

VERSION 2.1

**MERGER OUTCOMES: AN ANALYSIS OF THE PREDICTIVE POWER
OF EVENT STUDIES AND OF MARKET STRUCTURE**

John Kwoka

Chengyan Gu

Northeastern University

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1. Introduction

In contrast to most areas of antitrust enforcement, merger analysis is largely an exercise in prediction. Under the provisions of the Hart-Scott-Rodino Act, the Justice Department (DOJ) and Federal Trade Commission (FTC) must be notified of a prospective merger meeting certain size criteria, and then given a minimum period of time in which to evaluate the merger for possible competitive problems and perhaps to file a legal challenge. This process is unlike enforcement actions brought against cartels or against dominant firms engaging in exclusion or predation, where typically the alleged anticompetitive practice has already occurred and the remaining questions involve determination of causation and measurement of the extent of harm. In the case of mergers, except for the occasional challenge to a consummated merger, the relevant agency must be able to predict the outcome of an event that differs to varying degrees from all past experience and then, if it decides to challenge the merger, to convincingly explain the basis for that prediction to a court of law.

The task of prediction has generally been met by some combination of economic theory, empirical evidence, and past experience. At least since 1968, the prevailing approach has been summarized in the Horizontal Merger Guidelines issued by the Justice Department and the FTC. The first Guidelines articulated a fairly rigorous structural standard, that is, one that relied predominately on market shares and concentration as the basis for predicting the outcomes of mergers. This approach reflected the then-current understanding of the role of market structure in determining market performance, and formed the intellectual basis for a stringent merger policy.

Subsequent revisions of the Merger Guidelines altered the strict numerical standards of the original Guidelines and also broadened the analytical framework beyond market structure. Thus, market share and concentration thresholds were relaxed, and additional factors—notably, entry conditions and possible efficiencies—were more fully integrated into the analytical process. With the issuance of the 2010 Merger Guidelines, this more expansive and eclectic analytical approach reached its zenith. The new Guidelines

explicitly state that “merger analysis does not consist of uniform application of a single methodology”, but rather the application of a “range of analytical tools.”¹

Throughout, many observers have been concerned about the predictive power of a largely structural standard to merger enforcement, both its overall error rate and its supposed tendency toward incorrectly forecasting anticompetitive effects from mergers. Prodded as well by the insistence of the courts and enabled by advances in economics, they have sought alternative or supplementary methods of prediction. Over the past 25 years, one leading contender has been event study methodology. Event studies rely on evidence from changes in the stock prices of the merging firms and their rivals when the prospective merger becomes publicly known, to infer possible competitive problems with the merger.

The argument underlying event study methodology is simple: According to the efficient market hypothesis (EMH), stock prices capture all available public information about a firm’s financial prospects. Thus, a merger that is likely to increase market power can be expected to raise the stock prices of the merging companies relative to some benchmark group. By itself, examining merging companies’ stock prices is insufficient since a merger that promises increased efficiencies will also raise their profits and price of their stock. To distinguish market power from efficiencies, advocates argue that one should examine the stock prices of *rivals* to the merging companies. An efficiency enhancing merger should cause rivals’ stock prices to fall² since those rivals would be facing a more formidable merged competitor. But if rivals’ stock prices were to rise, the inference is that all firms in the market are expected to benefit from the merger, an outcome consistent with enhanced market power.

This logic motivated an effort to employ event studies instead of, or at least in addition to, traditional structural analysis of mergers (Eckbo, 1985). Event studies have their own limitations, however, and came under considerable criticism, especially from the economics community. A vigorous debate between its advocates and critics in 1989 did not settle the matter (Werden and Williams, 1989a, 1989b; Eckbo, 1989).

¹ Horizontal Merger Guidelines, Aug. 2010, p. 1.

²If the merger signals the possibility of industry-wide efficiency improvement, rival firms could earn some positive abnormal returns as well.

Some continued to utilize event studies for merger analysis³ (Prager, 1992; McGuckin et al, 1992; Schumann, 1993; Mullin et al, 1995; Cox and Portes, 1998; Warren-Boulton and Dalkir, 2001; Hosken and Simpson, 2001; Duso et al, 2006, 2007, 2010) and in making their cases before the antitrust agencies.

Largely lacking in all this has been actual evidence concerning the reliability of event studies as a method for merger evaluation. The reason has not been a dearth of event studies. The relative ease of conducting such research has spurred an explosion of published studies on merger and many non-merger “events” (Whinston and Collins, 1992; MacKinlay, 1997). Rather, there has been an acute shortage of reliable measures of the actual post-merger firm performance to which the predictions of event studies could be compared. A few papers have used accounting data to investigate the correlation between post-merger accounting profit and premerger share price, but accounting data also have some well-known problems. There appears to be only one paper in the literature that utilizes reliable post-merger data to draw the appropriate comparison. McAfee and Williams (1988) identify a merger shown in a prior study to have resulted in higher product price, and conduct their own event study for that merger. They find no indication that the merger was predicted by the stock market to be anticompetitive, and they conclude that at least for that merger reliance upon a premerger event study would have resulted in an incorrect prediction and policy determination.

The McAfee-Williams approach to evaluating event studies—retrospective comparison of the predictions against realizations—does not appear to have been replicated, and consequently it is unclear whether its finding is *sui generis* or correct in its cautionary conclusion with respect to the reliability of event studies. It is the purpose of this paper to substantially fill that evidentiary void. It does so by utilizing a data set on the actual effects of a considerable number of mergers—some 40, to be exact—on product prices. For each of these mergers we perform a new event study, and then compare the predictions of the event studies with the known outcomes of those mergers.

³For surveys of literature on using event studies in merger analysis, see Pautler (2003), Cichello and Lamdin (2006) and Duso, et al (2007).

Each of these steps will be detailed in the following sections. Ultimately the evidence supports the following two major conclusions. First, event studies are poor predictors of the actual outcomes of mergers, failing to correctly identify anticompetitive mergers in more than three-fourths of cases. Various statistical tests confirm that predictions based on premerger stock price movements of the relevant firms are statistically independent of actual post-merger price changes. Second, conventional market structure factors—concentration and related measures—are in fact statistically more closely related to the post-merger price changes, correctly identifying anticompetitive mergers in about twice as many cases as event studies.

This paper proceeds as follows: The next section provides a bit more background on event studies, both in theory and in practice. Section 3 describes the sample collection procedure, estimation methodology and the method to correct the sample selection bias. Section 4 presents the estimation results and Section 5 concludes.

2. Event Studies in Theory and Practice

Event studies date back to the 1930s, but their current methodology originated with Fama et al (1969) and in the merger context with Eckbo (1983), Stillman (1983), and Eckbo and Weir (1985). With a sample of 259 horizontal and vertical mergers, Eckbo (1983) first computes the abnormal returns for rival firms at the times of merger announcement and any complaint. He finds that rivals of the horizontal challenged mergers on average earn significantly positive abnormal returns at announcement, but do not receive significantly negative abnormal returns at complaint. As a result, Eckbo rejects the market power hypothesis and concludes that merging firm efficiencies lie at the heart of the association and that the structural standard errs in finding competitive problems with mergers that in reality are efficiency-enhancing: “All but the ‘most overwhelming large’ mergers should be allowed to go forward” (Eckbo and Weir, 1985). This conclusion was supported by Stillman’s(1983) study, among others.

