

**Behaviour, and how we know what we know (in social science generally,  
and in international business studies particularly)**

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**Abstract:**

**This paper is a fundamental critique of the observational positivist behaviourism which is the prevailing paradigm within business studies in general, and within cultural studies in international business in particular. The paper draws upon advances made fifty or sixty years ago in linguistics and social anthropology, and contrasts the intellectual lineage of ideas deriving from these, with the positivist behaviourism that gave birth to, and still dominates, social psychology (and which social psychology dominates cross-cultural studies in international business). The very idea of ‘behaviour’ is shown, in its most popular usages, to be deeply distorting, when we try to look at where our knowledge does (and does not) come from. Four sources of knowledge are briefly discussed – Common human nature (or intuitive psychology), Culture-specific semiotic systems, Experience, and Academic research about people and organisations. The positivist research paradigm encourages international business scholars to think that the bulk of their knowledge comes from the last of these – academic research. This paper argues that this is wrong, and that most of our knowledge (particularly that which we employ in the teaching room) is not derived from this (and an important debt to Pearce 2004 is acknowledged), but from the first three possibilities. The implications of this for the research paradigms that we inhabit, the respect we give to different kinds of knowledge, how we conduct and evaluate ourselves in the teaching environment, and how our research should be conducted, are fundamental; this paper argues for a major paradigm shift in respect of all of these issues.**

## **Behaviour, and how we know what we know**

This paper is about how a social anthropologist perceives one of the dominant research styles represented in JIBS and the Academy of Management school of publications. The conceptual issues relate directly to what we are doing when we carry out certain kinds of empirical work, and so have relevance both for theory and the realisation of theory in empirical work.

The paper begins by looking at 'behaviour', as a word and a concept which has a normal use in the Academy of Management Review (and Journal), but which is, from a social anthropological perspective, deeply problematic. The paper then looks at the research consequences of these problems.

The source discipline for many articles in the Academy of Management Review is psychology, perhaps qualified as social psychology. Social psychology is 'behaviourist', in origin and in current inclination (see Allport, 1985). By this, I mean that social psychology was born at a time when there was a wave of opinion, generalised across a number of social sciences, which held that a truly scientific approach to the study of people (and all their artefacts) would have to be *objective* and *observational*, and would have to renounce subjective understanding and any concern with *meaning*.

It is a measure of the puritan vigour of this approach, in its early days, that its practitioners were prepared to extend it even to the study of language. It is rather startling, on the face of it, that any would-be scientific concern with *language* should be prepared to renounce meaning. The dominant approach to linguistics in the U.S.A. in the middle-20<sup>th</sup> century, however, was that pioneered by Bloomfield (1933). Bloomfield's linguistics aspired to be scientific, objective and behavioural. Meaning could only be understood subjectively, and could not be measured or observed; as such, it could not be studied with the 'scientific rigour' that many linguists regarded as 'the supreme test of scholarship' (Ullmann, 1966). As such, meaning was sidelined and avoided: 'for many linguistic students the word *meaning* itself has almost become anathema' (Fries, 1954, p.58).

It is easy, in the early 21st century, to agree with Reid, that 'without meaning there can be no language and no linguistics' (Ullman, 1996, p.15, citing Reid, 1960, p.18). Indeed, it now seems almost unbelievable that a linguistics could have emerged which attempted to marginalize issues of meaning. The example from linguistics gives us, however, an idea of the conceptual and moral authority of the drive to scientific behaviourism in the middle of the 20<sup>th</sup> century (for commentaries on the linguistic aspects of this, see Hockett, 1958; Ullmann, 1966; Ardener, 1971). We can also see, from commentary on this, that the full consequences of objectivist behaviourism in linguistics were not always fully acted upon by those who gave themselves up to the idea: 'Meaning, as at least one linguist has expressed it, has become a "dirty word"; but if the name tends to be avoided, there is no doubt that every linguist employs the concept, though some would be unwilling to admit

to such improper thoughts' (Allen, 1957, p.22). This ideological refusal to admit particular analytical possibilities, while using them *sotto voce*, is one that has modern echoes, as we will see.

There were, of course, linguists who continued to be interested in meaning throughout the Bloomfieldian 'high-structuralist' period, but they were in a minority for a time.

Approaches that remained in contact with etymology continued an interest in meaning (for example, von Wartburg, 1943; Trier, 1931; Bally, 1940; Ullmann, 1973), as did those that were closely allied to the emerging sophistication of social anthropological accounts (for example, Malinowski, 1935, 1949; Ogden and Richards, 1949; Firth, J. 1957). The nature of meaning has also, of course, become a central issue in philosophy, generating a vast literature, some of which derives from, or overlaps with, the issues and references already discussed. No further attempt is made here to deal with this.

