

Methodological Developments in the Social Sciences: Some Implications for Interdisciplinary Study¹

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Methodological developments, as indicated by recent scholarly publications, in management theory, psychology, and social anthropology are analysed from a falsificationist perspective (Popper, 1968). It is argued that methodological developments in these disciplines were, and still are, out of phase with one another, and that this has resulted in wasteful repetition, and, in some instances, unnecessary methodological retardation. Some potentially damaging social implications of these developments are briefly discussed. Finally, a form of methodological eclecticism is proposed as a means of overcoming some of the problems identified, and as an approach of particular relevance to members of interdisciplinary sciences such as organizational behaviour.

The empirical basis of objective science has . . . nothing 'absolute' about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or 'given' base; and if we stop driving the piles deeper it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being. (Popper, 1968, p. 111)

In this, one of his better known metaphors, Popper succinctly describes the conditional nature of science, and his opposition to the naive inductivist idea that hypotheses and theories emerge from, and rest upon, a solid base of facts. Popper's solution to the problem of induction is considered by many to constitute his single most significant contribution to the philosophy of science although his views have not gone unchallenged (e.g., Grunbaum, 1976, 1978; Haack, 1976; Levinson, 1974). It is largely through the efforts of Popper and his students that many psychologists and sociologists by now have at least a nodding acquaintance with the frailties of science's empirical basis, and the logical fallacies of induction and verification (e.g., Popper, 1968). In fact, some psychologists have been concerned for some time with investigating ways in which Popper's epistemology and its more recent derivatives might be applied in their own

discipline (e.g., Meehl, 1967, 1978). However, the same sorts of awarenesses appear not to be widespread either among anthropologists or among the members of other disciplines such as management theory. Indeed, some of the most striking discoveries to be made in trying to come to terms with interdisciplinary study concern the wide discrepancies that exist *between* disciplines with respect to methodological sophistication, and the relative pace at which methodological developments have taken place *within* disciplines (in the present discussion "methodology" refers to both specific techniques of data collection and analysis, and the general philosophical principles underlying scientific research).

Although there are some notable examples of interdisciplinary debate on methodological issues (e.g., Borger & Cioffi, 1970), and there are several journals devoted to the philosophy of the social sciences, in many instances it seems still that social science disciplines have failed to benefit from each other's insights. Accordingly, the purpose of this paper is to provide a number

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of (admittedly isolated) examples, *within a functionalist framework* (see Burrell & Morgan, 1979), which show that there is insufficient methodological cross-fertilization between social science disciplines; and that this has stunted development in some instances, and led to the needless repetition, in different disciplines and at different times, of what amount to the same learning cycles. Part of what is being said here is that interdisciplinary sciences such as organizational behaviour (Pugh, 1969), which obviously stand to benefit from the factual inputs of other disciplines, could benefit in other respects, and in any case should not be seen as the sole beneficiaries. The older more established, "donor", disciplines could also benefit, particularly, it seems, methodologically. Finally, a major implication of the present analysis is that social science in general and in particular interdisciplinary sciences such as organizational behaviour have much to gain from Popperian critical rationalism², and that this method belongs "not to the *hors d'oeuvre*, but to the main dish" (Agassi, 1975) of research endeavours.

Owing to the limitations of space, in the following sections only two levels of methodological analysis are considered. Examples are provided which illustrate the kinds of methodological discrepancies which exist between some of the disciplines concerned with the study of organizations: in particular, social anthropology, psychology, and management theory. A final point which needs to be made at this juncture concerns the nature of the evidence employed in the present study. Little attempt is made to substantiate the arguments

presented below by marshalling evidence from empirical studies in the disciplines discussed. The major reason for this is that it is clearly safer to rely on explicit methodological statements by researchers than inferences drawn from examples of empirical research. A second reason is that this paper is concerned with the elaboration of an hypothesis and a partial test of it. There is not the room for the thorough going analysis and test such an hypothesis obviously requires.

Epistemology

Psychology. It is one of the ironies of social science research, and especially empirical research, that epistemology is so rarely acknowledged as worthy of careful consideration, yet much fuss is made of details such as the average age of subjects, their sex and socio-economic status, and whether or not t-tests achieve statistical significance. The dangers of ignoring epistemology and concentrating solely on, for instance, statistical significance have been pointed out a number of times (e.g., Lykken, 1968; Meehl, 1967). And there are other examples which illustrate the ways in which neglecting epistemological questions can be wasteful and act to the detriment of scientific development (e.g., Blunt, 1977). However, some social scientists are beginning to realize that epistemology is basic to the development of their fields. For example, there is a small but growing number of psychologists who are actively involved in analysing epistemological questions (e.g., Bethlehem, 1980; Farrell, 1978; Furedy, 1978; Heller, 1976), and although the importance of these and associated matters is still not acknowledged or reflected sufficiently widely (Maher, 1978), psychology's state of health, scientifically speaking, is bound to improve as a result of this type of analysis.

