

## Comment G. William Schwert

### Introduction

My understanding is that the theme of this NBER conference is to study a small number of acquisitions intensively with the goal of developing new insights into the market for corporate control. By interviewing managers, examining internal corporate records, and otherwise focusing attention on the details of a few transactions, we hope to learn the reasons why bidders are willing to pay large premiums to acquire target firms and whether their expectations at the time of the transaction are borne out by subsequent performance. One goal of this research is to develop hypotheses, methods, or data that could be applied to larger scale empirical analyses of the corporate control market. The goals for this conference are similar to the motivation for the Clinical Studies section of the *Journal of Financial Economics*, pioneered by Michael Jensen and Richard Ruback in 1989 (Jensen et al. 1989).

From this perspective, clinical studies are a good example of inductive inference, which is defined by Jeffreys (1961, 1) as “making inferences from past experience to predict future experience.” Zellner (1971, 5) describes reductive inference as a process whereby science develops new hypotheses: “unusual and surprising facts often trigger the reductive process to produce new concepts and generalizations.” Perhaps in some circumstances

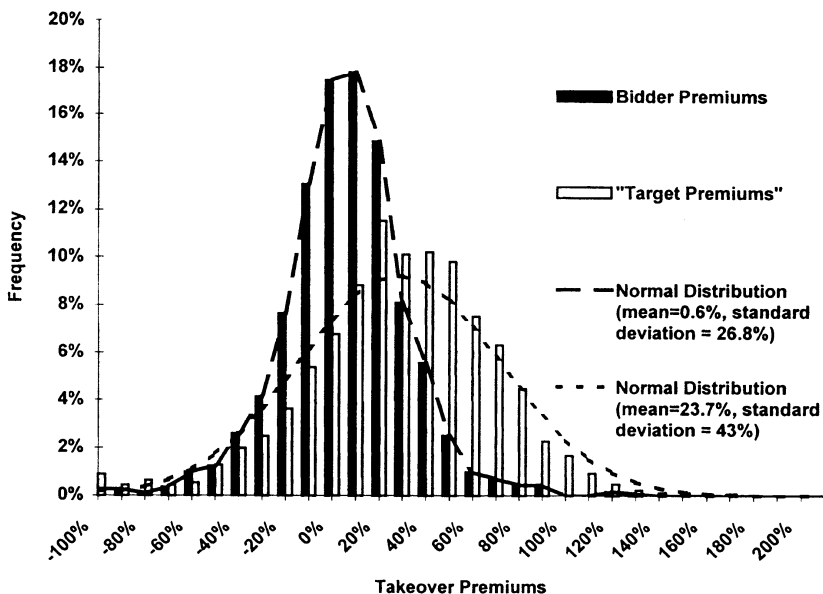
G. William Schwert is Distinguished University Professor of Finance and Statistics at the William E. Simon Graduate School of Business Administration of the University of Rochester and a research associate of the National Bureau of Economic Research.

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we will observe phenomena in detailed clinical studies that cause us to postulate new theories or hypotheses about economic behavior.

### What Is Success or Failure in Mergers and Acquisitions?

Large sample studies have frequently shown substantial changes in wealth for stockholders of target and (sometimes) bidder firms. These changes are usually measured around the announcement of the first public bid. Targets generally benefit, bidders often lose, but the net gains are usually positive, so the target gains are not just overpayment by the bidder. Figure 4C.1 shows the distribution of takeover premiums for 2,003 exchange-listed targets and 1,110 exchange-listed bidders from 1975 to 1994. The premiums are measured as market-model adjusted stock returns accumulated from three months before to six months after the first bid announcement (similar to the method used in many papers that study takeovers, including Schwert 1996). The average premiums are 23.7 percent for targets and 0.6 percent for bidders, with standard deviations of 43.0 percent and 26.8 percent, respectively. The large standard deviations mean that



**Fig. 4C.1** Distribution of premiums to exchange-listed target and bidder firms from 1975 to 1994, adjusted for market movements

*Note:* CRSP value-weighted market model residuals cumulated over trading days (-63, 126) around the first offer announcement date. Model estimated using days (-316, -64). Premiums are measured from three months before to six months after the announcement of the first public bid.

many realized premiums are negative. Figure 4C.1 also shows normal distributions with the same means and standard deviations represented as dotted and dashed curves. These approximate the histograms of target and bidder premiums well.

