Dimensions and Constructs: A Response to Dess and Rasheed

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In their thoughtful critique, Professors Dess and Rasheed raise several important issues about the conceptualization and measurement of the organizational environment in general and our work in particular. In this rejoinder, we discuss a fundamental difference between their position and ours regarding the place of theoretical constructs and observable dimensions in conceptualizing and measuring the environment. We also take issue with several of Dess and Rasheed's specific criticisms of our work.

Constructs Versus Dimensions

We wrote our article with the purpose of improving the state of conceptualization and measurement of the organizational environment. Our concern with previous research was that it was incomplete, rather than incorrect. Much of this research measures a single dimension of the environment (e.g., firm concentration) and uses that dimension to represent the entire construct (e.g., complexity). The justification for this practice has been that it is worth sacrificing theoretical accuracy for precision of measurement. We are not satisfied with that view. It was our intention to develop measures that represent as much of the construct space as possible, while simultaneously satisfying key methodological criteria.

The literature on the dominant environmental constructs (complexity, instability, and what we call competitive threat) is replete with references to multiple (distinct) elements within these constructs. Using single dimensions to represent multi-dimensional constructs does not do these ideas justice, and shortchanges their conceptual richness. This is the weakness in the literature that we set out to rectify.

We believe that a confounding of constructs and dimensions is one source of this problem. James, Mulaik and Brett (1982) distinguish between constructs (or latent variables) that are "abstract...associated with presumed but not directly observable events" (55), and dimensions: "manifest variables [that] are indicators of

latent variables" (109). Although perfectly reliable dimensions are preferable, dimensions do not always perfectly and reliably represent constructs. These authors also suggest that several dimensions may be necessary to fully represent any given construct and allow that dimensions may be "perfectly reliable but not strongly correlated" (117). If two or more variables are intended to measure the same dimension of a construct, it is reasonable to expect them to be correlated. This does not hold, however, when measuring different dimensions of a construct.

An example using environmental instability may help to clarify our position. Our measure of instability contains three indicators: variation in the value of shipments and number of employees in an industry, and the average number of patents granted. It is true that the first two elements (value of shipments and employees) are correlated, as they both relate to market-based instability. But there is no reason to expect or require that other indicators of instability such as the number of patents — which indicates technological instability — would be correlated with market-based indicators.

Industries experience substantial variation in sales independent of technological change. For example, business in the building trades rises and falls in conjunction with interest rates, and technological advances may have relatively little impact on the industry. In periods of rapid interest rate changes, the number of firms and the value of shipments would show commensurate levels of variation, whereas the number of patents might show little or no change.

Traditional validation methods suggest that we should only use the value of shipments and number of firms dimensions to measure instability, because these are correlated but the number of patents is not. This approach would leave out the indicator of technological instability, which is quite important in research-driven industries such as chemicals and electronics. We are not suggesting that convergence among indicators is not desirable. If one is measuring a dimension, such convergence is necessary. Our aim, however, was to measure the entire

construct space, as conceptualized on the basis of a rich vein in the organization theory literature. Dess and Rasheed apparently assumed we were trying to develop measures of single dimensions. As they point out, multi-dimensional dimensions are nonsensical. But as we are attempting to develop measures for constructs, multi-dimensional measures are acceptable, and in some cases, essential.

Our aim was not, as Dess and Rasheed suggest, to "establish theoretical and methodological convergence between objective measurements of organizational environments at the industry level and top management perceptions of industry environments at the firm level." Rather we were examining a set of industry-level measures in light of Schwab's (1980) suggestions on validation; establishing a relationship between our measures and managerial perceptions is but one step in this analysis. Because we were investigating construct level measures, validation techniques for individual dimensions (inter-item correlation and factor analysis) were not appropriate. We attempted to show the validity of our measures using alternative approaches. We believe that demonstrating a relationship between our measures and managerial perceptions is one indication of the validity of our construct-level measures.

Key Issues

Because of space limitations, we have organized the rest of our rejoinder around only two sets of issues. First, we discuss two of Dess and Rasheed's arguments about our competitive threat construct. Secondly, we address some of their criticisms of our predictive validity study.

Dess and Rasheed have reservations about the indicators we used in our competitive threat measure. They suggest that changes in the number of firms and average market share (AMSC) do not represent competition. They believe that market shares in highly competitive markets could remain stable over time. But if firms are strenuously competing, it is unlikely that market positions will remain stable (Klein, 1977). In industries with substantial exit and entry barriers, we would suggest that a stable distribution of market share is representative of weak competition.

