be for refinement of researchers' theories for undertaking their event studies by explaining the results of the standard model—that is, for relating the size and sign of the abnormal returns to specified firm and event characteristics.

3. Shareholder Wealth Implications of Corporate Lawsuits

In the 1980s and 1990s business frequently complained about a litigation explosion and the costs associated with legal disputes, raising concerns that the U.S. legal system affected firms' competitiveness in global markets. Surveying corporate legal department budgets, Economic Analysis Group, Craig Consulting Company, and Endispute estimated that salaries to in-house lawyers and fees to outside counsel for the 1,000 largest public companies hit $20 billion in 1991.[4] Large liability or settlement payments undoubtedly dwarf direct legal costs. Indeed, some mass torts, such as the breast-implant cases against Dow Corning and the Dalkon Shield cases against A. H. Robins, have threatened the existence of defendant firms, forcing them into insolvency proceedings.

It is, however, possible that estimates of business' legal costs are overstated, reflecting political agendas or overreaction to media coverage of a few spectacular cases. Many large publicized damage awards, for example, are overturned on appeal or significantly reduced in a settlement (Shanley and Peterson, 1987). In addition, much corporate litigation involves contract disputes between firms.[5] But concerns over litigation have continued into the 1990s: tort reform was one of ten points in the Republican party's "Contract with America" 1994 campaign platform, under which it gained a majority in the House of Representatives for the first time in 40 years, and successful litigation initiatives against tobacco companies that produced a settlement of over $200 billion have led to other industry targets, such as health care providers.

Event studies can be used to identify and measure the costs of lawsuits against firms, and they have been particularly used to evaluate the costs of interfirm litigation. The results are quite uniform: when the costs and benefits to both parties are computed, litigation is not a positive net present value event for both firms considered together. This result is not surprising:

4. An article in Forbes, citing statistics from a RAND study on tort litigation, estimated the direct costs of all lawsuits, including those involving business, to be as high as $117 billion a year (Spencer, 1992, p. 40). Another estimate (p. 41) placed litigation costs as high as 2.5% of GNP.

5. For example, a RAND study of Fortune 1,000 companies found that contract disputes between firms constituted the largest single category of federal civil suits (Dungworth and Pace, 1990).

it is an impetus motivating the successful move to greater use of alternative dispute resolution, particularly in the corporate context.

3.1. Wealth Effects of Corporate Litigation

The primary focus in the literature has been on "leakages" in the litigation process: negative wealth effects upon netting the parties' gains and losses. For example, Cutler and Summers (1988) examine the Pennzoil-Texaco lawsuit, which involved a claim of tortious interference of a merger contract, and find significant costs to both parties from the dispute, with the losses for the losing defendant Texaco being larger than the gains for the winning plaintiff Pennzoil. The combined drop in value for the two firms was $2 billion. They attribute the loss mainly to an increase in the probability of financial distress for Texaco. Engelmann and Cornell (1988) study the wealth implications around filings, settlements, and verdicts for a sample of five interfirm disputes. They too observe combined wealth losses, or leakages, to the litigating parties. Bhagat, Brickley, and Coles (1994) examine the market reaction to lawsuit filings and settlements for a much larger sample of 550 interfirm disputes. They observe combined wealth losses arising from lawsuit filings and find that these leakages are a result of increased probability of financial distress for the defendant. In addition, they find that defendant firms gain upon the announcement of a settlement.

Ellert (1975) examines the market responses to announcements of legal challenges to mergers under Section 7 of the Clayton Act by the Federal Trade Commission and Department of Justice over the period 1950–1972. During the month of the announcement of the suit, the market adjusts defendant firm value downward by about 2%. Bizjak and Coles (1995) analyze a more homogeneous but still large sample of interfirm disputes—private antitrust suits. To our knowledge, this is the only study to find a positive stock market reaction to plaintiffs upon any sort of lawsuit filing. They also find that the joint wealth effects associated with the announcement of a filing tend to be negative and that leakages in antitrust disputes are attributable to court-imposed behavioral restraints, the likelihood of follow-on suits, and an increased likelihood of financial distress. Moreover, they confirm that factors which affect the costs of litigation also affect behavior in suit, settlement, and trial. In their sample of antitrust lawsuits, the parties are more likely to settle when the suit involves poten-

tial restrictions on the defendant's business practices and when there is the potential for financial distress.

Event studies have also been used to address the validity of the government's antitrust actions against various corporations. The argument goes that for a corporation exercising market power, the government's antitrust action against it will lower its share price *and* increase the share price of its competitors. The competitors will experience a positive reaction since the government's antitrust action increases the odds that these competitors will be competing in an industry without a dominant company that might be exercising market power. Bittlingmayer and Hazlett (2000) use this intuition to evaluate the U.S. Department of Justice's recent antitrust action against Microsoft. They find evidence inconsistent with the joint hypothesis that Microsoft's behavior has been anticompetitive and that the antitrust enforcement enhances economic efficiency.

Finally, Bhagat, Bizjak, and Coles (1998) analyze a large sample of lawsuits in which at least one side, plaintiff or defendant, is a corporation. To estimate the implications of litigation for shareholder wealth, they examine the abnormal stock market reaction to filing and settlement announcements. They find that the average wealth loss for a defendant is 0.97% of the market value of the equity, or $15.96 million. They further test whether characteristics of the suit, such as legal issue, type of opponent, and firm characteristics (such as firm size and proximity to bankruptcy) have power to explain cross-sectional variation in these wealth effects.

Bhagat, Bizjak, and Coles find that no matter who brings a lawsuit against a firm, be it a government entity, another firm, or private citizen, defendants experience economically meaningful and statistically significant wealth losses upon the filing of the suit. Furthermore, they find some evidence that the identity of the plaintiff has an influence on the wealth effects upon filing. Defendants involved in government suits suffer larger declines in shareholder wealth (−1.73%) than defendants involved in lawsuits with other firms (−0.75%) or with private parties (−0.81%). This result is consistent with the notion that government agencies have more leverage and resources at their disposal to use in a legal battle or the type of suit most frequently filed by government agencies, such as an environmental action, is typically more serious—or both. Indeed, they do find that certain types of litigation are more costly for defendants. Environ-

mental suits (−3.08%), product liability suits (−1.46%), and violations of security laws (−2.71%) result in significantly greater wealth losses for defendant firms, compared to disputes involving antitrust or breach of contract issues. It appears that, at least for some types of suits, the actual or potential lawsuit is associated with a large decline in shareholder wealth and a corresponding nontrivial deterrent effect. The results of these and other studies that consider the impact of litigation on corporate value are summarized in Tables 1–3.

Bhagat, Bizjak, and Coles also find that the defendant wealth effect on announcement of a filing is significantly positively related to the size of the firm and, in some specifications, significantly negatively related to the firm's proximity to bankruptcy. One possible explanation for this effect of firm size is that larger firms can have more bargaining power or more resources to devote to the legal dispute (e.g., because of better access to capital markets or "deep pockets"). The results on proximity to bankruptcy are consistent with other work that has identified potential bankruptcy costs as an important indirect cost of a legal dispute (Bhagat, Brickley, and Coles, 1994; Bizjak and Coles, 1995; and Cutler and Summers, 1988).

For plaintiff firms, Bhagat, Bizjak, and Coles find no significant wealth effects associated with lawsuit filings. They also find that the identity of the defendant—that is, whether the defendant is another firm, a government agent, or private citizen—and the legal issue are not related to the stock price change of the plaintiff when a suit is filed. They are, accordingly, unable to detect in the data evidence of strong incentives for plaintiffs to sue.

Bhagat, Bizjak, and Coles' results indicate that when a defendant firm settles a suit with another firm there is a significant wealth increase. It is surprising that, in contrast, they can detect no significant wealth change for defendants upon announcement of a settlement when the opponent is a governmental entity or noncorporate private party. In addition, the wealth effect of a settlement for the defendant is unrelated to the legal issue. For plaintiff firms the wealth implications of settlements appear to be trivial. On average, they find no significant wealth gains or losses to plaintiff firms who settle a lawsuit, and neither legal issue nor the identity of the opposing party has power to explain variation in those returns. These data suggest that lawsuits are not positive net present value undertakings for plaintiffs, since the absence of positive abnormal returns on settlement

Table 1. Announcement-Period Abnormal Returns for Defendant Corporations by Opponent Type

Plaintiff	Study	Sample Period	Sample Size	Announcement Window: (Event Days)	Announcement Return (%)	*Z*-statistic
Another firm	BBC (1998)	1981–1983	239	Filing (−1,0)	−0.75**	−3.31
Government	BBC (1998)	1981–1983	110	Filing (−1,0)	−1.73**	−4.99
Private nonfirm	BBC (1998)	1981–1983	221	Filing (−1,0)	−0.81**	−2.67
Another firm	BC (1995)	1973–1983	343	Filing (−1,0)	−0.60**	−3.17
Stakeholders	KL (1993)	1978–1987	19	Allegation (−1,0)	−1.34	−1.21
Stakeholders	KL (1993)	1978–1987	25	Filing (−1,0)	−1.67*	−2.35
Government	KL (1993)	1978–1987	13	Allegation (−1,0)	−5.05**	−4.77
Government	KL (1993)	1978–1987	17	Filing (−1,0)	−0.93	−1.14
Stakeholders	KL (1999)	1979–1995	80	Filing (−1,0)	−1.02**	−2.86
Consumers	PR (2002)	1985–1995	15	Filing (−1,1)	−1.93**	−3.31
Another firm	BBC (1998)	1981–1983	12	Settlement (−1,0)	3.66**	3.29
Government	BBC (1998)	1981–1983	4	Settlement (−1,0)	−0.68	−0.22
Private nonfirm	BBC (1998)	1981–1983	12	Settlement (−1,0)	−1.06	−1.72
Stakeholders	KL (1993)	1978–1987	13	Settle/Verdict (−1,0)	−0.17	−0.49
Government	KL (1993)	1978–1987	10	Settle/Verdict (−1,0)	1.48	1.20
Stakeholders	KL (1999)	1979–1995	15	Verdict-Defense (−1,0)	−0.36	−0.51
Stakeholders	KL (1999)	1979–1995	193	Verdict-Plaintiff (−1,0)	−0.62*	−2.74
Stakeholders	KL (1999)	1979–1995	4	Settlement (−1,0)	−2.43	−1.35
Consumers	PR (2002)	1985–1995	25	Verdict-Plaintiff (−1,1)	0.33	0.73

Notes: Event day 0 is the publication date of the filing, allegation, or settlement. BBC (1998): Bhagat, Bizjak, and Coles (1998). BC (1995): Bizjak and Coles (1995). KL (1993): Karpoff and Lott (1993). KL (1999): Karpoff and Lott (1999). PR (2002): Prince and Rubin (2002).

*Significant at the 95% confidence level, two-tailed test.

**Significant at the 99% confidence level, two-tailed test.

Table 2. Announcement-Period Abnormal Returns for Plaintiff Corporations by Opponent Type

Defendant	Study	Sample Period	Sample Size	Announcement Window: (Event Days)	Announcement Return (%)	Z-statistic
Another firm	BBC (1998)	1981–1983	172	Filing (−1,0)	−0.25	−0.60
Government	BBC (1998)	1981–1983	26	Filing (−1,0)	−0.44	−0.80
Private nonfirm	BBC (1998)	1981–1983	51	Filing (−1,0)	0.71	0.34
Another firm	BC (1995)	1973–1983	86	Filing (−1,0)	1.24**	4.26
Another firm	BBC (1998)	1981–1983	8	Settlement (−1,0)	−0.77	−1.26

Notes: BBC (1998): Bhagat, Bizjak, and Coles (1998). BC (1995): Bizjak and Coles (1995).
**Significant at the 99% confidence level, two-tailed test.

