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(e.g., M&As) increase firm value based on the assumption that the stock market is efficient. On the other hand, in this chapter, the author says in the conclusion, “Our results may demonstrate that China’s stock market might not have reached the level of efficiency of the more-advanced economies, but its efficiency in assessing the value of M&A activities might have been improved from 2004 to 2005.” Does this chapter test whether China’s stock market is efficient given that M&As increase economic values?

Finally, the author may want to test formally whether the differences of CARs by ownership structures and by stock market’s boom-and-bust periods are statistically significant.

In sum, this chapter tackles a very challenging and important topic that no one else has ever investigated: the stock market responses to M&A announcements in China. Further improvements of analytical methods and the interpretations of the results based on Chinese regulations and practices will make this chapter more valuable to all that are interested in the functioning of emerging markets.

References

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Comment Julian Wright

Let me first provide some additional context and background to the chapter. The research question addressed by this chapter is quite simply “do mergers create or destroy value?” This is an important question for finance, industrial organization, and antitrust. There is a large literature that has looked at the question using data mainly from the United States but also from some other developed countries. The literature adopts two main approaches. One is to measure the abnormal stock return associated with merger announcements, and the other is to look at the change in accounting earnings following merger announcements.

The consensus from this literature is that the abnormal stock return to acquiring firms is not significantly different from zero, the abnormal stock return to target firms is significantly positive, and that the combined ab-

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normal return is about 2 percent of the total initial value. This 2 percent estimate is probably downward biased. Any equity financed merger involves two events—a merger and an equity issue. The latter may have a negative effect on stock returns for standard reasons, including that it may reveal negative information about the value of the acquirer. For example, an acquirer may use equity financing when it expects lower growth than the market does. Thus, cash- or debt-based mergers should reveal higher returns to acquirers. More generally, the fact a firm wishes to acquire another reveals some information about the acquirer (perhaps that it has few options to grow organically), and so the stock returns associated with the merger announcement will partly reflect this release of information.

So what are the innovations of this chapter? Put simply, the innovation is that this is the first study of mergers using Chinese data. Specifically, the author uses Chinese data from 2005, looking at 752 M&A events involving 587 companies.

Why is this interesting? One possible reason is the lack of formal antitrust law against mergers in China. Antitrust laws that make anticompetitive mergers illegal exist in the other countries that have been used to consider whether mergers increase firm value. This is potentially important because these laws mean the most profitable mergers are likely to be blocked. The absence of laws against anticompetitive mergers mean mergers in China could be substantially more profitable. A second reason this is interesting is that in China a large number of mergers involve public ownership, with 503 mergers involving state holding companies as the controlling shareholders (211 involved privately owned enterprises as the controlling shareholder). Because the government may have different objectives (such as bailing out failing firms) and may have more bargaining power with respect to private firms, this could also mean mergers have different implications for value creation in China.

So what are the main findings of the chapter? Compared to evidence from the existing literature, the combined return to mergers is surprisingly similar. What is more interesting is that the authors find the acquirers obtain a significantly positive abnormal return and that this is about equal to the target firm return. Recall this is in contrast with the United States and other countries, where the existing literature finds the entire positive return from mergers is generated from the return to the target firm.

If this difference in findings remains robust to considering a longer sample of Chinese mergers, it naturally raises the question of why acquiring firms do so much better in China compared to other countries. A possible answer is that in China, many acquiring firms are state firms, and they may have greater bargaining power, thereby extracting more of the surplus than would otherwise be the case. This could be tested by breaking up the results on abnormal returns to acquirers into private and state-controlled acquirers. Here the author finds that state controlled mergers have a

slightly higher abnormal return, but they do not break this up into acquirer and target firm returns. More generally, an interesting avenue for investigation is to explore the role of bargaining between acquirers and target firms. Does competition between acquirers or market position of the acquirer play a role? This could be addressed empirically, not just for this Chinese data set, but more generally.

There are, of course, several strong assumptions that are needed for the results of the stock market valuation approach to be valid. One always worries the market cannot efficiently calculate the change in value of firms as a result of a merger. If there is any bias in the market's perceptions of mergers, this will be directly reflected in the calculated abnormal returns. I mention this because at present, there are particular concerns about the rationality of the Chinese stock market. The alternative is to use the accounting methodology instead, but this raises more serious problems in my view—are the accounts reliable in China and are they comparable pre- and post-merger? Typically researchers do not put much weight on this accounting approach due to the large measurement errors involved, especially because a merger involves the firms merging their accounts. This measurement error problem is likely to be even more acute in China.

I have more serious concerns about the specific application of the accounting methodology in the chapter. The author compares whether earnings per share (and other accounting measures) increase in 2006 compared to 2005 for the 587 firms merging in 2005. This is meaningless unless there is some counterfactual of what earnings per share would otherwise have done in 2006. Otherwise, the increase in earnings per share after 2005 could well be an aggregate country-level effect. This indeed seems likely given the substantial rise in stock values in China since July 2005. Rather, in line with standard practice, the author should control for industry (or at the very least country-level) effects here.

Finally, I mention two important ways in which the chapter needs to be improved. For both accounting and stock return results, the author should follow the existing literature and break the results down into equity-based versus cash- debt-based transactions. As mentioned at the start, equity-based transactions involve an additional bias so that focusing on cash-debt-based transactions will give cleaner results. More critically, the author needs to extend the study to obtain more than one year of merger data. In doing so, the robustness of the results can be considered, and the analysis based on accounting indicators can be done properly.