To do justice to these issues would take volumes. The point which needs to be stressed here is that there was a vigorous attempt, in many areas of social science, to make the study of people objective, observational, and behavioural. This was true even within linguistics, where such an approach might seem obviously inappropriate. Bloomfield clearly tried to draw the object of linguistic science into the observable realm, by defining the linguistic form as 'the situation in which the speaker utters it and the response which it calls forth in the hearer' (Bloomfield, 1933, p.139).

This kind of behavioural objectivism sank deep into psychological science, and into the social sciences more generally. This was arguably more true in the U.S.A., than it was in the U.K. or in Europe more generally (this is controversial, but for comments on this, see Ardener, 1989, p.35; Casagrande, 1963). Social psychology, as stated above, is a subject still strongly marked by its 'behavioural' origin. The consequences of this, for the forms of expression often used in management research, are the focus of this article.

At a time when linguists were turning away from meaning, social anthropologists were making it central to their analysis. Different accounts would tell the story slightly differently, but in the UK at least, priority is often given to Evans-Pritchard (1937), or perhaps even to Malinowski (1935). By the early 1960s, British social anthropology was moving rapidly 'from function to meaning' (the phrase is originally from Pocock, 1961), and many monographs were published which gave substance to this idea (for example, Lienhardt, 1961; Turner, 1967; Douglas, 1963, 1966; Geertz, 1973; Crick, 1976).

Social anthropology turned away from behavioural positivism and towards 'meaning' in the 1960s. It has retained this orientation to the present, not of course without continuous internal argument. Social psychology, by contrast, was born as a study of 'behaviour', and has retained many habits of thought, enquiry and expression deriving from this. There is also a transatlantic flavour to the meeting of social anthropology and social psychology which is invoked here. As Allport says of social psychology: 'its present

flowering is recognised to be characteristically an American phenomenon' (Allport, 1985, p.2).

As a shorthand, we can say that British social anthropology in the 1960s became preoccupied with 'meaning', and that American social psychology from its origins was preoccupied with 'behaviour'. The study of 'meaning' led to interpretation, intersubjectivity, ambiguity, multiple accounts, and so on. The study of 'behaviour' led to attempts at observational objectivity, and to replication of the methods of the natural sciences.

What is wrong with conceiving of social psychology as a study of human 'behaviour'? The term seems, on the face of it, to be a borrowing from the study of 'animal behaviour', legitimately extended into the human realm. Desmond Morris, in his highly popular book *The Naked Ape* (1967) challenged the intellectual community to adopt, towards human beings, the same ethological research perspective that had emerged for the study of animals (see, for example, Tinbergen, 1951, 1953; Lorenz, 1952). To the ideologically puritan behaviourist, the only things that counted as knowledge were observations by an objective observer. There are good reasons for holding to something like this formulation in the study of 'animal behaviour'; we can not talk to animals, and we must suppose that we can not share meaning with them (although this probably needs arguing, at least at the fringes). The observation of 'behaviour' is our only access to intelligibility, usually argued within a Darwinian framework.

It is perhaps of interest that a degree course was begun in Oxford University, in 1970, called 'Human Sciences'. This was a pioneering attempt to bring together the natural sciences and the social sciences, as these were concerned with studying people (human beings, mankind, *homo sapiens*...). The natural scientists and the social scientists who were involved in this venture, sought common areas of interest and debate, where they hoped that their different perspectives could be mutually illuminating. One area of common interest that was proposed was 'behaviour'; after all, did not both humans and animals 'behave'?

Among Oxford social anthropologists of the time, Edwin Ardener was the one that took the closest interest in the new Human Sciences degree. He responded to the invitation to discuss 'behaviour' as a phenomenon common to both animals and humans, in a characteristically erudite and unsettling way. Ardener argued that the term 'behaviour', from its 15<sup>th</sup> century French derivation, came to mean 'socially prescribed or sanctioned conduct', with a positive high-prestige upper class association (Ardener, 1973; 1989, p.105). It came to form a semantic doublet with a parallel term, 'demeanour', which had a negative low-prestige lower class association. In as much as these terms still have an existence in natural language, we find enduring echoes of this. The natural language use of 'behaviour' evokes a context of good and bad, praise and blame. When a mother says to her child "behave yourself!", she is telling the child to be good, to conform to rules of social good conduct. 'Behaviour', in this unqualified sense, is always 'good behaviour'.

