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**Article**

## Comment: Kommentar zu: Warum Abwehrmaßnahmen gegen Übernahmen in Spanien

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**Comment to  
“Warum Abwehrmaßnahmen gegen  
Übernahmen in Spanien?  
(Why Takeover Defenses in Spain?)”**

By Robert S. Chirinko \*

This paper provides a highly interesting and thought-provoking analysis of takeover defenses in the Spanish equity market. The results shed light on the impact of the adoption of takeover defenses, an issue that is of general interest to students of corporate control. In addition, the paper provides interesting insights into the market for corporate control outside the well-worked UK and US equity markets. Several takeover defenses are considered (in order of frequency of adoption; see Table 2): voting caps, board membership, supermajority amendments, non-voting shares, and “golden” shares, as well as other measures. Professors Alemany and Crespi-Cladera employ an event study approach, and test hypotheses in terms of abnormal returns around the time of the adoption of these takeover defenses.

Let me begin my comments with an important caveat. All results in this paper derive from the benchmark market model that generates estimates of the “normal” return

$$R_{j,t} = \alpha_j + \beta_j R_{M,t} + \varepsilon_{j,t}$$

where  $R_{j,t}$  is the return of asset  $j$  in period  $t$ ,  $R_{M,t}$  is the market return over the same period,  $\alpha_j$  and  $\beta_j$  are parameters, and  $\varepsilon_{j,t}$  is the error term. Given estimates of the parameters ( $a_j$  and  $b_j$ ), it is straightforward to compute the abnormal return as the difference between  $R_{j,t}$  and ( $a_j + b_j R_{M,t}$ ) for the event window defined around the adoption of takeover defense. All results depend on the estimated model. Questions abound: are the residuals “well-behaved”?; are they free of serial correlation that might bias estimated returns over any given window? how much of the variation in  $R_{j,t}$  is explained by this model? It would be good to know more about this econometric equation and its performance over the sample.

As discussed in the paper, the adoption of takeover defenses has ambiguous effects on returns. Following from the standard principal/agent model, the *Entrenchment Hypothesis* implies that takeover defenses lower returns. By reducing the possibility of a takeover, managers have more latitude to take actions that benefit themselves at the expense of shareholders. By contrast, the *Shareholder Value Hypothesis* holds that takeover defenses have a positive impact on returns. Higher barriers to acquiring control by an outsider provide incumbent managers advantages during the negotiation process. Furthermore, these barriers protect managers’ firm-specific human capital, which also imparts value to the firm.

There are many interesting results in the paper, and I wish to focus on only two. First, the authors find that, eight days prior to the adoption of a takeover defense, returns fall by a statistically significant 64 basis points. Negative effects are absent elsewhere in the window. This is a puzzling result. The authors explore several plausible explanations, but the issue remains open.

The second finding, the one I wish to consider in more detail, is that returns rise by a statistically significant 57 basis points on the event day. This result is consistent with the *Shareholder Value Hypothesis*. However, the authors take a deeper look at this hypothesis in terms of the Spanish equity market. They note that, according to the Ruback (1988) model, any effect on equity returns depends on the takeover premium multiplied by the probability of a takeover bid. Based on ex-post data, the authors argue that the takeover probability is very low in Spain, so the search for an economic explanation of the positive abnormal return must continue. Before proceeding further, I might note that the assumption that the Spanish takeover market is inactive, can be challenged. Over a ten year interval, there were 111 takeovers (Table 3). I am not sure what this number implies for takeover possibilities and the pressures being exerted on incumbent managers, but it is not immediately obvious to this non-Spaniard that the corporate control market is inactive. Perhaps additional institutional features can make the case more persuasive.

With this point noted, I am intrigued by the authors’ behavioural finance explanation for the positive abnormal return. They argue that shareholders may be less concerned about managerial actions potentially diverting resources when returns are rising. Hence, the adoption decision may be correlated with positive abnormal returns, and this correlation, rather than a true wealth effect, may be at the core of the puzzling positive abnormal return. Table 7 (with an adjustment for industry effects) supports this interpretation. However, in the best of the scientific tradition, the authors test the other part of their behavioural finance hypothesis, and examine whether the relaxation of takeover defenses occurs when returns have been poor. While the sub-sample of firms relaxing takeover defenses is small, Table 8 suggests that takeover defenses are also removed when abnormal returns are positive. Thus, the selectivity associated with the “euphoria model,” while intuitively plausible, does not offer a complete explanation.

The authors then proceed to look at additional factors that might explain the pattern of abnormal returns and the adoption of takeover defenses. They consider the role of trading volume, ownership structure, the type of protection, and the sector of activity. Unfortunately, the puzzle of the positive abnormal returns remains.

