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Comment on Handlock, Houston, and Ryngaert

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1. Introduction

Over the past two decades, researchers in industrial organization in corporate finance have examined relationships between "corporate governance" and "firm performance". In an interesting and provocative paper, Charles Hadlock, Joel Houston, and Michael Ryngaert analyze the impact of proxies for corporate governance, managerial incentives, and performance on the likelihood of a bank's being acquired. Given the pace of activity in the bank acquisition market and concern in some quarters about consolidation in the US banking industry, the authors' study is timely.

After reviewing the key points in the paper, my comment proceeds in three steps. First, I place the authors' empirical approach in the context of recent work on equilibrium models of corporate performance. Second, I use that intuition to suggest ways to design and interpret tests of relationships among bank governance, performance, and acquisitions. Third, I suggest some "next steps" in the empirical research programs initiated by the authors. While many extensions are possible, the paper nonetheless is a nice contribution to our understanding of how managerial incentives affect bank acquisitions.

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2. Key points

Hadlock, Houston, and Ryngaert make essentially five key points. First, high levels of managerial ownership in banks reduce the likelihood of being acquired, all else being equal. Second, this effect is driven by acquisitions in which chief executives of the target banks disappear. Third, the authors find no evidence that banks with a low rate of return on assets are more likely to be acquired, other things being equal. Fourth, they conclude that, absent managerial ownership, "governance" variables are not importantly related to the probability of a bank's being taken over.

Finally, they put these points together to argue that their empirical evidence is consistent with an "entrenchment" explanation of bank acquisitions, wherein a link between managerial ownership and a preference for control would reduce the likelihood of being acquired. Hadlock, Houston, and Ryngaert contrast this explanation with a "financial incentive" explanation (in which managers with large ownership stakes have the most to gain from an attractive takeover premium, and are thus more likely to accept offers to be acquired), a "discipline" explanation (in which high managerial ownership is associated with a strong incentive to maximize shareholder value, thereby implying a negative relationship between managerial ownership and the probability of being acquired for disciplinary reasons), and an "irrelevance" explanation (in which acquisitions are unrelated to governance variables).

In reaching their conclusions, Hadlock, Houston, and Ryngaert generate an interesting set of facts from their small, but informative, sample of bank acquisitions. First, ownership by inside directors is much lower for target banks than for other banks. Second, board composition is quite similar for targets and non-targets. Third, target banks have a relatively high rate of management turnover, especially when insider shareholdings are low. Combining these facts with their evidence on managerial ownership, the authors conclude that high managerial ownership reduces the likelihood of being acquired, bolstering the "entrenchment" explanation of the relationship between managerial ownership and acquisition probability.

3. An equilibrium view of "governance" and "performance"

Before exploring the Hadlock–Houston–Ryngaert tests, I want to place the research program in the context of studies examining the relationship between firm "governance" and "performance." A basic question emerges: How should we think of an *equilibrium* relationship between governance and performance?

In equilibrium, firms choose a "mix" of or relative reliance on a set of "governance" variables – including, for example, managerial ownership, other elements of executive compensation, director composition, or capital structure

– to minimize deadweight losses from agency costs (see, e.g., Demsetz and Lehn, 1985; Nuttall, 1998; Himmelberg et al., 1998). [1] In this setting, choices of alternatives – say, managerial ownership and the probability of a takeover – are linked, and substitutability among alternatives likely varies across firms.

If there were no frictions in achieving this equilibrium relationship, an econometrician could predict the firm's mix of governance variables using variables in the firm's contracting environment. However, there would be no presumption of a *causal* link from a *change* in a governance variable (say, managerial ownership) to a *change* in performance (say, return on assets). While there may be a cross-sectional relationship between governance and performance – because both are determined by the same variables in the firm's contracting environment – estimating a meaningful "within" relationship between governance and performance is, in general, difficult. [2] Again, thinking of firms generally, predictable variation across firms between managerial ownership and the probability of a takeover would not necessarily imply a link from managerial ownership to performance.