The message that I get from figure 4C.1 is that there is a lot of “noise” in the cross-sectional distribution of premiums. Focusing on differences in average premiums can miss the point that there are many idiosyncratic differences across cases. Of course, the other side of this argument is that any small sample of cases is not likely to be representative of the complex population represented in the figure.

Kaplan, Mitchell, and Wruck looked at one deal that the market thought would be profitable and one that the market thought would be unprofitable. They focused on exchange-listed nonfinancial firms that completed deals from 1987 to 1994. Ex ante success was defined as an eleven-day abnormal return greater than 5 percent, while failure was defined as an eleven-day abnormal return less than 0 percent, based on a weighted average of announcement returns for targets and bidders, using equity capitalization as weights. There were thirty-four possible successes and fourteen possible failures. Next, they looked for cases where personal or institutional contacts or geographic proximity made it more likely that management would share private information. They pursued four firms as possible candidates and received favorable responses from two initially, although one later withdrew support.

### Selection Biases

Kaplan, Mitchell, and Wruck point out several biases that cause the selection of cases for detailed clinical study to be nonrandom. Selection biases may be (1) performance-related, in which case firms that experience poor performance are less likely to want to publicize their story; (2) privacy-related, where the choice of nondisclosure policies by firms is probably not random but is likely to be related to the value of information about the firm’s investment opportunities; or (3) institution-related, where the question may be asked whether, if personal or institutional contacts are necessary to conduct clinical research, this somehow slants the results. Is there an incentive to pull punches in describing corporate behavior to increase the likelihood of access to other companies in the future? All of these factors could limit the generalizability of clinical analysis and they are likely to be important in any clinical study.

There is evidence in the authors’ paper that managers are reluctant to criticize themselves. Most of the information about poor performance of Cooper comes from Cooper Cameron managers after they had been spun off from Cooper, and even they are reluctant to seem critical of former colleagues. Premark withdrew its cooperation early in the project, forcing Kaplan, Mitchell, and Wruck to rely on public sources of information,

rather than management interviews and internal records, to enrich the analysis of the Premark–Florida Tile transaction.

### Confounding Events

In measuring the ex post performance of these transactions, the authors track accounting and stock price performance for many years after the deal was announced. An obvious difficulty, which they discuss in detail, is to abstract from the many other factors that influence the performance of the merged firm.

Besides acquiring Cameron Iron Works, Cooper also took over Champion Products in 1989. This was part of a strategy of acquisitions that began in the 1960s. In fact, this strategy, called “Cooperization,” is often taught in strategy courses using Harvard cases (Collis 1991; Collis and Stuart 1991). Thus, the success or failure of the Cameron acquisition has to be viewed in the context of a long-term strategy that involved many acquisitions. In July 1995, Cooper spun off Cameron. Kaplan, Mitchell, and Wruck point out that Cooper Cameron increased focus, decreased bureaucracy, and increased incentive compensation after it split from Cooper’s control. The authors argue that Cooper Cameron has been much more successful than Cameron was as a wholly owned subsidiary inside a highly bureaucratic Cooper organization.

In Premark’s acquisition of Florida Tile, the authors argue that Premark was spending the cash generated by its Tupperware division to finance the acquisition in 1990. Shortly afterward, the Tupperware market was adversely affected by factors unrelated to the acquisition. The authors argue that solving the Tupperware problem probably distracted Premark management from focusing on Florida Tile, which contributed to the failure of this transaction.

### Ex Ante Profitability—Stock Market Reaction to Bids

Kaplan, Mitchell, and Wruck use the initial stock market reaction as a measure of whether the transaction was likely to be successful. I have much sympathy with this approach, but there are also some potential limitations. As noted by the authors, part of the change in the bidder’s stock price can be a reaction to information about the bidder’s alternative uses for capital. For example, the Florida Tile transaction might have been a neutral deal for Premark (i.e., it could have been a zero net present value investment), but if the market had expected a better alternative, Premark’s stock price would fall.