Popper's influence has been considerable in this regard, and his criteria (the central one being the notion of falsifiability) for the demarcation of science are among the most widely received in the social sciences (e.g., Borger & Cioffi, 1970). In particular, increasing numbers of psychologists and sociologists refer with approval to Popper's falsificationist construal of the hypothetico-

² Briefly, the term critical rationalism captures Popper's views on the importance in science, and generally, of openness to criticism or "readiness to be criticized, and eagerness to criticize oneself" (Popper, 1976b, p.115). For criticism to be effective, problems must be stated clearly, and solutions expressed in a definitive form which allows them in turn to be critically discussed. In science, therefore, theories must be criticizable or testable, that is, logically coherent and empirically falsifiable. For a clear and concise introductory account of Popperian falsification (in its various forms), neo-Popperian thinking (Lakatos), Kuhnian paradigms, the problem of induction, and one or two other points, see Chalmers (1976).

deductive method (e.g., Furedy, 1978; Guntrip, 1972; Meehl, 1967), and more and more attempts are being made to analyse social science research in terms of either the Popperian or neo-Popperian (e.g., Lakatos, 1970; Meehl, 1978) proposals. To some degree this has been achieved by Meehl who is noted for his methodological contributions to psychology. In one of his more recent articles, Meehl (1978) has addressed, in a preliminary way, the broad question of the conjoining of theories and auxiliary theories in psychology (following, among others, Lakatos, 1970, 1974). This might be considered to be a part of the vanguard of epistemological development in psychology, and it is worth considering in some detail. Doing this will enable rough comparisons to be made in subsequent parts of this discussion, between this level of debate and the levels achieved in other disciplines.

Following Meehl, the crux of the "auxiliary" problem is that it is rarely possible in the social sciences, and in many instances other sciences, to test a theory, T, directly through observation. This is because the detailed description of the logical structure of the observational test necessarily entails the intervention of auxiliary theoretical statements, A, plus the stipulation of experimental conditions, C, between, as it were, the theory T and the observation O. Thus the formerly relatively simple conjunction T-O becomes (T.A.C.)-O. That is, the falsifying observations do not impinge directly on the theory T but imply instead the much more complicated, not (T.A.C.). This means that instead of falsifying the theory T alone, one or more of T.A.C. may have been falsified, and more likely still the conjunction of (T.A.C.). The situation described is a particularly difficult one for social scientists to deal with because the characteristics of the auxiliaries A and the conditions C are frequently as problematic as those of the theory T itself. For instance, many auxiliary theories in soft psychology involve the validation of psychometric tests, and the connections between such tests and the substantive theory are frequently neither completely validated nor straight forward, as Meehl has shown (1978, pp. 819-820). In the social sciences in general, weak linkages

of this sort are commonplace and it means that, strictly speaking, auxiliary theories need to be independently validated first. Unfortunately, however, the validation of A above would, in most instances, say, in soft psychology, involve additional auxiliaries, A₁, and additional conditions, C₁, with the same problems which afflicted A above likely to attend A₁ and C₁, and so on, almost inevitably, into an infinite regress. Although Meehl (1978) does not arrive at the same conclusion, he does say: "Almost nothing we know or conjecture about the substantive theory helps us to any appreciable degree in firming up our reliance on the auxiliary" (p. 819).

One way of circumventing this problem is to treat the conjunction (T.A.C.) as the "theory" under test. The snag with this idea, though, is that falsification of (T.A.C.) makes it difficult to apportion responsibility for failure precisely among the component parts. Accordingly, the soft psychologist is put in the position of having to say that "If the empirical research does not pan out as predicted, one does not abandon T; instead . . . either T is incorrect, A is incorrect, or the diagnoses were untrustworthy" (Meehl, 1978, p. 821). This leaves soft psychologists (and other social scientists who face similar difficulties) in the unhappy position of having to say in effect that one will be pleased with T if (T.A.C.) is corroborated, but will not necessarily be displeased with it if (T.A.C.) is falsified. But this is not as irrational a stand as it might appear at first sight. In simple terms, because, in the social sciences, of the lack of clear and strong connections between the components T.A.C., it would be very unlikely indeed that a theory T would have low verisimilitude (nearness to the truth, Popper, 1976a) and still be able to generate, say, accurate point predictions through the conjunction (T.A.C.). If, therefore, corroborations of point predictions are made through such high risk conjunctions it is reasonable to infer that the theory T has been corroborated, even though, in effect, one backs the theory both ways by saying that one would not necessarily discard it in the face of falsifying evidence. It is worth noting that this position conflicts with neither Popper (1974) nor

Lakatos (1970), providing there are no obviously superior competing theories about. Lakatos (1970) has argued that the core of theory — T in the above example — should be protected from falsification by auxiliaries such as A, and perhaps (A.C.), above. In his view, science should comprise research programmes which proceed in a way which allows them to deal as they go along with anomalies and potential falsifiers which could impinge on the substantive theory. Such theories or research programmes can be overthrown, objectively, only “by a rival research programme which explains the previous success of its rival and supersedes it by a further display of heuristic power [the power of a research programme to anticipate theoretically novel facts in its growth]” (Lakatos, 1970, p. 155). Without meaning to imply anything but the slightest symmetry between the views of Popper and Lakatos, probably less well known is the fact that Popper has made it clear that he is not an advocate of the conflation of refutation with rejection, as the following passage shows.

It is a typical matter of conjecture and of risk-taking whether or not we accept a refutation and, furthermore, of whether we ‘abandon’ a theory or, say, only modify it, or even stick to it, and try to find some alternative, and methodologically acceptable, way round the problem involved (Popper, 1974, p. 1009).

At the very least, the analysis described above illustrates that although epistemological debate in psychology is still not as pervasive or as thorough as one might wish, there are strands which are well advanced.

Social anthropology. In this discipline, the few nodes of epistemological debate that there are seem to be directed at problems of a more basic nature. There are at least two fairly recent examples of this in the literature (Jarvie, 1967; Vermeulen & de Ruijter, 1975).