Table 3. Announcement-Period Abnormal Returns for Defendant Corporations by Type of Legal Issue

Legal Issue	Study	Sample Period	Sample Size	Announcement Window: (Event Days)	Announcement Return (%)	Z-statistic
Antitrust	BBC (1998)	1981–1983	62	Filing $(-1, 0)$	−0.81	−1.52
Breach of contract	BBC (1998)	1981–1983	48	Filing $(-1, 0)$	−0.16	−0.59
Corp. governance	BBC (1998)	1981–1983	154	Filing $(-1, 0)$	0.08	0.64
Environment	BBC (1998)	1981–1983	27	Filing $(-1, 0)$	−3.08**	−5.32
Exclusive dealing	BBC (1998)	1981–1983	27	Filing $(-1, 0)$	−0.14	0.28
Patent infringement	BBC (1998)	1981–1983	33	Filing $(-1, 0)$	−1.50*	−2.42
Product liability	BBC (1998)	1981–1983	38	Filing $(-1, 0)$	−1.46**	−3.12
Disclosure laws	BBC (1998)	1981–1983	46	Filing $(-1, 0)$	−2.71**	−4.49
Antitrust-horizontal	BC (1995)	1973–1983	117	Filing $(-1, 0)$	−1.45**	−4.88
Antitrust-vertical	BC (1995)	1973–1983	105	Filing $(-1, 0)$	0.27	1.29
Fraud of stakeholders	KL (1993)	1978–1987	19	Allegation $(-1, 0)$	−1.34	−1.21
Fraud of stakeholders	KL (1993)	1978–1987	25	Filing $(-1, 0)$	−1.67*	−2.35
Fraud of government	KL (1993)	1978–1987	13	Allegation $(-1, 0)$	−5.05**	−4.77
Fraud of government	KL (1993)	1978–1987	17	Filing $(-1, 0)$	−0.93	−1.14
Financial reporting fraud	KL (1993)	1978–1987	4	Allegation $(-1, 0)$	−4.60*	−2.00
Financial reporting fraud	KL (1993)	1978–1987	7	Filing $(-1, 0)$	−4.56*	−1.99
Punitive damages	KL (1999)	1979–1995	80	Filing $(-1, 0)$	−1.02*	−2.86
Product liability	PR (2002)	1985–1995	15	Filing $(-1, 1)$	−1.93**	−3.31

Notes: Event day 0 is the publication date of the filing, allegation, or settlement. BBC (1998): Bhagat, Bizjak, and Coles (1998). BC (1995): Bizjak and Coles (1995). Horizontal antitrust issues include horizontal price fixing, merger/joint-venture, asset accumulation, predatory pricing, and monopolization. Vertical antitrust issues include resale price maintenance, exclusive dealing, tying, territorial restrictions, dealer termination, and refusal to deal. KL (1993): Karpoff and Lott (1993). Fraud of stakeholders occurs when the firm is accused of cheating on implicit or explicit contracts with suppliers, customers, or employees. Fraud of government occurs when the firm is accused of cheating on implicit or explicit contracts with government agencies. Financial reporting fraud occurs when the firm is accused of misrepresenting the firm's financial condition. KL (1999): Karpoff and Lott (1999). Punitive damages are sought in cases involving product liability, fraud, business negligence, breach of contract, insurance claims, employment claims, asbestos claims, and vehicular accident claims. PR (2002): Prince and Rubin (2002). Product liability claims involving auto manufacturers.

*Significant at the 95% confidence level, two-tailed test.

**Significant at the 99% confidence level, two-tailed test.

cannot be explained by investor anticipation upon the lawsuit filing (there was no significant positive gain at the earlier date).

3.2. The Effect of Litigation on the Value of Brand Name, Trademarks, and Corporate Reputations

Klein and Leffler (1981) argue that a company's investments in brand names and trademarks provide implicit guarantees to consumers of quality products. These authors posit that consumers are willing to pay a premium for branded and trademarked products. They suggest that firms and consumers have an implicit contract—firms that produce higher-quality products have consumers who are willing to pay high prices. If the firm reneges and fails to maintain consistent quality, consumers do not repeat-purchase, and the company will not be able to recoup its investment in the brand name or trademark.

One event study has indicated that the value of a brand name is, indeed, substantial and related to quality assurance: Mitchell (1989) studied the stock price effect of Johnson & Johnson's recall of its Tylenol capsules after serious product tampering and found that by far most of the loss ($1.24 billion of $1.44 billion, a loss estimation based on the stock's relationship with the over-the-counter drug market, whose firms also were negatively impacted by the incident) represented a decline in the value of the brand name of the firm and the product, as out-of-pocket costs of the recall were about $200 million. This was a 14.3% decline in its stock price relative to its forecasted value. Although Tylenol's market share eventually recovered to close to pretampering levels, it never reached the level that was forecasted before the event, the sale of Tylenol tablets declined even though they had not been subject to the tampering, and the company delayed the introduction of new drugs.

Landes and Posner (1987) suggest a framework of company-consumer interaction that makes a brand name or trademark valuable to both consumers and corporations. First, consumers value a trademark because it reduces their mental and time costs of identifying and describing the product they want. Second, trademarks help reduce search costs for consumers. Suppose a consumer had a positive experience with a prior purchase of a brand or was recommended a brand by a friend who was satisfied from the use of the brand. The consumer would like to purchase that brand again with a minimum of effort searching for it. In the absence of a brand

name or trademark, the brand can be found by evaluating all attributes or searching among numerous brands. In contrast, search costs for the consumer are much lower when she is looking for a specific brand name or trademark and then purchases the brand.

Third, the consumer will find it worthwhile to search for the branded product that previously provided a positive experience, if the prior experience is a good predictor of future experience. In other words, the consumer will look for a trademarked brand only if it is of consistently high quality. Consumers cannot be repeatedly fooled about the quality, for instance, by false advertising about the quality of attributes. If the brand's quality is inconsistent, the consumer will not be able to use the brand name or trademark to relate past experience to future consumption experiences. The brand name or trademark will not lower search costs, because the consumer will have to search and evaluate attributes and brands. Consequently, the consumer will be unwilling to pay more for the branded product over the unbranded product. Firms will not be able to charge high prices for brands that have substandard quality. Hence, a firm that produces a brand with poor or inconsistent quality will not find a brand name or trademark to have as much value. This suggests that aside from potential bankruptcy costs, court costs, and punitive fines, corporations will incur a reputational cost if a lawsuit has an adverse impact on the value of their brand name and trademark.

The Klein and Leffler model, and its extension by Landes and Posner, would suggest that related-party corporate crime, where customers and other related parties may stop dealing with the corporation or otherwise change their willingness to pay for the defendant's product, will impose reputational damages, while third-party offenses will not. Alexander (1999) shows that this is in fact the case: corporations committing related-party crime (i.e., contract fraud) experience significant reputational losses, whereas those committing third-party offenses (i.e. violations of environmental law) do not. She further provides evidence on the reason for the observed reputational losses: for example, in 57% of the contract related-party criminal cases, customer dealings were suspended or terminated, whereas this occurred in only 14% of the third-party crime cases.

In a series of papers, Karpoff and Lott (1993, 1999) document the importance of reputational costs imposed on defendant corporations. On the basis of their empirical evidence they argue that criminal restitution,

civil penalties, and court costs comprise only about 7% of the shareholder wealth loss. They argue that the remaining 93% can be attributed to the reputational loss suffered by the defendant firms. The market thus appears to impose significant costs on firms for engaging in criminal conduct. This is also true for firms subject to punitive damage award lawsuits, as the median loss in market value over the announcement period regarding the litigation is far greater than the nominal cost of the awards; but the absolute dollar amount of reputational loss in these cases is about half as large as that for firms involved in criminal or civil fraud lawsuits.

Michael Block (1991) compares the stock price effects of corporate fraud with those of certain crimes referred to as *malum prohibitum* crimes, that is, crimes that have negligible effect on parties in contractual relations with the firm, such as tax evasion, money laundering, and currency-reporting violations. He finds significant negative price effects only for the fraud cases. This suggests that the reputational costs of corporate crime are fairly specific. As Block puts it, "Simple conviction of a criminal act does not generally stigmatize" (p. 414). Block further examines the stock price effects for certain civil fines—federal safety regulation violations by airlines. He again finds a significant negative price effect, of the same magnitude as that experienced by the firms charged with criminal fraud, −2.2%. This suggests that civil enforcement may be equally as effective in imposing reputational penalties as criminal enforcement.

Private civil litigation does not, however, appear to have similar reputational consequences (at least, in the absence of punitive damage awards): Prince and Rubin (2002) examine product liability litigation, and offer data suggesting that the significant negative stock price declines experienced by defendant firms upon lawsuit filing approximate the out-of-pocket costs of the litigation and therefore do not seem to include additional reputation losses. This differs markedly from the impact of government-mandated product recalls: Jarrell and Peltzman (1985), for instance, find that the stock price losses to firms upon the announcement of a recall are substantially greater (as much as ten times) than the out-of-pocket costs. One possible explanation of these disparate results is that the market does not view the filing of a product liability lawsuit, compared to a government recall, as evidence of a defective product that would diminish the value of a corporate brand name or reputation.

4. Summary and Recommendations

4.1. Summary of Wealth Effects of Corporate Lawsuits

Defendants experience economically meaningful and statistically significant wealth losses upon the filing of the suit. Defendants involved in government suits suffer larger declines in shareholder wealth than defendants involved in lawsuits with other firms or with private parties. Plaintiff firms experience no significant wealth effects upon filing a lawsuit. Also, when a defendant firm settles a suit with another *firm* there is a significant wealth increase for the defendant. In contrast, no significant wealth effects are observed for defendants upon announcement of a settlement when the opponent is a governmental entity or noncorporate private party. For plaintiff firms the wealth implications of settlements appear to be trivial. These findings suggest that, at a minimum, lawsuits are not a value-enhancing way for corporations to settle their disagreements with other corporations. Finally, the market appears to impose a higher sanction on firms than actual criminal sanctions, and the reputational losses are of equal magnitude for civil fines as for criminal ones.

Two caveats are in order regarding these findings. First, the announcement-period abnormal return understates the expected decline in shareholder wealth. The reason is that information about the forthcoming suit may already have reached the market (before announcement in the press) and therefore already be reflected in the market price of the firm's stock. Most of the studies have attempted to reduce the severity of this problem by excluding cases where there was indication in published news reports that information about the suit had previously reached the public. Second, event studies of litigation report the average market response associated with the filing or settlement of a lawsuit. Under what circumstance would a court, corporate manager, or corporate legal counsel use such information? Virtually no litigation situation is an average situation. Each suit represents a unique set of costs and benefits, and managers deciding whether to launch or defend a suit will consider the specific costs and benefits of their situation, rather than the average market response to a collection of suits that may or may not share similar characteristics. However, it is precisely information in a wide spectrum of suits that is most useful for the ex ante formulation of public policy and corporate strategy.

4.2. Recommendations for Use of the Event Study Methodology

The standards for conducting an event study are well established. A researcher can increase the power of an event study by increasing the sample size, narrowing the public announcement to as short a time frame as possible, or both.

How large should the sample size be? In general, the larger the better. This said, the recommended sample size would depend on the magnitude of the abnormal return that one is trying to detect. If the abnormal return is about 1% (and the announcement window can be narrowed to one day) then a sample of 100 firms would be sufficient. If the abnormal return is only 0.5% (and the announcement window can be narrowed to one day) then we would recommend a sample of 200 firms. On the other hand, in general, a sample of just one firm would be quite inadequate in detecting an abnormal return of even 2%.

Regarding the length of the announcement window: the shorter the better. If one is using daily return data, an announcement window of one day is quite feasible and the window that we recommend. However, in going from one to two or three days, the loss in statistical power is not serious. But it is very difficult to have much confidence in the results of event studies that consider long-horizon returns of several years.

Many topics of interest to legal researchers involve events that will produce a data set that does not fall into these extreme cases. For instance, if the topic of investigation is the wealth effect of a specific state law, it may be impossible to identify a one-day event interval. Given the nature of the legislative process, statutory changes typically occur over an interval significantly longer than one day, encompassing at least several months. In this setting, the researcher should try to narrow the event interval as best as he or she can: for instance, by examining the impact on returns only of specific event days (introduction of the bill, committee hearing, chamber vote) over the longer legislative interval. But identification of a single event day is not always possible. In addition, the number of firms affected by one state statute is likely to be substantially below 100 in all but a few states. Inability to increase sample size or narrow the event interval does not indicate that the methodology cannot or should not be used: rather, it means that interpretation of results, such as a finding of insignificance, should be undertaken with care. For a sample of

50 firms and an event date consisting of a one-week interval, for example, the event would have to produce an abnormal return of about 4% to be reliably detected, although there may be a further question whether a smaller level of abnormal returns would be considered economically significant.