If a mother asks “did you behave?”, the answer “yes” means “yes, I was good”. A negative reply would mean “no, I mis-behaved, I was bad”. Similarly, someone asked to perform a task which they considered beneath their dignity, might still say ‘I would not so demean myself’ (although the usage is starting to feel archaic).

‘Behaviour’, then, meant ‘(good) behaviour’ - human activity that conformed to whatever social rules and conventions were operative. The term spoke of a commitment to, and an aspiration towards, the orderliness of social life. Within this usage, ‘there could be no such thing as random behaviour’ (Ardener 1989, p.106). Ardener then argued that the first uses of the terms ‘behave’ and ‘behaviour’, in the discourse of the natural sciences, were in Chemistry in the 1850s and 1860s:

These early examples have still some of the direct living metaphor about them. The very model of orderly discrimination of the conditions under which things acted as they did, was derived from social behaviour. Behaviour was marked therefore for its knowability in advance: an image or aspiration for the natural order. When in 1878 T.H. Huxley talked of the ‘behaviour of water’, he was reducing to orderly terms the activities of a supremely unpredictable element. No doubt it was the continual use of ‘behaviour’ in contexts in which the activity was far from understood, that led to its association with ‘activity in general’, and even (‘behaviour problems’) towards relatively violent activity. The generalization of ‘behaviour’ to the inanimate world has since then gone so far that we tend to think of it as ‘action that is not yet understood’ rather than as ‘action that is supremely understood because prescribed’.

It is ironical that the use of the term ‘animal behaviour’ probably owes more to its natural science uses than it does to its original social use. Paradoxically, then, we are offered ‘behaviour’ as a quantifiable universal, a mere century after its metaphorical use in natural science began. Of course, there has been retained throughout the essential component of ‘constraint on action’. At all times ‘behaviour’ has been conceived of as rule-governed: the natural science shift has moved the locus of the rules. At one time behaviour is expressly the subject of rules, at another it is the subject of an aspiration that it will turn out to be governed by rules (Ardener 1989, p.106)

The term was borrowed into studies of animals, where the subject of ‘animal behaviour’ developed. The conduct of animals had long been used by human society as a source of images of disorderly or random phenomena, contrasted to the orderliness of human good conduct (see Chapman, 1993). The application of the term ‘behaviour’ to animals reflected a hope, an aspiration, that animal conduct would turn out to be orderly - would be, that is, rule-governed, like human society (whatever the source of the rules). Other areas of the natural world - physical, chemical, geological and so on - had proved to be orderly, and apt to the scientific investigation of the modern age. Why not animals too? Ardener, however, suggests that by the time the term was applied to animals, it had

already found its new natural science usage - as 'all action', and 'action that is not yet understood'.

Chemical elements, rock formations, animals - they do not talk. Or, if they do, we cannot understand them. Because we cannot communicate with animals through speech, we must, in order to try to understand them, have recourse to other methods - primarily, to observation of their activities, and to attempts to relate these activities to reproductive success (bearing in mind that the entire enterprise of animal behaviour is underwritten by genetic evolutionism, of which more below). The term 'behaviour', naturalised within animal studies, expressed this mode of research - 'behaviour' was 'all action', 'action that was not yet understood'; it was observable, physical, material; it could be seized in objective totality by scientific observation, without need for any medium of communication; and everything an animal did, good or bad, was 'behaviour' (adaptive or maladaptive as it might be).

The language of positivist behaviourism has been borrowed extensively into the social sciences generally, and is everywhere in management and business studies. For most writers it is self-evident and unproblematic; they are looking for a theory, or theories, of 'human behaviour' (or, perhaps, 'consumer behaviour', 'investor behaviour', 'behaviour in organisations', 'behaviour of organisations', and so on). We could even say that the language of positivist behaviourism has become the most common dialect of scientific modernism within human studies.

Those that have attempted to adopt an objective observational stance in the human sphere, have hoped to replicate the success of animal studies. However, both the *need* for this approach, and the *success* of this approach, in animal studies, are not similarly configured when we move into human studies.

The *need* for this approach in animal studies was primarily based on the fact that human beings cannot communicate with animals. In human studies, there is no such need: human beings *can* communicate with other human beings. Why, then, should we deny that such communication exists in the human realm, and limit ourselves to the observation of behaviour? If a herring gull (let's be scientific about this - *larus argentatus*) could give us an extensive and detailed verbal description of why it liked to peck red spots, would we decline this information and limit ourselves to the observation of behaviour? Of course not. By saying or implying that our human studies are 'behavioural' we are, however, making just such a limiting and pointless choice.