Jarvie’s (1967) paper was concerned with “Theories of Fieldwork and the Scientific Character of Social Anthropology”. He began with the following statement: “Theories of fieldwork explain why anthropologists do fieldwork. They are theories of method, since fieldwork is a method of doing

anthropology” (Jarvie, 1967, p. 223). The topic of his epistemological discussion, then, may be summarised as, “what is the importance of fieldwork to the scientific development of anthropology?” According to Jarvie, among anthropologists conventional wisdom dictates the following reasons — which he sought to combat — for carrying out fieldwork. In essence:

- (1) Fieldwork should be conducted to collect facts about moribund societies.
- (2) Fieldwork helps to correct ethnocentric bias, “the unarticulated assumption that the outlook and way of life of your own culture is ‘normal’ and every other pattern is deviant” (Jarvie, 1967, p. 226).
- (3) Fieldwork is the only means of correctly interpreting strange customs and beliefs because it allows them to be perceived in context.
- (4) Fieldwork enables the collection of background information (facts) relating to research.
- (5) “Fieldwork is the only way one can get to think like the member of another society” (Jarvie, 1967, p. 229).
- (6) Conducting fieldwork allows one to perceive the difficulties of cross-cultural communication, that is, of conducting fieldwork in an alien culture.
- (7) Fieldwork teaches one the proper scientific attitude.

An obvious feature of the reasons anthropologists give for conducting fieldwork is the emphasis placed on the collection of facts as an important exercise in itself (rather like butterfly collecting). Another implicit feature is that fieldwork provides an important source of theory (Jarvie, 1967, p. 224). It seems almost trite these days to say that this view is mistaken and that how one dreams up theories, whether it be from the security and comfort of one’s own study or the wilds of Borneo, matters not in the slightest. Jarvie (1967) has aptly described the inductivist approach — which in important respects parallels that described above — as one in which “facts are conceived of as meat, induction as a sausage machine, and science as the product of pushing the one through the other. No conjectures, no imagination, no web of guesses in this conception of science —

which, incidentally, is far more widespread [among anthropologists] than the frequent disavowals of it suggest" (Jarvie, 1967, p. 232).

An additional rationale for conducting fieldwork is that it is considered possible, and desirable, to do away with individual theoretical frameworks or, more plainly, interpretation. As we have seen, for many anthropologists fieldwork is the means for objectifying perception; for putting the researcher into other people's shoes, and also for allowing him to step out of his own to observe them, "objectively" of course, from a distance. The counter to this view is that it is impossible to make observations which are not theory-laden. Indeed, much of what has been said here would seem uncontroversial were it not "put against the standard view that we should approach the study of alien societies without prejudice and preconception" (Jarvie, 1970, p. 231).

The epistemological remedy prescribed by Jarvie for social anthropology has much in common with the stance adopted in the present paper; it constitutes a straightforward account of the Popperian viewpoint.

The theory of fieldwork I propose states that: (a) anthropology is the study of problems, which can be discovered by fieldwork or any other method; (b) the job of the anthropologist is to put forward explanatory theories which solve these problems; and (c) to criticize and test these theories as severely as he can by fieldwork or any other means. This theory of fieldwork assigns it the primary roles of providing problems and testing theories (Jarvie, 1967, p. 235).

Vermeulen and de Ruijter (1975) made very much the same criticisms of social anthropology as Jarvie (1967) did about ten years earlier. Basically, they argued that most research in social anthropology was founded on induction along with the empiricist idea that it is possible to distinguish phenomenal language — that is, theory-free language — from theoretical language. The case put forward by Vermeulen and de Ruijter is well supported by recent examples from the literature, and there is no need to repeat their arguments

here. Their most important conclusions may be summarised best in their own words:

The cross-cultural survey method is characterized by what — depending upon the context — may be called a positivist, empiricist, or inductivist epistemology . . . The nature of the cross-cultural method and especially the accompanying epistemological presuppositions of the proponents of this method have resulted in research which has hampered the development of theory. The idea that science consists in the collection of facts and their categorization and mutual association has resulted in a bag full of rules of restricted theoretical interest. The belief that such generalizations are verified or 'true' statements rather than provisional conjectures has led to an uncritical attitude towards the results of comparative research (Vermeulen & de Ruijter, 1975, p. 36).

As is customary with the journal of *Current Anthropology*, major articles are followed by a series of comments and criticisms provided by leading international scholars. The comments made on the paper by Vermeulen and de Ruijter are, in many respects, as interesting and revealing as the article itself. The criticisms raised were limited to a few hundred words, so, no doubt, corners were cut and statements made more plainly than they might otherwise have been. There was a wide variety of response, ranging from dogmatic refusal to accept the fallacies of induction — or even to argue its case, as if the criticisms had made no contact whatever — through apparent bafflement, to guarded, and in a few cases complete, agreement. Driver and Lane were two of the most unrepentant and unyielding inductivists: "The main thrust of Vermeulen and de Ruijter's argument is that it is better to have a series of broad and woolly propositions and theories conjured up intuitively and unverified by extensive comparative research than to have the more explicit and particularistic results of cross-cultural research. I disagree" (Driver, 1975, 38–39). Lane also reacted strongly: "Cross-cultural method has its strengths and its weaknesses, its appropriate and inappropriate uses, and it can be used well or

poorly. The conceit of those anthropologists who believe that only the paths of their choice lead to salvation and that those of others lead to damnation probably contribute more to sterility in anthropological theory than the practice of any particular method" (1975, p. 41). A final illustrative critical comment is derived from Otterbein's contribution: "Vermeulen and de Ruijter err when they argue that the cross-cultural survey method is inductive rather than deductive. The method is, and should be, both" (1975, p. 43). There were a number of positive responses to the paper, notably those of Rosenblatt, and Rossi. The latter agreed with the importance of epistemological discussion in general but questioned the applicability, to social science, of Popper's criteria for scientific endeavour and implied that neo-Popperian criteria (e.g., Lakatos, 1970) might be worthy of consideration. Most importantly, however, he acknowledged that epistemological debate was essential, and hoped "that essays of this kind will sensitize the anthropological audience to issues which up to now have only marginally attracted its interest" (Rossi, 1975, p. 45).