The event study methodology, accordingly, can be useful to analyze a variety of issues of interest to both lawyers and economists, or more generally, to public policy analysts. In the companion article to this one, we review its extensive use in illuminating the policy debates in corporate law and corporate governance, as well as issues in its application to the study of regulation.

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Event Studies and the Law: Part II: Empirical Studies of Corporate Law

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This article is the second part of a review of the event study methodology, which has proved to be one of the most successful uses of econometrics in policy analysis. In this part we focus on the methodology's application to corporate law and corporate governance issues. Event studies have played an important role in the making of corporate law and in corporate law scholarship. The reason for this input is twofold. First, there is a match between the methodology and subject matter: the goal of corporate law is to increase shareholder wealth, and event studies provide a metric for measurement of the impact upon stock prices of policy decisions. Second, because the participants in corporate law debates share the objective of corporate law, to adopt policies that enhance shareholder wealth, their disagreements are over the means to achieve that end. Hence, the discourse can be empirically informed. The article concludes by sketching the methodology's use in evaluating the economic effects of regulation. While event studies' usefulness for policy analysis is by now familiar in the corporate law setting, we hope that our two-part review will suggest appropriate applications to other fields of law.

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1. Introduction

This article continues our survey of the event study methodology and its strengths and limitations for policy analysis by reviewing in detail how event studies have been used to evaluate the wealth effects of corporate and securities law and corporate governance.[1] Corporate and securities law are areas in which the event study methodology has had a prominent role in shaping policy debates. This is no doubt because there is a natural fit between the methodology and these fields of law: the benchmark for evaluating the benefit of corporate and securities laws is whether they improve investor welfare, and this can be ascertained by what event studies measure, whether stock prices have been positively affected. While the corporate area is the policy domain in which event studies have been most widely employed, and it is therefore the focus of our review, event studies have also been used to evaluate the wealth effects of regulation. Event studies of regulatory change raise more difficult methodological issues than those involving corporate law and corporate governance, because of the typically long time period in which the event of interest occurs. The article concludes with a brief sketch of the use of event studies in the field of regulation and, in particular, event study–based empirical evidence on producer- and consumer-protection theories of regulation in the banking area, where, given the nature of the regulatory change, some studies are able to avoid the most serious technical difficulties.

2. Event Studies in Corporate Law

Event studies have had a major impact on corporate law. The explanation for this influence is straightforward. The objective of U.S. corporate law is furthering the interest of the owners of the firm, and the event study methodology, measuring the unexpected change in stock price due to new information about firm value, such as adoption of a new corporate law or a firm decision, provides a metric for identifying whether a specific corporate policy or action has the legal regime's desired beneficial impact on firm owners. Moreover, the event study literature serves as a helpful

1. See Bhagat and Romano (2002) for the first part of this survey of the event study literature, which reviews the methodology and its use to evaluate the wealth effects of corporate litigation.

arbiter of corporate law debates because all sides hold the same normative conception of corporate law, shareholder wealth maximization. This is a setting in which quantitative research can fruitfully be used in policy making: data can inform debates that are over the means of implementing public policy, as opposed to debates over the ends of public policy.

Given the fit between the methodology and the legal benchmark—increasing shareholder wealth and measuring the relation between a particular corporate policy and share value—and the relative ease in operationalizing the technique with contemporary computing, since the 1980s event studies have been used by commentators to further debate over central issues of corporate law, have been used by litigants in federal securities cases to establish liability and damages, and have even influenced courts' fashioning of legal doctrine. There have been far more event studies on takeovers than on any other topic, but no important topic of corporate governance has been untouched by the methodology, including the most fundamental question, the production of corporate law itself—specifically, whether the federal system in which states compete for corporate charters is for the better.

2.1. Event Studies and the Debate over State Competition for Corporate Charters

In the U.S. corporate law is largely a matter for the states. State corporation codes consist primarily of enabling provisions that supply standard contract terms for corporate governance. Firms choose their state of incorporation, a statutory domicile that is independent of physical presence. Midstream domicile changes require the approval of a majority of the shareholders. Firms consequently can particularize their governance arrangements both by the choices made in their charters under state law and by their choice of domicile.

One small state, Delaware, has come to dominate the incorporation process, serving as the domicile for most publicly traded corporations. Its profits from providing corporate charters are significant: for example, franchise fees averaged 17% of total tax revenues over the past 30 years (Romano, 2002, Table 4.1). Delaware's success has fueled a running debate among corporate law commentators, mirroring the more general U.S. political debate over the benefits of federalism: are the aims

of corporation codes—protecting the interest of the shareholders—best achieved by firms' ability to choose among domiciles compared to a centralized national regime?

A little over 25 years ago the unquestioned consensus among corporate law scholars followed the position best articulated by William Cary, that the states were competing in a race "for the bottom," in which Delaware led the pack to produce corporate laws that decidedly favored managers' over shareholders' interests (Cary, 1974).[2] But today Cary's position is no longer accepted as a self-evident proposition. Indeed, even adherents to Cary's position in the contemporary discourse advocate federal law as an option in addition to state law, rather than preemption of state law (Bebchuk and Ferrell, 2001). What accounts for such a seismic shift?

Judge Ralph Winter first articulated the flaw in Cary's position as the omission of markets from his analysis of firm behavior. As Winter explained, were managers to choose to incorporate in states whose codes disadvantaged shareholders, they would encounter a higher cost of capital and ultimately a lower job retention rate, compared to competitors operating under codes more favorable to shareholders (Winter, 1977). While Cary's position can be amended to join Winter's argument by asserting that markets are imperfect at disciplining managers when it comes to domicile choice, Winter's insight motivated empirically oriented scholars to study the effect of incorporation choices on firm value, for the purpose of arbitrating the debate.

The event study methodology meshed neatly with Winter's analysis of the issue. This is because a good proxy for ascertaining whether the legal regime decisions made by firms under competition benefit investors is the effect upon shareholder wealth of a change in domicile. If a change in domicile increases firm value, it would be exceedingly difficult to maintain that charter competition and, particularly, Delaware's legal regime, is harmful to shareholders, because the overwhelming majority of firms reincorporate in Delaware (Daines, 2001; Romano, 1985).

There have been eight event studies investigating the effect on stock prices of a change in incorporation state. The event day zero is identified as the date of the proxy mailing announcing the proposed reincorporation. All of the studies find positive abnormal returns, with four finding

2. For example, 80 law professors signed a letter endorsing a national corporation law in 1976 (Romano, 1993a, p. 14 n. 2).

a significant positive stock return at the time of the announcement of the domicile change (although one of these, the earliest study, employs a variant of the event study methodology and uses a difference-in-mean test between price changes of reincorporating firms and the S & P index) (Bradley and Schipani, 1989; Hyman, 1979; Romano, 1985; Wang, 1995), one finding a significant positive return for only a subset of reincorporations on the announcement date, with different results on the subsequent shareholder meeting date (Heron and Lewellen, 1998), another finding a significant positive return over two years before the reincorporation (Dodd and Leftwich, 1980), and two finding positive returns significant at the 10% confidence level, albeit one of these is only for a subset of reincorporations (Netter and Poulsen, 1989; Peterson, 1988)(see Table 1). As indicated in the table, the sample size in many of the studies finding significant positive abnormal returns is large (over 100 firms), whereas some of the studies that report a significant abnormal return at only a 10% confidence level have small samples (fewer than 40 firms). Hence the difference could be attributed to the more limited power of the test for small samples, as discussed in Bhagat and Romano (2002). Thus, the event study literature suggests that Winter's core insight is accurate: competition for corporate charters benefits investors. One certainly cannot read the event study literature and conclude that firms reincorporating are reducing their shareholders' wealth, as Cary's position contends.

Bebchuk, Cohen, and Ferrell (2001) criticize this conclusion by contending that, except for the finding of a significant abnormal return of 4% in Romano (1985), the reincorporation event studies' results are only a "modest" 1%.[3] However, an investment project that generates positive abnormal returns of even 1% is, in fact, considerable in competitive capital markets. The magnitude of the price effect of announcements of capital expenditures, joint ventures, product introductions, and acquisitions by acquiring firms is less than 1% (Andrade, Mitchell, and Stafford, 2001, p. 119). In addition, their negative assessment of state competition is based on their view of state takeover statutes, but they do not similarly

3. Romano's sample is larger than that of other studies and consists of domicile changes in an earlier period. A possible explanation of the different magnitude of the abnormal returns in the studies is that in later years Delaware's legal regime was less distinctive as other states revised their codes, reducing some of the value obtained in a domicile change.

Table 1. Announcement-Period Abnormal Returns for Firms Changing Their State of Incorporation

Study	Sample Period	Sample Size	Announcement Window (Event Days)	Announcement Return (%)	Z-statistic
BS (1989)	1986–88	32	0	1.04*	2.21
Hyman (1979)	1968–76	26	Announcement Week	2.73*	2.01
Romano (1985)	1961–83	150	(−1,+1)	4.18**	11.92
Romano (1985)	1961–83	63 (m & a)	(−1,+1)	6.94**	11.44
Romano (1985)	1961–83	43 (to)	(−1,+1)	0.05	0.77
Wang (1995)	1986–94	145	(−1,+1)	0.97*	1.99
Wang (1995)	1986–94	94 (Del)	(−1,+1)	1.12	1.65
Wang (1995)	1986–94	51 (Non-Del)	(−1,+1)	0.69	0.87
HL (1998)	1980–92	294	(0,+3)	−0.15	−0.51
HL (1998)	1980–92	45 (to)	(0,+3)	−0.51	−1.08
HL (1998)	1980–92	59 (ll)	(0,+3)	1.20	1.66
HL (1998)	1980–92	49 (oth)	(0,+3)	−1.23	−0.72
DL (1980)	1927–77	140	0 (Month)	1.58	N.r.
NP (1989)	1986–87	36	(−1,+1)	0.93	1.61
NP (1989)	1986–87	19 (Cal)	(−1,+1)	0.96	1.57
NP (1989)	1986–87	17 (non-Cal)	(−1,+1)	0.89	0.68
Peterson (1988)	1969–84	30	−1	0.27	1.35
Peterson (1988)	1969–84	14 (to)	−1	−0.16	−0.20
Peterson (1988)	1969–84	16 (no to)	−1	0.65	2.04

Notes: Event day 0 = proxy mailing date announcing meeting with reincorporation vote. N.r.: test statistic not reported. BS (1989): Bradley and Schipani (1989). Romano (1985) subsamples: m & a: reincorporations accompanied by acquisition programs; to: reincorporations accompanied by takeover defenses. Wang (1995) subsamples: Del: reincorporations into Delaware; non-Del: reincorporations to states other than Delaware. HL (1998): Heron and Lewellen (1998), subsamples: to: reincorporations accompanied by takeover defenses; ll: reincorporations accompanied by director liability limits; oth: reincorporations not accompanied by takeover defenses or director liability limits. DL (1980): Dodd and Leftwich (1980). Hyman (1979): calculates AR as difference in mean changes in stock price compared to S & P index. NP (1989): Netter and Poulsen (1989), subsamples: Cal: reincorporations from California; non-Cal: reincorporations from states other then California. Peterson (1988) subsamples: to: reincorporations accompanied by takeover defenses; no to: reincorporations not accompanied by takeover defenses.

*Significant at the 95% confidence level.

**Significant at the 99% confidence level.

emphasize that most of these event studies find insignificant or even more "modest" (less than 1%) negative abnormal returns than the reincorporation event studies (see Table 2 and the discussion of these studies below).