Prior to the 1970's, the U.S. steel industry had the kind of stability Dess and Rasheed describe. Market positions were established and market share changed little. These firms also engaged in little competitive behavior because each company had a comfortable market share. We argue that the industry at that time was characterized by little competition. The competitive turmoil that has haunted the industry in the ensuing decades has resulted in substantial market share changes (see Hoerr, 1988).

Dess and Rasheed further criticize us via example. They show two very different cases where the MSC would come out to zero. In the first case, every firm in the top eight except one changes during the interval of study, and that one firm maintains its exact same market share. In the second example, all of the firms remain the same, and they all retain their exact same market share over a 7-year period. Because there are no changes in market share in either case, both scenarios result in a numerator (NF x MSC) of O, despite the differences in turnover of firms in the top eight.

This criticism rests on the fact that when market share changes are exactly (not just close to) zero, firm turnover has no impact on the calculation of competitive threat. Granted. The question is, how likely is this ever to occur, especially in conjunction with significant turnover of firms in the top eight? Our data shows that even in the least competitive industries, there are invariably changes in market share: Our sample had *minimum MSC of .41 percent.* We concede that there may be industries where even less change occurs, but we argue that zero MSC is highly unlikely, and were it ever to occur, would indicate very weak competition.

Moving beyond such technical issues, Dess and Rasheed's mathematical critique of our competitive threat measure ignores two key points. First, from a theoretical standpoint, our measure is a close approximation of (the inverse to) Aldrich's (1979:64) concept of the carrying capacity of the environment, much closer than could be achieved by ignoring the competition dimension and focusing on munificence alone. Second, from a practical standpoint, our measure is strongly correlated with managerial perceptions of competitive threat. Although the measure could probably be strengthened, it certainly appears from this early data that we are moving in the right direction.

Dess and Rasheed also criticize our predictive validity study on several

grounds. First, they suggest that a relationship should be found between industry performance and competitive threat due to "definitional dependence." When this does not occur, they suggest that the reason is that our measure is poor. It would then follow that because the original Dess and Beard (1984) munificence measure was also found to be uncorrelated with performance, it also must be a poor measure. We suggest that neither of these charges is correct. Rather neither measure is related to performance because, Dess and Rasheed point out, an organization's (and by implication the industry's) ability to take advantage of environmental munificence is probably a more complex function than our simple correlation analysis would suggest.

Dess and Rasheed also criticize our inclusion of a liquidity measure in the industry-level performance scale. They suggest that there is little theoretical reason to believe that the liquidity measure would be related to the other measures. But Rappaport (1986) in his shareholder value model (a more comprehensive view of performance than ROI/ROS analysis) uses cash flow as an important indicator of the overall value that a firm has created for its shareholders. Because liquidity measures are preliminary indicators of cash flow, we included them as part of our measure. Further, the alpha coefficient of the performance scale (mean alpha = .72) demonstrates a high level of intercorrelation between liquidity and the other items, further substantiating its inclusion as an indicator of firm performance.

Another portion of our predictive validity model with which Dess and Rasheed take issue is our predictions concerning dynamism, performance, and industry-level performance, suggesting that they are not consistent with theory. The theoretical basis for our predictions in this area came from Thompson (1967), who argues that firm performance increases as the effects of the environment are minimized. Because the environment/performance relationship was not the primary focus of our study, perhaps this perspective was underdeveloped. The fact that our results run counter to this theory, however, does not undermine our analysis: it simply supports an alternative view — one which we expressed in our discussion.

Conclusion

Dess and Rasheed have conscientiously examined our work, pointing out flaws and places where further research is needed. For this we appreciate their efforts. In some cases, we suggest that they may have misunderstood what we were trying to say. Some of these cases were due to a lack of clarity in our original manuscript. We hope that this rejoinder has clarified such issues. However, there is still a basic disagreement — or at least a difference in emphasis — between us and our critics. Dess and Rasheed argue that Dess and Beard (1984) concentrated on simplicity and generalizability while sacrificing accuracy. In our research, we chose to let accuracy drive our construct-level measures, thus sacrificing some simplicity. It is clear from the Dess and Rasheed critique that some middle ground may be needed. In order to truly develop a useful literature on the nature of the organizational environment, we must find measures that fully cover the construct space, in addition to meeting important methodological criteria.

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