However, evidence to show that induction is alive and well continues to appear in the literature. For example, Rodin, Michaelson, and Britan (1978) have advocated recently that a particular theory must satisfy the following "scientific and intellectual standards of adequacy: It must be founded on empirical data — either the qualitative observations of a trained ethnographer or the quantitative data of census statistics or social surveys" (p. 749).

All this brings to mind another paper by Jarvie (1970) in which he discussed the idea that "in saying the beliefs of primitives do not accord with objective reality we are talking in terms of reality seemingly conceived of outside language and culture" (p. 233). He disagreed with this view, and employed the example of Azande magic to illustrate many of his arguments. The point to be made in relation to the present discussion is that problems of interpretation — as the above discussion illustrates — are not peculiar to so-called primitives who believe in magic. It may be a hoary drum to beat, but questions of interpretation are

just as pronounced in social science itself. It certainly seems that different forms of rationality may be at work in anthropology, and perhaps other social science disciplines. And just as Azande who have been educated abroad return to their homelands with shaken beliefs in magic so too might social scientists, taken early in their careers, avoid some of the epistemological problems described here were they educated more widely and more thoroughly in these matters.

Management theory. There is also a dearth of epistemological debate in management theory, which, in any case, would not be regarded by some as having scientific status (e.g., Drucker, 1954). There are, however, a few examples of epistemological discussion in the literature (e.g., Greenwood, 1974; Gribbins & Hunt, 1978; Gulick, 1965). The article by Gribbins and Hunt (1978), which is illustrative of the level of debate in management theory, sought, in five pages, to unravel the question, "is management a science?" They concluded that management is a science.

The notion of science propounded by Gribbins and Hunt contains views derived from various sources, among them, Braithwaite (1955), Buzzell (1963) — who wrote on the scientific status of marketing! — Kerlinger (1964), and Rudner (1966). On the other hand, Popper and Kuhn, perhaps the two most outstanding epistemologists of our time, were only referred to in passing. The picture of science which resulted was an unclear one, but one in which, nevertheless, it was possible to discern some sympathy for the importance of theory testing (inadequately explained though it was) and the significance of intersubjective testing. However, in their eagerness to embrace as much as possible, the authors contrived to introduce with approval the notions of induction and theory free observation as well. They did this by referring to Berelson and Steiner (1964), and reproducing their view of science along with its insistence on "precise definitions" and "objective data-collecting".

Precisely the same approach was employed with the other end of the argument, that is, the management theory end. Instead of analysing in detail one strand of development in management theory and relating

this to a single view of science, the authors chose, again, a broad and less cohesive perspective. By doing this, they managed to avoid problems such as having to confront individual theories with, say, their susceptibility to refutation, a test which few would have survived. In this respect, theories in management are very similar to those in soft psychology and, indeed, some have been derived directly from soft psychology. One does not have to look very far for examples. Take, for instance, one of the most popular theories of motivation in management, Maslow's need hierarchy theory. There appears to be no valid and reliable way of measuring the need categories described in this theory (see Blunt, 1977; Blunt & Denton, 1979), let alone a means of testing the theory itself.

But this is not a question which will be addressed here. The scientific status of management theory has yet to be subjected to detailed analysis, although there has recently been some well informed debate on the status and methods of the closely allied disciplines of organizational theory and organizational behaviour (e.g., Behling, 1980; Morgan & Smircich, 1980). The likelihood is, however, that, rather like soft psychology, it is not at its present stage of development, and judged by orthodox scientific (say, Popperian) criteria, a science. This, of course, does not mean to say that it could not achieve scientific status at some time in the future.

Concluding remarks. So far in this discussion, it has been shown that a single, though fairly broad, epistemological perspective — in this case, various related though by no means unitary interpretations of Popperian critical rationalism (Meehl and Jarvie) — when made to include the epistemological experiences of a number of

social science disciplines highlights different critical development problems. So, the studies referred to above serve not only to illustrate the degree to which epistemological debate has progressed in psychology, social anthropology, and management theory, but they also typify the *kind* of epistemological debate which takes place within each discipline.

Among psychologists, for example, there seems to be a greater concern with the status of *existing* theory, with questions pertaining to matters such as testability, and logical consistency (e.g., Cioffi, 1970; Farrell, 1978; Meehl, 1967, 1978). Anthropologists, on the other hand, appear still to be preoccupied with data collection and theory construction (Jarvie, 1967, 1970; Vermeulen & de Ruijter, 1975). No doubt these different points of focus are due partly to the greater theoretical richness of psychology and its longer history as a semi-scientific discipline. But this is not the main point. More important is the fact that, like anthropology, the futures of psychology and management theory lie in the development of new theories which are more testable, and the refinement of existing theories in the same direction. When this activity becomes more pronounced among psychologists and management theorists, it may well turn out that induction, as it is in anthropology, is a guiding force in these disciplines too, as the directional hypothesis testing tradition and "nose counting"³ (see Blunt, 1980a, p. 47; Meehl, 1967, p. 112) suggest it might be. Moreover, one might find, if one were to scratch the surface hard enough, that the assumptions underlying fieldwork in soft psychology are much the same as those of anthropology. It is just that psychologists have not had explicitly to articulate these questions recently. But the relatively new interest in epistemology is bound to produce theory development, and it might behove psychologists to consider carefully the points made by, for example, Jarvie (1967) and Vermeulen and de Ruijter (1975) in relation to the problems of induction.

