



Size, ownership and the market for corporate control

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ARTICLE INFO

Article history:

Received 3 September 2008

Accepted 3 September 2008

Available online 9 September 2008

JEL classification:

G30

G34

Keywords:

Mergers and acquisitions

Corporate governance

ABSTRACT

This paper is written with two goals in mind. The first is to offer a critical discussion of papers by Bauguess, Moeller, Schlingemann, and Zutter [Bauguess, Scott, Moeller, Sara, Schlingemann, Frederich and Zutter, Chad, 2009. Ownership structure and target returns. *Journal of Corporate Finance*, this issue], and Offenbergs [Offenberg, David, 2009. Firm size and the effectiveness of the market for corporate control. *Journal of Corporate Finance*, this issue], both of which appear in this special issue. The second goal is to offer some perspectives about new questions that these papers bring to light.

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1. Introduction

This paper has two goals. The first is to offer a critical discussion of papers by Bauguess et al. (2008, hereafter BMSZ), and Offenbergs (2009-this issue), both of which appear in this special issue. Then, using these two papers as a starting point, the second goal is to offer some thoughts about the new questions that these papers raise.

Both BMSZ and Offenbergs (2009-this issue) are concerned at some level with the disciplinary role of mergers. While they differ in terms of methodology, research design, sample construction, and indeed the specific issues addressed, they both fundamentally address the broad question of how firm characteristics relate to the possibility that a firm will become the target of a takeover attempt. Indeed, they look at the market for corporate control from two highly complementary perspectives: BMSZ start inside the firm, and look out onto the market for corporate control, asking how ownership structure affects target returns. Offenbergs (2009-this issue) starts outside the firm, looking at the incidence of becoming a target, and focuses inward, looking how the firm's past behavior makes it more or less likely to be a takeover target.

The bottom line from both papers is that, in general, firms cannot readily exempt out of the market for corporate control. The evidence is that, first, big firms engaging in bad takeovers are more likely to be takeover targets. Second, the CEOs of these big firms are more likely to be replaced. Third, concentrated activist ownership seems to provide incentives for monitoring that raise ex ante firm valuations, even if the subsequent acquisitions appropriate more takeover synergies to the bidder.

These findings raise fascinating questions about the market for corporate control. To discuss these papers and explore these questions in greater detail, the remainder of this paper is structured as follows. I devote Sections 2 and 3 to a critical appraisal of BMSZ and Offenbergs (2009-this issue), respectively. Then, in Section 4, I conclude with some thoughts about the broader questions that these papers raise.

2. Does ownership structure affect returns?

BMSZ ask a question that has received a good deal of theoretical attention, but less empirical attention: namely, how does the concentration of ownership in the target firm affect the returns it receives in an acquisition? This revisits a question first studied empirically in Stulz et al. (1990), but it does so in a way that was not possible when the Stulz et al. paper was written. In particular,

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BMSZ disaggregate “all officers and directors” in such a way that they can distinguish owner-managers from large shareholders who are essentially outsiders to the firm. Theory offers sharp distinctions between these two groups in terms of their incentives and likely behavior. The finer granularity that BMSZ offer is not only critical for teasing out distinct predictions of theory, but the different time period helps us understand how the market for corporate control has changed since the 1980s.

The results that BMSZ find line up well with the theoretical predictions of [Shleifer and Vishny \(1986\)](#) and [Stulz \(1988\)](#). Increasing active outsider ownership generally lowers target returns. On the one hand, this could indicate that active outsiders were monitoring the firm ex ante, lowering the gains that are available to outsiders. On the other hand, it could indicate that active outsiders were more willing to share the gains from takeovers with a potential suitor, raising the likelihood that a merger would occur.

In contrast, increasing insider ownership raises target returns. This is consistent with a variety of theoretical explanations. Perhaps ownership captures entrenchment, and entrenched managers drive down ex ante valuations, creating scope for higher returns. Or perhaps they reduce the likelihood of takeover, lowering the anticipation premium already built into ex ante stock prices (see [Song and Walking, 2000](#)).