Because reincorporations are typically accompanied by changes in business plans (Romano, 1985, p. 250), there is, however, a question whether the positive stock price effects are evidence of the market's assessment of the change in business plan rather than the change in domicile. The issue is whether there is a confounding effect, one that muddies the interpretation of stock price effects, requiring a more probing examination of the findings. To investigate whether the positive price

Table 2. Event Studies of Takeover Statute Enactments

Study	Statute(s) studied	Sample Size	Announcement Window (Event Days)	Announcement Return (%)	Z-statistic
KM (1989)	40 statutes, 26 states, 1982–87	1,505	(−1, 0)	−0.29	−2.43**
KM (1989)	38 statutes, 26 states (no to)	1,107	(−1, 0)	−0.39	−2.54**
KM (1989)	33 statutes, 23 states (to)	368	(−1, 0)	−0.13	−0.87
Mahla (1991)	49 statutes, 30 states, 1983–89	678	(0)	−0.12	−1.85
PJ (1990)	5 statutes, 1 vetoed bill, 4 states	245	(0, +1)	−0.46	−1.62
KM (1989)	11 BC statutes	1,030	(−1, 0)	−0.47	−2.70**
Mahla (1991)	BC statutes	248	(0)	−0.24	−2.35*
KM (1989)	Del BC statute, 1987	N.r.	(−1, 0)	−0.44	−1.10
JP (1991)	Del BC statute, 1987	920	(0, +1)	−0.09	−0.19
PJ (1990)	Ind BC statute, 1986	15	(0, +1)	−0.94	−1.12
Broner (1987)	NJ BC statute, 1986	51	(−1, +1)	−0.55	−1.13
PJ (1990)	NJ BC statute, 1986	26	(0, +1)	0.48	0.71
Schumann (1988)	NY BC statute, 1985	94	(−1, +1)	−0.96	−2.37*
KM (1989)	NY BC statute, 1985	N.r.	(−1, 0)	−0.22	−0.60
PJ (1990)	NY BC statute, 1985	72	(0, +1)	−0.72	−1.71
KM (1989)	12 CSA statutes	271	(−1, 0)	−0.01	−0.89
Mahla (1991)	CSA statutes	236	(0)	−0.017	0.18
KM (1989)	Ind CSA statute, 1986	N.r.	(−1, 0)	−2.14	−3.46**
PJ (1990)	Ind CSA statute, 1986	15	(0, +1)	−1.8	−2.15*
SW (1990)	Ind CSA statute, 1986	19	(0)	−5.91	1.97
Romano (1987)	Mo CSA statute, 1984	14	(−1, +1)	−0.01	−0.72
PJ (1990)	OH CSA statute, 1986	45	(0, +1)	−0.35	−0.67
KM (1995)	Penn DG, 1990	57	(0, +1)	−1.43	2.89*
Margotta (1991)	Penn DG, 1990	55	(0)	−0.6	−2.47*
ST (1992)	Penn DG, 1990	56	(−1, +1)	−3.33	−4.80**
ST (1992)	Penn DG, 1990	44 (no to)	(−1, +1)	−3.94	−5.06**
ST (1992)	Penn DG, 1990	12 (to)	(−1, +1)	−1.11	−0.65
KM (1989)	11 FP statutes	329	(−1, 0)	−0.27	−1.30
Mahla (1991)	FP statutes	74	(0)	0.06	−0.05
Romano (1993c)	25 OC statutes (pre-1991)	361	(0)	0.02	0.30
ASM (1997)	IN, NY, and OH OC statutes (pre-1993)	318	(0, 1)	−0.33	V.n.r

Table 2. Continued

Study	Statute(s) studied	Sample Size	Announcement Window (Event Days)	Announcement Return (%)	Z-statistic
RN (1988)	Oh OC/PP statute, 1986	37	(−1, +1)	−2.08	−2.18*
MMM (1990)	Oh OC/PP statute, 1986	53	(−1, +5)	1.43	1.69

Notes: Event is first press report, unless otherwise indicated below. N.r.: number of firms not reported. V.n.r.: value not reported for *z*-statistic: significance level is reported. BC: Business Combination statute; CSA: Control Share Acquisition statute; FP: Fair Price statute; OC: Other Constituency statute; PP: Poison Pill statute; Penn DG: Pennsylvania takeover statute including disgorgement, other constituency, control share acquisition, and labor protection provisions. KM (1989): Karpoff and Malatesta (1989); they report insignificant abnormal returns on legislative event dates; subsamples: to: firms with takeover defenses when statute adopted; no to: firms without takeover defenses when statute adopted. Mahla (1991): event is introduction of bill. PJ (1990): Pugh and Jahera (1990), event is introduction of bill. JP (1991): Jahera and Pugh (1991); event is eight legislative events; significant positive return reported using excess returns model. Broner (1987): event is committee release of bill. SW (1990): Sidak and Woodward (1990), 14 legislative events. Romano (1987): event is introduction of bill. KM (1995): Karpoff and Malatesta (1995). ST (1992): Szewczyk and Tsetsekos (1992), event is measured over four legislative events; subsamples: to: firms with takeover charter defenses when statute adopted; no to: firms with no takeover charter defenses when statute adopted. Romano (1993b): three legislative events. ASM (1997): Alexander, Spivey, and Marr (1997); subsample: to: firms with takeover defenses when statute adopted. RN (1988): Ryngaert and Netter (1988), event is legislative action. MMM (1990): Margotta, McWilliams, and McWilliams (1990), event is legislative action.

*Significant at the 95% confidence level.

**Significant at the 99% confidence level.

effect was a function of investors' responses to other changes in business plan accompanying the reincorporation and not their evaluation of the new legal regime, Romano (1985) compared the returns of sample firms grouped by the type of activity accompanying or motivating the reincorporation—engaging in a mergers and acquisitions program, undertaking takeover defenses, and a miscellaneous set of other activities, including reducing taxes. Although one might have expected the impact to vary across firms, with the antitakeover reincorporations experiencing negative returns, because prominent commentators have viewed takeover defenses as adverse to shareholders' interest (e.g., Easterbrook and Fischel, 1981) while announcements of acquisition programs produce positive price effects (Schipper and Thompson 1983), in fact, not only was the sign on the former group's abnormal return positive, but there was no significant difference across the groups (Romano, 1985, p. 272). This finding implies that the significant positive returns upon reincorporation can be attributed to investors' positive assessment of the change in legal regime, not a confounding of the impact of reincorporating firms' other future projects.

In contrast to Romano's study, Heron and Lewellen (1998, pp. 557–59) find a different price reaction, depending on whether the reincorporation is undertaken to limit directors' liability (positive) or to erect takeover defenses (negative). However, the event date they use to obtain this result is problematic for an event study analysis. The takeover defense firms' abnormal returns are negative only on the shareholder meeting day, and James Brickley's investigation of the event study methodology found that, in contrast to random samples of proxy mailing dates, random samples of annual meeting dates—that is, a sample on which there is no a priori reason to find a significant price effect—produce significant abnormal returns (Brickley, 1986, pp. 346–47).[4] In addition, there is no theoretical basis for expecting a significant price effect (related to the reincorporation) on the meeting date, because the number of contested management proposals is so small that little uncertainty exists over reincorporation voting outcomes and thus no new information is revealed on the meeting date for the overwhelming majority of reincorporating firms.

Some corporate law scholars have further questioned the appropriateness of using event study methodology for assessing the efficacy of state competition beyond the issue raised by the potential confounding effect of a domicile change and a change in business operations at the time of reincorporation. For instance, Lucian Bebchuk (1992, pp. 1449–50) has asserted that stock price studies are not probative on whether state competition benefits shareholders, because state competition may produce some provisions that are harmful to shareholders even if the overall package of provisions is not, and hence we would not detect any statistically significant price effect upon reincorporation.

Bebchuk's bundling critique is not a troubling objection to the use of the methodology. Bebchuk's premise of shareholders' being forced to choose between bundles of offsetting good and bad statutes is founded on

4. Brickley's explanation of the finding of abnormal returns on randomly selected meeting dates in contrast to mailing dates is that annual meeting dates are known in advance and often contain important management announcements (such as earnings forecasts), which can produce abnormal returns because "risk and expected returns can increase around predictable events likely to contain information" (Brickley, 1986, pp. 347–48). Heron and Lewellen use a four-day interval to measure abnormal returns around the proxy mailing date and a one-day interval for the meeting date, without justification. For consideration of the desirability of examining returns over similarly lengthed intervals with arbitrary dates see Brickley, Bhagat, and Lease (1985, p. 126).

the premise that the statistical findings of the event studies are insignificant; yet, as noted, many event studies report significant positive stock price effects. In any event, from the perspective of shareholders, it is the net wealth effect of a code that is important.

Bar-Gill, Barzuza, and Bebchuk (2001) have extended Bebchuk's bundling objection by a further contention that the positive price effect of a Delaware domicile is due to network effects unrelated to the content of the legal regime (that is, investors value the presence of a stock of legal precedents independent of whether the precedents benefit investors or managers). However, to the extent that the event studies find positive abnormal returns for all moves and not solely for migration to Delaware, the network effect is not a satisfactory explanation of the data. In addition, it is not obvious that the value of the stock of precedents—the network externality—should be inversely correlated with the value of the substantive law to investors, yet that must be the case for Bar-Gill, Barzuza, and Bebchuk's explanation to be consistent with the empirical evidence. If the network effect simply reinforces the positive value of the legal regime, rather than offsets an otherwise negative impact, then it is of no particular import for an evaluation of state competition; in their model that is, of course, what happens: there is a tradeoff of a positive network effect as against a legal regime whose rules are adverse to shareholder interests.

Another class of event studies that provides more tangential evidence on the state competition debate than the reincorporation studies is event studies of changes in Delaware law. Such studies are less reliable tests than studies of domicile switches of the wealth effects of state competition, for several reasons. First, reincorporations are more difficult for investors to anticipate and therefore easier to date for statistical testing than legislative changes. Second, reincorporations are firm-specific events, so the endogeneity of the event's occurrence is automatically controlled for by the composition of the test portfolio: it includes only firms experiencing the event. This is not true for the enactment of statutes, which are applicable to all domestic corporations but may actually have divergent effects on different corporations. When their impact is examined for a portfolio of domestic firms that does not control for the potential heterogeneity across firms of the effect of the legal rule change, the test may simply aggregate offsetting effects and therefore not be able to identify any wealth effect.

State takeover statutes have been examined intensively, a legislative context in which Delaware is a laggard rather than the leader of competitive activity (Romano, 1993a, p. 59). One Delaware statutory change has been closely studied, enactment of a statute permitting firms to limit outside directors' liability for negligence.

The results on takeover statutes are less uniform than the reincorporation studies: there are findings of negative, positive and insignificant price effects (see Romano, 1993a, pp. 60–68). But the most comprehensive study, by Jonathan Karpoff and Paul Malatesta (1989), which has the largest sample size because it includes 40 statutes enacted in 26 states, finds that the statutes have a significant, albeit small, negative price effect on domestic corporations (−0.4%), when the event date is the earliest newspaper report of the legislation. They find no significant price effect when days on which specific legislative events occurred, such as bill introduction, final passage and signing into law, are used as the event dates. Much of the difference in the event study findings can be explained by the type of statute: statutes more likely to raise the cost of a bid tend to produce negative price reactions compared to statutes less likely to affect a bid (compare the results for disgorgement, business combination, and control share acquisition statutes [e.g., Karpoff and Malatesta, 1989; Szewczyk and Tsetsekos, 1992] with those for fair price and other constituency statutes [e.g., Karpoff and Malatesta, 1989; Romano, 1993c]). Differences also depend upon the event interval chosen (compare Ryngaert and Netter, 1988, with Margotta, MacWilliams, and MacWilliams, 1990), which cautions against drawing strict conclusions from any one study without strong justification of the researchers' interval choice.[5] Finally, some differences depend on firm characteristics: Karpoff and Malatesta (1995), for example, find that controlling for firm size eliminates the significance of the negative price effect of the Pennsylvania takeover statute found in other studies.

