The Role of Stock Market Studies in Formulating Antitrust Policy Towards Horizontal Mergers: Comment

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"By contrast, antitrust enforcement authorities can and do pool information from all major sources and should have better information than investors. The enforcement authorities compel production of confidential documents and answers to interrogatories from the merging firms, their rivals, and their customers. *As a consequence, one might seriously question the idea that it is possible to learn any more from the stock market.*" Werden and Williams (1989) (emphasis added).

1 Introduction

Since 1950, the Department of Justice (DOJ) and the Federal Trade Commission (FTC) have filed more than 500 antitrust suits against firms involved in mergers. The charge has been that these mergers reduce competition and thus violate Section 7 of the Clayton Act, the principal federal antitrust law regulating corporate mergers and acquisitions. Eckbo (1983) and Stillman (1983) offer a new approach to examining the anticompetitive significance of challenged mergers. This approach is to analyze the abnormal stock price performance of merging firms and their close *competitors* during acquisition and prosecution periods. The idea is that if a horizontal merger is expected to change competition in the merging firms' industry, the resulting change in product and factor prices will be anticipated at the time of the merger announcement, causing the merging firms and their rivals to earn abnormal stock returns. The direction of the abnormal returns to the rivals can be used to discriminate between alternative theories explaining the nature of the expected change in industry competition. Based on samples of horizontal mergers which were challenged under Section 7 prior to 1978, this methodology leads both Eckbo and Stillman to reject the proposition that the typical challenged merger would have been anti-competitive.

How can the government's apparent failure to prosecute anti-competitive mergers be explained? Eckbo and Wier (1985) examine the proposition that legal constraints in effect during the Eckbo and Stillman sample periods essentially prevented the agencies from obtaining the information needed for accurately judging a merger's competitive impact before filing a complaint. The implementation of the Hart–Scott–Rodino Antitrust Improvements Act in September 1978 significantly relaxed those constraints. A major purpose of this Act was to increase the precision with which defendants are chosen by providing the agencies with more information about potential Section 7 violations and more time to analyze the information before they take legal action.

In particular, the Hart–Scott–Rodino Act requires advance notification of mergers above a specified size, and it allows the agencies to defer legal completion of the transactions for a limited period time. Furthermore, it allows the agencies to request information of relevance for judging the competitive impact of a merger from third parties such as *competitors* of the merging firms. The response to such requests has apparently been of value to the agencies: in its 1983 annual report to Congress, the FTC states that this information "usually is sufficient . . . to make a prompt determination of the existence of any antitrust problems raised (by the merger)." By analyzing cases brought after the introduction of the Hart–Scott–Rodino Act, Eckbo and Wier test the proposition that the additional time and information granted

the enforcement agencies in fact have improved their selectivity.

In section 2 of this comment, I summarize the basic methodology and findings of Eckbo and Wier (1985). In section 3 I critically examine several of the speculations made by Werden and Williams (1989) with respect to the quality of the event study methodology in general, and the evidence in Eckbo and Wier in particular. Some concluding remarks are given in section 4.

2   Summary of Eckbo and Wier (1985)

2.1   Horizontal Mergers and Industry Competition: Hypotheses

Two general hypotheses explaining the sources of economic gains from horizontal mergers are the "market power" hypothesis and the "productive efficiency" hypothesis. The predictions of these two hypotheses for the abnormal returns to the merging firms and their rivals are summarized in Table 1. The abnormal returns are associated with events that either increase or decrease the probability that a merger will be full consummated. Probability–increasing events are merger proposal announcements and announcements of pro–defendant Section 7 decisions. Probability–decreasing events are announcements of Section 7 complaints against mergers and pro–government Section 7 decisions. Since the "market power" and "productive efficiency" hypotheses have identical predictions for the abnormal returns to the *merging* firms, the abnormal returns to the *rival* firms are used to distinguish between the two hypotheses.

According to the "market power" hypothesis, a horizontal merger will encourage industry–wide collusion or dominant firm pricing, which causes an increase in (quality–adjusted) product price and/or a reduction in factor price. The resulting price change benefits *all* firms in the industry. Therefore, when a collusive merger is proposed, the market will impound the expected increase in future monopoly rents in the stock prices of the combining firms and their product market rivals. Conversely, the announcement of a subsequent antitrust complaint against the proposed (anticompetitive) merger reduces the probability that the merger will take place, leading to abnormal losses (i.e., reduced monopoly rents) for all industry members' shareholders. Further losses to the rival firms will occur when a pro–government decision about the suit is announced and the market realizes that there is a zero probability the merger will survive. A victory for the defendant will increase the probability of merger survival and produce positive abnormal stock returns to the rival firms.