When BMSZ try to distinguish between alternative explanations for the results they obtain, the picture that emerges grows somewhat more complicated. Firms with higher active outside ownership are not only associated with lower target returns, but higher bidder returns, lower target relative gains, and higher overall deal synergies. Therefore, there is no evidence that they bargain for larger fractions of the total pie being created through the merger, although they may well prefer bidders that offer greater synergies, ceteris paribus. BMSZ argue that outsider activists have greater willingness to share the synergy gains with bidders.

Likewise, they find evidence that insider ownership is also related to higher bidder returns. They interpret this to mean that insiders are engaged in self-dealing, consistent with evidence in [Moeller \(2005\)](#) or [Hartzell et al. \(2004\)](#). Unlike the case with outside owners, however, greater inside ownership is associated with larger target relative gains.

While in general BMSZ provide a sound empirical analysis, their findings are open to an alternative explanation that they have not considered. To understand more completely the mergers that do occur, we must also consider the mergers that could have occurred but did not occur. In my view, the BMSZ findings are most interesting when we interpret them in the context of the possibility that increases in ownership are associated with targets making better choices among a set of many potential bidders.

This alternative explanation is partly rooted in the empirical evidence found in [Boone and Mulherin, 2005](#), who demonstrate that a great many potential suitors are considered, vetted and screened prior to an actual suitor being announced. It is also rooted in theoretical work by [Brusco et al. \(2007\)](#), which explores the conditions under which the economically efficient (in the sense of maximizing synergy creation) mergers will and will not occur based on the information structure in the market for corporate control.

[Brusco et al. \(2007\)](#) study the M&A process from a mechanism design perspective. In particular, they model the problem facing a potential target firm who faces many potential suitors. Each firm possesses private information about its value, and the goal is to develop a mechanism that facilitates efficient transfer of assets.¹ The critical takeaway from their analysis is that the source of the information asymmetry is critical for determining whether efficient mergers occur, even in a world where complex bundles of securities can be used as means of payment. If asymmetries are about the stand-alone values of bidders and targets, adverse selection prevents efficient mergers from occurring — too often mergers that raise social surplus do not occur because there is no way to overcome the informational impediments to efficient trade.

However, if stand-alone values are easily observable and the asymmetric information is about the synergies created by the transaction, then informational asymmetries can be overcome, and the first best can always be achieved.

There is no scope for moral hazard in the [Brusco et al. \(2007\)](#) model, since it focuses solely on the role of asymmetric information in the takeover process. But it is easy to see how the analysis might be modified to include a role for effort, and thus ownership, to affect the findings. For example, suppose that managers must incur a search cost as they learn about potential suitors, and they can either maximize firm value or they can choose a partner who best suits their private interests. Managers with larger pecuniary stakes would naturally have more incentives to choose a suitor that maximizes monetary gains, but in a world where agents must be compensated to reveal private information, what would the predictions be?

To square such a story with empirical findings in BMSZ requires some careful consideration of some subtle issues. But doing so may shed new light on the interaction between two alternative governance structures. A key, but subtle factor, is the nature of the asymmetric information. In a world where most of the uncertainty surrounds stand-alone values, we may well expect higher insider ownership firms to allocate a larger fraction of the transaction surplus to the successful bidder, precisely because better bidders must be rewarded with better terms to overcome the lemons problem. In contrast, in a setting in which most of the uncertainty surrounds synergy creation, we should expect to see higher quality bidders being more eager to offer transaction surplus to the target. Indeed, the nature of the asymmetric information may well determine whether increased ownership eases or exacerbates the informational impediments to efficient mergers.

3. Who's afraid of the big bad bidder?

Moving from BMSZ to [Offenberg \(2009-this issue\)](#) moves from questions about how the internal ownership structure of the firm affects its place in the takeover market to questions about how firms' prior behavior in the takeover market affects the likelihood of being an acquisition target.

¹ Their analysis allows for stock and cash as means of payment, which complicates the analysis but brings the predictions much closer to the empirical evidence, especially given that they study settings in which the true values of the uncertain assets may or may not be revealed ex post. See [Fuller et al. \(2002\)](#) or [Officer et al. \(in press\)](#) for empirical evidence on the role that alternative means of payment plays in environments with information asymmetries.

Offenberg (2009–this issue) draws inspiration from the findings of Moeller et al. (2004) and returns to a question first posed by Mitchell and Lehn (1990) and Lehn and Zhao (2006). In particular, Offenberg notes the evidence from Moeller et al. (2004) pointing to wealth destruction being concentrated in large acquisitions. Then, returning to Mitchell and Lehn, he poses the following question: If big bidders are bad bidders, and bad bidders become good targets, then are large bidders prone to becoming takeover targets when they make bad acquisitions?