The *success* of the objective observational approach in animal studies is, as already noted, underwritten by the survival and inheritance issues configured by Darwinian evolutionary theory and modern genetics. Despite a great deal of effort over a long period, human societies and organisations have not been shown to have any clear analogues of these survival and inheritance features. The historical evolutionism and social Darwinism of the late 19<sup>th</sup> century (see Spencer, 1876-96; Müller, 1856; Maine, 1861; McLennan, 1865;

Tylor, 1871; Morgan, 1877) was made outmoded by the development of field research, and came to seem ethnocentric, naïve, over-ambitious and ill-informed (Chapman, 1993; Jarvie, 1963). The asynchronic functionalism that succeeded it retained an idea of purposive adaptation at its core, but the idea of ‘function’, so powerful at first, dwindled into tautology as more and more examples of function were found (see Malinowski, 1922; Radcliffe-Brown, 1957; Gluckman, 1964). Sociobiology flourished briefly, before it was politically, morally, and to some degree intellectually, occluded (Wilson, 1975; Fox and Tiger, 1971; Kitcher, 1985). Evolutionary economics has nibbled at the edges of the analysis of organisations, without ever assuming centre stage (Nelson and Winter, 1982; Vromen, 1995). Only religious fundamentalists can doubt that human beings, and human social organisation(s), are the product of evolution, but exactly how to tie this insight into the analysis of modern social forms remains controversial, to say the least. Ardener puts the problem nicely, when he says: ‘all human societies existing today may be soberly stated to have survived in spite of the most grotesque misjudgements of events in the past’ (Ardener, 1989, p.126).

The relatively recent emergence of evolutionary psychology offers plenty of interest. The essential premise, that ‘human nature’ exists, and is a product of evolution, will scarcely seem controversial to many. The ‘blank slate’ theorists, of whom Pinker is so justifiably critical, have moral and political objections to the idea, but only the ideologically committed would deny all credence to the underlying insight (Pinker, 2002). It remains to be seen, in the study of management and organisations, whether evolutionary psychology will grow out of being a truism, and become analytically useful. It is near the beginning of its trajectory, and we must wait and see. Some acceptance of the idea of ‘human nature’, backed up by evolutionary psychology, is important to this paper, as will become clear.

The evolutionary and adaptive frameworks, that have been applied to animal behaviour, are therefore neither necessary nor appropriate when applied to human beings and human organisations. They are a useful source of metaphors, but it is not yet clear that they are a good source of science.

It seems to be characteristic of students of the animal world, that they will, sooner or later, wonder whether their methods and conclusions might not also be applicable to humanity. Human use of the animal world as an analogy for the human world, and the human world as an analogy for the animal world, has a history as long as recorded thought (see Chapman 1992b, for the origins in animal metaphor, in the works of Homer and Pindar, of some of our modern would-be analytical vocabulary). Twentieth-century animal behaviourists, of all kinds, were ready prey to the temptations. ‘Behaviour’ had been borrowed into natural science, and into animal studies, and naturalised itself there; then, forgetful of its origin, it began to try to recolonise human studies.

Many other terms have followed a similar route. Animal studies does not, of itself, throw up any native analytical vocabulary; all the terms and ideas are, in the first place,

borrowed from natural human vocabulary and conceptions. We can take a few examples – ‘altruism’, ‘frustration’, ‘domination’, ‘submission’, ‘aggression’, ‘clan’, ‘troop’, ‘family’, ‘sentry behaviour’, ‘territory’ and so on; all of these are deeply socialised, with their origins in human natural language. They were perhaps always subject, in their animal employment, to interference from this social origin. What is most important, however, in this context, is that they, like ‘behaviour’, gradually forgot where they came from. Specialist animal behaviourists grew used, in the animal context, to talking about ‘altruism’, ‘aggression’, ‘sentry behaviour’ and the like. As far as the animal behaviourists were concerned, these phenomena were tied to the animal world, objective and observable. Animal behaviourists of this cast of mind did not necessarily have any great knowledge of organised thought about human activity. When they turned their attention to such activity, however, they discovered that the same phenomena seemed to be present in human behaviour - humans, like animals, had ‘altruism’, ‘aggression’, ‘territorial behaviour’ and the like. For the naive behaviourist, this had all the delight of discovery - humans really were just like animals. Callan, in a study of these problems, has called the excited (re-)discovery of animal traits in humans the ‘Aha! reaction’ (‘Aha! human societies are territorial just like baboons’) (see Callan 1970, p.43). To the animal behaviourists, the rediscovery seemed like a guarantee that the positivist research methods which had worked for animals, would work for humans as well. At root, however, this was the merest tautology - a re-discovery of the original analogies.