In contrast, the "productive efficiency" hypothesis suggests that mergers take place because the merger partners can lower production and/or distribution costs by combining their productive resources. The fact that a merged firm under this hypothesis becomes a tougher competitor –placing the rivals at a competitive disadvantage– tends to reduce the market value of the rivals when the merger proposal is announced. However, the prediction of the efficiency hypothesis is more complex: The merger announcement can also signal opportunities for productivity increases available to non–merging rivals, or the existence of undervalued resources owned also by the rival firms. Thus, depending on the relative magnitudes of the

negative competitive disadvantage effect and the positive information signaling effect, the *net* effect on the rival firms of the announcement of an efficient merger can be positive, zero or negative. What gives predictive content to the efficiency hypothesis is the fact that the announcement of a subsequent antitrust complaint (or a pro–government Section 7 decision) reverses the competitive disadvantage effect without removing any valuable efficiency–oriented information disseminated through the earlier proposal announcement. Thus, the efficiency hypothesis predicts zero or positive rival firm abnormal performance in response to the antitrust complaint.

The two studies by Eckbo and Stillman reject the market power hypothesis because rival firms do not show the predicted sequence of abnormal performance explained above. For example, Eckbo finds that rival firms of mergers challenged by the FTC show statistically significantly *gains* in response to the *complaint* announcement. As explained above, this is inconsistent with the collusion argument but consistent with the productive efficiency argument. This result is also consistent with a "pork barrel" model of the antitrust process, in which competitors of the merging firms are successful in invoking the public–administrative process to attack the efficient merger.

2.2 Data Selection: Merging Firms and Competitors

The FTC and the DOJ gather information about mergers from numerous sources. In addition to the merging parties themselves, these generally include other firms potentially affected by the merger. Since 1978, when the Hart–Scott–Rodino Antitrust Improvements Act was implemented, merging companies must inform the agencies of a pending acquisition prior to completion. The enforcement agencies select cases largely on the basis of market share and industry concentration. The Justice Department's *Merger Guidelines* of 1968 contain bidder and target market shares that, until 1982, were likely to trigger an antitrust complaint. For example, a merger between two firms each having four percent of the sales in a market with a four–firm concentration ratio of 75 percent or more was likely to be challenged. The Department's current merger guidelines are somewhat "looser" than the old guidelines, but their focus remains on market structure.

The sample collected by Eckbo and Wier consists of 82 mergers challenged under Section 7 of the Clayton Act between January, 1963, and December, 1981. In each case, the bidder or the target firm and at least one rival were listed on the NYSE or AMEX at the time of the merger proposal. In order to examine the sensitivity of the earlier results to the selection of economically relevant rival firms, *two* sets of rivals were collected, one based on Eckbo's original *five*–digit Standard Industrial Classification (SIC) procedure and another set identified *by the prosecuting agencies* as rivals to the defendant firms. Identifying the latter set of rival firms involved reading case summaries in the American Bar Association *Merger Case Digest* and the Commerce Clearing House *Trade Regulation Reporter*. Where no rivals were mentioned, references given in the summaries to actual published reports of the cases were pursued. ALL NYSE–or AMEX–listed rivals mentioned in these sources are included in the second set of rivals. The total data base contains 82 portfolios of "SIC–based" rivals with an average of five rivals per case. Furthermore, there are "agency–based" rival

portfolios for 36 of the 82 cases. The 36 agency–based rival portfolios contain an average of three rivals per case.

2.3   The Abnormal Performance of Bidder and Target Firms

For each merger in the sample, Eckbo and Wier compute abnormal share price performance (the cumulative average abnormal return or CAAR) relative to each of the three consecutive announcements of the merger proposal, the antitrust complaint, and the case outcome. The estimation period is always 200 trading days before the event announcement day (day 0) through 10 trading days after the event day. To capture pre–announcement leakage of merger–related information, as well as any immediate post-announcement developments, they report results based on the period extending from 20 days before to 10 days after the event as well as the period from one day before to one day after the event.