5. The price impact of a statute may be related to the absence of firm-level defenses. When Karpoff and Malatesta's sample is divided according to whether the firm has antitakeover charter amendments or a poison pill, only the portfolio without defenses experiences a significantly negative effect on the event date (p. 308). However, not all studies find the same abnormal return pattern controlling for firm-level defenses (e.g., Jahera and Pugh, 1991; Romano, 1993c). We thus are hesitant to conclude that characteristic differences across firms in sample portfolios explain the variance in the studies' results.

Most importantly for the state competition debate, Karpoff and Malatesta (1989) find that Delaware's takeover statute had an insignificant stock price effect. A study by Jahera and Pugh (1991) finds a significantly positive effect of the Delaware statute on legislative event dates for some but not all of the excess returns models that they investigate; they also find no significant price reaction on the newspaper announcement dates (Karpoff and Malatesta do not provide information on the price effect of the Delaware statute on legislative event dates). Contrary to Karpoff and Malatesta's finding on firm defenses, Jahera and Pugh find that Delaware firms with antitakeover charter defenses experienced a negative price effect; those without defenses, a positive price effect on several event dates: the cumulated effect is insignificant for the former group and significant for the latter group only at 10%. Thus, they conclude that the statute did not adversely affect the wealth of investors. Table 2 summarizes the results of these event studies and additional studies evaluating the same statutes as those discussed in the text; for a more complete tabulation of takeover-statute event study results see Table 4-1 in Romano (1993a).

The nonnegative impact of the Delaware takeover statute is a fact of itself favorable to an assessment of state competition, because Delaware is the leading incorporation state and this result indicates that its action did not adversely affect shareholders, compared to that of other states. Indeed, the findings on takeover statutes are the strongest (and sole) empirical evidence against the efficacy of state competition for charters: they suggest that states other then Delaware may and do enact specific laws that are not in shareholders' interest. Ironically, then, these data cast Delaware in a highly positive light for investors. A fair conclusion from these event studies is that for at least some firms in some states, legislative initiatives making takeovers more difficult were bad news (wealth-decreasing events) for investors.

Delaware's limited liability statute, in contrast to other states' takeover statutes, did not have a significant stock price effect (Bradley and Schipani, 1989; Janjigian and Bolster, 1990; Romano, 1990). Delaware firms did experience significant negative returns on the effective date of the statute, which was two weeks after its enactment and two months after the first legislative event date, the day when the corporate law section council of the Delaware Bar Association approved the provision (for corporation

code revisions, the Delaware legislature acts upon recommendations of the corporate bar). But the statute's effective date is not a meaningful event date, because there was no new information released on it—the statute's enactment was well publicized, and there was no uncertainty regarding whether the statute would become effective on the stated date.

Because the coverage of the limited liability statute was optional, one explanation of the finding of insignificance, besides the length of the legislative interval's creating imprecision as to the dating of events, is that the effect of the statute would not be incorporated into stock prices until investors determined whether or not their firm would elect to be covered. The abnormal returns experienced by firms opting into the statute vary, however, depending on the event window examined or the portfolio of firms. One study finds significant positive returns over two-, three-, and five-day intervals, and insignificant returns over a seven-day interval (Romano, 1990), another study finds significant negative returns over a seven-day interval (Bradley and Schipani, 1989), two studies find insignificant returns over a variety of time intervals (Janjigian and Bolster, 1990; Netter and Poulsen, 1989), and a final study, which uses a four-day interval, finds insignificant positive returns for its full sample of 120 firms and positive abnormal returns for poorly performing firms (Brook and Rao, 1994). Brook and Rao's explanation of this finding is that shareholders of poorly performing firms value limited liability provisions more highly than shareholders of other firms because it is more important for such firms to "attract and retain" the services of high-quality outside directors.[6]

One conclusion, given the findings of these studies, is that the limited liability statute did not adversely affect shareholders. Providing further support for this interpretation of the data is the fact that shareholders vote overwhelmingly to opt into the limited liability statute. It is, of course, possible that shareholders vote for management-sponsored proposals that adversely affect firm value; see Bhagat and Jefferis (1991). But the Bhagat and Jefferis study investigated management-sponsored proposals related to takeover defenses, proposals that institutional investors have vigorously opposed in the same time period in which they have supported the limited

6. Although these firms also had a smaller percentage of outside directors, there was no relation between the number of outside directors and the value of a limited liability provision (Brook and Rao, 1994, p. 495).

liability provisions. Consistent with this distinction, these investors have also sponsored proposals to overturn takeover defenses constructed by management but not to overturn limited liability charter provisions. In the reverse legislative situation, a takeover statute that shareholders did not wish to have applied to their firm (the Pennsylvania disgorgement statute), institutional investors have successfully pressured managers to opt out of the statute's coverage (Romano, 1993a, pp. 68–69). The event studies of that statute report a negative wealth effect, in contrast to those of the limited liability statute.

Finally, in addition to the event studies of legislation, there have been event studies of judicial decisions (Bradley and Schipani, 1989; Kamma, Weintrop, and Wier, 1988; Ryngaert, 1988; Weiss and White, 1987). Because courts play an important role in Delaware's market position (e.g., Romano, 1993a, pp. 39–41), determining whether investors benefit from judicial decisions could proxy for determining whether they benefit from state competition.

However, judicial decisions are not "events," except for the litigants for whom a decision effects a wealth transfer. Decisions in corporate law cases are not likely to effect firms other than the litigants, because other firms and investors are able to contract around the rule and recalibrate the costs and benefits. They are therefore only of limited value as subjects for the event study methodology: we can use the methodology to learn how a specific decision affects the parties, but it will not be valid for analyzing the decision's impact on nonlitigants.

Further complicating event studies of judicial decisions is the interaction between the court and state legislature in Delaware, which is a byproduct of the competition for charters. A judicial decision with a significant adverse impact on firms stands an excellent chance of being overturned by the Delaware legislature: the limited liability statute, for instance, was a reaction to a judicial opinion holding outside directors liable for accepting too hastily a takeover premium (Romano, 1990). Since investors can anticipate the legislature's response to a judicial decision that is adverse to their interests, one cannot expect to find a negative stock price effect for a portfolio of Delaware firms after a wealth-decreasing decision. Thus, the stock price reaction would not be distinguishable from such a decision compared to a decision that did not diminish share value.

Not surprisingly, event studies of judicial decisions find insignificant price effects for portfolios of Delaware firms (Bradley and Schipani, 1989; Weiss and White, 1987). These studies are not informative about the value of Delaware law, because the methodology is not a good fit for addressing the question. Judicial decisions produce significant abnormal returns to the litigants, and, when the decisions uphold (or invalidate) a specific takeover defense, to concurrent takeover targets (Kamma, Weintrop, and Wier, 1988; Ryngaert, 1988). The use of the methodology in these latter studies is equivalent to that of the litigation studies discussed in Bhagat and Romano (2002). The reincorporation studies provide, by contrast, a clean, and indeed the best, measure of the wealth effect of state competition.

2.2. The Role of Event Studies in Public Policy toward Takeovers

There was an intellectual revolution in corporate law scholarship in the 1980s. Before then, corporate law was a dead field of research (Manning, 1962, p. 245 n. 37). But there was a burst in new acquisitive activity at that time and because corporate law scholarship tends to follow deals, corporate law became one of the more active and sophisticated fields of interdisciplinary legal scholarship.

Event studies became an important source of information with which to ground policy recommendations in the new context of hostile leveraged bids. The explosion in acquisitions, which occurred shortly after the development of modern finance theory, of which the event study technique is a spinoff, created a cottage industry of event studies. There was a plethora of studies of the price effects of acquisitions and review articles were repeatedly updated in order to keep up with the literature (e.g., Jarrell, Brickley, and Netter, 1988; Jensen and Ruback, 1980). These studies highlighted that there were uniformly large and significant positive price effects for shareholders of targets. There is also consensus in the literature that, on average, bidding shareholders do not experience any significant wealth effect upon announcement of such mergers. Depending on the sample period and sample considered, studies document average bidder returns that cover the range from positive, economically small, and statistically insignificant, to negative, economically small, and statistically insignificant. Studies that have aggregated the wealth effects of both the

target and bidder firms find, however, that despite the lower returns to the generally larger-sized bidders, the combined target and bidder return is positive (e.g., Bhagat, Hirshleifer, and Noah, 2001; Bradley, Desai, and Kim, 1988; Kaplan and Weisbach, 1992).

Concern has also been raised over the impact of takeovers on other stakeholders, notably, employees, customers, and suppliers (see Akhavein, Berger, and Humphrey, 1997; Bhagat, Shleifer, and Vishny, 1990; Kim and Singal, 1993). A policy-relevant question is whether the large positive returns to target shareholders are offset by negative (or nonpositive) returns to employees, customers, and suppliers. Several studies have attempted to measure the losses to these nonshareholder interests, and the average effect is generally small and often statistically insignificant, in striking contrast to the significantly larger average target shareholder gain (see, e.g., Asquith and Wizman, 1990; Dennis and McConnell, 1986; Marais, Schipper, and Smith, 1989; Pontiff, Shleifer, and Weisbach, 1990; Rosett, 1990). We are not aware, however, of any study that has attempted to address the question with a consistent sample. A study that considers the impact of a sample of takeovers on target and bidder shareholders and bondholders, employees, customers, and suppliers would be a valuable contribution to this literature.

Analogous to the shifting sentiment on state competition, the conclusion from the event study research regarding the benefits of takeovers for target shareholders led commentators and policy makers alike to conclude that takeovers should be encouraged rather than obstructed (e.g., Council of Economic Advisors, 1985, p. 215; Easterbrook and Fischel, 1991, pp. 175–205), and the Delaware courts took note, tightening the fiduciary standard applicable to takeover defenses (*Unocal Corp. v. Mesa Petroleum Co.*)

The Delaware courts did not, however, go as far as the position advocated by some prominent commentators that all defenses should be banned (e.g., Easterbrook and Fischel, 1981). Indeed, they eventually adopted an approach that provided managers with substantial discretion to react to a takeover so long as the bid is not precluded (*Unitrin v. American General Corp.*) This restrained, fact-intensive judicial approach is, in fact, consistent with the inconclusive empirical evidence on the efficacy of defenses, despite legal commentators' support for more active judicial intervention. The event study literature does not uniformly find that the

adoption of defenses produces negative price effects. Stock price reactions vary not only with the type of defense, but also with the type of firm. For example, adoption of golden parachutes produces positive price effects (Lambert and Larckner, 1985), elimination of cumulative voting produces negative ones (Bhagat and Brickley, 1984), and the effects of poison pills vary, being negative in the early to mid-1980s (e.g., Ryngaert, 1988) and insignificant in later years (e.g., Comment and Schwert, 1995). In addition, Brickley, Coles, and Terry (1994) found positive price effects for poison pill adoptions by firms with independent boards, and Datta and Iskandar-Datta (1996) found pill adoptions produce insignificant effects except for firms subject to a takeover bid, for which the price effect is negative. Bhagat and Jefferis (1991) argue that the earlier studies find conflicting or insignificant results since they do not control for the anticipation of the antitakeover proposal. After controlling for this anticipation effect they find a statistically negative 1% return. The Delaware courts moved to an increasingly restrained approach to managerial resistance over the same time frame as the range in the findings of event studies of defensive tactics increased.

A more troubling issue for corporate law was presented by the event study results regarding the stock price of acquirers. Event studies indicated a change in acquiring firms' abnormal returns from positive or insignificant to negative from the 1970s into the 1990s, paralleling the increasing use of defensive tactics to encourage auctions (e.g., Jarrell, Brickley, and Netter, 1988). As corporate law is directed to the shareholders of targets rather than bidders, the owners least likely to benefit from an acquisition as the decade progressed—the shareholders of the acquirers—did not have the opportunity for legal recourse. Courts did not change their traditional response, deferring to management on acquisitions, compared to the defensive tactic setting, where the conflict of interest is more clear-cut. But because even commentators concerned about this issue were divided on whether there ought to be a legal response (compare, e.g., Dent, 1986, with Coffee, 1984; and Black, 1989, pp. 651–52), legislatures' and courts' maintenance of the status quo is unexceptionable.