It need hardly be said that the complexities of theory testing, as discussed by Meehl (1978) and others, should be considered at the same time as those surrounding theory construction. In fact the two processes are

³ "Nose counting" refers to the common practice of assessing a theory's worth by simply counting up the number of supportive and unsupportive studies. If there are found to be more of the former than the latter, the theory in question is often taken to have survived scientific scrutiny. Falsificationists would argue that this view is mistaken and that on the contrary a small number of falsifying studies do a theory much more harm than a larger number of supportive studies do it good.

inseparable, and it is disciplinary peculiarities more than anything else which have brought about the *artificial* distinctions mentioned. This fairly obvious fact needs wider recognition among social scientists so that equal emphasis may be given to both activities.

Developmental characteristics similar to those described above are evident at other levels of methodological debate. This is illustrated in the following section where two important aspects of data analysis are discussed.

Data Analysis

The ways in which data are analysed and collected — in short, the observations made — indicate the sorts of (logically organized) evidence a researcher might be likely to accept as a test of his or her conjectures. Very often — particularly among psychologists — acceptability has assumed two distinct guises. Some psychologists argue that explicit subjective interpretations should be encouraged, and that evidence should only take the form of long, detailed, and carefully compiled case analyses. The emphasis in this approach, quite clearly, being placed on qualitative material, frequently referred to as “soft” evidence by those more concerned with the statistical or quantitative aspects of data, so-called “hard” evidence. The history of this debate in psychology is a long and turbulent one, and it is too well known to discuss here at any length. In anthropology, on the other hand, the history of this debate is much shorter, as indicated below.

The second aspect of data analysis examined has to do with the extent to which different disciplines have shown, over time, an open-systems view of the phenomena studied; that is, their *Weltanschauungen*. The open-systems perspective is widely received among organizational researchers, and it is important therefore to investigate the extent to which related disciplines employ the notion.

Qualitative Versus Quantitative Approaches

Psychology. More so in the past than in recent years, psychologists have tended to muddy debates on this issue with largely emotional disagreements as to which

methods — hard or soft — yield valid information. Kaplan's (1971) statement on this topic illustrates the flavour of these disputes:

Possibly more widespread than the mystique of quantity, and certainly more pernicious in its effect, especially on behavioural science, is a corresponding *mystique of quality*. This mystique, like its counterpart, also subscribes to the magic of numbers, only it views their occult powers as a kind of black magic, effective only for evil ends, and seducing us into giving up our souls for what, after all, is nothing but dross (p. 121).

By now, among most psychologists, although there may not always be complete acceptance of one by the other, “hard noses” and “soft noses” seem to have reached a methodological stalemate, the evidence for both viewpoints being so compelling that even the most ardent protagonists of one cannot help but see a glimmer of truth in the arguments of the other. It is recognized that neither approach is totally right nor wrong for the social sciences in general and psychology in particular, and the growing acceptance that this fact enjoys is producing psychologists in small but increasing numbers who are methodologically eclectic at this level. The need for such eclecticism is particularly apparent in certain interdisciplinary fields such as organizational behaviour, and recently considerable emphasis has been given to the relevance and appropriateness of a wide range of alternatives to the conventional statistical methods commonly employed in studies of organizational life (e.g., Argyris, 1979; Downey & Ireland, 1979; Mintzberg, 1979; Van Maanen, 1979; Webb & Weick, 1979). This implies that it is the nature of the problem, and its setting, as well as the abilities of the investigator which should do most to determine the data gathering techniques employed, as this quote from Rohner (1977), an anthropologist, amply illustrates.

Every methodology has certain advantages and disadvantages, strengths and weaknesses, as well as the potential for certain kinds of bias. Each yields certain kinds of information but does not yield other

kinds. Just as the kind of fish one catches depends on the net that is used, and the net that is employed is designed to catch the fish one values, so the kind of data one collects depends on one's methodology, and the methodology is selected to gather the *kind* of data one values (p. 126).

However, there are signs which suggest that these views are not widely distributed among anthropologists.

Social anthropology. In the 1950s, a number of prominent anthropologists (the evidence presented here refers primarily to Africanists although it may also be applicable to other social anthropologists) began to show some concern over what they saw as the growing limitations of their traditional methodology, participant observation. More than anything else, this mood of concern was brought about by the gradual disappearance of clearly distinguishable ethnic units or tribes as objects of study. In Africa, the progressive breakdown of such unitary systems was a result of many factors, among them, increasing contact with white colonists, the rapid spread of new technologies after World War II, and the new attitudes and knowledge displayed by returning black war veterans who had been drafted to fight the white man's war in far away places. As a result of these changes, it became increasingly difficult for anthropologists to find a traditional culture untainted by the effects of rapid development: the "noble savage" was fast becoming the stuff of folklore and legend. The most dramatic, and perhaps most significant, aspects of the new dynamism were manifested in the rapid growth of towns and cities, and the increasing demands for migrant labour on the one hand and the growing needs for Western consumer items on the other. Indeed, in this regard Little (1973) has noted that "city growth in Africa seems now to be proceeding more rapidly than in most other regions of the world and exceeds even that of developed countries during their own period of fastest urban growth" (p.12). These developments prompted among anthropologists a search for new and "more sophisticated" methodologies. Banton (1957), for example, felt that:

The relatively simple life of a tribal

village can perhaps be adequately described in purely verbal terms but the uniformities found in urban life can for the most part be expressed only statistically. In the town few generalizations of any validity can be obtained without the use of social survey techniques — taking a sample of the populations and showing the variations of behaviour and circumstances in a numerical form (p. xv).