Note first that both the market power and productive efficiency arguments presented above presume that managers act in their share–holders' interests. In other words, both arguments imply that the typical merger is a value–increasing investment project for the bidder and target firms. The results listed in Table 2 clearly support this assumption: Target firms earn, on average, large and significantly positive abnormal returns relative to the merger proposal date, with an abnormal return of 25.7 percent over day -20 through day 10, and 10.5 percent (more than fifteen standard deviations from zero) over the three–day interval -1 through 1. This result is generally consistent with most recent capital market based studies on mergers. Furthermore, the average bidder earns significantly positive abnormal returns of 3.0 percent over day -20 through day 10, and .80 percent (more than two standard deviations from zero) over the three–day period surrounding the announcement day. This performance is somewhat stronger than that typically observed for bidder firms in a randomly selected sample of merger proposals. However, this sample is clearly not randomly selected: almost every merger proposal in the data base was "successful" in the sense of being accepted by stockholder vote; the bidder and target firms are horizontally related in product markets; and the mergers were all chosen by the government for prosecution under Section 7 of the Clayton Act. It is possible that this selection process in fact captures the most profitable mergers regardless of their anticompetitive impact.

Eckbo and Wier also find that the abnormal performance of the merging firms relative to the antitrust complaint and final outcome announcements supports the hypothesis that the relief imposed in antimerger cases is costly for the defendant firm's stockholders. Bidder firms, and the targets which remain listed around the antitrust complaint and final outcome announcements, experience significantly negative abnormal returns in response to both announcements. For example, over the three days around the antitrust complaint announcement, bidder firms earn on average -1.4 percent abnormal return (significant at a 1 percent level) while on average target firms experience -7.6 percent abnormal losses over this event period. The negative abnormal return most likely reflects expected case–related (legal) expenses, interruption of productive activity during the case period, and cost associated with regulatory conditions frequently imposed on convicted or settling firms. Interestingly, they find that the magnitude of capitalized losses associated with final outcome announcements

4

depends on the actual outcome. Only cases resulting in merger cancellation or divestiture orders lead the market to further reduce the market value of defendant firms. Announcements of dismissal, or final judgments that do not involve divestiture, do not on average cause statistically significant changes in the equity value of defendant firms.

Given this evidence, one can reasonably expect the announcement of a complaint or final decision against a truly anticompetitive merger to affect the market value of the *rival* firms as well. Under the market power hypothesis, the larger the losses to defendant firms associated with complaints or final case outcome announcements, the larger the unanticipated increase in the expected costs of acquiring market power, and the larger the negative impact on rival firms. Tests of this prediction are summarized next.

2.4   The Abnormal Performance of Rival Firms

Eckbo and Wier pool the rival firms associated with a given merger into one (equally weighted) industry portfolio in order to account for any correlation of returns across firms in the same industry. The evidence indicates that rivals of challenged mergers earn significantly positive abnormal returns around *merger proposal* announcements, regardless of the procedure for selecting rival firms. As reported in Table 2 (which, for the purpose of this summary, focus on cases for which we could identify **both** sets of rival firms), portfolios of SIC–based rival firms earn on average 3.0 percent abnormal returns over day -20 through day 10 relative to proposal announcements (more than two standard deviations from zero.) A virtually identical picture emerges from the agency–based portfolios of rival firms. The 36 equal–weighted portfolios earn, on average, 3.3 percent abnormal returns over the same 31–day period.

Furthermore, the typical antitrust complaint and pro–government final outcome announcement *fails* to significantly *reduce* the equity value of rival firms, regardless of the rival firm selection procedure. In fact, the SIC–based rival firms earn a .3% *positive* average abnormal return over the three–day interval relative to antitrust complaint announcements. Furthermore, if anything, pro–government decisions are associated with *larger* (positive) abnormal returns for rival firms than are pro–defendant decisions.

When checked against the predictions listed in Table 1, it is clear that the evidence in Table 2 contradicts the market power hypothesis. On the other hand, the positive abnormal performance by rival firms relative to merger proposal announcements does suggest an *information* effect. Thus, Eckbo and Wier conclude that the typical merger proposal in their sample most likely reveals to the market that some resources also owned by the non–merging rival firms are currently undervalued, or that opportunities for productivity increases (which may have motivated the merger in the first place) are available also to the rival firms. Since the value to the rivals of this type of information is unlikely to depend on the eventual consummation of a proposed merger, a subsequent antitrust action against the proposed merger does not harm rival firms.

