

COUNTERPOINT

Reply:**The Need for Theoretical Coherence
and Intellectual Rigour in
Corporate Governance Research:
Reply to Critics of Donaldson and Davis**

by

Lex Donaldson †**James H. Davis** ‡**1. Introduction**

We published an article (Donaldson and Davis 1991) in an earlier volume of the *Australian Journal of Management (AJM)*. In a subsequent number of the AJM two commentaries have appeared in the *Counterpoint* section that critique our paper (Arthur, Garvey, Swan and Taylor 1993; Whittred 1993). Herein we would like to offer our reply. We will consider first the commentary by Arthur et al. (1993) and second the commentary by Whittred (1993).

2. Reply to Arthur, Garvey, Swan and Taylor (1993)

The article by Arthur et al. (1993) is an oddity. It is cast as a critique of our paper yet its main target is a position that we attacked. Thus, there is a large measure of agreement, or potential for agreement, between us and them, but this is swept aside by Arthur et al. (1993) in favour of a critique. This is but one aspect of the way in which their commentary lacks internal consistency and coherency.

2.1 Agency-Control Theory

The main positive point of Arthur et al. (1993) is that the agency theory perspective on corporate governance is defective and that a “costly contracting” approach is a superior version of agency theory. This leads them to criticise present-day policy reforms that seek to impose a more independent board on corporations. They caution that superior corporate governance mechanisms arise

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endogenously rather than being imposed exogenously. This distinction is quite acceptable and indeed was pointed out in an earlier statement by the senior author (Donaldson 1990a). It seems a pity that Arthur et al. (1993) had not taken time to read this earlier statement.

Arthur et al. (1993) are at pains to distinguish this superior version of agency theory from the simplistic version that we will herein call the agency-control theory. This latter states that managers as agents require controls, in particular by the board of directors, to prevent them inflicting losses on their principals, the outside shareholders. We criticised this agency-control theory in our article (Donaldson and Davis 1991). Arthur et al. (1993) also criticise it. Since we are all criticising the same thing, one might have expected that Arthur et al. (1993) would welcome our critique, but instead they attack it. This seems inconsistent.

Arthur et al. (1993) assert that we mislead in our characterisation of agency theory. The version of agency theory on which we focused (Donaldson and Davis 1991) was the agency-control theory. As Arthur et al. (1993, pp.95, 97) themselves make clear, this is the version associated with Jensen and Meckling (1976). We focused on this version of agency theory because this version has been influential in management theory research [e.g. Kosnik (1987, 1990); Kosnik and Bettenhausen (1988)], as has the similar theory (as regards corporate governance) of Williamson (1985). Such high-profile contributions deserve inquiry even if other scholars such as Arthur et al. (1993, p.97) consider those contributions flawed. We would beg to suggest that, in many academic circles still today, it is the writings such as Jensen and Meckling (1976) that are considered as authoritative and seminal statements of agency theory—more so than the later writings that Arthur et al. (1993) prefer, such as Garvey and Swan (1992a,b), however meritorious the latter may be.

These seminal statements of agency theory (i.e. Jensen and Meckling 1976) deserve attention also because they provide academic, intellectual support for the presently popular policy reforms of requiring companies to have more independent boards of directors to control managers. Arthur et al. (1993, p.94) acknowledge the affinity between these calls for reform and agency-control theory. Hence, considerations of prevailing academic theory and public policy debate make the control version of agency theory salient.

Our focus upon agency-control theory does not mislead, it is highly pertinent. If others such as Arthur et al. (1993) wish to take a different theoretical approach and to develop different policy implications then that is their prerogative, but it is invalid to castigate those who choose to investigate the agency-control theory. Moreover, it is ill-judged in that a critique of the control version of agency theory opens the door for those who would assert other theories of corporate governance, including the costly contracting theory favoured by Arthur et al. (1993).