Other leading anthropologists shared Banton's view (e.g., Fortes, 1949, p. 59). There were a few, however, who disagreed with this sort of reasoning, although not until some years later. Mitchell (1966) was one of these:

The belief that quantitative methods must be used in urban studies because the data are so complicated is mistaken . . . It may well be that the social situations in which a town-dweller interacts are more varied than those in the life of a tribesman, but, so far as sociological analysis is concerned, it seems that the behaviour of a townsman in a given social situation is not likely to be more 'complex' than that of a tribesman in a rural situation (p. 40).

Southall (1961) has argued along similar lines: "The first point to make is that small group analysis of the type under discussion gives knowledge of the fabric of society which statistical sampling . . . can never provide. The alternative at this level is not between representative or unrepresentative data but between some knowledge or none" (pp. 26-27).

These limited examples suggest that anthropology has the makings of the same sort of controversy which raged for so long in psychology, with similar proportions of heat and light being generated. Meehl (1978) has put into place some of the major components of this issue, and the implications are as relevant to anthropology as they are to psychology or any other related discipline.

It is a fuzzy open question, in the present state of the metatheoretician's art, just when a mass of non-quantitative converging evidence can be said to have made a stronger case for a conjecture than the weak kinds of nonconverging quantitative

evidence usually represented by the significance testing tradition. I say 'when' rather than 'whether', because it is blindingly obvious that *sometimes* qualitative evidence of certain sorts is superior in its empirical weight to what a typical social, personality, or clinical psychologist gets in support of a substantive theory by the mere refutation of the null hypothesis (Meehl, 1978, p. 830).

Open-Systems Theory

The idea of an open-system has been around for so long in some fields of social science research (one of the first fully developed systems approaches being that of Parsons, 1951) that, by many, it is simply taken for granted. This is not universally so, however. In some respects, anthropology provides an example of slower growth and spread. At the other end of the continuum, those involved in the study of formal organizations have employed open-systems theory for many years, beginning in the early 1950s with rapid diffusion taking place in the middle and late 1950s and early 1960s.

In this section, some of the developments which have taken place within the following disciplines are compared and contrasted: first, organizational psychology; second, social anthropology; and third, management theory. A number of implications of these developments are also discussed.

Organizational psychology. In this domain, open-systems theory implies that organizations are dynamic systems comprising repeated cycles of input, transformation, output and renewed input. Thus, formal organizations may be seen as a special class of open-system displaying individual properties, but having other properties in common with all open-systems. In different words, these common properties include the importation of energy from the environment, the transformation of such energy into some form which is characteristic of the organization, the exportation of these products into the environment, and subsequent re-energizing from the environment. Open systems also have in common the characteristics of negative entropy, feedback, homeostatis, differentiation, and equifinality (Katz & Kahn, 1966).

In their explanatory endeavours, open-

systems theorists typically allow for multiple causation and a high degree of uncertainty. Closed-systems theorists, on the other hand, attempt to reduce the range of independent variables and to state causal relationships with much less equivocation. Researchers in the field of organizational psychology have long been aware of the limitations of a closed-systems approach. In America, the seeds of this awareness were sown by the work of Elton Mayo and his associates (Pugh, 1969). But it is probably in England that open-systems theory has its strongest roots. The foundations were laid by Trist and Bamforth (1951) in their pioneering study of Durham coal miners, where they developed explicitly the notion of "socio-technical systems". This marked the beginning of a long line of similar research conducted by members of the Tavistock Institute (e.g., Emery & Trist, 1969; Miller, 1975; Miller & Rice, 1967). The open-systems approach continues to enjoy widespread appeal and is firmly embedded in the traditional core of organizational studies (Burrell & Morgan, 1979).

Social anthropology. In anthropology, open-systems theory has developed much more slowly. As recently as the mid-1960s, for example, there were a number of leading social anthropologists who openly advocated what amounted to a closed-systems approach. The examples set out below are drawn from the study of rural-urban migration in Africa (a research domain of considerable importance in modern social anthropology) where some notable researchers have argued that rural-urban migration should be seen in terms of separate rather than interlocking spheres of behaviour. Gluckman (1961) and Mitchell (1966) are probably best known for this stance. Gluckman's (1961) view was, simply, that there was no relationship between a migrant worker's behaviour in town, and his behaviour at home in the rural area. He saw so-called "de-tribalization" as an instant change which took place the minute the tribesman left the tribal boundary and was outside the political control of the tribe. For Gluckman, therefore, an African miner was a miner just as anywhere else. He concluded: "Our examination of town and rural areas shows that it is possible for men

to dichotomize their actions in separate spheres, and this may be an important contribution to the working of the embracing social field" (Gluckman, 1961, p. 81). Mitchell (1966), a few years later, was even more outspoken in this regard:

The starting point of the analysis of urbanism must be an urban system of relationships, and I would go further than Gluckman (1961, p. 80) and say that the tribal origins of the population in so far as these imply tribal modes of behaviour *must* be — not 'may even be' — regarded as of secondary interest (p. 48).

The opposing view looked upon town and country as integral parts of one social system in which townsmen and tribesmen were linked in networks of relationships (e.g., Epstein, 1967; Mayer, 1961, 1962; Van Velsen, 1961). But it is only in the last decade or so that this debate appears to have been resolved, by implication, in favour of an open-systems approach (e.g., Parkin, 1975; Shack, 1973). Parkin, for example, begins his book from what he refers to as "the now generally accepted view that the rural and urban areas of East and Central Africa have to be regarded as part of a single field of relations made up of vast criss-crossing of peoples, ideas, and resources" (1975, p. 3).