‘Behaviour’ itself was perhaps the most pernicious and pervasive of all the products of the ‘Aha! reaction’. It had come from ordinary social use, as an expression of society’s sense of its own orderliness; it was a linguistic category, comprehensible in language, carrying its meaning in language. It imposed itself upon animal studies, as an aspiration that they too would be orderly. Within animal studies, it came to be a figure for studies which proceeded not through language and understanding, but through observation alone; it also came to be a tool for drawing order out of randomness, rather than an already specified idea of orderliness. Given all these things, it is genuinely extraordinary that it should have re-imposed itself so thoroughly upon human studies, where it had become so deeply inappropriate. For understanding of humans proceeds not *only* from observation, but also (and usually in much larger part) from language and understanding, and human orderliness is already contained within human systems of knowledge, thought and action - it does not need to be teased out from random data by observation. And, as always, humanity can communicate its presence and nature through complex multi-dimensional symbolic systems. The idea that ‘observation’ should become the most privileged mode of inquiry is genuinely bizarre.

Most of our understanding of other people derives from a complex sharing of information, through many semiotic media. Language is the most obvious and probably the most important of these, as well as being the one which most clearly challenges the uniquely behavioural approach. The attempt to limit understanding of other people to the ‘observation of behaviour’ is an extraordinary self-limitation, both comical and lunatic. It is rather as if somebody with a sophisticated understanding of music, and a love of it as



well, had decided henceforth to listen to music only from the other side of a wall, to wear ear-plugs while listening, and to limit their experience of music to studying the amount of dust raised out of the carpet by the vibrations coming from the next room; and arguing, in justification, that this would allow a truly objective, empirical measure, uncontaminated by subjective, aesthetic or emotional considerations.

There is a tendency, in the literature, to speak of the pair 'language and behaviour' as if this exhausted the range of human activity: behaviour is the objective action, while language encodes this reality in communicable form. Against this, I wish to argue that 'behaviour' itself is already highly encoded, and a medium of communication in its own multifaceted way. I should stress that the novelty of this position is in the context of its argument, within behavioural positivism in the management and business field. Outside this field, the position is well-established, and my own presentation draws from many sources, variously cited throughout this piece.

We can take a very simple example. Suppose we take the example of somebody walking. We might like to say that we could specify this as an objective unit of behaviour - a particular kind of bipedal locomotion, at a particular pace, sustained unidirectionally for a specified period. Let it be all those things. We can take a sample of people moving in such a way that we are able to observe them according to the behavioural unit specified.

What, however, were these people doing? If we say 'walking', or indeed 'exhibiting walking behaviour' we have told ourselves nothing of any use for social analysis; and also, of course, nothing that was not contained in our original specification of the activity. So what is the person walking really doing (let the person be a man)?

He is a managing director walking into the boardroom

He is a managing director wearing a suit and tie walking into the boardroom

He is a managing director wearing a suit and tie walking into the boardroom at 4 p.m.

He is a managing director walking naked into the boardroom at 4 p.m.

He is a managing director walking naked into the boardroom at 4 a.m.

He is a managing director walking naked along a nudist beach near St Tropez

He is a managing director in a suit and tie walking along a nudist beach near St Tropez

We have kept constant a minimal behavioural unit – walking (same pace, unidirectional), and embellished it a little. It is clear that only limitations of space and imagination put any restrictions to the embellishments that might be possible. It is also clear, however,

that to make sense of the 'walking' (the behaviour, that is), we can not do without the context, the social and symbolic content. We might want to say, from a purely objective and observational point of view, that the walk of the naked managing director is the same in all three instances in which it is mentioned – on the nudist beach, in the boardroom at 4 p.m., and in the boardroom at 4 a.m. In the first instance, however, he is getting himself a tan and doing something quite ordinary; in the second he is perhaps committing, for whatever reason, a grotesque error, which may lose him his job, and haunt him for the rest of his life; and in the third, well...

The behaviourist might say, in this case, that the 'behaviour' was objectively the same in all three instances, but that something else had changed: perhaps he might say that 'the context had changed', which gave the behaviour 'a different meaning'; or perhaps he might say that 'the behaviour was perceived in different ways in the different environments'. These two rescue formulas mean more or less the same thing.

