

NEW PARADIGMS IN BUSINESS RESEARCH – A COMMENTARY

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In his paper in Analysis II December 1986; Md. Zabid Abdul Rashid puts forward a theoretical construct which defines three dimensions and eight typologies of business management research. The purpose of this commentary is not to discuss his theory of inter-related disciplines and methods but to address the problem he outlines at the end of his paper.

'Until Academia begins to indulge in more research in business, and until the business sector is willing to co-operate and indicate their appreciation in research, the starting point of research in business management will never see the light at the end of the tunnel.' This sounds like a willing seller seeking an unwilling buyer in the dark.

It is a commonplace of business activity that a satisfied customer will continue to buy, all other things being equal. The problem is the initial 'sell-in'; and for this a clear understanding is necessary by the seller of the benefits to the customer of what he or she is trying to sell.

The rest of this short commentary therefore concentrates on the nature of academic research which the business would might find useful. Attempts are also made to indicate ways for a better customer contact on the part of academia in Malaysia.

It can be argued that to see research into business management as primarily a scientific discipline is unhelpful to both business and academia.

Proper considers that the essence of scientific theory is its inherit 'verifiability' or 'falsifiability'. Thus if the initial premises and methodology are clearly explained, the apparatus capable of being replicated, the results accurate and the theory rationally developed, then the theory can be tested again and again until it becomes the nearest approximation to the truth which the scientific community of that day and age can arrive at. The essence of business activities is that they are not scientific in this sence. Business judgements have often to be taken on the basis of partial, or obsolete data and in most cases it is impossible either to turn back the clock and try again or ever to carry out a verifying experiment in a similar comparable

environment with similar sets of conditions. It is usually impossible for a business man to know with any degree of scientific accuracy whether it would be better to merge or not to merge, to centralise or decentralise, to hedge currency or not. He has to move on towards varying degrees of success or failure for which there is no real scientific yard stick. At best his or her efforts become another case study to add to the store of experience about how, in general, business operates. Thus I would suggest that whilst scientific rigour and method should be an important aspect of enquiries into business activities, business research cannot be called scientific or a science, and that it is unhelpful to think of it this way.

I would suggest that business studies are more akin to political philosophy or the study of politics on the Aristotelian model. Michael Oakeshott said that in politics there is no starting point, or certainty of return, or anchor or clear destination;- 'the enterprise is to keep afloat'. Most business men on reflection would probably agree with this in relation to their businesses — Thus they would be less interested in hiring someone who could talk about progress in the development of the chronometer and its relationship to celestial navigation, than someone who could keep them off the rocks and moving roughly at the right speed and in the right direction — as seems best from time. Another aspect of business is that few business men or women could express the theory of what they are actually doing at all accurately; (although a theoretical basis could probably be found for it—possibly an ill-understood or obsolete one). Thus when presented with alternative ideas in a theoretical form they not only have a tendency to resist the content but also the form of presentation.

It is probable, therefore, that academia in Malaysia is wandering around in the tunnel, without a clear idea of what it has to sell and how to sell it.

The following is therefore a brief outline of a possible approach to improving the situation in Malaysia.

1. Effective research into business activities has to be needs based and action oriented. Pure research for the sake of increasing knowledge as such is likely only to be funded and appreciated by one's academic peers. It is also likely to encounter problems of access to really hard business data. (The 'what's in it for me syndrome'). There is a place for 'philosophers of business and industry' like Peter Drucker but he has built consultancy and personal friendship on hard academic discipline and his books are admired as much for the detailed and relevant examples he can quote as for their fertile imagination and lively style.
2. A central problem for academics is therefore how to get access and how to

offer help which is seen to be effective to business people on their terms i.e. 'to keep afloat' and to prosper.

3. Allied to this is the even deeper problem that business people, because their time is limited, (largely because of the 'action oriented' ethos of business), do not have sufficient time for reflection and very often are not even aware of the needs they have and the help which might be available to them. How much better for Malaysia many academics might think if businessmen could manage cash flow better, carry out project evaluation on a risk analysis basis, know more about the markets in which they operate, or understand on a practical basis the strengths and weaknesses of 'information technology'.
4. It seems to me therefore that academics who wish to engage in business research should sit down with their colleagues and analyse the business needs of industries which are reasonably close to them and assess their own skills and knowledge in meeting these needs through research and consultancy. Some of these needs will be obvious. For example whether or not to relax price controls on paddy which is a live issue and a complex intellectual problem. Others are latent and not yet clearly defined for themselves by the businesses concerned;- such as the effects of slower growth and higher unemployment in the Malaysia of the 1990's. There is also an important and valuable academic task in recording accurately and dispassionately business events which have already taken place; for example the lessons of the 'Freeport' and free manufacturing area status afforded to Penang. Having carried out this 'potential mutual benefits analysis' they should seek by running relevant seminars and by seeking personal contacts to persuade business people that there is a mutual benefit to such research. They should also, whilst retaining academic integrity, be prepared to shape their research in such a way that it will be seen by the business people concerned as being relevant to their needs and potentially helpful in improving business performance. If one considers the commercial, industrial, agricultural and 'invisibles' aspects of a developing and newly industrialising country in terms of understanding where it has been and where it should be going, the potential is enormous. But I am suggesting that real data, other than that provided by government departments (which are usually out of date and very generalised and sometimes unavoidably inaccurate) can only be generated if research is directed, at least in the first place, towards helping businesses to satisfy what they perceive, or can be persuaded to perceive, as their real needs.

Once a body of relevant data, case studies, histories, performance measurements, organizational studies, attitude surveys etc., have been published, then there may be a basis for the development of broad academic theories, which,

as I have suggested above, will not usually be scientific in the fullest sense of that term but tentative and indicative in the fashion of Douglas Macgregor's 'The Human side of Enterprise' or Peter Drucker's 'Innovation and Entrepreneurship.'

Only then will there be a hope of persuading business people of the truth of Kurt Lewins remark that 'nothing is so practical as a good theory'.