2.5 Cases Brought After the Antitrust Improvements Act

It is reasonable to expect that the increase in enforcement powers due to the Hart–Scott–Rodino Act has increased the precision with which the relevant markets and market shares of the merging firms can be identified. If the decision rules used by the agencies to select defendant firms can in fact identify truly anti–competitive mergers, improved ability to apply the rules should be reflected in the evidence based on mergers challenged after 1978, when the Act took effect. However, Eckbo and Wier fail to find any evidence consistent with the market power hypothesis in cases brought after September 1978. The abnormal returns to the bidders, targets and rival firms in these cases support at most the productive efficiency hypothesis. As indicated in Table 3, they find that rival firms earn a statistically significant 2.40 percent average abnormal return over the 31–day period surrounding proposal announcements both before and after September 1978. However, this positive rival firm performance does not on average represent monopoly rents since there is no evidence of significant reactions to the subsequent antitrust complaints or pro–government decisions. Since there is no significant difference between cases brought before and after September 1978, one cannot conclude that the Antitrust Improvements Act has improved the agencies' ability to select truly anti–competitive mergers for prosecution.

3 Comment on Werden and Williams (1989)

Werden and Williams are critical to the idea that one can use stock prices to second–guess the policy decisions of the antitrust enforcement agencies. Unfortunately, they do not present any evidence to shed further light on the basic validity of the collusion hypothesis. Thus, their criticism is purely speculative and, as pointed out below, they frequently confuse the issues. Further, their reference to "data errors" in Eckbo and Wier (1985) is plainly misleading.

3.1 Methodological Issues

(1) "Merger events may be anticipated by the market" (p. 4).

True, but be careful: While bidder firms are often expected to undertake acquisitions as part of a merger program, the identity of the *targets* is largely unanticipated, as evidenced by the uniformly large and significant merger–induced revaluation of the target shares. If the target cannot be anticipated, then the identity of the *rivals* is a surprise too.

(2) "[T]he antimerger law is largely self enforcing" (p. 4).

As argued in the original papers, the issue is not so much whether the law deters monopolistic mergers from being proposed but rather *how many* such mergers are being deterred. The burden of proof is on proponents of the deterrence argument to show *empirically* that the

benefits of deterence exceed the evidenced costs of antitrust enforcement. And don't forget that *efficient* mergers are detered too.

(3) "[U]nions [as opposed to stockholders] capture the lions share of monopoly profits" (p. 5).

This is an empirical allegation which no one has yet put to a serious test. However, if the statement is true, it undermines the concern that monopolistic mergers will be undertaken by wealth maximizing managers even in an unregulated environment.

(4) "[M]any rivals ... derive only a small portion of their revenues from the market or markets affected by the merger" (p. 5).

What Werden and Williams have in mind is that one cannot expect to detect a reaction to a merger event in the rival firms' stock prices. However, this ignores the significantly positive average rival firm performance documented by the original papers. In particular, it ignores the fact that Eckbo and Wier (1985) continue to strongly reject the collusion hypothesis based on the product market rivals cited in court documents prepared by the DOJ and the FTC.

(5) "[F]ailure to find collusive effects does not imply that a merger is not collusive" (p. 5).

This point is trivial. The evidence in the original papers have increased the *likelihood* that the collusion hypothesis is, in fact, false.

(6) "[A] pattern of rivals' abnormal returns consistent with collusion easily could occur for non-collusive mergers" (p. 6)

This is a restatement of the fact that the tests based on rival firms are one-sided: While the collusion hypothesis can be inconsistent with evidence based on rival firms (and is therefore testable), the efficiency/information hypothesis probably cannot (and is therefore not refutable). Since none of the original papers purport to *test* the efficiency argument, it is not clear why Werden and Williams insist that this point is "critical".

(7) "The benefits or costs of a merger generally will be distributed quite unevenly across rivals" (p.6).

While this is certainly true for efficient mergers, the collusion hypothesis implies that all rival firms' stock prices will change in the same *direction*, which is essentially what the literature attempts to test. Eckbo (1985) also present direct test of the hypothesis that the abnormal return to all rivals are the same. This hypothesis is overwhelmingly rejected based on a standard chi-square (seemingly unrelated regression) test statistic.

(8) "A merger may produce efficiency effects in some affected markets and collusive effects in others" (p.6).