More recent statements on this topic are contained in a paper entitled "Systems Theory in Anthropology" by Rodin, Michaelson, and Britan (1978). This paper reported on the proceedings of the panel on "Systems Analysis in Anthropology" of the World Anthropology Conference, held in Houston, Texas, November 28–29, 1977. The comments made by these authors give some indication of the present position of anthropology *vis a vis* open-systems theory. To begin with, Rodin *et al.* felt that "modern systems approaches first came to the attention of social scientists in the late 1950s" (1978, p. 747). It is not altogether clear what the authors mean by "modern systems approaches", but open-systems theory was quite well known by the late 1950s. An example for further enquiry consisted in "distinguishing between formal and informal structure in centralised bureaucracies and outlining the effect of this

distinction on the diffusion of innovations" (Rodin *et al.*, 1978, p. 748). Now the question of informal activities in organizations is clearly of some importance, but it is a subject which has been well researched in the past (e.g., Crozier, 1964; Trist & Bamforth, 1951) and hardly warrants special mention these days.

As Dow noted at the end of the article, "systems theory [still] has an alien connotation in anthropology" (Dow, 1978, p. 753). Perhaps, as Dow suggested later, it is not so much a question of bringing systems theory into anthropology, but more a question of making "anthropologists aware of the extent to which systems concepts already exist, to help them to find techniques to prove or disprove their theories, and, thus, to move anthropology ahead as a science" (1978, p. 753). Alternatively, the "difficulty with anthropological participation in systems oriented research that cuts across disciplines is a tendency toward complacency on our part. We tend to make an issue of the fact that our discipline has been since its inception holistic, and, so the argument goes, essentially systems-oriented. While this has been a goal of anthropology, it has seldom been accomplished" (Moran, 1978, p. 756).

A possible consequence of such developments is the apparent readiness on the part of some anthropologists to accept, almost as immutable, the technical and structural characteristics of Western industrialized society. Change is perceived as being uni-directional, as if the only possibility, to quote Price (1975), is "for the modern organizational [bureaucratic] role-set to merge with and dominate the traditional [in this case, Ghanaian] corporate role-set" (p. 209). The same sort of attitude is evident in Mitchell (1966): "the focus of sociological interest in African urban studies must be on the way in which the behaviour of town dwellers fits into, and is adjusted to, the social matrix created by the commercial, industrial, and administrative framework of a modern metropolis" (p. 38). The trouble with these views is that they do not acknowledge explicitly the possibility that modern systems (architectural, organizational or whatever) might well be improved

if *they* were able to adapt to the people and environments they operate in.

Another consequence of slow and uneven development may be seen in the restricted field of vision which some anthropologists display in relation to modern organizational systems: more often than not their analyses are Weberian and nothing else. The considerable impact of Weber on anthropology has been noted by Gluckman and Eggan (1966): "But it would seem that, leaving aside Durkheim, whose school's influence on social anthropology has always been marked, the influence of Weber on younger social anthropologists in recent years has been considerable" (p. xviii). No doubt this has served to reinforce the special position accorded bureaucracy by anthropologists, almost as if it is a structure which should be regarded as a more or less permanent and universally necessary feature of modern organizational life. In the face of views of this sort, it becomes necessary to say, as Child (1977) has done so cogently in his recent book, that:

There is nothing sacrosanct about bureaucracy or any other particular model of organization. The ways in which the various dimensions of structure can be designed are extremely varied. The choice is wide and can suit the circumstances which prevail, as well as the ways in which members of organizations wish to perform their work and relate with colleagues. Given this breadth of organizational choice, it is necessarily an oversimplification to offer any classification scheme which is limited to only a few categories (p. 231).

But if some anthropologists appear to suffer from a certain amount of tunnel vision when it comes to organizational systems, so too do some organizational psychologists when it comes to considering cultural systems, as Roberts (1977) has recently made clear: "Despite their diversity, the anthropologists strive to reach a theoretically useful description of culture. Psychologists, on the other hand, fail entirely to address the problem. They view culture as a vague entity, cast it as their independent variable, and forget it" (p. 62).

At first sight, the above examples may

seem to stretch the limits of methodological consideration a little far. But it is probable that such questions prevent, on the one hand, anthropologists from attempting to come to terms with, for example, a wider array of organizational systems, and on the other, psychologists from confronting questions like culture. If there were fewer (methodological) disciplinary peculiarities about, what are, after all, increasingly common problems could be approached, in a more unified way, by researchers from a variety of backgrounds to their collective advantage. More importantly, perhaps, the practical application of social science research would also be liable to benefit.

Management theory. A number of writers have referred to the apparent gulf between the wide theoretical acceptance of the open-systems concept and its limited practical application (e.g., Blunt, 1978; Clark & Ford, 1970; Emery & Trist, 1969; Roberts, 1977; Schollhammer, 1969). For example, Schollhammer has described the situation as follows:

The traditional management literature is largely confined to the description, evaluation, and prescription of structural conditions and functional processes from the point of view of an effective utilization of the production factors which the organization employs. It is thus emphasizing the 'internal environment' of a firm's operations and takes the external environment largely as given or at least not as being within the realm of business management theory (1969, p. 84).