Much debate has followed these initial findings, with considerable attention to possible limitations and other interpretations of the finding of abnormal returns. Among the generally recognized limitations are the following (Eckbo, 1985; MacKinlay, 1997):

(1) For merging firms that are large and diversified, it may be difficult to identify abnormal returns to a merger affecting one product. Further, to the extent that rivals are small or have thinly traded stocks, their stock price effects may be difficult to estimate reliably. These issues are more serious when the merger effects on rivals are small.

(2) Information about mergers and other events often leaks out prior to the official announcement, so that too close a focus on the announcement date may erroneously imply the absence of effect. This issue is now commonly addressed by using various event windows or by using the date at which abnormal returns first appear as the exact date around which to construct the window.

(3) Stock returns logically reflect not just the merger but also the likelihood of an antitrust complaint and, further, its likelihood of prevailing. A conventional fix is to adjust the measured stock price change for those probabilities (the Heckman procedure), or alternatively to use announcements of events associated with the antitrust review process—initiation of an investigation, closure of the investigation, issuance of a complaint, etc.—as additional date points for analysis.

In addition to these general issues about methodology, concerns have been raised about the interpretation given to the key finding of abnormal returns to rivals. At least two additional interpretations seem plausible, with fundamentally different implications: (1) The announcement of a planned merger between two firms in an industry might highlight other attractive merger opportunities within that industry, thereby causing rivals' share prices to increase. That increase would therefore reflect improved information about prospects for other firms in the industry, rather than any expected anticompetitive effect from the merger. (2) A merger that increased the likelihood of exclusionary or predatory conduct by the merged firm would diminish the profit outlook for its rivals.⁴ A decline in the latter's share prices at

⁴ These concerns over possible competitive effects of mergers appear explicitly in the Merger Guidelines for the first time in 2010.

announcement would therefore signal competitive concerns from the merger, which will lead to a price rise in the long run.

Although these concerns have diminished enthusiasm for event studies, some research has continued to utilize them, with mixed results. As noted before, McAfee and Williams cast considerable doubt about the reliability of event studies in the Xidex-Kalvar case in the 1970s, merger that eliminated its biggest rival in the market. Warren-Boulton and Dalkir, on the other hand, examine the proposed merger of Staples and Office Depot in 1996. In addition to their careful examination of the effects of various events in the antitrust review process on share prices, they report a close correspondence between the product price increases implied by share price changes, and the likely product price increases from the merger from other estimation techniques introduced into the trial record.

A number of authors have used stock market data in conjunction with accounting measures of merger outcomes. Healy, et al (1992), Kaplan and Weisbach (1992), Sirower and O'Byrne (1998), and Duso et al (2010) all find that stock market returns are correlated with post-merger asset productivity, cash flow returns, and accounting profitability. Duso et al, for example, report a positive and significant correlation between the abnormal returns of 482 merging or rival firms in the EU between 1990 and 2002 and accounting measures of post-merger profitability. While they conclude that event studies might therefore be useful tools in merger analysis, it would seem more accurate to conclude only that event studies and accounting profitability are similarly affected by mergers, but that result does not test or cast light on the relationship to true post-merger performance.⁵ A similar qualification applies to other accounting-based performance measures.

Finally, Beverly (2007) examines four recent mergers investigated by the UK Competition Commission. Examining stock prices of prospective merging parties and of their rivals, she finds as a threshold matter “few cases where the stock price movement was large enough to be conclusively linked” to events in the Commission’s investigatory time line. She goes on to note that normal stock price volatility, lead and lag

⁵Furthermore, Duso et al find this correlation only for what they term “a reasonably long window before the merger—...50 days” (Duso et al, p. 191). A 50-day window is atypically long. Whether it is “reasonably” so is a matter of conjecture.

effects, unrelated events, and other factors imply that event studies in merger cases are likely to produce results that are inherently “ambiguous.”

3. Data and Methodology

3.1. Sample Selection

This study utilizes three different sets of data on the same group of mergers: (a) post-merger price changes, (b) pre-merger stock price changes, and (c) market characteristics such as concentration. The first of these—the actual price changes resulting from mergers—represents the binding data constraint and so will be discussed first. The information used in this study derives from a compilation of merger retrospectives on some 53 mergers that were consummated during the period of 1976 through 2006 (Kwoka, 2013). Merger retrospectives typically rely on a methodology such as difference-in-difference to isolate the effect of a merger on product price. This approach has a number of methodological and substantive advantages. Methodologically, difference-in-difference can circumvent the need for elaborate structural modeling. Substantively, the use of price as the performance variable does not encounter the difficulty of disentangling market power and efficiencies—a price increase implies an adverse net effect on consumers.

The growing body of retrospectives is described in Kwoka. That study screened all candidate studies for acceptability based on a number of criteria, specifically, the following. (1) The study must examine a purely or substantially horizontal transactions. Some "partial" mergers as joint venture and airline code-sharing agreements are included as well, since these are often thought to have effects at least qualitatively similar to those from pure mergers, but vertical mergers are omitted since they raise different economic and policy issues. (2) Given the focus on domestic policy, only transactions involving U.S. companies and markets are included. (3) Since policy takes place at the level of the individual merger or transaction, studies that only report the average outcome for a group of mergers⁶ are excluded. (4) The study must use a recognized technique such as difference-in-difference, which meets modern standards of

⁶ For example, Kim and Singal (1993) report the average price effect for fourteen airline mergers. Such studies may shed some light on the mergers collectively, but they do not permit matching the individual transactions to policy actions and hence are not included for current setting.

research design.⁷ (5) Lastly, it must have appeared in a peer-reviewed journal in economics or closely related discipline, or in a respected working paper series such as NBER, FTC, or DOJ. Some 53 usable estimates of the price effects from identifiable mergers are identified in Kwoka and used in the present study.⁸

From each of the qualifying retrospectives is extracted a measure of the magnitude of the price change resulting from the merger. In many cases multiple results are reported in a study of a single merger—for example, due to alternative model specifications, different levels of aggregation, or a multiplicity of products. In these cases any guidance provided by the author as to the most reliable summary estimate(s) is followed, or if necessary, an average is taken of estimates across the key non-nested and non-duplicative results. In cases where the same merger was studied in more than one retrospective, the summary statistics from all retrospectives are averaged into a single overall value. All of these procedures are designed to avoid any subjectivity when recording the price effects.

We also develop and record a summary measure of the statistical significance of the price effect for each merger. In the simplest case where the effect of a merger is given as a single number, the statistical significance of that number is recorded. Where multiple results of varying significance are reported, we use the author's final conclusion with respect to the whole transaction and record its significance.⁹ For those few mergers studied in more than one retrospective, we record the price change as insignificant whenever at least one of those studies reaches such a conclusion. While there is some arbitrariness in this assignment of statistical significance, it seems better than treating estimates of widely differing statistical significance as if they are identical. Robustness checks fail to disclose major differences in the results due to these assignment rules.