True, but don't forget that the original studies all focused their tests on the product market listed in the case-related court documents. According to the enforcement agency's complaint, the merger would "monopolize" this particular market. Thus, the argument is spurious to the central issue of these papers.

(9) "It is likely that some of the mergers in the portfolio were collusive and some were efficient" (p.8).

First the word "likely" is empirically unfounded. Second, Eckbo (1983, 1985) and Eckbo and Wier (1985) present several statistics (in addition to the average abnormal return) which address this issue. For example, significance tests are performed on *each* individual case before the pooling of mergers take place. As a result, one obtains a *distribution* of t-values which reveals the number of cases with a collusion–consistent pattern of abnormal return within the sample. Third, for *each* case, Eckbo (1983) specifically conditions the test of the abnormal return associated with the complaint announcement on the market reaction to the previous merger proposal announcement (and finds that the market reaction to the two announcement appears to be independent, which contradicts the collusion hypothesis). Fourth, Eckbo (1985) performs a set of cross-sectional regressions based on industry concentration and market shares without finding further collusion-consistent evidence.

(10) "Eckbo and Wier arbitrarily consider several [event] 'windows' because they did not know exactly when the market received the new information ... Thus, aggregation implies that an incorrect window will be used for some, probably most, events" (p.8).

First, all event studies center the event on a predetermined news date, usually the *Wall Street Journal* announcement of the event. Clearly, the press date is the last date for which one can argue that the news have not yet been released to the market. This allows for a systematic selection of the event day across cases (i.e., a replicable sampling procedure). Thus, the "arbitrariness" alluded to by Werden and Williams is the fact that most event studies –including ours– also report several multi-day event periods around the event day in order to check whether the conclusions based on day 0 are biased in favor of the null hypothesis of zero abnormal returns. In other words, a study would be criticised for *not* providing the reader with information an abnormal stock returns behavior around the pre-selected event day. Thus, Werden and Williams put this process on its head. As is apparent from Eckbo (1983) and Eckbo and Wier (1985), the rejection of the collusion hypothesis is robust with respect to the choice of such multiple-day event windows.[1]

Finally, on p.6, Werden and Williams rediscovers the information argument developed by Eckbo (1983) and which is the main reason why the efficiency hypothesis cannot be refuted based on rival firms' abnormal returns alone. In particular, they emphasize that the rival firms' stock price, due to the information effect discussed in Eckbo and Wier, can increase in response to the complaint announcement even if the merger is collusive. That is, blocking

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[1]In footnote 8, Werden and Williams confuse the portfolios in Brown and Warner (1985) with the portfolios of rivals used in Eckbo and Wier. In fact, our study is quite representative of the type of studies where, according to Brown and Warner, the procedure for detecting even small abnormal returns is quite powerful.

a collusive merger is bad news for the rivals since the expected product price increase will not materialize. However, the possibility that one of the *rivals* might now become the target instead is good news.[2] The difference between Werden and Williams and Eckbo and Wier on this point is that only the former two authors believe that the net effect of these two opposing forces is *likely* to be positive. For the net effect to be positive, it must be the case that the actual benefits from becoming a target are substantially larger than the benefits of the initially proposed collusive merger.[3] The problem with this argument is that is difficult to explain why the initial merger was actually preferred by wealth maximizing bidders and targets in the first place, a point which is ignored by Werden and Williams.

3.2   Data Problems in Eckbo and Wier (1985)?

(11) "There are many serious problems with the Eckbo and Wier [(1985)] study....[one of which is that].. their most recent merger occurred prior to the Justice Department's issuance of revised Merger Guidelines in 1982" (p. 9) ?!

This statement raises the suspicion that when Werden and Williams uses the words "data problems" they are really referring to the *relevance* of the evidence in Eckbo and Wier for *current* antitrust policy. In other words, very little of what they call "serious problems" concern the actual data used in our study. This suspicion is confirmed by reading their section 4.

More than thee years ago, The DOJ received (free of charge) the entire sample of 82 cases in Eckbo and Wier. Apparently, Werden and Williams proceeded to "throw out" 62 cases:

(12) "We initially screened the mergers to make sure that at least one of the merging firms' stock prices was significantly affected by both the announcement and the complaint. This was necessary to assure that sufficient new information was released to the market on those dates to have prompted significant rival stock price movements' (p.9).