The uniformity in the empirical findings on takeovers for target shareholders also affected interpretation of the mandate of the securities laws. The Securities and Exchange Commission (SEC) issued rules to overturn defensive tactics (see Securities Exchange Act Rules 13e-4(f)(8),

prohibiting selective self-tenders, and 19c-4, requiring one share, one vote), although the federal courts did not always find it had authority to do so (*Business Roundtable v. S.E.C.*, overturning rule 19c-4). Over 20 years earlier, the agency had successfully lobbied to advantage incumbent managers over bidders in the enactment of the Williams Act. It would be fair to say that the transformation in perspective on hostile bids was not simply a function of a change in agency personnel, but was caused by a more diffuse change in attitude toward bids that was, no doubt, in part influenced by the event study literature demonstrating the benefits of takeovers to target shareholders.

The event study findings of the positive impact of takeovers on targets also formed the backdrop for the Supreme Court's decision in *Basic v. Levinson* (1988), which held that merger negotiations were sufficiently material to investors that disclosure could be required before the firms reach an agreement in principle, a bright-line standard that several appeals courts had adopted. The Court's drive to disclose such information as early as possible is an acknowledgment of the significance of the information, which was underscored by the salience of the value of bids as measured by event studies. In reaching this conclusion, the Court rejected the view of the importance of maintaining secrecy until a firm agreement was reached, which had been adopted by some appeals courts, stating that the view that secrecy "maximize[d] shareholder wealth" was "at least disputed as a matter of theory and empirical research" (p. 235). Although the Court did not specifically cite the economic literature on takeovers, it is plausible that the event studies detailing the benefits to shareholders of takeovers had an impact on its decision making as the opinion evidences an awareness of the finance literature.

2.3. Event Studies and Securities Litigation

Basic v. Levinson (1988) had an even more profound impact on the conduct of securities litigation in relation to the event study methodology than it had on firms' acquisition negotiations. It articulated a doctrine, known as the "fraud on the market theory," that permits plaintiffs to establish reliance, a necessary component of securities fraud, by reference to the integrity of the market price. Namely, rather than having to show that the plaintiff actually saw or heard misleading information from the defendant, the presumption is that the market price of a security reflects

its value, and an affirmative misstatement or omission that distorts that value is fraudulent even if the shareholder had no knowledge of the statement: the fraud is on the market as a whole, on whose determination of value the individual shareholder is entitled to rely.

The "fraud on the market theory" is, in essence, a statement of the semi-strong form of the efficient market hypothesis, the theory on which the event study methodology is grounded. Event studies, relying on the efficient market hypothesis, assume that public information is incorporated into stock prices and that stock prices change when new information is revealed. This is precisely how the Supreme Court's reliance presumption operates. Because stock prices are understood as reflecting the value dependent upon a defendant's fraudulent statement, when the misstatement or omission is corrected, the stock price is expected to adjust to the true value, which evinces the harm to investors who had relied on the prices formed by the mix of information that included the fraudulent statement. This presumption dovetails with the Court's definition of materiality, another precondition for establishing liability—a substantial likelihood that the information would have affected the investor's decision to buy or purchase a security (*TSC Industries, Inc., v. Northway, Inc.*).

The doctrine makes plain that event studies have a dual role in securities litigation. They can be critical for determining both liability and damages. The event study technique permits a demonstration of the change in value due to the fraud, by measuring the firm-specific movement when the fraud was revealed (when the information that counteracted the alleged material misstatement or omission was disclosed). Hence it facilitates the establishment of liability by showing that the information was in fact material—it demonstrates that there was "a fraud on the market." Without adjusting for general market movements, it would be exceedingly difficult, if not impossible, to ascertain the true effect of the information on the firm. The event study technique provides an anchor for making this determination and eliminates the need to rely on ad hoc judgments concerning information effects. In addition, the demonstration of materiality by the methodology is also useful for the calculation of damages (what the defendant's gain is for the statutory disgorgement penalty in SEC cases, and what the plaintiff's loss is in civil liability cases).

Following this reasoning, the SEC has used the event study methodology in its enforcement of the prohibition on insider trading, both to determine the materiality of information and to calculate the profits that an insider has to disgorge (see Mitchell and Netter, 1994). Private parties have similarly used the event study methodology as well in civil litigation. Courts have, for instance, relied on expert witnesses' analyses of stock price movements due to corporate press releases and announcements in their decision making on liability (e.g., *Sibley v. Mary Kay Cosmetics, Inc.*). Indeed, even before *Basic v. Levinson*, commentators advocated the use of event studies for measuring damages for securities fraud under the *TSC v. Northways, Inc.,* standard (e.g., Fischel, 1982).

Cornell and Morgan (1990) provide a comprehensive overview of the strengths and weaknesses of the use of event studies for private securities litigation. In particular, they detail when an event study will under- or overestimate damages in a class action. In brief, when there is leakage of the true information before a public announcement by the corporation, the event study technique will understate the damages, because part of the impact of the information has been incorporated into the stock price before the announcement date. One way to compensate for this problem is by extending the event window, to include the longer period in which the true information was being released, but identifying the appropriate endpoints of the wider interval can be quite difficult. In addition, if there is more than one source of misrepresentation in a securities fraud, and the timing differs for the correction of the misstatements, then the methodology will not be particularly useful, because it does not provide a basis for allocating the damages across the different sources (the price impact will be concentrated on the date of the disclosure of the final fraud). Another methodological problem occurs when the securities claim involves a material omission rather than misstatement: it may be difficult to specify on what date the omission occurred, a date that is necessary to determine a predicted price to calculate damages from prices when the fraud was revealed.

While Cornell and Morgan's article is a constructive cautionary note regarding the appropriate application of the event study methodology in adjudicating key issues in securities fraud cases, the greater precision that the methodology offers for measuring the effect of information on stock prices is, nevertheless, an enormous advance over the ad hoc techniques that were formerly used to establish materiality and damages. In addition,

the adversary process should mitigate many of the methodological concerns that Cornell and Morgan identify, as experts employed by the parties will highlight them in the litigation. It is therefore safe to conclude that, with regard to securities litigation, the methodology's appropriateness for valuation issues is a settled part of the landscape and that it will disappear from litigants' repertoire only when a statistical technique offering even greater precision is developed.

In addition to the application of event study methodology to litigate securities fraud cases, the methodology has been used to assist in policy analysis, as in the state competition and takeover debate contexts, in the evaluation of procedural reforms wrought by the Private Securities Litigation Reform Act of 1995. This congressional initiative was intended to render it more difficult to bring a civil action under the federal securities laws (H.R. Conference Report, 1995). Because the legislation was unexpectedly vetoed by President Clinton and the veto was overridden shortly thereafter (see Johnson, Kasznik, and Nelson, 2000), in contrast to legislation that comes to fruition over a long period of time, fairly clean event dates for the act can be identified.

Event studies have found that the act had a significantly positive stock price effect (Johnson, Kasznik, and Nelson, 2000; Spiess and Tkac, 1997). This result is interpreted as validating the congressional impetus for the legislation, concern over the incidence of nonmeritorious lawsuits, because the market valued the legislation's benefits from curtailing frivolous suits as greater than its costs in restricting meritorious suits, although Ali and Kallapur (2001) contend that the positive price effect was in reaction to President Clinton's veto and not its override. Further supporting the conclusion that the act's curtailment of litigation benefited investors is a study of the price impact of a court decision adopting the most stringent interpretation of the act's pleading requirement, which furthered Congress's goal of making filing of a nonmeritorious suit more difficult. This study found that the decision had a statistically significant positive effect on the stock prices of a sample of high technology firms, firms that operate in an industry sector with a high probability of securities litigation (Johnson, Nelson, and Pritchard, 2000).[7]

7. In contrast to state corporate law, the federal securities laws consist of mandatory rules. Firms cannot therefore contract around a court decision in this context as they

2.4. Event Studies and Corporate Governance

Virtually all of the important mechanisms of corporate governance have been subjected to event study analysis. These include boards of directors, shareholder proposals, derivative lawsuits, and executive compensation. Although all of these devices have been posited to perform a critical function of reducing the agency costs of the separation of ownership and control in the U.S. public corporation, empirical studies do not provide strong support for this viewpoint. Neither shareholder proposals nor lawsuits have a significant positive price effect. A positive stock price effect is associated with appointment of an independent director to the board, but board composition has not been found to impact positively on performance. By contrast, the incentive-aligning device of stock-based executive compensation has been found to affect stock prices positively. These findings suggest that widely shared beliefs concerning what are essential components for effective corporate governance may be mistaken and that affirmative policies to foster such devices ought to be reconsidered.

2.4.1. Boards of directors. Directors are seen as performing a pivotal role in the corporation: ensuring that management acts in furtherance of the shareholders' interest. As the repository of the shareholders' agents to monitor the agents directly running the firm, the board structure interposes an additional strata of agency problems on the more basic agency relation between managers and owners of the firm. Accordingly, commentators have emphasized the desirability of a board composed of independent or outside directors—directors without a financial or personal connection to

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can in the corporate law context, and decisions can therefore impose wealth effects on nonlitigants. It should be noted that some studies of corporate litigation have focused on securities lawsuits. One motivation for this research was to investigate the issue of concern to Congress in enacting the 1995 reform legislation, whether the suits are typically supported by meritorious or nonmeritorious claims. Most of the empirical studies of securities litigation do not, however, use the event study methodology, and they are accordingly not reviewed here. For example, Alexander (1991) compares settlement value and potential damages for litigation following initial public offerings of a small number of high technology firms. She concludes that the settlement amounts do not depend on the merits of the cases. For other studies suggesting instead that settlements are related to the merits, see Francis, Philbrick, and Schipper (1994) (finding positive correlation between settlements and potential damages) and Skinner (1995) (finding more untimely disclosures of adverse earnings produce less favorable litigation outcomes).

management—to ensure that the board structure is not simply creating a further layer of agency problems that is one step removed from operations (e.g., Eisenberg, 1976). This position has been incorporated into the legal system: stock exchanges require listed firms to have independent directors on audit committees, and courts take the board's independence into account when assessing claims in shareholder lawsuits.

Consistent with the monitoring view of outside directors, the market views such directors favorably. An event study of the appointment of an outside director reports a significant positive price effect, even when most of the board was already independent (Rosenstein and Wyatt, 1990). This increase, while statistically significant, is economically small and could reflect signalling effects. Appointing an additional independent director could signal that a company plans to address its business problems, even if board composition does not affect the company's ability to address these problems.

Bhagat and Black (1999) recently surveyed the literature on how board composition affects firm performance or vice versa. Prior studies of the effect of board composition on firm performance generally adopt one of two approaches. The first approach involves studying how board composition affects the board's behavior on discrete tasks, such as replacing the CEO, awarding golden parachutes, or making or defending against a takeover bid. This approach can involve tractable data, which makes it easier for researchers to find statistically significant results. But it does not tell us how board composition affects overall firm performance. For example, there is evidence that firms with majority-independent boards perform better on particular tasks, such as replacing the CEO (Weisbach, 1988) and making takeover bids (Byrd and Hickman, 1992). But these firms could perform worse on other tasks that cannot readily be studied with this approach (such as appointing a new CEO or choosing a new strategic direction for the firm), leading to no net advantage in overall performance. Also, events such as CEO turnover and takeovers are rare occurrences for firms. The greater and more positive contribution of boards may be in the ongoing advice they give to senior management in private meetings; it would be difficult to study this with the traditional event study method.

The second approach consists of examining directly the correlation between board composition and firm performance. This approach allows

us to examine the "bottom line" of firm performance (unlike the first approach), but involves much less tractable data. Firm performance must be measured over a long period, which means that performance measures are noisy and perhaps misspecified, as discussed in Bhagat and Romano (2002).

Bhagat and Black (2000) found a reasonably strong correlation between poor performance and subsequent increase in board independence. The change in board independence seems to be driven by poor performance rather than by firm and industry growth opportunities. However, there is no evidence that greater board independence leads to improved firm performance; if anything, there are hints in the other direction. The conventional wisdom that supports a very high degree of board independence and may explain why poorly performing firms increase board independence, appears to rest on a shaky empirical foundation.