This tendency is particularly evident in developing countries in Africa, where, by and large, it seems that Western management systems have simply been transplanted wholesale into an alien environment with little or no attention being paid to their impact and appropriateness (Blunt, 1978). In many instances, this has exacerbated, unnecessarily, job alienation and employee dissatisfaction (Blunt, 1980b, Note 1). This implies very strongly that the theoretical advances of organizational research have yet to penetrate the corpus of management theory sufficiently to allow for their widespread application, especially in developing countries. And this is made worse when one

considers that there has been in existence, for almost twenty years now, a sub-discipline known as "comparative management theory" whose express purpose has been "the detection, identification, and evaluation of uniformities and differences of managerial problems in different countries or regions" (Schollhammer, 1969, p. 82). In part, these short-comings are due to the traditional alliance between organizational researchers, management theorists, and management *per se*, an alliance which aimed to protect managerial prerogatives and increase the efficiency of managerial controls. Although, as Pugh (1969) and others have noted, there has been a growing awareness of the need to break this alliance, such changes are notorious for the time they take to effect.

Moreover, it is sad but true that innovations in managerial thinking — such as the widespread concern in many developed countries to evaluate and implement various forms of worker participation — take a long time to reach the developing world. It is as if such countries have become not only the dumping grounds for outmoded and inappropriate technologies, but also the backyard of managerial innovation, where, so to speak, the scrap is kept. Instead of encouraging indigenous peoples in the developing countries to learn from our mistakes, and allowing their cultures to contribute their own insights, we have simply planted a few bureaucracies randomly about and left it at that: thus forcing such countries to endure the some dreary and drawn-out learning cycles as those experienced in the West. Victor A. Thompson is an advocate of better adapted organizational systems and structures in developing countries. In his view, if developed societies have anything to offer the developing world, "it most definitely will not come from the doctrines of management or administration most widely prevalent in the west. . . . The ideal must be *adaptation*, and this involves creativity and a looseness of definition and structure" (1964, pp. 92-93). The chances of this sort of recommendation being implemented would be much greater if there were a more general awareness among social scientists of the practical implications of methodological developments like open-

systems theory. Clearly, this is not the only obstacle, but overcoming methodological parochialism must be given high priority as it is a necessary, if not sufficient, precondition for bringing about social as well as scientific change.

Conclusion

To return to Popper's analogy, it would appear from the evidence provided here that the piles which bear the structure of social science research not only are driven into a swamp but are suffering, in addition, from a severe attack of woodworm. That is to say, the tentative nature of theory is compounded in the social sciences: first, by the uncertain hold which many social scientists appear to have on fundamental methodological advances that have been made; second, by the apparent inability among social scientists to learn from each other's mistakes and insights; and third, by placing too little store by methodological debate.

In summary, the present study shows how:

- (1) The experience of anthropology clearly illustrates the dangers of induction (e.g., Jarvie, 1967; Vermeulen & de Ruijter, 1975).
- (2) The recent work of Meehl in psychology and Lakatos in the philosophy of science re-emphasises some of the problems connected with auxiliary theories (Lakatos, 1970; Meehl, 1978), and hypothesis testing (Meehl, 1967).
- (3) Development administrators, management theorists, and anthropologists could well apply open-systems theory more widely, following the lead of organizational researchers at the Tavistock Institute (e.g., Miller & Rice, 1967; Trist & Bamforth, 1951).
- (4) The emerging preoccupation among anthropologists with quantitative methods might well be tempered by the experiences of, for example, psychologists (e.g., Meehl, 1954) and organizational researchers (e.g., Campbell, 1978).
- (5) Management theory needs to be more self-critical methodologically if its aspirations to science are to bear fruit.

All this implies that some type of

methodological eclecticism might be an effective way of overcoming the problems here identified. More precisely, it may be that social scientists should be encouraged to enquire into methodological problems and developments in a number of disciplines and at a number of levels, so that their own methodological positions are as well informed as possible. In particular, social scientists in interdisciplinary fields such as organizational behaviour should be alive and responsive to this suggestion. In other words, in proposing some form of methodological eclecticism, it is intended to convey:

- (1) The need, at the epistemological level, for greater breadth of vision — knowing your opposition — and the adoption of a consistent, well defined and defended position: a position which may be informed and in some cases reinforced by analysing epistemological developments in other disciplines. This, by itself, does not imply eclecticism in the strict sense of the word. However, taken in conjunction with the next proposal, eclectic recovers some of its accepted meaning.
- (2) At the levels of data analysis and collection: again, greater breadth of vision, encompassing as many methods as possible and allowing their use to be determined by the nature of the problem, its setting, the researcher's abilities and weaknesses, time, and the availability of other resources. But, importantly, a greater vertical scope of vision is needed. That is to say, one which allows epistemological considerations to filter down and affect the lower-level methods employed and the way findings are interpreted.

A final reason for more inclusive methodological perspectives is related to the social scientist's role as social engineer or action researcher (Clark, 1972). That is to say, social life may be perceived through the same logical structures as scientific method (Popper, 1961). Both are, or should be, founded on the method of trial and error. In both, progress is only achieved through the recognition and critical analysis of past mistakes. In this way, Popper links his epistemological doctrine of falsification to

his social philosophy of piecemeal social engineering. This is not the place to enter into a discussion of piecemeal social engineering (see Freeman, 1975 for a recent analysis), but the unity and breadth of Popper's thinking as portrayed in his scientific and social philosophies exemplify precisely an important element of the methodological eclecticism advocated here as a way of overcoming some of the problems identified in this paper.

Reference Notes

1. Blunt, P. *Work alienation and adaptation in sub-Saharan Africa: some evidence from Kenya*. Manuscript submitted for publication, 1981.

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