Since Werden and Williams are not interested in testing the collusion hypothesis, I will refrain from elaborating on the obvious selection bias resulting from screening the data by "looking for outliers". It appears that the authors simply want to reduce the sample in order to provide some specific comments on the underlying data. Consistent with this, while they provide t-values for these 21 cases (without, however, providing information on exactly how the estimation was done), they never purport to make sample-wide inferences. Thus, my comments are restricted to the alleged "data problems".[4]

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[2]This precludes, as noted in the original papers, landmark cases where the entire industry is pushing against the antitrust constraint.

[3]It must be substantially larger since the comparison involves a certain loss against an uncertain (expected) gain.

[4]Although their screening of the data precludes general inferences, it is notable that based on their t-values alone, there is no systematic collusion-consistent evidence in this sub-sample either. Werden and Williams do not make the reader aware of this point. Notice also that in footnote 15, they state that "[F]or a few cases, the CRSP tape we used did not contain information for one of the rivals Eckbo and Wier used,

(13) "The theories of the challenge in at least seven of the [21] mergers ... were not properly characterized as traditional horizontal theories based on increases in market concentration" (p. 10).

While this statement is essentially unintelligible, it also misses the point of the case selection procedure in Eckbo and Wier. We selected a case as long as the *court documents* stated that the case was challenged on the basis of a threat to "horizontal competition". The point was precisely *not* to second-guess this opinion but to let the *evidence* speak for itself. Instead, Werden and Williams want to preselect cases and exclude a challenge from analysis if it was based on, e.g., "potential competition". Again, this defeats the purpose of the scientific inquiry since it is equivalent to granting the DOJ the ability to fully understand the competitive nature of a merger (which, incidentally, the agency decided to challenge) *before* looking at the evidence.

(14) "In our view, mergers challenged but not blocked should be analyzed separately *or not at all*" (p. 11, emphasis added).

This refers to three of the 21 cases where the challenge was either withdrawn or rebuffed by the courts. They go on to state the following:

(15) "It is of limited value to assess initial enforcement decisions when those decisions were reversed without having had any real effect. Rather, it is much more useful to assess whether the legal system produced the proper outcomes. If the legal system determined that a merger did not pose a danger to competition, it is inappropriate to treat it like a merger for which the opposite conclusion was reached" (note 17).

Again, as in the quote at the beginning of this paper, the two authors reveal their fundamental bias: They simply refuse to believe that it makes any sense to *unconditionally* second-guess the DOJ's antitrust decisions.

Finally, Werden and Williams argue that not all of the rivals identified through the 5-digit SIC code can be considered as "relevant" rival firms. Again, any procedure for selecting rival firms –however replicable– is open to criticism, since it depends on how narrowly one wishes to define the relevant product market. Again, it is misleading to characterize this as a "data error". Also, in this context, it is important to mention (which Werden and Williams stubbornly refuse to do) that Eckbo and Wier collected rivals defined by the DOJ itself in order to test the sensitivity of the results to the rival firm selection procedure. And, as pointed out before, the rejection of the collusion hypothesis is even stronger when based on these agency-identified rival firms.[5]

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and such rivals were omitted". In other words, Werden and Williams threw out rivals for which the CRSP changed the CUSIP number after 1984. For their information, no firms are ever dropped from the CRSP tapes.

[5]Werden and Williams do indeed point out one real data error in Eckbo and Wier: Unfortunately, in one case we punched in the wrong date for the merger proposal. I have redone the analysis, and can report that correcting this error has no detectable impact on the earlier results.

4 Conclusions

Stock prices set in a rational, efficient market contain information which is useful in the process of diagnosing anticompetitive mergers. The accumulated evidence on antitrust enforcement indicates that the typical challenged merger work likely would have produced efficiency gains. In line with the results of Eckbo (1983) and Stillman (1983), Eckbo and Wier find that the evidence based on capital market data generally rejects the market power hypothesis. As long as the enforcement agencies continue to insist on rigid structural standards for evaluating the competitive effects of mergers, it is reasonable, given the evidence, to suspect that special interest groups, including those representing relatively inefficient producers and/or a rigid work force, will continue to attempt to take advantage of the regulatory process.