By exploring the governance–performance link in the context of bank acquisitions, Hadlock, Houston, and Ryngaert implicitly are interested in characterizing an out-of-equilibrium relationship. (Again, in a frictionless equilibrium, firms would choose a mix of governance variables, but there should be no necessary link from a change in governance variables to a change in performance.) One likely culprit in explaining a departure from equilibrium is a friction in the market for corporate control (e.g., regulation). In this case, a promising empirical strategy would be to identify an exogenous change in the market for corporate control which could shift both the "governance mix" (including managerial ownership) and firm opportunities and performance. Changes in the corporate control environment at the state level could be particularly useful sources of variation (in a different context in banking, see Hubbard and Palia, 1995).

4. Mapping the equilibrium view to bank acquisitions

Once one starts from the perspective that governance and performance are determined in equilibrium by a similar set of (observable and unobservable)

[1] The authors argue that endogeneity of ownership is not important in their case because the firms are in the same industry with similar risk levels. They address neither observable determinants of ownership other than risk nor unobserved heterogeneity in determinants of ownership. Simply arguing that "a firm's ownership structure is not always optimal" (footnote 27, page 21) does not justify treating ownership as exogenous, as I argue below.

[2] Charles Himmelberg, Darius Palia, and I formalize this intuition in our paper (Himmelberg et al., 1998).

variables in the firm's contracting environment, one can motivate the kinds of tests considered by the authors, though not necessarily with the same economic interpretation. I consider three exercises below.

First, managerial ownership levels may be inefficiently high. Suppose there is some significant "fixed cost" in the market for corporate control that inhibits takeovers. In this case, high managerial ownership levels could reduce the likelihood of a takeover (as in the entrenchment explanation favored by the authors). The authors' empirical evidence does not obviously fit this explanation, however. Banks with high levels of managerial ownership do not appear to have a low return on assets (though they might have a low "Q" measure of performance).

Second, managerial ownership levels may be inefficiently low. A friction in the market for corporate control (say, because of a regulatory change) may lead to acquisitions and changes in management (see, e.g., Hubbard and Palia, 1995). This explanation would be bolstered by an analysis of whether "performance" improves after a takeover.

Third, a change in investment opportunities could explain both a change in governance and performance. Suppose that the initial governance mix is chosen as a constrained optimum given regulation. A change in regulatory constraints may, then, lead to a different governance mix and a different required talent for the chief executive officer (CEO) – that is, takeovers and managerial turnover. Variation across states and time in corporate control regulation would be useful here, and empirical analysis would focus on whether performance improves following being acquired.

These three stories have in common their emphasis on the joint determination of governance (including managerial ownership and the probability of a takeover) and performance and the need to isolate an exogenous shift in a variable determining governance and performance (e.g., regulation). Exercises like this offer ways to extend the careful tests of Hadlock, Houston, and Ryngaert and to facilitate economic interpretation of these tests.

Stepping outside of this broad point, the authors may also want to explore a simple "life-cycle" explanation. Many banks are family enterprises (or, at least, private firms with large individual or family shareholders). Might family shareholders in such banks use an acquisition as a means of cashing in on the expanded liquidity of the bank corporate control market? Such a story is consistent with the consequences of outsider-ownership and CEO-age variables in the authors' tests.

5. Next steps

To conclude, Hadlock, Houston, and Ryngaert have given us a thoughtful and careful analysis of links among corporate governance, performance, and the likelihood that a bank is acquired. I believe their work could be profitably extended by focusing more sharply on the objective of modeling the choice among governance mechanisms and the link between governance and performance. The key challenge in that extension will be the identification of exogenous changes in firm's contracting environment.

In the banking application on which Hadlock, Houston, and Ryngaert focus, three questions then remain. First, what factors explain a "suboptimal" governance mix? Second, what is the economic link between an "ownership change" and "performance"? Third (and most important, in my view), are there efficiency gains from shifts in the contracting environment for banks?

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