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I would like to offer an alternative explanation of why the adoption of takeover defenses are welcome by the equity market. This hypothesis is mentioned in the Introduction of the paper, but deserves further elaboration. In the United States, the 1980's witnessed great concerns about equity finance. This system was considered flawed and plagued by "short-termism," the need for managers to forego profitable long-term projects in favor of projects paying lower returns but whose profits arrived relatively quickly.<sup>1</sup> Undue pressure from equity investors forced managers to adopt sub-optimal, short-term investment horizons. There was a perceived lack of long-term patient investors, and alternative financial systems, such as those in Japan and Continental Europe, were viewed very favorably. The success of the U.S. economy and the robustness of equity markets in the industrialized world during the 1990's may be the primary reasons why "short-termism" has received little attention in recent years,

This demise may well be premature and unwarranted. At a formal level, there are several models that support the intuitive notions of "short-termism."<sup>2</sup> These models are generally based on the notion of "signal jamming" by managers who have an information advantage relative to investors. The latter are aware of both this discrepancy and managers' incentives to manipulate performance signals. Consequently, biased signals are expected and, depending on the exact nature of the information asymmetry, can be an equilibrium outcome of the strategic interactions between managers and investors. I might speculate that the adoption of takeover defenses serves as a coordinating device that allows dispersed investors to coordinate and refocus management's energies on pursuing wealth maximization. Such a story is consistent with the positive abnormal returns reported in this paper. Moreover, a related result was obtained by Chirinko, Garretsen, and Sterken (2000) for Dutch firms, whose performance (measured by the return on assets) improved when anti-investor protections were adopted. "Short-termism" offers a compelling explanation for the positive abnormal return reported in this paper, and throws cold water on the notion that equity markets are the preferred means of providing finance.

While I find the results in this paper interesting and interpretable, I would ask for more. The sample contains forty instances where firms adopted takeover provisions, and seven where the defenses were removed. The disadvantage of a small sample is well known. However, the advantage — the ability to dig deeply into the specifics of each case — should be exploited. The authors begin such an analysis with their interesting discussion of Telefonica. I would ask them to expand their discussion to many more firms in the sample. This is no small amount of work, but I believe that the additional insights to be gained from a closer examination of the institutional details will yield a very favorable benefit/cost ratio.

A second extension would be to analyze the adoption decision itself through a formal probit analysis. The data are already available, so this request will be relatively easy to execute. The paper currently analyzes the consequences of takeover defenses on stock market performance. The proposed probit analysis would allow the authors to provide a deeper understanding of why these devices are adopted. In addition, they would create some instruments that might prove useful in addressing the selectivity problems noted above.

While I learned a good deal from the paper by Professors Alemany and Crespi-Cladera, lingering puzzles remain. Note that these puzzles pertain more toward the corporate control literature than this particular paper. Are hostile takeovers likely to be an effective disciplining device? Most hostile takeovers are not motivated by poor performance of the target company. Thus, one of the elements for proponents of the market for corporate control seems to be missing. Furthermore, how badly are deposed managers treated? If the past few years in the United States is representative, the answer to the question is not badly at all! What discipline potential takeovers apply to incumbent managers is not at all clear.

How does this literature explain Richard Notebart? The operating framework in most of the takeover/control literature is the principal/agent model; anecdotal evidence suggests that the model does a poor job explaining the behaviour of top managers. Richard Notebart was the Chief Executive Officer of Ameritech, headquartered in Chicago and one of the seven "Baby Bells" created from the breakup of AT&T in 1984. Ameritech was acquired by SBC, another Baby Bell headquartered in Texas. The purchase of Ameritech was finalized on October 8, 1999, and Notebart profited handsomely. He donated a small part (\$4 million) of his windfall to a local museum located on the lakefront. By all indications, Notebart's marginal utility from an additional dollar of income is zero. Yet, shortly after leaving Ameritech, Notebart took another job as a Chief Executive Officer of a technology company that was much smaller than Ameritech. The principal/agent model, which is the workhorse of the corporate control literature, is based on financial incentives dangled in front of top executives. Yet, as the stories of Richard Notebart, Robert Rubin (who took a job at Citicorp/Travellers after leaving the U.S. Treasury), and undoubtedly many other leading executives demonstrate, financial incentives cannot be the primary driving force behind their decisions.

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<sup>1</sup> For an example of these concerns, see Grundfest (1990), Hayes and Abernathy (1980), Jacobs (1991), and Porter (1992a, 1992b).

<sup>2</sup> See the survey by Bohlin (1997) and the models in Bebchuk and Stole (1993), Narayana (1985), Stein (1989), and Webb (1993).

The principal/agent model would thus seem to handicap rather than help researchers. For many executives, the marginal utility of income is vanishingly small, a fact that needs to be reflected in the models we use to explain executive behaviour and its responses to economic and non-economic forces.

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<sup>3</sup> Other than those contained in the paper.