On the policy side, Arthur et al. (1993, p.94) are critical of reformers who prescribe a more independent board of directors to govern the company (i.e. Bosch, Cadbury and American Law Institute). They write that: “Donaldson and Davis’

view . . . appears to have increasingly permeated the regulatory perspective” (Arthur et al. 1993, p.94). Indeed, it has, but this is not because of our own writings. Arthur et al. (1993, p.93) continue: “Donaldson and Davis’ [sic] provide potential support for this narrow focus on the separation of ownership and control”. Now it sounds as if in some way we are at least partly to blame for erroneous thinking among reformers. While we have focused on the issues of separation of ownership and control, this no more than reflected the authoritative agency theory literature, as noted earlier. More importantly, our contribution was to critique that agency-control theory. We expressed skepticism and sought to call that view into question both theoretically and empirically. Hence, if our writings (and associated verbal pronouncements) on corporate governance have any impact it would be to dispute agency-control theory and the related policy prescription of adopting a more independent board. Thus, linking our writings to erroneous ideas enjoying currency among policy reformers is tendentious and unfair, for we have sought to combat just such ideas.

The same, however, cannot be said for Arthur and Taylor (1992). While the commentary by Arthur et al. (1993) inveighs against agency-control theory, elsewhere Arthur and Taylor (1992) take a position that is in the agency-control theory tradition. They argue that an independent board is an alternative form of control on managers to corporate takeover (Arthur and Taylor 1992). This is consistent with the agency-control theory that managers unless checked will gratify their self-interest at cost to the owners. It is the idea of market discipline, here the market for corporate control, that is held to provide a control on managerial consumption of owners’ wealth. Again, in Arthur et al. (1993) several controls on managers are discussed as ways to constrain managerial self-aggrandisement: “independent directors” (p.96), “the market for corporate control” (p.96), “large shareholders” (p.96) and “high levels of involvement of financial institutions in monitoring management” (p.97). These are the sorts of external controls on managers that agency-control theories postulate (Jensen 1989). Thus, Arthur et al. (1993) are inconsistent in that they invoke agency-control theory arguments while simultaneously criticising agency-control theory and calling for a version of agency theory that eschews simplistic control notions. Thus, it seems doubly unfair that they criticise us for giving attention to agency-control theory, for, whereas we attacked it (Donaldson and Davis 1991), Arthur et al. (1993) are using its arguments and thereby lending it support.

2.2 Misunderstanding of Stewardship Theory

Arthur et al. (1993, p.94) write that: “. . . the distinctions drawn by Donaldson and Davis between agency and stewardship theory are at best arbitrary, and potentially very misleading”. Yet, their view is that: “. . . managers motivated by (among other things) debt and capital structure are put in charge” (Arthur et al. 1993, p.93). Stewardship theory does not see debt and capital structures as the motivators of managers (Donaldson 1990a; Donaldson and Davis 1991). It draws inspiration from a tradition of psychological theorising and research that sees the

job that the manager does as being the most powerful shaper of his or her work behaviour (Argyris 1964; McGregor 1960). It is the activation of such intrinsic motivation and of needs for achievement and responsibility and the like that are the main drivers of managerial effort and performance (Herzberg 1966; McClelland 1961). Moreover, belief in the organisation and its mission, loyalty, deference to authority, social esteem and so on all play a part in determining managerial work behaviour (Etzioni 1975). These factors are the central motivators under stewardship theory rather than the narrow calculation of self-interest (in largely pecuniary terms) that agency theory asserts. Thus, stewardship theory is very different from agency theory. The difference between the two theories draws upon, and reflects, a long-running split between economic models of people and socio-psychological models of people. It is based on different assumptions about the nature of people and how they are best organised. Thus, it is untrue to state that the difference between agency and stewardship theory is “arbitrary” or “potentially very misleading” (Arthur et al. 1993, p.94).