⁷ In fact, all the studies in our data base employ some kinds of difference-in-difference analysis. A few offer alternative measures such as structural modeling as well as difference-in-difference estimates.

⁸ The final data set in this study uses a slightly smaller number, for reasons described below.

⁹ For example, in the New Hanover-Cape Fear hospital merger case, Thompson (2011) finds two of the insurers experience price increases after merger while the other two face price decreases, and concludes that "it is difficult to draw conclusions about the impact of this merger on inpatient pricing". In this case, we record the price change for this merger as insignificant.

Then second necessary data series is stock market data for the relevant firms affected by these merger events. For reasons described above, abnormal returns to the merging firms are ambiguous indicators of the expected effects of the merger, but a finding of positive abnormal returns to rivals of the merging firms at the time of merger disclosure is a necessary condition for predicted anticompetitive effects.¹⁰ Relevant stock market data are collected from the Center for Research in Security Prices (CRSP) database as follows: We first identify the announcement date of the proposed merger using the *Wall Street Journal* and *New York Times*.¹¹ The day that news of the event was first published in either source is defined as the event date. Other information about the merging parties and their rivals is compiled, including such things as the number of competitors, whether the merger involved firm with financial difficulties, whether the transaction was reviewed by antitrust agencies, and the date at which an antitrust complaint challenging the merger may have been filed. Also, unsuccessful bidders and other firms that are actively involved in another takeover either as a target or as a bidder within the period from 20 before to 10 days after the merger announcement are excluded from the rival sample. The result of the above procedure is to reduce the sample size from 53 to 40 cases—still a sizeable number on a topic where previously even a single case has seemed informative.

The third data set consists of market characteristics for each merger, in particular, pre-merger and post-merger HHI, entry conditions, and the number of direct competitors.¹² In some instances the actual necessary data are provided in the retrospective studies or the FTC or DOJ docket sheets. For example, Ashenfelter and Hosken (2010) list the competitors with their market shares for five mergers, and FTC Docket No. 9150 provides detailed market share information of rivals for the merger between Weyerhaeuser and Menasha in 1980. Many other cases, however, lack such public information and a

¹⁰ As also noted above, this is not a sufficient condition, since there are other explanations for positive abnormal returns to rivals. But absent such a finding, the event study methodology does not predict competitive harm (Eckbo, 1985).

¹¹ In fact, there are two early cases that we cannot identify the event dates through WSJ or NYT. For the Xidex/Scott Graphics (1976) case, we use the date showed on FTC's complaint docket as the event date, and for Xidex/Kalvar (1979), we use the event dates identified by McAfee and Williams (1988). Our results for the latter case are consistent with the outcomes in their study.

¹² In some cases, the number of competitors differs from those for which there is stock price data, since some rivals—notably, nonprofit firms—may not have stock data. This results in the elimination of all five hospital mergers.

variety of techniques appropriate to the circumstances are employed. Within the markets which raise the antitrust concerns, we begin by identifying competitors of the merging firms and then seek measures of their sales or capacity. From this information, their market shares, overall concentration, and its change can be computed. The data sources for each case are listed in Appendix Table. A.1.

By way of illustration, for airline industry mergers we seek to examine the principal locus of competitive concerns with each merger, whether that be routes, hubs, regional, or national scope. This is achieved by using the DB1A and DB1B data to identify rivals. Following Singal (1996), the percentage of an airline's revenue passenger miles (RPM) on the routes served by both merging entities is calculated, with rivals that have less than 25% of total route RPM excluded as less likely affected by the merger.¹³ For mergers involving clearly national carriers, we use airlines' nationwide market share to calculate HHI unless the study specifies which regional market raises the antitrust concern.¹⁴ For the latter, a set of hubs may serve as a proxy for the sphere of competition.

In the case of biomedical journals, market shares are calculated using number of journals in that category controlled by each publisher over the total number of biomedical journals listed in McCabe (2000). Similarly, for the law journal industry, the direct competitors are found in McCabe (2002) and Svengalis' *Legal Information Buyer's Guide & Reference Manual* (2005). Then market shares are calculated as the number of relevant titles (including six kinds of legal series) held by each firm, divided by total number of such titles.¹⁵ For the petroleum industry, besides the HHI information supplied by FTC (2004) on several merger cases (such as Shell/Texaco, BP/Amoco, Exxon/Mobil), we use U.S. Energy Information Administration's annual *Refinery Capacity Reports* to get direct competitors and their capacity shares for

¹³ Note that while Singal distinguishes the route served by bidder and by target to identify competitors, we only focus on the rivals on the overlapped routes.

¹⁴ For example, Borenstein (1990) emphasizes the issue of shared hubs for the Northwest-Republic merger. In this case, we use the market shares at the Minneapolis/St. Paul (MSP) airport to calculate HHI. Such data is collected from Maldutis (1987) which provides the HHI for 50 U.S. largest U.S. airports for the period of 1977-1987. Note that DOT's Form 41 data only start from 1990 on its website.

¹⁵ McCabe (2002) and American Association of Law Librarians define a set of the "480 most important" titles from eight broad categories of commercial, print legal serials. Here we use 480 as the total number of titles, as the other "less important" titles may not be close substitutes and will be less affected by the merger. Clearly these measures do not include weights, and hence measure shares and concentration with error.

each wholesale merger.¹⁶ Data at the retail level for individual cities are not available, so at best it may be hoped that wholesale concentration is correlated.

For two acquisitions in the railroad industry, the competitors are defined as the rivals who have routes that overlap with the merging entities, and their market shares are calculated using firms' miles of track from Surface Transportation Board's *Class I Railroad Annual Reports*. That, of course, will not capture concentration at the shipper level. For the Fleet/BankBoston case, besides the market shares provide by the study, information on competitors is gathered from the Federal Reserve Bank of Boston's *Banking Structure in New England 1996-1999 and 1999-2001 (Report 75 and 76)* which have detailed lists of banks and their market shares information for each state and county in the New England area.

Clearly, this third data set on market characteristics contains some imperfect estimates. Nonetheless, within those limitations it will be useful to compare event studies and structure-based predictions. None of these limitations affect the more fundamental comparison between event studies and structural analysis in terms of their reliability in predicting the actual outcomes of mergers from retrospective studies. A summary of these market structure characteristics is reported in Table 1.