As another example, the strong negative reaction to Kodak’s “white knight” takeover of Sterling Drug in 1988 was likely to be more than overpayment because the loss in value for Kodak exceeded the premium offered to Sterling by a substantial amount. The market probably inferred

something negative about the future profitability of Kodak's main line of business (chemical photography) from the eagerness with which Kodak pursued Sterling.

Thus, the use of initial stock market reaction as a basis for identifying successful or unsuccessful deals has some difficulties.

#### Ex Post Profitability—Stock and Accounting Returns

Kaplan, Mitchell, and Wruck measure profitability after the deal using both accounting and stock returns. Accounting returns (EBITDA relative to sales, assets, and value) based on private data show poor performance for the Cameron division in table 4.3. In contrast, the public data based on the consolidated performance of Cooper hide the poor performance of Cameron in table 4.4. This is an important example where the methods of clinical studies, including access to internal records and information from companies, provides substantially different information from the publicly available data.

Nevertheless, the problem of identifying abnormal accounting performance over long time periods is difficult. Barber and Lyon (1996) provide simulation evidence that shows the difficulties and imprecision associated with testing for unusual accounting performance.

A similar problem arises in measuring abnormal stock returns over long time periods. Barber and Lyon (1997) and Kothari and Warner (1997) use different simulation methods to show the difficulties and imprecision associated with testing for unusual stock price performance. Mitchell and Stafford (1998) also show the wide dispersion of abnormal stock return measures when calculated over multiyear horizons.

The linkage between the expected net benefits from an acquisition based on announcement period stock returns and the realized benefits based on accounting performance is likely to be weak, simply because there is much noise in both measures. If the correlation is small, it would be difficult to measure in a large sample of cases. It would be unlikely to see a relation between these measures in a small sample of cases (e.g., two), except by chance.

Finally, in an efficient market one would not expect a correlation between the abnormal stock return at the time of the acquisition announcement and the abnormal return measured over subsequent periods. Thus, if the Cooper and Premark stock returns are disappointing after the acquisitions, this can be viewed as a reflection of new negative information, but not as confirmation or contradiction of initial market reactions (to the extent that markets are efficient). This point is worth reiterating, not so much because Kaplan, Mitchell, and Wruck interpret it incorrectly, but because much of the discussion at the conference for many of the papers often overlooked this point.

## Conclusions and Implications for Future Research

The authors conclude that both of these deals failed. Cooper's acquisition of Cameron failed because they were in a different business (despite SIC code similarities) and because Cooper's centralized organization and incentives were inappropriate. Premark's acquisition of Florida Tile failed because they were in different businesses, because Premark overpaid (spending free cash flow), and because other divisions had problems.

One lesson the authors advocate is that bidders must understand their targets. It is hard to argue with this conclusion, but it is also hard to implement it. It seems clear that SIC codes are not helpful, because different sources at different times yield different answers. On the other hand, no suggestions are offered for alternative methods of identifying target companies that are similar to the bidders.

Kaplan, Mitchell, and Wruck argue that organizational design or corporate cultures are important, but they offer few guidelines for identifying problems. The challenge here is to develop a measurable, implementable, replicable method that could be used in large samples.

The authors also argue that incentives are important. They point to the "high powered" equity incentives used by Cooper Cameron after the spinoff, but not by Cooper or Premark. On the other hand, my sense is that this is typical of the distinction between "focus-increasing" going private or recapitalization transactions versus acquisitions. I doubt that Kaplan, Mitchell, and Wruck would conclude that acquisitions would fail unless such incentive compensation is part of the transaction.

Another question that concerns many of the clinical studies in this conference was whether bidding firms were making mistakes. Roll (1986) hypothesizes that managers often become carried away with the bidding process and overpay. Jensen (1986) argues that firms with abundant free cash flow often waste it, perhaps by overpaying in acquisitions. From this perspective, negative stock price reactions for bidders could reflect systematic free cash flow mistakes that are recognized by the outside world at the time. This raises the question of whether managers have better information about the likely success of a possible acquisition than security analysts. It would be interesting to augment the retrospective analysis by managers with reports of securities analysts (or other disinterested parties) at the time of the transaction. This would give clinical studies an additional dimension of information beyond the usual public databases without the problem of translating through the "20/20 hindsight" of managers who were involved in the transaction.

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