A public policy that leads to prosecution of efficient mergers harms consumers and benefits at most the competitors of the merging firms. Furthermore, if merger proposals typically convey new and valuable information to rival firms (as suggested by the positive rival firm performance), delays in execution due to complaints filed by the antitrust enforcement agencies effectively give these rival firms additional time to capitalize on the information, perhaps by competing for the target firms. The additional competition for the target shares reduces the value of a merger to the initial bidder firm, whether the merger is anti–competitive or efficient. In sum, while Eckbo and Wier are unable to identify a material benefit (in terms of improved selectivity) from granting the government significant enforcement powers such as those under the Hart–Scott–Rodino Act, the evidence indicates past antitrust law enforcement has quite possibly resulted in a substantial social cost in the form of a reduced incentive to undertake *efficient* mergers.

Of course, evidence that antitrust policy is costly does not necessarily rule out the possibility that the same policy is socially optimal: It is possible that the threat of a challenge also deters a sufficient number of collusive mergers from even reaching the state of a merger proposal. The benefit of the previous studies is then to refocus the debate on the perhaps most important remaining issue: What is the likely social value of the deterrent effect? Empirical evidence on this difficult issue is sparse and awaits further research.[6]

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[6]Some preliminary evidence is presented in Eckbo (1988) based on horizontal mergers in Canada. Up to 1986, there were virtually no antitrust constraints on horizontal mergers in that country. In Eckbo (1988) I fail to detect any evidence of anticompetitive mergers in that country over the period 1964–1983.

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Table 1
Predicted Abnormal Returns to the Merging Firms and their Rivals[1]

Theory Predicting the Source of the Merger Gains	Type of Event	Abnormal Returns to Merging Firms	Abnormal Returns to Rival Firms
Market Power (Collusion)	π up	Positive: Monopoly Rents	Positive: Monopoly Rents
	π down	Negative: Loss of Monopoly Rents	Negative: Loss of Monopoly Rents
Procedure Efficiency	π up	Positive: Cost Savings	Negative: Competitive Disadvantages
	π down	Negative: Loss of Cost Savings	Positive: Avoiding Competitive Disadvantages
Information	π up	Positive: Undervalued Resources	Zero or Positive: Undervalued Resource and/or Possible Productivity Increases
	π down	Zero	Zero

1. "π up" denotes events which increases the probability of merger consummation, such as the merger proposal announcement or the announcement of a pro–defendant decision by the court. "π down" denotes probability–decreasing events, such as the antitrust complaint announcement, announcement that the merger has been abandoned, a pro–government consent decree, or a pro–government decision by the court.

Table 2
Abnormal Performance of Bidders, Targets and Rivals/1963–1981 [1]

Events	Observation Days	Bidders	Targets	SIC Rival Portfolios	Agency Rival Portfolios
Merger Proposal	-20 through +10	3.0*	25.7***	3.0***	3.3**
	-1 through +1	.8**	10.5***	.3	.6*
Antitrust Complaint	-20 through +10	-1.8	-6.5***	1.3	1.1
	-1 through +1	-1.4**8	-7.6***	.9***	.6
Cancellation or Divestiture	-20 through +10	-1.2	-1.2	-.2	-.1
	-1 through +1	-.6**	-6.1***	.2	.7*
Dismissal or No Divestiture	-20 through +10	-.5	Fewer than 5 firms in sample	-3.2	-3.3
	-1 through +1	-.5		-1.3	.4

1. Percentage cumulative average abnormal returns are measured over 31-day intervals (-20 through +10) abd 3–day intervals (-1 through +1) containing *Wall Street Journal* announcement days (day 0). *, **, *** denote that the hypothesis of zero abnormal returns is rejected at the 10, 5, or 1 percent level of significance, respectively.

Table 3
Abnormal Performance of Rivals Before and After September, 1978 [1]

| Event | Observation Days | SIC Rivals | |
		January 1963– August 1978	September 1978– December 1981
Merger Proposal	-20 through +10	2.4***	2.4*
	-1 through +1	.2	.3
Antitrust Complaint	-20 through +10	.8	-.5
	-1 through +1	.1	.6
Cancellation or Divestiture	-20 through +10	.1	.4
	-1 through + 1	-.4	-.5
Dismissal or No Divestiture	-20 through +10	-1.7	Fewer than 5 firms in sample
	-1 through +1	-1.0	

1. Percentage cumulative average abnormal returns are measured over 31–day intervals (-20 through +10) and 3–day intervals (-1 through +1) containing *Wall Street Journal* announcement days (day 0). *, **, *** denote that the hypothesis of zero abnormal returns is rejected at the 10, 5, or 1 percent level of significance, respectively.