3.2. Estimation Methodology

We use the same methodology as in Eckbo and Wier (1985) and McAfee and Williams (1988). For the i th merger in our sample, we compute abnormal returns to the rival firms at the time of the announcement of the merger proposal and, for those firms subject to antitrust challenge, at the time that the formal antitrust complaint was filed. Abnormal returns are obtained by estimating the coefficients of the following regression equation:

$$r_i = \alpha_i + \beta_i r_m + \gamma_i d + \varepsilon_i \quad (1)$$

¹⁶ This method to calculate the market share is consistent with the method used by most of gasoline studies in our sample. See Hosken et al (2011) for example. For some transactions involved only wholesale-level concentration changes (such as terminals and marketing), we identify competitors through FTC (2004, 2011) which provide PADD-level and state-level wholesale concentration estimates for each year from 1994-2010. For a few cases which have concentration changes in several markets, we use the concentration change for the market emphasized by the studies to make them comparable with the subsequent analysis on price changes.

where r_i is the daily continuously compounded returns to the equal-weighted portfolio of rival firms, and r_m is the daily continuously compounded returns to the value-weighted CRSP market index. The term d is a dummy variable that takes on the value of unity for days in the event window and zero otherwise, and ε_i is the daily random error which is assumed to be independent of r_m and d , serially unrelated, and normally distributed with mean zero and constant variance. Note that pooling the returns of the rival firms associated with a given merger to create one equal-weighted industry portfolio allows us to avoid any contemporaneous correlation of returns across firms in the same industry.

The estimation period for the regression is 200 trading days before an event date to 10 trading days after an event date. An event date (the merger proposal announcement date and in some cases the complaint filing date) is defined as day zero. To capture any prepublication leakage of relevant information, we employ five event windows: (-20, 10), (-10, 5), (-3, 3), (-1, 1) and day (0). The dummy variable d takes on the value of zero for the comparison period (-200, -21), and takes on the value of one for each day in an event window. For the event windows that lie inside (-20, 10), we delete the returns outside the event window but inside (-20, 10). For example, for the event window (-3, 3), the returns in the periods (-20, -4) and (4, 10) are dropped.

In a few cases the announcements for a merger (usually in the airline industry) is close in calendar time to other merger announcements. Although we exclude all firms that are actively involved in another takeover either as a target or as a bidder within (-20,10) days around merger announcement from the portfolio of rivals, this situation could still veil the true effect of a merger. As such, we use the method adopted by Knapp (1990). An additional dummy variable is added to isolate the overlapping effects. For example, if two mergers have some rivals in common, and merger 1 makes a proposal announcement 20 days before merger 2's announcement, then a dummy taking on the value of one in the period (1,10) is added for merger 1, and a dummy taking on the value of one in period (-30, -10) is added for merger 2.¹⁷

¹⁷ Robustness tests show that the coefficients changes a little bit but the signs and significances scarcely change when we remove these kinds of dummy variables from the regression.

The average daily abnormal returns to the rival firms for each merger case are shown in the Appendix table. Note in these results that the estimate of γ_i represents the average daily abnormal return (AR) to the portfolio of rival firms. The total abnormal effect of a merger announcement can be computed by multiplying γ_i by l --the length of the event window. For example, for the period of (-20, 10), a total abnormal return equals $31 \times \gamma_i$. It is these variables that represent the predictions of the stock market for each of the mergers in our sample.

3.3. Correction for Sample Selection Bias

One drawback of current sample is that the mergers and other transactions are all consummated cases, and thus not randomly selected from the pool of all proposed and even deterred mergers. In fact, the abnormal return to the rival firms resulting from a merger announcement can be viewed as a mixed expectation about future benefits (the market power effect vs. the efficiency effect) and the uncertainty about whether the government will prevent the merger from taking place.¹⁸ A higher post-merger HHI and a higher Delta (the change in HHI) increases the expected gains in the future, but also drives up the probability of a merger being challenged by the antitrust agencies. This leads to the classical problem of sample selection bias. As a result, following Eckbo(1985), a transformation of Heckman's two-step estimator can be used to correct for such selection bias.

If we define p_i as the predicted probability of a merger been challenged by antitrust agencies, then the adjusted abnormal return (AAR) can be written as $\gamma_i \times l / (1 - p_i)$, where $(1 - p_i)$ is the predicted probability, conditional on the merger proposal, that the merger will be consummated finally. To derive this probability, a dummy variable-*CHALLENGED*-is constructed, taking on the value of one if the merger case was challenged by either the FTC or DOJ,¹⁹ and then it is used to regress on the market characteristics variables. The *logit* estimation results are listed in Table 2. The results show that the probability of

¹⁸ In our sample, all the merger proposals were approved by stockholders. Thus, we do not consider the probability that the proposal can be rejected by the stockholders of the merging firms.

¹⁹ Due to the size of our sample, here we do not distinguish the actions enforced by the agencies. When a larger sample available, a multinomial model could be used to predict the probabilities of different enforcement actions, such as opposed, conditions, structure(divestiture) and conduct. For the differences among those four categories, see also Kwoka (2013).

challenge increases in a highly concentrated post-merger market with a high Delta and substantial entry barriers, where M1 is a dummy taking on the value of one if the post-merger HHI>2000 and Delta>300.²⁰ With this foundation we can proceed to the major issues having corrected for possible bias in the results.

4. Empirical Results

4.1. *Abnormal Returns and Observed Post-merger Price Changes*

Our first and perhaps most important question concerns the reliability of event studies as a method to predict merger effects prospectively. We take as our measure of the actual outcome, the calculated price change determined by the corresponding retrospective study and infer an anticompetitive merger from a finding of a significant price increase subsequent to the merger.²¹ We begin simply by noting the success and failure rates for detecting anticompetitive mergers by using the event study methodology on stock prices of rivals to the merging firms at the time of announcement—a necessary but not sufficient condition for an anticompetitive merger. As shown in Table 3A, there are a total of 22 anticompetitive cases in our sample. Of these, in exactly six cases do event studies find positive abnormal returns to rivals of the merging firms. Thus, in only 6 of 22 cases—27 percent—would the actual anticompetitive outcome have been correctly predicted by premerger stock price movements.

Moreover, of the 18 mergers without ex post anticompetitive price changes, event studies predicted 3 or 16.7 percent of them as problematic. These data imply a 73 percent Type II error rate and a 16.7 percent Type I error rate from reliance upon event studies to determine competitive problems with mergers.

These error rates from event studies can be compared to those that in fact result from the approach used by the FTC and DOJ—the latter presumably reflecting the factors identified in the Merger Guidelines.

²⁰ The numbers used here are chosen arbitrarily but the results have been checked for robustness. In fact, they are largely unchanged to alternatives such as POSTHHI>1800 and Delta>200, or POSTHHI>1800 and Delta>100.

²¹ This is in fact a sufficient but not necessary condition for an anticompetitive outcome in these data, since many of these mergers were subject to challenge and attempts at remedial action by the antitrust agencies. If that action reversed the price effect of a merger, a finding of no price change would be ambiguous, whereas in fact in this sample almost all successfully challenged cases experience some price increases after the merger. In only two cases, the merger between Weyerhaeuser and Menasha, and the code-sharing agreement between Northwest and Continental, experienced some price decrease given the challenge from antitrust agencies.

The success rate for the agencies would be measured by the frequency with which they correctly identify mergers that prove to be anticompetitive. As also shown in Table 3A, among the 22 anticompetitive mergers, the agencies acted against 11 mergers, for a 50 percent “success” rate. While not overwhelmingly high, this rate is nearly twice that for event studies. Moreover, with respect to the 18 mergers that proved not to be anticompetitive, the conventional approach erred by treating 5 of them (27.8 percent) as anticompetitive.