So the behaviourist might find himself arguing that the object of observation should not be behaviour alone, but an interaction of behaviour and environment. This only pushes the same problem a step further into the material, however, since we can do to the 'environment' or the 'context' what we have already done to the 'behaviour'. We can take the boardroom (although the beach would do just as well). If the MD is walking naked at 4 p.m. into the boardroom when it is full of expectant fully dressed directors, waiting for the meeting to begin, then something has gone seriously wrong: maybe a very strong political statement is being made; maybe the MD has gone mad; maybe the MD has found a much better source of money, and wishes to show amused contempt for the conventional morals of his soon-to-be-erstwhile colleagues. If the MD is walking naked at 4 a.m. into an empty boardroom, however, then something very different may be happening: perhaps a lovers' tryst, or a short cut to the loo? Again, there are no limits to our potential creativity in the area.

So, O.K., maybe the behaviourist says 'we have to allow for the behaviour of other people, and to allow for a time dimension, and to allow for the possibility of different interpretations of the same environment in relation to other contextual variables like time and people'. Can't you just hear it? In fact, if you want to hear it, read the following aloud:

While they are beyond the scope of this study, more detailed future explorations of such contextual considerations and the effects of status incongruence would certainly enhance scholars' understanding of impression management. This finding also suggests that demographic category salience may shift from context to context depending on which dimensions of an individual's overall demographic profile are triggered. Future research should examine the effects of the organizational context on demographic category salience and the implications of this salience for the effects of demographic dissimilarity on impression management. (Barsness, Diekmann, and Seidel, 2005, p.415)

Whatever we are looking at, we can pursue the same problem further and further into the research material, through different people, times, places, and combinations and interpretations of these. The behavioural observational research programme, chasing vainly along in pursuit, rapidly becomes impossibly complicated. In order to specify and observe all the relevant parameters in any situation, covering all combinations and eventualities, the behavioural programme would (it is no exaggeration to say) develop a set of observations that were more complicated than social life itself, and immeasurably more complicated to interpret. This would be an interesting result, of course, if it were possible to achieve; it is not what was intended, however, and it is not possible to achieve.

I make the statement at the end of the last paragraph unequivocal. Total 'behavioural' knowledge, of the kind that would ultimately be required for the analysis of social life of even only modest complication, is not available. Its gathering would require social activity on a scale greater than the social activity which it was trying to record, and would, in short, be in itself a new kind of social activity. As Ardener says, of a related kind of empirical imperialism:

This Faustian aim is beyond the competence of any single model; with the computer engineers, we should remember that ultimately the only effective store of the natural order is the natural order (Ardener, 1989:41).

We could extend this, to say 'the only effective store of the social order is the social order'. There is no positivistic alternative to the store of information that the social order is. Perhaps we can put the problem this way: in the case of our managing director, in the context and time of his actions, understanding was built into the events - into their context, timing, personnel, 'behaviour' and all the rest. This was not behaviour in an environment, in the animal sense. It was, rather, a performance in which the environment, the timing, the personnel, the material embellishments, were already highly pre-structured by the actors - in which these things were, indeed, already part of the performance.

How did we know the room was a boardroom, as we looked at it? It may, after all, have been only a part-time boardroom, serving at other times as dining room, reception room, interview room, cloakroom and so on. We knew that it was a boardroom at that time, because the directors were getting together to have a meeting of the board: the decision as to the definition of the room was in their hands. They also knew what was appropriate dress for a meeting of the board, and how it was appropriate to conduct oneself (how to 'behave', in the vernacular sense). The Managing Director shared this knowledge; he could hardly have become managing director if he did not. His appearance naked, then, was a very flagrant message, and one of disruptive intent. He knew that (unless he was unbalanced of mind), and the other directors knew it too. Our own interpretations, as

observers of these events, depend not upon objective judgement, but upon the fact that we share, with the actors, an understanding of what is going on - we too know the difference between a mediterranean beach at noon and a boardroom in England at four o'clock in the afternoon; we too know what a boardroom ought to look like, more or less; we too know how a managing director ought to behave, within reason; we too have a very sophisticated understanding of the appropriate intersections of people, places, actions, words, time, objects and events, that go to make up the regularities of our social world.

It is, therefore, strangely mendacious for us to pretend that we understand what is going on because we have been observing 'behaviour', as if we had been observing rats in a maze. I suggest that we do so because of misguided scientific piety, the remnants of the power of the behavioural research programme as it was first articulated.