It is possible that the failure to find that independent boards improve performance is because not all outside directors are truly independent from management and empirical researchers cannot distinguish between "effective" and "ineffective" independent boards. But a more compelling reason why increasing board independence does not result in improved performance is that having inside directors could add value in strategic planning or evaluation of potential successors for the CEO (e.g., Vancil, 1987).[8] In addition, independent boards at best improve corporate decision making in certain extraordinary situations, such as management buyouts or poor performance (e.g., Lee et al., 1992; Weisbach, 1988), which are very low probability events for most firms.

These data suggest that it would be prudent for companies to consider experimenting with modest departures from the norm of a "supermajority independent" board with only one or two inside directors. The independent directors will still numerically dominate the board, and can take appropriate action in a crisis. In addition, effort should be focused on devising mechanisms to enhance director independence, or otherwise improve their incentives to monitor, by encouraging greater equity ownership.[9]

8. This is consistent with Klein's (1998) evidence that inside director representation on investment committees of the board correlates with improved performance.

9. Hall and Liebman (1998) provide evidence of the sensitivity of management's financial wealth to firm performance. The hypothesis that director incentives affect firm performance is consistent with the evidence in Bhagat, Carey, and Elson (1999).

2.4.2. Shareholder proposals. A mechanism of corporate governance used increasingly by certain institutional and individual investors is shareholder proposals, which are included in a firm's proxy materials under SEC Rule 14a-8 and voted on at the annual shareholders' meeting. The most active institutional users of this tool, public pension and union funds, sponsor a variety of proposals that they assert will improve performance, including proposals to enhance the independence of the board, reform executive compensation, remove takeover defensive tactics, and adopt confidential voting (see, e.g., Del Guercio and Hawkins, 1999). Under the SEC rule, the institutions must notify management of their intent to submit a proposal in advance of the meeting, a requirement that has the beneficial effect for the sponsor that management will frequently negotiate a compromise in order to avoid the proposal's submission (see Del Guercio and Hawkins, 1999).

Numerous event studies have been undertaken to determine whether the introduction of a shareholder proposal affects firm value. The uniform finding is that they do not (see Romano, 2001).[10] The absence of a significant effect is not likely due to imprecision in the event study methodology, because in these studies the sample sizes are large and the event dates are precise (see Bhagat and Romano, 2002). A plausible explanation of the absence of a price effect is that the objects of many shareholder proposals—independent boards, limits on executive compensation and in particular on incentive pay, and confidential voting—do not, when investigated by event studies, significantly affect firm value (see Romano, 2001). It is improbable that a proposal to undertake a governance strategy that does not itself significantly affect prices will produce a price effect.

It is troubling that institutional investors, who are in most cases, after all, fiduciaries, would spend significant effort sponsoring proposals that are not likely to improve firm performance. A lack of information regarding the appropriate governance policy to adopt does not seem a plausible explanation for the behavior of at least the most prominent sponsors of

10. Negotiations with management, by contrast, have been found to produce both significant positive and negative price effects. As discussed in Romano (2001), the difference may be explained either as evidencing that management selects the highest valued proposals for negotiation (or lowest for the negative effect studies) or that negotiation, by indicating management's responsiveness to certain investor concerns, provides a signal of management's quality (the price effect reflects market updating regarding management quality rather than the value to the firm of the omitted proposal).

proposals, who are sophisticated institutions. Public pension fund managers might well be informed about which proposals are useful and still champion fruitless proposals, however, if the managers obtain private benefits from submitting such a proposal, given the absence of strong incentives of boards of public funds to monitor their staffs (or the presence of similar private benefits for board members).

The fact that, in contrast to public pension funds, private pension and mutual funds do not engage in activism has been explained by the competitive nature of the industry, or as cost-conscious private funds' free-riding on the expenditures of activist public funds (e.g., Black, 1998, p. 460). This may be so. But there is a further, complementary explanation, that private institutions' managers are less likely to obtain private benefits from engaging in shareholder activism than public and union fund managers.[11] Both explanations are provided with support from survey data indicating that private fund managers perceive the costs and benefits of shareholder activism differently than public pension fund managers (Downes, Houminer, and Hubbard, 1999, pp. 32–4).

In short, financial economists have not been able to identify a positive performance effect of shareholder activism because much of that activism would appear to be misdirected. To the extent that this mismatch is due to problematic behavior on the part of fund managers sponsoring proposals involving private benefits, potential solutions are the adoption of better internal control devices for fund boards, such as program audits, or a reduction in the current subsidy of the presentation of proposals by requiring sponsors of losing proposals to reimburse the corporation, in whole or in part, for the cost of the proposal (see Romano, 2001).

2.4.3. Shareholder derivative suits. Several studies have examined the stock price effects of the filing and disposition of shareholder derivative suits. Fiduciary duties enforced by derivative suits in theory perform an important deterrent and compensatory function in the agency cost perspective on the firm. But, because the cost of such litigation is generally greater than a shareholder's pro rata benefit, the legal regime relies on

11. For examples of possible private benefits relevant to public pension or union fund officials in contrast to private fund managers, related to furthering political reputations or collective action goals, see, for example, Romano (1993b, p. 822) and Thomas and Martin (1998, pp. 61–62).

the incentives provided by plaintiffs' attorneys' fees to ensure that such lawsuits will be brought. In contrast to the conventional rule in U.S. litigation that parties bear their own legal costs, when the action provides a "substantial benefit" to the corporation, the plaintiff's legal expenses are reimbursed by the defendant firm, even if there is no cash recovery fund from which to pay out such fees. Although this legal response solves the shareholders' collective action problem in policing fiduciary duties, it creates a host of new incentive problems, because the interests of the plaintiff's attorney and the shareholders are not necessarily aligned (see Coffee, 1985).

Indeed, the agency problem between attorney and client is exacerbated in the derivative suit setting by the terms of directors' and officers' liability insurance. The policies reimburse individual defendants' legal expenses, as well as any required payments to shareholders, in the absence of an adjudication of deliberate fraud. Thus, even if the probability of an adverse judgment is very small, individual defendants have a powerful incentive to settle, as that strategy guarantees that they will avoid any personal expenditures on the litigation. The attorney-client agency problem, when coupled with the incentives of defendants to settle, appears to produce two problematic trends: frivolous claims tend to be overcompensated; meritorious claims, undercompensated. The plaintiffs' bar is the principal beneficiary of the system (see Romano, 1991).

Researchers have undertaken event studies of shareholder suits to get a handle on the scope of the agency problem. The hypothesis is that if a suit is nonmeritorious or, to put it another way, if a suit will result in a substantial payment to the plaintiff's attorney with only trivial benefits going to shareholders, then its filing will not produce a positive price effect, and may well produce a negative effect on the firm. The effect of the underlying conduct should not be confounded with any effect upon a lawsuit filing because the fraud (whether by affirmative misstatement or omission) or negligence will have been revealed before the filing and hence will have already been impounded in the stock price: without revelation of the misconduct no suit would be brought. But if investors anticipate the lawsuit's filing upon revelation of the misconduct, then there will be no additional impact on its occurrence and the returns around the filing date

will not be able to identify investors' views of the efficacy of shareholder litigation.[12]

Romano (1991) examines the impact of lawsuit filings. The abnormal returns upon derivative suit filings are insignificant. One explanation of this result is that the market anticipates the outcomes of such suits: derivative suits typically result in no or very low monetary rewards. In Romano's sample, for instance, most derivative suits did not produce a monetary recovery, and for those that did, the value was less than 0.5% of firm assets, or $0.15 per share net of attorneys' fees (Romano, 1991, pp. 61–62). The principal beneficiary of the litigation was the plaintiffs' attorneys. The data on derivative suit recoveries, in conjunction with the earlier noted distinctive incentives for settling in this context, provide an explanation of the difference between the wealth effects of derivative suits compared to corporate litigation in general, discussed in the first part of our review (see Bhagat and Romano, 2002): derivative suits do not have a significant negative impact, in contrast to most corporate litigation, because the indirect financial distress costs from such disputes are extremely low.

Fischel and Bradley (1986) investigate the stock price reaction to court decisions on motions to dismiss a derivative suit. They find a significant negative reaction to suit terminations and an insignificant positive reaction to decisions not to dismiss. Although this result suggests that the market views derivative suits positively, the authors conclude that the suits have no significant wealth effects because when returns are cumulated around the filing date they are insignificant. In contrast to their study, Romano finds no significant price effect for lawsuit dispositions: dismissed derivative suits have insignificant negative returns, but so do settled suits. These data are in accord with Fischel and Bradley's conclusion that the wealth effects of derivative lawsuits are negligible. Of course, this is simply confirming the aforementioned disposition data, that shareholders do not receive substantial compensation in derivative suits.

The event study methodology is not directed at measuring any potential deterrent effect from the existence of derivative lawsuits. Such a

12. Such anticipation could affect the stock price reaction to the news of the underlying misconduct, exacerbating a decline if investors believe that the lawsuit will reduce value (e.g., if they believe it will transfer corporate funds to outsiders, such as the plaintiff's attorney), and mitigating a decline if investors believe that the lawsuit will produce a significant recovery.

third-party effect would not be incorporated in a sued firm's stock price. Because, in order for lawsuits to deter misconduct generally, managers who are sued need to suffer a penalty or sanction (there must be specific deterrence), Romano (1991) investigated whether top management of sued firms experienced a decline in compensation, an increased frequency of termination, or a decrease in directorships held on other companies' boards, compared to management of firms, matched by industry and size, that were not sued. Romano failed to find any significant differences across management on all of the dimensions she measured—compensation, employment, and directorships—and therefore concluded that derivative suits do not provide specific deterrence. This finding throws into question the possibility that much is provided in the way of general deterrence.

The data indicating negligible wealth effects from derivative suits supports consideration of proposals to reform the procedural rules to better align the incentives of plaintiffs' attorneys with the interests of investors. One possible solution proposed by Macey and Miller (1991) is to eliminate the named plaintiff to underscore that the attorney is the real party in interest and to have a court auction the lawsuit to law firms in order to ensure that the litigating attorney has an incentive to obtain the highest recovery, as well as to screen for meritorious claims. The event study findings are not probative on whether any particular proposal, such as the lawsuit auction idea, would work, but they do bolster the frequently expressed view that current rules do not compensate shareholders as well as they do plaintiffs' attorneys, which lends credibility to the belief that the claims of misconduct underlying many of the suits are insubstantial and the regime ought to be reformed.

2.4.4. Executive compensation. A final mechanism of corporate governance that has been the subject of event study analysis is incentive-based executive compensation.[13] Incentive compensation plans that grant stock or stock options to senior management must be approved by shareholders under stock exchange rules, and new plans or amendments to existing plans are accordingly described in the proxy materials distributed before

13. For a review of the empirical research on executive compensation, which consists of numerous approaches in addition to event studies, see Murphy (1999).

the shareholders' annual meeting. Thus, as in the event studies of changes in board composition or shareholder proposals, proxy statements can be used to identify the specific events of interest. Brickley, Bhagat, and Lease (1985) report a 2.4% positive abnormal return for the adoption of stock-based compensation plans. This finding follows from a careful screening of proxy materials to ensure exclusion of firms for which there are confounding events disclosed in the proxies, as well as the use of a variety of event dates, given the potential ambiguity over whether the proxy statement is the first public announcement of the compensation plans. In addition, Yermack (1997) finds significant price increases after not publicly announced grants of executive stock options.

Both of these studies' results are consistent with the view of stock compensation as a critical mechanism for reducing agency costs, because they indicate that the market expects incentive-aligning compensation to improve performance. An alternative explanation, which is less benign, however, is that management adopts stock option programs or approves specific grants when it expects events that are likely to increase stock prices to occur. But this second explanation seems to be somewhat less plausible of the two because Yermack does not find that executives receive larger quantities of stock when the grants occur before favorable announcements.