Returning to the issue of the reliability of event studies, we note a further prediction of that methodology. Specifically, in the case of an anticompetitive merger, the filing of a complaint by the antitrust agencies should result in negative abnormal returns to rivals of the merging parties, as their profit prospects, as well as of the merging firms, are diminished. We test this prediction by examining all 16 cases²² in which either the FTC or DOJ filed an antitrust complaint, computing the average abnormal returns to rivals of the merging firms at the time a complaint was issued. The event study will be said to correctly identify an anticompetitive merger when rivals’ stock prices exhibit significant positive abnormal returns for at least one of five event windows at the time of merger announcement followed by significant negative abnormal returns at the time of the complaint. By this stricter rule, none of these 16 mergers would have been correctly identified as anticompetitive using the event study approach. Some of these mergers show negative abnormal returns at the time of merger announcement, while others exhibit positive abnormal returns at complaint, but in combination this rule would have resulted in the failure to correctly identify all the anticompetitive mergers in this sample.

We conduct further tests of the predictive power of event studies vs. conventional structural approaches using a pairwise correlation analysis and a Chi-square test. The first three rows of Table 3B report the correlation between the observed price change and the abnormal return in alternative windows, while the next three rows report the same for adjusted abnormal returns. The first comparison shows that there is no

²²We note that this sample is defined by cases with complaints, since regardless of whether the complaint was meritorious or not, profits of merging and rival firms are diminished in the case of an anticompetitive merger. In fact, 11 of these 16 mergers eliciting complaints are anticompetitive.

significant relationship between abnormal returns as a measure of expected profitability and ex-post measure of price change at the 5% level of statistical significance. This conclusion persists for the adjusted abnormal returns as well as for the simple abnormal return.

Table 4 reports the overall frequency distribution of cases for which the retrospective study and the event study separately found various effects, namely, anticompetitive, competitively harmless/helpful, and insignificant effects. This two-way tabulation of outcome frequencies can be tested to determine whether the observed price effects are not just pairwise statistically independent of the estimated abnormal returns. The appropriate Chi-Square test indicates that we cannot reject ($p=0.462>0.05$) the hypothesis that the pattern of observed price effects and estimated abnormal returns are due to chance. As with the pairwise correlation analysis, this result confirms the absence of correlation between ex-ante measure of profitability and ex-post measure of price change.

We note that these findings run counter to some previously-cited results in the literature that report a correlation between abnormal returns and post-merger accounting profitability. This contradiction may be due to some details of the different studies, such as differences in samples, different performance measures, and different time periods. Perhaps more importantly, a research design-based price change is simply a more direct and reliable measure for detecting anticompetitive effects.

4.2 Market Structure and Anticompetitive Price Effects

To further test the robustness of these findings, we can analyze whether the stock market foresees the anticompetitive price effects. Causation here runs from the actual competitive effect of the merger, as measured by its price effects, to the abnormal returns to rival firms at the time of merger announcement: in an efficient capital market, the market should anticipate the subsequent price effects of the mergers and current stock prices should change correspondingly. In other words, a price decrease, which is expected to hurt rival firms, should lower the rival firms' abnormal returns, and a price increase that is anticipated to benefit competitors would drive up the current stock prices of rival firms.

Table 6 presents regressions of the adjusted abnormal returns to rival firms for five event windows on observed post-merger price changes. Results consistently show that none of the coefficients is significant

at the conventional levels. Moreover, even when we focus only on the 22 anticompetitive cases or 7 pro-competitive cases, the coefficients are still insignificant at conventional levels. Again, those results are consistent with the findings using pairwise and Chi-square tests. The abnormal returns to rival firms at the time of merger announcements do not correctly anticipate the actual post-merger price change of the merging firms.

For the comparison purpose, we also test the predictive power of the structural standard used by the antitrust agencies. The observed post-merger price changes are regressed on various market structure characteristics. The hypotheses to be tested here are that a merger is more likely to have anticompetitive effects in the market with a higher level of concentration ratio and a higher level of merger-induced concentration change.

The two columns of Table 7A list the results using the pre-merger HHI and the post-merger HHI as the explanatory variables. The result shows that an increase in concentration by 100 points in a highly concentrated industry will lead to a 1.0861 percent (or 0.6613 percent if using post-merger concentration) increase in observed post-merger price, after adjusting the sample selection bias. Similarly, Table 7B displays the results using merger-induced HHI change (DELTA) as independent variable, after controlling for the post-merger level of HHI and other market characteristics. Results show that merger-induced HHI change increases merging firms' price significantly: a 100 points increase in DELTA will result in a 2.5833 percent increase (column 3 in Table 7B) of post-merger price.

To sum up, market structure criteria correctly predict post-merger price change with greater frequency and precision than do event studies. Higher levels of concentration and greater changes in concentration in fact have significant predictive power with respect to post-merger price increases.

4.3. When should we trust event studies?

Even if generally inconsistent with post-merger price changes, it is logically possible that event studies can predict the effects of mergers under certain conditions. If those conditions could be identified, then it would be of some research and policy interest to understand when reliance upon stock market data is not misplaced, i.e., when it is capable of correctly predicting merger outcomes. We address this question here

by examining the market conditions associated with correct predictions of event studies. We begin by defining a dummy variable-Successful Prediction-taking on the value of one if : (1) there is a significant positive abnormal return to rivals (for at least one of five event windows)when a significant price increase is observed; or (2) there is an insignificant abnormal return to rival firms when the post-merger price change is insignificantly different from zero. In addition (3), all cases that find significant post-merger price decreaseare grouped as a successful event study prediction, since the sign of the abnormal returns to rivals depends on the relative magnitudes of the negative competitive disadvantage effect and the positive information signaling effect due to an industry-wide efficiency increase.²³ Note that the "success" should be qualified until we can combine the results at announcement with the estimates at complaint, to test the market power hypothesis vs. efficiency hypothesis.

Table 8 reports the results of regressing the successful prediction variable on all the merger-related information at the time of the merger proposal announcement. The pre-merger and post-merger HHI have been divided by 100. The results consistently show that successful predictions by event studiesare not significantly associated with any identifiable market characteristics. In other words, there is no clear pattern or cutoff of the market features between the successful prediction and the failed prediction, and there is no clear pattern that can tell when the market will successfully predict the merger effect.

This finding casts additional doubt on the efficient market hypothesis as a reliable and appropriate method for predicting the outcome of a particular event. This outcome might be due to characteristics of stock market: in a noisy market, many other factors besides these merger-related factors (such as union strikes in airlines industry and shocks from related industries) could affect peoples' correct predication of merger effects, and even where market makes the correct prediction, the current event method cannot identify those effects separately. Some of these possibilities could be tested with a larger data base of retrospective mergers, but there is nothing in these results to suggest that event studies represent a good predictiontool.

5. Conclusion

²³Eckbo and Wier (1985) and Eckbo (1989) provide detailed discussion on these two opposite efficiency effects. The signal effect is called as "precedent effect" by Whinston (2007).