The difficulty that Arthur et al. (1993) have in distinguishing between agency and stewardship theory stems from their mischaracterisation of stewardship theory. Again, they write: “The distinction between stewardship and agency theory is not clear because every manager, like every person, will want to do a good job within the confines of the incentives, essentially property rights, facing him or her” (Arthur et al. 1993, p.95). This characterises the agency theory view, but not the stewardship theory, which sees the basis of motivation as not residing in incentives nor in property rights. Here we see the difficulty of anyone in the economics paradigm comprehending a theory from outside that paradigm (Kuhn 1970).

2.3 Method

Arthur et al. (1993, p.94) also criticise our method. This is curious because our results found against agency-control theory. Since Arthur et al. (1993) profess to be against this theory they might normally be expected to welcome our findings. They welcome the findings from several other studies that go against or qualify agency-control theory (Arthur et al. 1993, pp.96–98). Again, Arthur et al. seem inconsistent and internally incoherent in their argument.

Inconsistency is seen once again in the treatment of the study by Rechner and Dalton (1991). This found in favour of agency-control theory and yet is quoted with approval (Arthur et al. 1993, p.98), despite the avowed mission of Arthur et al. (1993) to replace agency-control theory with a more sophisticated version of agency theory.

Arthur et al. (1993) criticise our use of ROE as a measure of firm performance. Yet, Arthur et al. (1993, p.98) do not criticise Rechner and Dalton (1991) who also use ROE, nor call their ROE results invalid. Moreover, the results of Rechner and Dalton (1991) using ROE were similar to their results using ROI and profit margin. Arthur et al. (1993) accept the validity of these results, but since the ROE results were so similar, it follows logically that the ROE results must

be equally valid. ROE reports firm performance about as validly as ROI and profit margin in Rechner and Dalton (1991). Thus, there cannot be some method defect that afflicts the ROE results more than the ROI and profit margin because if there were the results obtained would not have been similar. Hence, the objection by Arthur et al. (1993), which joins with the criticism made by Whittred (1993; see below), that ROE is invalid because of contamination by leverage, fails.

Arthur et al. (1993, p.100) criticise our use of accounting measures of firm performance on the grounds that they may be manipulated by management accounting methods. But the same is true in the Rechner and Dalton (1991) study on which Arthur et al. (1993) rest their case. Again, we see a failure to mount a consistent argument.

Arthur et al. (1993, pp.99–100) make an argument about the impossibility of share price registering any effect of board structure. They argue that the post-1985 share price changes will be caused only by surprises that were not anticipated and included in the price by 1985. But adoption of a new board structure (e.g. shift from independent board chair to CEO-chair) after 1985 would be an event that would not necessarily have been anticipated beforehand by the market and so would effect the share price post-1985 (if there were an effect, as agency-control theory posits). Arthur et al. (1993, p.99) argue that the absence of a significant finding is because the method could not register one, but our view is that the method could and that the failure to register a positive stock market effect of an independent board chair is because the theory is false.

Rechner and Dalton (1989) also used stock market measures of firm performance to test the theory, which implies that, unlike Arthur et al. (1993), Rechner and Dalton do not believe that there is some inherent problem that prevents board structure effects ever being captured by stock market measures. Rechner and Dalton (1989) failed to find the positive effect that agency-control theory led them to expect. Thus, neither our use of a stock market measure nor the lack of a finding supportive of agency-control theory with that type of measure are idiosyncratic to us. Indeed, it has occurred with Rechner and Dalton (1989), the researchers whose other results (Rechner and Dalton 1991) Arthur et al. (1993) quote with approval.

Again, Arthur et al. (1993, p.100) call for more variables to be controlled in order to make a better estimation. We are in favour of increasing method rigour and would hope that this can be attained in future research on this topic. But we must remind ourselves that no study can control all variables and that the only variables that require control are confounds, that is, exogenous causes that register as spurious effects. Simply stating a list of variables that might seem to affect the dependent variable *prima facie* is easy, but not necessarily helpful, since they may not all be confounds. To be a confound, a variable that is a cause of the dependent variable also has to be correlated with the independent variable, with a specific sign and sufficient magnitude, to generate the spurious relationship between independent and dependent variable. Whether the supposed confound variable actually has such a correlation with the independent variable is an empirical matter

that needs to be ascertained for the data in each study.