In general, we can all take comfort from the findings of Offenberg (2009–this issue): the market for corporate control works for these firms too. They are more likely to become targets and their CEOs are more likely to be replaced.

To reach this conclusion, Offenberg builds a data set from the time-series behavior of firms who engage in takeover activity by focusing on the last acquisition that a firm makes. He then looks backwards through time over two years to build a return series for the firm around any prior acquisitions that have occurred, and looks forward over the next four years to determine whether the company becomes a takeover target. This research design allows him to relate the returns from past bidding activity to the likelihood of subsequently becoming a takeover target. During this four-year window he is also able to examine whether a CEO is replaced, irrespective of becoming a takeover target.

This is an interesting empirical strategy aimed at addressing a well-posed question, but the analysis ultimately leaves me wishing for more. In particular, I found myself wanting to see a more detailed breakdown of the time horizons in question, especially given the observations in Holmstrom and Kaplan (2001) pointing to the distinct differences between the takeover markets in the 1980s and 1990s. On a closely related point, I also found myself wanted to see a more detailed analysis of the timing of the event that led the bad bidder to become the good target.

On the first point, it would be interesting to explore whether the results that Offenberg finds are limited to the 1980s, when we know that takeovers had a distinctly more hostile flavor, or whether they also extend into the 1990s, when a variety of alternative governance mechanisms had evolved. The answer is not obvious, because it depends on both supply-side factors and demand-side factors. If there are very few alternative governance mechanisms available in the market for corporate control, then one might reasonably expect a limited amount of bad bidding from large firms. Yet this is precisely when the disciplinary effects of mergers might well be the most severe. Thus, if we saw little in the way of big bidders making bad acquisitions, how could we distinguish a well-functioning market for corporate discipline from a paucity of potential acquisitions? Likewise, if non-disciplinary mergers are more common, as is the case in the 1990s, then this opens the door for more bad bidding to occur, but may blunt the responsiveness of the takeover market to such behavior. Thus, the analysis is ultimately more difficult than it seems at first blush because it is difficult to disentangle periods of high potential for bad takeovers from periods of weak intervention from disciplinary takeovers, and periods of low potential from periods of strict takeover discipline. At some level, this is a classic identification problem that cries out for an instrumental variables strategy. Perhaps the analysis of CEO turnover may provide just the empirical mechanism required to disentangle these two effects.

A four-year window of time is also a long period over which many firm characteristics can change, potentially blunting the connection between being a bad bidder and a good target. Thus, it would be fruitful to consider how the passage of time affects the connection between prior bidding behavior and subsequent target likelihoods. One way to do this would be to consider modeling the hazard of being acquired during this four-year window as a function of past bidding behavior and other characteristics. Are bad bidders especially prone to being acquired soon after their last acquisition, or does the firm need to experience a series of negative shocks after the last acquisition before it is a takeover target?² If the latter holds, can we be sure that the acquisition behavior was the real cause of becoming a takeover target, or is it reflective of deeper problems in the firm?

This last point raises more general questions. Are big bad bidders good targets because they are bad bidders or because they are bad firms? There are a number of questions that arise when we rely heavily on stock returns as an explanatory variable in an analysis of subsequent takeover activity. Both theoretical and empirical works illustrate the fact that the connection between stock returns and acquisition quality is at best a subtle one. For example, McCardle and Viswanathan (1994) argue that a takeover decision signals a firm's otherwise lack of ability to initiate a corporate strategy internally. Thus, it can send bad news to the market about a firm's internal prospects. Hietala et al. (2003) show that stock price changes surrounding takeover events confound three factors: news about stand-alone values, news about synergies, and news about payments. Typically these confounding sources of value change cannot be disentangled into their constituent parts.

Moreover, there are significant issues associated with using a takeover announcement date as a time point for measuring valuation changes. In multiple papers, Moon Song and Ralph Walking show that markets anticipate future acquisition activity, therefore the focal announcement may not capture the news release date (Song and Walking, 2008; Song and Walking, 2000). Indeed, Boone and Mulherin, 2005 give us one reason why this is so — a lot is happening behind closed doors before “markets” learn about the announcement.