Despite various shortcomings, event studies have long been advocated as a tool for predicting the competitive effects of prospective mergers. The present study is the first to compile more than isolated evidence concerning the reliability of event studies for this purpose. A unique data base of 43 post-merger price changes from well-designed retrospective studies is used, together with new event studies on the abnormal returns to rival firms at the time of the merger announcement. The comparisons between the two represent a test of whether abnormal returns to rival firms correctly predict actual ex-post price changes.

The evidence is clear and overwhelming: event studies systematically underestimate the actual anticompetitive effects of these mergers. Event studies make considerably more Type II errors than the current system used by FTC or DOJ. The pairwise correlation test and chi-square test provide further evidence that ex-ante profitability is in reality independent of ex-post price change. Regression analyses of abnormal returns on observed price change consistently suggest that post-merger price changes do not affect abnormal returns to rival firms in any significant way. Tests of the market concentration doctrine on post-merger prices, on the other hand, support the logic of the structural standard used by antitrust agencies.

Overall, these results cast serious doubt on the efficacy of using event studies as a predictive tool of prospective mergers. In an antitrust system that emphasizes the consumer surplus standard, event studies are found to be unable to provide convincing predictive power to foresee the potential of ex-post price increases. As pointed out in previous research, event studies suffer methodological and identification problems: rival firms might be too large or too diversified for their stock price to be informative; the merger might only have very small effects on rivals; a merger that resulted in predatory or exclusionary behavior would have a similar pattern of efficiency hypothesis; signal effects of future merger opportunity cannot be distinguished from the market power hypothesis; and so forth. These and broader issues with event studies simply corroborate the finding of this study—that at the end of the day, and regardless of specific limitations of event studies or of conventional structural methods, reliance on the stock market for insights into the effects of a merger is misplaced.

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Table 1. Summary of the Cases

Variable	Definition	Mean	Std. Dev.	Min	Max
PREHHI	Pre-merger HHI	2372	1360	846	5275
POSTHHI	Post-merger HHI	3318	2474	892	9363
DELTA	Change of HHI	945	1215	15	4234
ENTRY	Dummy=1 if entry is difficult	0.60	0.50	0	1
NCOMP	No. of direct competitors	7.95	3.50	1	15
FINANCIAL	Dummy =1 if target had financial issues	0.15	0.36	0	1
CHALLENGED	Dummy =1 if the case was challenged by FTC or DOJ	0.40	0.50	0	1
ΔPRICE	Observed price change in percentage	5.50	9.17	-7.3	29.4

Table 2. Probability of being challenged

Dependent Variable: Challenged		(1)	(2)
M1	POSTHHI>2000 and Delta>300	2.7781*** (2.903)	2.7750*** (2.879)
ENTRY	Dummy=1 if entry is difficult	2.3569** (2.239)	2.2770** (2.128)
NCOMP	No. of Direct Competitors	0.1625 (1.264)	0.1637 (1.262)
FINANCIAL	Firms with financial issues		-0.9405 (-0.666)
CONS	Constant term	-4.8070*** (-2.757)	-4.6403*** (-2.640)
Log-likelihood		-16.426	-16.190

Note:t-statistics in parentheses, and * p<0.1, ** p<0.05,***p<0.01.

Table 3. Goodness of Fit for Predictions of Event Studies

A. Counts of Successful Detection of Anti- and Pro-competitive Mergers						
		Total	Challenged	Percent	Non-Challenged	Percent
Anticompetitive Cases (price increase significantly)	A. FTC and DOJ	22	11	50.0	11	50.0
	B. Event Study	22	6	27.3	16	72.7
Non-Anti Cases (price decrease or no change)	A. FTC and DOJ	18	5	27.8	13	62.2
	B. Event Study	18	3	16.7	15	83.3
B. Pairwise Correlation (for all mergers)						
	AR1	AR2	AR3	AR4	AR5	
Observed Price Change	-0.1005	0.0972	0.1434	0.1397	0.0242	
Significance of Price Change	0.0863	0.0800	0.0576	0.0412	0.0012	
	AAR1	AAR2	AAR3	AAR4	AAR5	
Observed Price Change	-0.015	0.0596	0.0752	0.0256	0.0605	
Significance of Price Change	0.2294	0.1736	0.1170	0.1043	0.0452	

Table 5. Chi-Square Test

	Insignificantly Different	Significantly Negative Price Effect	Significantly Positive Price Effect	Total
Insignificantly Different	8	3	11	22
Significantly Negative Abnormal Return	1	3	5	9
Significantly Positive Abnormal Return	2	1	6	9
Total	11	7	22	40
Pearson Chi-Square(4)=3.6088			Pr=0.462	

Note: H_0 : The estimated price effect is independent of the estimated abnormal return; H_1 : The estimated price effect is associated with the estimated abnormal return. The p-value here is greater than the conventionally accepted significance level of 0.05, we fail to reject the null hypothesis.

Table 6. Regression Results

Sample	Dependent Variable Variable	AAR1 (-20,10)	AAR2 (-10,5)	AAR3 (-3,3)	AAR4 (-1,1)	AAR5 (0)
All Cases	Δ PRICE	-0.0010 (-0.092)	0.0030 (0.368)	0.0021 (0.465)	0.0005 (0.158)	0.0011 (0.373)
Anticompetitive	Δ PRICE	-0.0060 (-0.768)	0.0002 (0.025)	-0.0005 (-0.069)	-0.0034 (-0.538)	-0.0007 (-0.117)
Pro-competitive	Δ PRICE	0.0061 (0.316)	-0.0007 (-0.021)	-0.0032 (-0.115)	-0.0038 (-0.352)	-0.0051 (-0.332)

Note: t-statistics in parentheses, and * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7A. Tests of Structural Standard

Dependent Variable: Adjusted Price Change	(1) Pre-Merger HHI	(2) Post-Merger HHI
Low HHI	0.9964 (0.321)	0.0326 (0.009)
Medium HHI	1.6799 (1.188)	1.0399 (1.013)
High HHI	1.0861** (2.095)	0.6613*** (2.836)
Constant	-13.5978 (-0.741)	-7.8933 (-0.669)
R-Squared	0.163	0.237

Note: t-statistics in parentheses, and * p<0.1, ** p<0.05, ***p<0.01. The three variables of Low HHI, Medium HHI and High HHI are obtained by a dummy variable that takes on a value of one if pre-merger HHI<1000, 1000≤HHI<2000, and HHI≥2000. All HHIs are adjusted by dividing 100.

Table 7B. Tests of Structural Standard

Dependent Variable: Adjusted Price Change	(1)	(2)	(3)	(4)
POSTHHI	0.6097*** (3.382)	-0.5587 (-0.944)	-0.6433 (-0.938)	-0.6247 (-0.894)
DELTA	-	2.4922** (2.065)	2.5833* (1.862)	3.0165 (1.425)
POSTHHI×DELTA	-	-	-	-0.0054 (-0.274)
ENTRY	-	-	3.2271 (0.324)	3.2080 (0.318)
NCOMP	-	-	-0.0293 (-0.021)	-0.0391 (-0.028)
FINANCIAL	-	-	-4.3231 (-0.334)	-4.1750 (-0.318)
Constant	-4.5593 (-0.614)	10.6688 (1.040)	11.5605 (0.628)	10.1410 (0.523)
R-Squared	0.231	0.311	0.316	0.317

Note: t-statistics in parentheses, and * p<0.1, ** p<0.05, ***p<0.01. POSTHHI and DELTA are adjusted by dividing 100.