As observers and interpreters of human activity, we make use of our knowledge of that activity, derived from whatever source. We have seen above that for some linguists, 'meaning' became a "dirty word" – 'but if the name tends to be avoided, there is no doubt that every linguist employs the concept' (Allen, 1957, p.22). We could say something rather similar for objective behavioural scientists – they employ the riches of their knowledge and understanding, while implying that they are not doing so.

In her 2003 Presidential Address to the Academy of Management (Pearce, 2004), under the title 'What do we know and how do we really know it?', Pearce argues that, as academics concerned with business, we inhabit two worlds - one, the scholarly world of ideas and research, and two, the world of 'folk wisdom about management and organisations'. These two worlds she calls 'parallel', in the sense that they run side by side but do not meet. She regards the world of 'folk wisdom' as one that is 'not as openly acknowledged', 'not as well understood', and 'underappreciated'. She then qualifies this by saying that she can only speak with certainty for herself. For herself, however, she describes the experience of being interim dean of her management school, which she describes as 'a real managerial job'. She elaborates:

So I asked myself: Was anything I learned studying, teaching, and thinking about management and organisations all these years useful? Yes, sure, some of it was. Am I a better manager because of those years of study? Yes, I think so. Although I have made many mistakes, I probably would have made more had I been studying molecular biology instead.

But as I think about what was useful, and which mistakes I was able to avoid, I became aware that very little of this useful knowledge about my most important challenges came from our scholarly world. Rather, the really useful insights, the knowledge that helped with the tough problems, came from what I am calling our world of shared folk wisdom about management and organisations.

The more I thought about it, the more I came to believe that we do not sufficiently recognise that we are steeped in a rich wisdom that is often useful in addressing the really tough organisational problems individuals face. Unfortunately, I fear that very little of that wisdom is based on our claims to be social scientists. It comes from a different world – a world we have not examined as thoroughly as we have our scholarly world. (Pearce, J., 2004, pp.175-6)

Pearce's argument is pregnant with interest. We can perhaps push the argument, for present purposes, a little further.

Try this:

There are four major sources for our understanding of people and organisations:

- 1) Common human nature, or intuitive psychology.
- 2) Shared culture-specific semiotic systems.
- 3) Experience.
- 4) Academic research about people and organisations.

Pearce's argument, with which I agree in most respects, seems to suggest that the great bulk of our useful knowledge about people and organisations comes from the first three of these, and that only a small fraction comes from the fourth – from 'academic research about people and organisations'.

Of course there are many different kinds of academic research about people and organisations. We have been exploring a particular possibility, however – that of objective observational behaviourism. We have seen that such an approach, in its original and militant form, eschewed all knowledge arising from the first three sources; indeed, regarded such knowledge as 'subjective', 'non-scientific'. So objective observational behaviourism was committed to the view that scientific knowledge would derive from 'academic research about people and organisations'. If Pearce is right, however, and if my interpretation of Pearce is admissible, then objective observational behaviourism was looking in the wrong place in the wrong way. The behaviourists brought to their studies a great body of knowledge about people and organisations, derived from the first three sources – common human nature, shared culture, and experience – and then put themselves into the position of trying to pretend that they did not have these.

Let me reiterate that this paper is written by a social anthropologist, who began about twenty years ago to be exposed to the literature of management research. The first and abiding impression of the management research literature, to the social anthropologist, was that it seemed to be steeped in tautology, to the point of comedy.

This tendency to tautology makes sense, however, if what the (would-be) behavioural scientists were discovering (academic research about people and organisations), was something that they already knew (common human nature, shared culture, and experience). We might expect the hypotheses of positivist research in this area to be phrased in terms of abstractions, derived from sources 1, 2, and 3, and expressed in a language appropriate to source 4 – these abstractions would then be ‘discovered’ in the empirical data. Source 4 would find that it had achieved success, and would continue as before, continuing to deny sources 1, 2 and 3 as the real inspiration for insight.

These are harsh criticisms. Let’s have a few examples (I should stress that what I am criticising here is a general tendency within social psychologically inspired management research. The particular examples and articles that I choose to illustrate the problem are there to serve the general point, and I do not intend my criticism to be taken as directed at the individuals who produced the examples and articles which I cite).

A favourite, and one which first gave me a handle on the problem, comes from an article from the *Journal of International Business Studies*, entitled ‘When in Rome? The effects of cultural adaptation on intercultural business negotiations’ (Francis, 1991). Francis says ‘being liked appears to induce positive reactions’, and this is exactly the kind of tautology to which I am referring. It has a natural language element – ‘being liked’. It has a behavioural science paraphrase of this – ‘positive reactions’. And it has the implied separation of these, and a causal link between them – ‘appears to induce’. The causal link is masquerading as a behavioural scientific discovery, when it no more than a connector between two expressions of the same thing.