It is not established from the arguments of Arthur et al. (1993) that our results are invalid nor that they should be dismissed in the cavalier fashion advocated by them.

The main contribution of our work on corporate governance to date has been to seek to break the present domination of thought enjoyed by agency-control theory. This has been progressed by advancing an opposite theory, stewardship theory, and showing that empirical tests by no means invariably favour agency-control theory and can support stewardship theory (Donaldson 1990a; Donaldson and Davis 1991). This in turn challenges those policy prescriptions that call for companies to adopt an independent board. We would submit that this is, at the least, a step in the direction of querying such present-day reforms and urging that their scientific base be established. As such our work is precisely in the direction that Arthur et al. (1993) advocate. But their rhetoric rings somewhat hollow in that even in their own contemporary statements they slide back into the traditional control version of agency theory (Arthur and Taylor 1992; Arthur et al. 1993). Their theorising is built upon a presumption that managerial motives are anti-organisational unless checked but provide no evidence. As we have seen, the commentary by Arthur et al. (1993) is also riven by contradiction and inconsistency at many places. This sits uncomfortably with the present-day posture of economics as the only discipline in social science that provides rigour in theoretical argument and empirical method [see Donaldson (1990b)].

3. Reply to Whittred

Whittred (1993, p.107) similarly concludes that our research "... sheds little real light on the issue". This is because of supposed defects in the method.

Specifically, Whittred (1993, p.107) lists three method defects: "Ambiguously defined dependent variables, ineffectual controls for confounding variables and important omitted variables ...". Yet, omitting variables is only a problem if this leads to confounds, so there are really just two points here.

The main criticism of Whittred (1993) is that our analyses failed to control variables that are confounds. This means that effects on firm performance (the dependent variables) attributed to board structure (CEO duality, the independent variable) are spurious. On this view, the real cause of firm performance resides in the confounding variables and failure to control for them gives the appearance of an effect of CEO duality where there is either nil effect or an effect opposite to the positive one we reported. Whittred (1993) argues that leverage and risk are uncontrolled confounds in our study.

3.1 *Leverage*

Whittred (1993) gives most space to the idea that leverage is a confound, so we will give this issue the most attention here. His argument is that ROE is affected by leverage, so that ROE is more volatile in firms with higher leverage. He hypothesises that if firms with CEO duality have more leverage, then in periods of

good economic conditions their ROE will be higher than firms with independent board chairs and that in periods of adverse economic conditions CEO duality firms will have lower ROE than those with independent chairs. In this way he offers an explanation of why Rechner and Dalton (1991) obtained results opposite to our own. This is an ingenious idea and potentially an interesting contribution to this literature and other scholars may well wish to explore it in future work.

The first point to make is that it assumes that CEO dual firms have higher leverage than independent chair firms. This would need to be shown empirically in each study before the moderator argument could begin to be applied in that study. We are not aware at this time that this has been demonstrated.

Whittred (1993, p.105) hypothesises that the source of the superiority of independent firms on ROE in Rechner and Dalton (1991) is the recession in the second half of their study period (1978–1983). If this were true then the independent firms would be more superior to the duality firms in the second than in the first (more prosperous) part of that study period, but an investigation of the graphs in Rechner and Dalton (1991, pp.158–159) shows that there is no such shift. Rechner and Dalton (1991, p.157) themselves comment that:

The pattern of these relationships is stable both across measures and across time. It is especially notable that a comparison of periods with relatively high financial returns (e.g. 1978–1980) with those of more modest returns (e.g. 1981–1983) indicates that firms with independent governance still consistently outperformed the CEO duality firms.