Table 8. Probability of Successful Prediction by Event Study

Dependent Variable: Successful Prediction	(1)	(2)	(3)
PREHHI	-0.0501 (-0.968)	-	-
POSTHHI	-	-0.0495 (-0.955)	-
MI	-	-	-1.0623 (-1.499)
DELTA	0.0148 (0.255)	0.0638 (0.607)	-
ENTRY	-0.5993 (-0.790)	-0.6003 (-0.791)	-0.7229 (-0.982)
NCOMP	-0.0572 (-0.532)	-0.0564 (-0.525)	-0.0466 (-0.467)
FINANCIAL	1.2410 (1.041)	1.2425 (1.042)	1.4898 (1.247)
CONSTANT	1.8112 (1.264)	1.7955 (1.255)	1.2367 (1.207)
Log-likelihood	-24.1967	-24.2097	-24.0285

Note:t-statistics in parentheses, and * p<0.1,** p<0.05,***p<0.01.

Appendix: Average Daily Abnormal Return to the Portfolio of Rival Firms

No	Type	Bidder	Target	Industry	Agency	Action	Remedy	Data Sources of Rivals and Market Structure	Date ^a	Days Relative to Proposal Announcement (Day 0)				
										(-20, 10)	(-10, 5)	(-3, 3)	(-1, 1)	(0)
1	Merger	Northwest	Republic	Airlines	DOJ	Opposed	Overruled	DB1A (1986Q1); Maldutis (1987)	01/24/86	0.0023 (1.121)	0.0055* (1.958)	0.0103** (2.520)	0.0226*** (3.737)	0.0546*** (5.495)
2	Merger	Trans World	Ozark Airlines	Airlines	DOJ	Opposed	Overruled	DB1A (1986Q1); Maldutis (1987)	02/28/86	-0.0119*** (-2.626)	-0.0156** (-2.428)	-0.0110 (-1.502)	-0.0101 (-1.152)	-0.0109 (-0.839)
3	Merger	USAir	Piedmont	Airlines	DOJ	Cleared	None	DB1A (1987Q1)	02/18/87	-0.0003 (-0.138)	0.0019 (0.696)	-0.0016 (-0.380)	0.0031 (0.496)	0.0010 (0.098)
4	Merger	Continental	People Express	Airlines	DOJ	Cleared	None	DB1A (1986Q3)	07/03/86	-0.0036 (-1.414)	-0.0025 (-0.711)	-0.005 (-1.016)	0.0052 (0.696)	0.0001 (0.005)
5	Merger	Delta	Western	Airlines	DOJ	Cleared	None	DB1A (1986Q1)	09/11/86	0.0043* (1.826)	0.0035 (1.079)	0.0002 (0.046)	0.0025 (0.327)	-0.0015 (-0.110)
6	Code-Share	Continental	America West	Airlines	DOJ	Cleared	None	DB1B (1994Q1)	02/22/94	-0.0047** (-2.000)	-0.0063* (-1.960)	-0.0041 (-0.853)	-0.0042 (-0.579)	-0.0094 (-0.752)
7	Code-Share	Northwest	Alaska Airlines	Airlines	DOJ	Cleared	None	DB1B (1995Q3)	08/22/95	-0.0005 (-0.133)	-0.0010 (-0.220)	0.0044 (0.645)	0.0032 (0.310)	0.0142 (0.809)
8	Code-Share	Northwest ^b	Continental	Airlines	DOJ	Opposed	Overruled	DB1B (1997Q4); DOJ (98-74611)	12/16/97	0.0006 (0.223)	-0.0030 (-0.851)	-0.0059 (-1.119)	-0.0056 (-0.711)	-0.0230* (-1.696)
9	Code-Share	Delta	Continental +Northwest	Airlines	DOJ	Cleared	None	DB1B (2002Q3)	08/08/02	-0.0045 (-1.039)	-0.0115** (-2.081)	-0.0101 (-1.266)	0.0062 (0.52)	0.018 (0.871)
10	Merger	America West	USAir	Airlines	DOJ	Cleared	None	DB1B (2004Q4)	04/20/05	0.0027 (0.748)	-0.0006 (-0.118)	-0.0030 (-0.418)	-0.0048 (-0.442)	0.0101 (0.534)
11	Merger	P&G	Tambrands	Feminine Hygiene	DOJ	Cleared	None	Ashenfelter and Hosken (2010)	04/09/97	-0.0008 (-0.397)	-0.0036 (-1.371)	-0.0054 (-1.373)	-0.0095 (-1.595)	-0.0485*** (-5.109)
12	Merger	Guinness	Grand Metropolitan	Spirits	FTC	Consent	Structure	Ashenfelter and Hosken (2010)	05/13/97	-0.0016 (-0.920)	-0.0030 (-1.306)	-0.0041 (-1.228)	-0.0043 (-0.841)	0.0056 (0.634)
13	Merger	Pennzoil	Quaker State	Conventional Motor Oil	FTC	Cleared	None	Ashenfelter and Hosken (2010)	04/16/98	0.0016 (0.957)	0.0003 (0.128)	0.0011 (0.328)	0.0055 (1.092)	0.0200** (2.329)
14	Merger	General Mills	Ralcorp	RTE Cereal	FTC	Consent	Condition	Ashenfelter and Hosken (2010)	08/14/96	-0.0006 (-0.381)	0.0015 (0.676)	0.0030 (0.892)	0.0030 (0.580)	0.0113 (1.287)
15	Merger	Aurora Foods	Kraft (Log Cabin)	Breakfast Syrup	N/A	N/A	N/A	Ashenfelter and Hosken (2010)	05/02/97	0.0050 (1.255)	-0.0007 (-0.305)	-0.0032 (-0.870)	-0.0064 (-1.149)	-0.0046 (-0.473)
16	Merger	Whirlpool	Maytag	Home Appliances	FTC	Cleared	None	Ashenfelter et al (2011)	07/19/05	-0.0029** (-2.496)	-0.0020 (-1.278)	-0.0016 (-0.702)	-0.0049 (-1.423)	-0.0038 (-0.640)
17	Merger	Wolters Kluwer	Lippincott	Biomedical Journal	DOJ	Cleared	None	McCabe (2000; 2002a)	05/22/90	0.0041 (1.467)	0.0015 (0.411)	0.0014 (0.253)	0.0057 (0.680)	0.0052 (0.356)
18	Merger	Elsevier	Pergamon	Biomedical Journal	DOJ	Cleared	None	McCabe (2000; 2002a)	03/29/91	0.0054 (1.634)	0.0063 (1.418)	0.0065 (0.983)	0.0233** (2.344)	0.0315* (1.832)

Appendix: Average Daily Abnormal Return to the Portfolio of Rival Firms (continued)