One potential strategy for testing the relation between stock returns and value creation is to relate the magnitude of subsequent takeover premia that obtain when the bad bidder is acquired to the size of the CCAR calculated over the two-year window during which the firm is behaving as a bad bidder. Alternative sources for stock price changes should indeed translate into alternative predictions about how these sets of returns are related.

Thus, Offenberg (2009–this issue), in spite of being built as a continuation of ideas present in existing papers, is a great first look at an important question in the role that mergers play in the overall corporate governance of firms. But the analysis raises as many questions as it answers. I take this as yet another sign that mergers and acquisitions are an enduring area for future research.

² Of course, this is somewhat arbitrary, since the decision to make no further acquisitions is presumably endogenous to the very factors that are driving the relation between bad bidding and becoming a takeover target.

4. What is the market for corporate control anyway?

One of the reasons why I am excited by both of these papers is that it is easy to imagine that they would have found exactly the opposite results. Given the results in [Moeller et al. \(2005\)](#), and in light of the corporate governance scandals of the early 21st century, it is easy to imagine David Offenberg finding totally contrary results, demonstrating that once a firm was above a certain size, it was essentially exempt from the takeover market.

It is equally easy to imagine a system in which increased ownership in a firm simply provides the owners with extended means to expropriate other shareholders and enjoy the private benefits of control. Indeed, [Gan \(2008\)](#) argues that this is precisely the problem that the Chinese system has faced as it has privatized once state-owned firms.

But not so – at least, not according to these papers. Thus, in spite of whatever concerns I may have about the particular details of each analysis, the answers that emerge from these two papers are important. They are neither foregone conclusions, nor do they put an end to the sets of questions raised.

If bad bidders continue to be good targets, even when they are large firms, then why do large firms continue to be bad bidders? This question is even more pertinent when juxtaposed with the analysis of ownership and target returns provided by BMSZ.

I can only speculate, but I see two possible sources for answers to this question. One rests on factors inside the firm, while another rests on the external workings of the market for mergers.

Looking inside the firm, I think that one reason why big firms continue to be bad bidders in spite of the apparent success of the takeover market as a disciplining device is endemic to the structure of executive promotion. To a large extent, internal labor markets inside firms operate like tournaments: they promote people who experience extreme positive outcomes and demote, fire, or pass over people who experience extreme negative outcomes. Managers who rise through the corporate ranks to positions of prominence are almost by definition the very people who suffer most from biases in judgment but experienced good fortune that justified their biased judgment. This is because success tends to be measured *ex post*, rather than *ex ante*.

Looking outside the firm, I think there are several forces at work. As [Jensen \(2005\)](#) points out, over-valued equity creates its own agency problems. If easy liquidity and high earnings expectations – common features of merger waves (see [Harford, 2005](#); [Rhodes-Kropf and Viswanathan, 2004](#)) – drive managers to search excessively for growth opportunities, then characteristics of merger waves may well exacerbate the forces at work inside the firm that naturally promote those with biased judgment *ex ante* who experience good fortune *ex post*.

It is also important to consider the micro-workings of the merger market. Research in finance is beginning to embrace the idea that the market for mergers operates like a search market ([Rhodes-Kropf and Robinson, 2004](#)).

But the market for mergers is considerably more complicated than simply a market where buyers and sellers bump into each randomly and choose whether to transact or continue searching in the hopes of finding a better deal. Instead, this is an intermediated search market. The fact that the search process is intermediated creates a second layer of agency. Thus, the market for solving agency problems inside the firm is characterized by its own set of agency relationships. Why should this work at all?

Perhaps it does not work as well as it should. Perhaps it works far better than any conceivable alternative that lacked intermediation, in spite of the agency costs that intermediation introduces. To know the answer to this, we need a considerable amount of additional work, both theoretical and empirical, that explores the role of search in the merger process. But until then, I interpret these papers as telling me that on balance the disciplinary role of mergers is still there, even when crowded out by alternative motives for mergers and blunted by faulty intermediation.

Acknowledgements

I am grateful to the JCF for the opportunity to contribute this paper to the special issue. I received helpful feedback from Pino Lopomo and Mike Stegemoller on an earlier draft of this paper.

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