Thus, the results in Rechner and Dalton (1991) fail to conform to the pattern expected on the Whittred hypothesis.

The second point is that the effect of leverage on ROE is to make ROE more volatile than ROA (Whittred 1993, p.105 and Table 1). But the actual ROE would therefore depend on the ROA. If the true effect of board structure on ROA was nil then in prosperity ROE would be greater for duality than independent and in recession ROE would be lesser for duality. But agency (-control) theory argues that ROA is *lower* as a result of duality. For ROE to be greater for dual than independent in prosperity the gain from leverage would have to more than off-set the negative effect of duality on ROA. Therefore, in prosperity the superiority of duality on ROE would be small, as it is net of the true negative effect on ROA that underlies it. By contrast, in recession the superiority of independent over dual would be larger because the leverage of the duality firms would further depress their ROE below the already lower ROA that results from their duality. Thus, agency theory leads to the hypothesis that the superiority on ROE of duality in prosperity is smaller than the superiority of independent in recession.

This hypothesis may be tested by comparing the results from the recession years as nominated by Whittred (1981–1983) from Rechner and Dalton (1991, p.158, Figure 1) with those from the prosperous years as nominated by Whittred (1985–1987) from Donaldson and Davis (1991, p.57, Table 1). [Since Rechner and Dalton (1991) did not control for industry this comparison cannot either.] By interpolation from Figure 1 of Rechner and Dalton (1991) we obtain the

approximate figures that, in the recession, average ROE was higher in independent than in dual firms by about 3% ROE. In Donaldson and Davis (1991) in the prosperous years, the average ROE in the dual firms was higher by 3.26% ROE. Thus, there is no evidence here that the superiority of dual in prosperity is smaller than the superiority of independent in recession.

If one re-expresses the superiority of the dual relative to independent as a percentage of the independent ROE (i.e. the figure supposedly less contaminated by leverage), then in recession the dual is 21% less than independent and in prosperity the dual is 28% greater than independent. Once again there is no evidence that the superiority of dual in prosperity is smaller than the inferiority of dual in recession. The hypothesis is not supported.

Even if one supposes that the ROE results are afflicted by differences in leverage, the pattern of the results in these two studies is not consistent with the idea that there is an underlying superiority of ROA caused by independent chairs in Donaldson and Davis (1991).

Arthur et al. (1993, p.99) state about the research of Rechner and Dalton (1991) that: "The ROI measure, being return on total assets, is not confounded by leverage problems". Thus, the ROI results from Rechner and Dalton pick up the true ROA effects of broad structure, while the ROE results would be expected to behave in the manner Whittred (1993) depicts, that is, being more volatile. The superiority of independent over dual would be constant in the Rechner and Dalton (1991) study for ROI but not for ROE which would show fluctuation over the years, with the superiority of independent being smaller in the prosperous than in the recession years. But a comparison of the graph for ROI with that for ROE in Rechner and Dalton (1991, p.158) fails to find such a pattern. The superiority of independent over dual is fairly stable across the six years for both ROI and ROE; there is no sign of greater volatility of the independent-dual difference for ROE as compared with ROI. Thus, the idea of Whittred that ROE is less reliable an indicator than ROI is not borne out in the results of Rechner and Dalton wherein ROE yields the same results as ROI. Thus, the use of just ROE in our study is not a problem.

In summary, the objection by Whittred (1993) to our use of ROE is based on a series of conjectures that remain speculative at present. The available empirical results do not lead to the view that ROE masks a true negative effect of CEO duality on firm performance (as argued by agency theory), nor that ROE is a less reliable measure of firm performance than ROA, ROI or similar, in these studies.

3.2 Risk

Whittred (1993, p.105), more briefly, also criticises our use of a stock market measure of shareholder returns because risk was not controlled. Whether risk accounts for findings such as Donaldson and Davis (1991) needs to be ascertained empirically in future research. There is no basis for a presumption that these results are due to risk until there is evidence of such an effect.