The positive market assessment of the use of stock-based incentive compensation not only confirms the agency models but also provides additional support for rethinking the shareholder proposal process. Studies of the aftermath of shareholder activism on executive compensation have found that stock-based incentive compensation declines when firms receive such proposals or are targeted by the California Public Employees' Retirement System (see Romano, 2001). Because stock-based compensation is positively valued by investors, proposals that result in a decrease in such compensation are not likely to be in shareholders' interest. These proposals do involve, however, a politically charged subject—high levels of executive pay—that has considerable cachet value for labor union leaders and public pension fund managers with an eye on higher public office.

2.5. Summary of the Role of Event Studies in Corporate Law

Event studies have been influential in the making of corporate law and in corporate law scholarship. They have informed the major policy debates over the production of corporate laws and takeovers, and the jurisprudence on securities law. The impact of empirical research on these issues can be overstated: the strength with which particular corporate law commentators hold priors concerning the appropriate policy will cause them to update those priors differentially. But over time empirical research does have an effect, and its effect has reached beyond the academy to corporate law decision makers. This is precisely what has occurred in the state competition and takeover debates over the past two decades: academic consensus shifted to a more favorable assessment of state competition and of takeovers as well as defenses, and the approach of the SEC and the Delaware courts has changed accordingly. A similar process may well occur in the current debates over the efficacy of corporate governance devices.

3. Event Studies of Regulation

Since the pathbreaking work of George Stigler (1971), commentators have questioned whether regulation should be interpreted as serving the public interest, or understood as benefitting the interests of the regulated or a subset of the regulated. Event studies can be used to assess the wealth effects of government action on regulated firms and thereby test these different theories of regulation. For instance, if regulated firms suffer stock price declines from unexpected agency action, it is not probable that such firms have captured the regulators. If the action instead produces abnormally positive stock returns, this would suggest that regulation has provided a net benefit to regulated firms (the market expects future cash flows to increase), and it is supportive of Stigler's producer-protection theory of regulation, rather than a consumer-benefit theory.

Because, to be applied, the event study methodology requires that the regulation under study be unanticipated, it will not be useful for evaluating existing regulation.[14] Moreover, the methodological difficulties regarding

14. In such a context a different empirical approach is necessary. If firms earn economic rents due to regulation because, for instance, regulation creates artificial

the dating of events that arise in event studies of legislation are equally relevant to those of regulation, as regulatory change often unfolds over a lengthy time interval. Despite these caveats, researchers have been able to identify regulatory changes that can be profitably examined through the lens of the event study methodology. Space does not permit us to provide a detailed review of the use of event studies to arbitrate the debate over theories of regulation. We provide instead a highly selective review of the literature by discussing a subset of event studies of changes in banking regulation. Other regulatory areas that have been studied include changes in the regulation of stock exchanges (Jarrell, 1994) and airlines (Beneish, 1991).

Event studies have been used to estimate the wealth effects of numerous banking rules, and therefore to determine whether banking regulators are captured by regulated financial institutions or act in the public interest. The deregulation of interest rate ceilings in the 1970s has been a particularly interesting focus of research. Constraints on interest rates payable on deposits can be characterized as a government-administered price-fixing agreement that subsidizes depositary institutions at the expense of depositors. In this scenario, the removal of the interest rate ceilings in the 1970s will reduce the subsidy, and hence the value, of the thrifts. If banking regulators are captured by financial institutions, however, we would not expect the lifting of ceilings on different types of deposit accounts to reduce the regulated firms' values. Rather, the regulatory changes should result in a net inflow of funds to the thrifts, preventing disintermediation that would otherwise occur as the difference between the bank's rate ceilings and market interest rates increased, and we would expect a positive stock price reaction.[15]

Dann and James (1982) investigate the price impact of three rate ceiling changes from 1973 through 1978. These changes were adopted without

barriers to entry, this value will be reflected in stock prices. The value of the rent can be measured either by subtracting the estimated replacement cost of assets from the stock price, or, by equating it with the price of a saleable license where one exists, as with a taxicab medallion (Schwert, 1981).

15. A third hypothesis, the "ceiling circumvention" hypothesis, predicts there would be no change in value from removal of interest rate ceilings; in this characterization, any subsidy that thrifts received from the rate ceiling in the form of limiting price competition would be fully dissipated through non–price competition, and depositors would obtain the equivalent of the market interest rate despite the existence of the rate restrictions (see Kane, 1981).

prior public announcement, which increases the accuracy of the event study methodology since the regulatory action is both unexpected and readily datable. They find that savings and loan institutions experienced significantly negative stock price reactions on the removal of interest rate ceilings for small accounts and on the introduction of short term variable rate money market certificates (MMCs) but not on the removal of rate ceilings for large accounts. Dann and James conclude from these data that the thrifts had been earning economic rents from the restrictions on small saver accounts. The losses to the thrifts from the regulatory change is evidence against the capture theory of regulation, because under such a theory regulatory actions should not adversely affect regulated firms.

The insignificance of the removal of the ceiling on large accounts is consistent with either the capture theory or the ceiling circumvention hypothesis. Dann and James therefore examine more closely the thrifts' business to determine which effect, small or large accounts, is more important, in order to evaluate which regulatory theory best explains the data. They note that the subsidy reduction or consumer protection interpretation of a rate ceiling removal is relevant only where the institutions were previously able to engage in price discrimination (that is, for inelastic deposit sources), since only under such circumstances would a rate ceiling produce a subsidy or rent to a thrift. If large accounts are more elastic, then they would not be a significant source of rents under a ceiling restriction. In fact, large deposits constituted less than 2% of the sample firms' financing. In addition, the proportion of small deposits drops dramatically from 1978 through 1980 by a fraction equal to the increase in deposits subject to market-based variable rate ceilings (which are largely MMCs). These facts suggest, as Dann and James conclude, that the data are most consistent with the subsidy reduction hypothesis as opposed to the producer protection or capture theory of regulation in explaining the interest rate ceiling removals.

However, in another study, James (1983) investigates the stock price effects of the same events by type of financial institution. He finds significant differences across banks: while wholesale banks (banks with less than 10% of their deposit portfolio in passbook accounts) experienced significant positive abnormal returns from the regulatory changes, the abnormal returns to retail banks (banks with 20% or more of deposits in passbook accounts) were significantly negative. This suggests that the regulatory

regime had substantial redistributive effects across banks, benefiting small retail banks at the expense of wholesale commercial banks, and, analogously, that deregulation (liberalization of interest rate ceilings) affected an intra-industry wealth transfer. It further demonstrates the difficulty of testing a simple capture theory of regulation: conflicts of interest regarding particular regulatory policies within a regulated industry may make identification of a proper hypothesis test difficult. Thus studies of the wealth effects of regulation must be careful to consider the possibility of divergent interests within a regulated industry. A researcher may fail to detect significant wealth effects associated with a regulatory change if he or she does not control for potential intra-industry effects, and might therefore incorrectly conclude that he or she has provided evidence in support of or against the economic theory of regulation.

Other studies have also found that changes in banking regulation effect a redistribution across financial institutions and have related such an impact to the regulatory capture theory. Cornett and Tehranian (1989, 1990) find that 1980 banking deregulation and 1982 banking reforms produced positive abnormal returns for large commercial banks but negative abnormal returns for small banks and savings and loans. Moreover, at the one point in the sequence of legislative events when the 1982 reform movement seemed to falter, the signs of the abnormal returns, which were significant, reversed for the two sets of institutions.

The larger institutions were expected by investors to fare better under the regulatory reforms because, as more cost-efficient producers of financial services, they would be less likely to experience failure in the more competitive environment fostered by the legislation.[16] Cornett and

16. It should be noted that the divergent interests across the banking sector between big and small banks is a reason why the event study methodology proved useful in this regulatory reform context. Congressional regulatory reforms are likely to be anticipated by the market because of the lengthy legislative process (see Binder, 1985), and hence will not be useful subjects for event study analysis. But when the regulated industry is divided over reform, there is uncertainty over the content and likelihood of enactment of reform legislation, and hence specific legislative actions resolve uncertainty and are genuine "events." Indeed, in the course of the 1980s' banking legislation, there were numerous disagreements among congressional committees, and judicial rulings invalidating regulators' reform-oriented actions, which created substantial uncertainty over statutory enactment; Cornett and Tehranian believe that this is the reason why, in contrast to other studies of regulatory changes, there are so many significant events associated with the passage of the banking reforms.

Tehranian contend that the wealth redistribution within the industry from these regulatory reforms is consistent with Stigler's capture theory. The reasoning follows two steps. First, large banking institutions were unable to compete with unregulated nondepository financial intermediaries (such as mutual funds) under the existing regulatory regime, which included interest rate ceilings, and the reform legislation was intended to remedy that situation. Second, the large banks' positive abnormal returns indicate that the market expected them to benefit from the legislation permitting competition, a regulatory reform that was backed by the banking regulators. In other words, their large size indicated that they would not be likely to fail when confronted with increased competition, in contrast to the small banks that experienced negative returns; thus these banks obtained all the benefits and few of the costs of competition. Because the wealth transfer was intra-industry, in this instance evidence supporting the capture theory of regulation is not necessarily evidence that the regulatory action was contrary to consumers' interest. A recent comprehensive review of event studies of specific regulatory, legislative, and judicial actions that reduced the restrictions on banks' product lines and geographical reach from 1970 through 2000 offers a less positive assessment: Carow and Kane (2001) conclude that the reforms are not likely to have benefited the public, because the relaxation of restrictions significantly benefited large banks, redistributed wealth across financial sectors (in particular, adversely affected the incumbent competitors of large banks authorized to enter securities and insurance markets), and negatively affected bank customers in the event studies that were able to investigate such an impact.

4. Conclusion

This two-part review has sought to demonstrate how event studies can have, and have had, an important role in the making of public policy. Using conventional financial econometric techniques, event studies provide an anchor for valuation: they enable a researcher to measure the wealth effect on investors of events, such as litigation, statutory change, or regulatory change, as well as firm-level actions (actions undertaken by corporate managers, such as an acquisition or a dividend payment). Event studies are, by definition, limited to policies affecting publicly traded

corporations, since the analysis is of stock returns. However, by dollar amount, such entities are the source of the vast majority of business transactions in the economy, and much government regulation is directed at their activities. This is also the reason why the most prevalent use of the methodology in law has been in the corporate law field: there is a natural fit between the legal regime's objective of protecting shareholders' interests and event studies' ability to quantify the impact of firm and governmental policies on their interest.

As the first part of this review (Bhagat and Romano, 2002) emphasized, it is important to identify accurately the date of the event under study and to have a sizeable number of firms subject to the event, in order for the methodology to work well (that is, to detect abnormal performance only when it is present). If these conditions are not met, an event must have a comparatively large wealth effect of several percentage points to be detected by the methodology. Thus, both the practitioner and user of the event study must keep in mind the setup of the study when evaluating its results. What can be gleaned from the results of a properly conducted event study in the areas of corporate law and regulation? Policies that adversely affect the value of corporations or redistribute wealth across different sectors of an industry might be worthy of greater scrutiny than those that do not.

In this article, the second part of our survey, we have reviewed in detail how event studies have informed long-standing debates in corporate law, supporting a positive assessment of the competition across states for corporate charters and the benefits to target shareholders from takeovers and suggesting that the wealth effects from much of shareholder activism are insubstantial. The event study methodology has had an even greater influence on securities litigation than on corporate law. The Supreme Court has taken the premise of the methodology—that stocks trade in efficient markets—as the basis for establishing reliance in civil actions for fraud under the federal securities laws and, paralleling the Court's line of reasoning, the SEC has used the methodology to establish liability and the measure of damages in insider trading actions. Because of the limitations of the methodology when dating of the event is imprecise, the results of event studies evaluating the wealth effects of regulation are likely to be less clear-cut than those of event studies evaluating corporate law and governance matters. But some researchers have been able to identify suggestive

evidence of distributional effects of regulation in the banking area, favoring more producer- than consumer-oriented explanations of policy change. Undoubtedly, new uses of the approach will emerge in the future beyond our survey, which is admittedly incomplete regarding the diversity of current practices. It is, in fact, our hope that this survey will spark such innovations.

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