A second illustration can be taken from Shaw (1990), ‘A cognitive categorization model for the study of intercultural management’. In this, the reified abstractions on display are ‘scripts’ and ‘schemas’; we are also in the realm of Hofstede’s ‘dimensions’. Shaw says:

Hypothesis 3: the magnitude of differences in schema prototypes will be positively correlated with the extent to which the cultures represent different levels along cultural dimensions (Shaw, J. 1990:635).

Would it be unfair to translate that as ‘the more different they are, the more different they are’? Or:

Hypothesis 8: when examining the cognitive structures among individuals from very collective societies, there will be a high similarity in schema structure across individuals. For individuals from highly individualistic societies, there will be a greater variability among people in their individual schema structures (Shaw, J. 1990:639)

Would it be unfair to translate that as ‘the more similar they are, the more similar they are, and the more different they are, the more different they are’?

Let us move on to 2005. Here we have a proposition from ‘Power dynamics in negotiation’ (Kim, Pinkley and Fragale, 2005):

Proposition 2: A negotiator’s potential power will positively influence each party’s perception of that negotiator’s potential power in the relationship (Kim, Pinkley and Fragale, 2005, p.807).

Not much risk of being wrong about that, perhaps.

Ardener said:

It is surprising that many intellectual persons of a scientific bent, whose own minds and lives are a continuum of nuances and subtleties, are often easily satisfied with ideas of social systems made up of simple causal chains, which would not safely guide them through ten minutes of real life (Ardener, E. 1989:126).

I am inclined to regard some, at least, of the cross-cultural work in business studies as ‘would-be scientific’. I am not, therefore (I hope), being anti-scientific. It is rather that the scientific pretensions involved seem to be a genuine obstacle to thought - an appearance rather than a substance of science. I have cited Edwards above, in relation to the social psychological ambition of producing ‘a 1:1 map which reproduces social reality’. It will perhaps be clear that I do not think any such map is logically possible. Positivist studies of the kind that I have discussed tend to judge themselves, in a concluding paragraph, as having contributed one small brick to the unitary edifice of total knowledge; they tend, also, to demand ‘further research’ in the same direction, in order that the building can be completed. I do not think that this is a proper assessment of their achievement; not do I think that their research programme is leading to this end.

J. Edwards (himself a social psychologist) has said of the work of Giles and Tajfel:

An important question is whether or not the theories advance our understanding of the processes discussed or simply restate or formalise, from a particular perspective, existing knowledge. The answer, as is usually the case with social psychology, is the latter (Edwards, J. 1985:155)

If this is so (and I agree with Edwards here), what are we to make of the ‘hypotheses’ which are so commonly found in such work. Edwards goes on:

The danger (again endemic to much of social psychology) is that theoretical perspectives may become prematurely solidified, with a great deal of internal

coherence at the expense of on-the-ground explanatory value. Things are often made neater than they really are. [...] It sometimes seems as if the final, if unattainable, goal of social psychology is destined to be some 1:1 map which essentially reproduces social reality. This is clearly an undesirable state of affairs which itself prompts the often premature, altogether too simplistic, but eminently understandable drive to 'theory' (Edwards, J. 1985:155-6)

### **Emic and Etic**

I have noted a strong agreement with Pearce's argument (see 2003) that a great deal of our knowledge is based upon sources, like experience and culturally specific semiotic systems, which we do not fully acknowledge or admit or recognise in our academic work. The social psychological behaviourist dream has been objective, observational, scientific, and all the rest. The reality, as demonstrated in this paper has perhaps at least suggested, is that a great deal of our knowledge is not derived from scientific observation of this kind. We have also, however, tried to demonstrate the means through which a pretence of scientific objectivity is maintained. The pretence is often empty and shallow, it produces few useful results, but it is an enduringly successful pretence nonetheless.

The 'emic/etic' distinction is bound up with the problems discussed above. The distinction is frequently discussed as though it were synonymous with the 'culture specific / human universal distinction'. As such, we could parcel out the study of the 'emic' and the 'etic', such that one brought cultural knowledge to the study of the 'emic', but was a detached scientist in the study of the 'etic'. If, however, we acknowledge that most of our knowledge is *not* derived from our would-be scientific stance, then the study of 'emic' phenomena as properly understood dominates our science, and until we acknowledge this we will continue to misrepresent ourselves to ourselves.

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