Overall, it is not established at the present time that our research is damaged by the omission of the variables of leverage and risk.

3.3 Incentive Alignment

A further concern of Whittred (1993, p.104) is that another confound was not controlled, despite our attempt to do so in this case (Donaldson and Davis 1991). This refers to our proxy for incentive alignment, that is, long-term compensation. Whittred states that this could be just tax-efficient remuneration. He writes that its real nature cannot be determined from our information, nor perhaps from any such data [see Whittred (1993, p.104)]. It seems that, on this point, there is no information that would be sure to be satisfactory to Whittred. Long-term compensation may enjoy tax benefits, but this does not preclude it from also providing a degree of incentive alignment with the interests of outside owners, through emphasising longer-term rather than short-term performance. The issue here is whether our measure captures fully incentive alignment. It is admittedly a somewhat crude proxy. There may well be scope for subsequent analysis to refine the measurement of incentive alignments. But given the remarks in Arthur et al. (1993, p.97), that measures of incentives in the economic literature may actually index managerial entrenchment and thus have quite opposite properties, obtaining satisfactory measurement of incentive alignment may be far from readily feasible.

Even on the most extreme interpretation of our measure, as totally failing to capture any incentive alignment and thereby not controlling at all for incentive which is held to be the underlying cause, this means that CEO duality produces positive returns to outside shareholders through the intervening variable of CEO incentives. Therefore, on this interpretation, an independent chair would prevent higher levels of CEO incentives from being installed and so would depress returns to shareholders. This would tend to confirm the incentive alignment argument in agency theory but would also question the argument about the necessity for an independent board chair. This, in turn, would undermine the control version of agency theory that sees an independent board monitoring management as essential. It is this aspect of agency theory that underpins the boards reform moves that disturb Arthur et al. (1993). And it is this theory of management always requiring control by a board of outsiders that has been at the centre of our own critical efforts.

On the matter of the definition of performance variables, we relied upon the source from which we took them, and this also led to the set of organisations that was studied. There is scope for improvement here, but it is far from clear that any imprecision introduced by these shortcomings is more than minor. Most importantly, it is not established that the limitations in our study cause crucial differences in results of such magnitude that erroneous judgements about the validity of agency theory ensue.

Whittred (1993, p.103) opens his commentary by setting aside theoretical considerations and proceeds to discuss the research as if it is a purely technical matter of accuracy in each aspect. This leads in turn to a mechanical application of

methodological strictures. We believe that this has the danger of losing sight of the purpose of social scientific enquiry. Social science is always guided by theory and empirical analysis is always primarily geared to probing the adequacy of theory rather than simply seeking to attain technical accuracy, however desirable (Popper 1945). Research is, necessarily, the business of sorting through the imperfect materials available and seeing which of the competing theories is supported best. On the matter of the effect of CEO duality on firm performance and shareholder returns, our research remains a critical challenge to the agency theory of corporate governance. The attempt by Whittred (1993) to dismiss our findings is insupportable. The great disjunction between our findings and those of Rechner and Dalton (1991) remain and constitute a research challenge for future scholars.

4. Finale

Theoretically, there is merit in retaining, in discussions of corporate governance, Stewardship theory which is a “stark alternative” to agency theory (Arthur et al. 1993, p.93). This will allow researchers to consider a broad range of possibilities in corporate governance and avoids a premature narrowing of the discourse by exclusive reliance on agency theory (even granting variants within that approach). Two economists have recently challenged the presumption that an economic analysis invariably leads to agency theory (Castanias and Helfat 1991). Their own economic analysis, in terms of the critical resources theory approach, leads them to a position that they see as having affinities more with stewardship than agency theory (Castanias and Helfat 1991). The most valid theory of corporate governance may lie in between the two extremes of stewardship and agency theory. But, without an extreme theory such as stewardship to counter the equally extreme theory of agency theory the middle ground will not be open to examination by scholars.

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