19	Merger	Thomson	Shepard's	Law Journal	N/A	N/A	N/A	McCabe (2002b; 2004) Svengalis (2005)	11/30/95	0.0002 (0.071)	0.0035 (0.743)	0.0094* (1.887)	0.0076 (1.289)	-0.0042 (-0.512)
20	Merger	Thomson	West Publishing	Law Journal	DOJ	Consent	Structure Conduct	McCabe (2002b; 2004) Svengalis (2005)	10/23/95	-0.0008 (-0.368)	-0.0013 (-0.496)	-0.0008 (-0.225)	-0.0031 (-0.542)	-0.0028 (-0.288)
21	Merger	Reed Elsevier	West Publishing	Law Journal	DOJ	Cleared	None	McCabe (2002b; 2004) Svengalis (2005)	09/13/96	-0.0015 (-0.641)	-0.0014 (-0.443)	0.0029 (0.720)	-0.0034 (-0.583)	-0.0091 (-0.899)
22	Merger	Wolters Kluwer	CCH	Law Journal	N/A	N/A	N/A	McCabe (2002b; 2004) Svengalis (2005)	11/28/95	0.0011 (0.674)	0.0052** (2.400)	0.0053 (1.623)	0.0098** (1.982)	0.0111 (1.300)
23	Merger	Wolters Kluwer	Little Brown	Law Journal	N/A	N/A	N/A	McCabe (2002b; 2004) Svengalis (2005)	08/29/96	0.0016 (0.513)	-0.0021 (-0.834)	0.0016 (0.426)	0.0019 (0.335)	0.0074 (0.759)
24	Merger	Tosco	Unocal	Petroleum	FTC	Cleared	None	EIA (1995); Hosken et al (2011)	11/19/96	-0.0006 (-0.128)	0.0006 (0.103)	0.0027 (0.329)	-0.0034 (-0.266)	-0.0234 (-1.091)
25	Merger	UDS	Total	Petroleum	FTC	Cleared	None	EIA (1995)	02/25/97	0.0009 (0.234)	-0.0002 (-0.031)	0.0019 (0.233)	-0.0099 (-0.827)	0.0029 (0.143)
26	Joint Venture	Marathon	Ashland	Petroleum	FTC	Cleared	None	Taylor and Hosken (2007)	04/01/97	0.0001 (0.075)	0.0009 (0.398)	-0.0038 (-1.143)	-0.0003 (-0.062)	-0.0046 (-0.562)
27	Joint Venture	Shell	Texaco I	Petroleum	FTC	Consent	Structure	EIA (1997); FTC (2004)	03/19/97	0.0000 (0.002)	-0.0026 (-0.243)	-0.0033 (-0.205)	-0.0016 (-0.065)	-0.0356 (-0.842)
28	Joint Venture	Shell	Texaco II +Saudi Arabia	Petroleum	FTC	Cleared	None	EIA (1997); FTC (2004)	03/19/97	0.0005 (0.394)	0.0022 (1.358)	0.0056** (2.423)	0.0029 (0.814)	0.0024 (0.395)
29	Merger	BP	Amoco	Petroleum	FTC	Consent	Structure	EIA (1998); FTC (2004)	08/12/98	0.0008 (0.345)	0.0024 (0.756)	0.0067 (1.498)	0.0074 (1.092)	-0.0141 (-1.216)
30	Merger	Exxon	Mobil	Petroleum	FTC	Consent	Structure	EIA (1998); FTC (2004)	11/26/98	-0.0014 (-0.578)	-0.0025 (-0.745)	-0.0028 (-0.559)	-0.0014 (-0.181)	0.0138 (1.056)
31	Merger	MAP	UDS	Petroleum	FTC	Cleared	None	Simpson and Taylor (2008)	05/25/99	0.0007 (0.231)	-0.0021 (-0.517)	0.0027 (0.443)	0.0006 (0.063)	-0.0024 (-0.148)
32	Merger	Sunoco	El Paso's Eagle Point	Petroleum	FTC	Cleared	None	EIA(2003); FTC (2004) Silvia and Taylor (2010)	12/31/03	-0.0012 (-0.481)	-0.0003 (-0.102)	-0.0005 (-0.096)	0.0050 (0.658)	0.0070 (0.526)
33	Merger	Valero	Premcor	Petroleum	FTC	Cleared	None	EIA(2003); FTC (2004; 2011) Silvia and Taylor (2010)	04/25/05	-0.0009 (-0.408)	-0.0018 (-0.644)	-0.0035 (-0.867)	0.0027 (0.439)	0.0087 (0.824)
34	Merger	Burlington Northern	Santa Fe	Rail	DOJ	Consent	Structure	STB (1996) Firms' websites	07/01/94	-0.0009 (-0.585)	-0.0010 (-0.485)	-0.0019 (-0.591)	-0.0022 (-0.455)	0.0105 (1.269)
35	Merger	Union Pacific	Southern Pacific	Rail	DOJ	Opposed	Overruled	STB (1996) Firms' websites	08/03/95	0.0019 (1.348)	0.0009 (0.512)	0.0023 (0.965)	0.0072** (2.034)	0.0128** (2.085)
36	Merger	Weyerhaeuser	Menasha	Corrugating Medium	FTC	Opposed	Overruled	FTC (Docket 9150)	09/12/80	-0.0035* (-1.950)	-0.0026 (-1.029)	-0.0025 (-0.655)	-0.0035 (-0.613)	-0.0031 (-0.311)
37	Merger	SCM	Gulf & Western	Titanium Dioxide	FTC	Cleared	None	Schumann et al (1992)	07/16/83	-0.0012 (-0.372)	0.0026 (0.596)	0.0127** (2.005)	0.0118 (1.217)	0.0190 (1.140)
38	Merger	Fleet	BankBoston	Banking	DOJ	Consent	Structure	Federal Reserve Bank of Boston (1999; 2001)	03/19/99	0.0013 (0.633)	0.0001 (0.021)	-0.0015 (-0.338)	-0.0053 (-0.814)	-0.0041 (-0.366)

Appendix: Average Daily Abnormal Return to the Portfolio of Rival Firms (continued)

39	Merger	Xidex	Scott Graphics	Microfilm	FTC	Consent	Structure	FTC (Docket 9146);	06/01/76	-0.0013	-0.0025	-0.0095**	-0.0117	-0.0162
				Diazo			Conduct	McAfee and Williams (1988)		(-0.552)	(-0.792)	(-1.997)	(-1.626)	(-1.295)
40	Merger	Xidex	Kalvar	Microfilm	FTC	Consent	Structure	FTC (Docket 9146);	02/08/79	-0.0041**	-0.0050**	-0.0048	-0.0058	-0.0042
				Vesicular			Conduct	McAfee and Williams (1988)		(-2.447)	(-2.149)	(-1.386)	(-1.106)	(-0.462)

Note: t-statistics in parentheses, and * p<0.1, ** p<0.05, ***p<0.01. a. The date of merger proposal announcement. b. Northwest Airlines acquired 14% stake of Continental Airlines in this code-sharing agreement transaction.

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