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Psychology in context

Cross-cultural research trends in South Africa

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Editors

With an introduction by Simon Biesheuvel

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SERIES PREFACE

In human sciences research we strive to increase our understanding of man: to discover and interpret the meanings and symbols of social life, to explore the causes which underlie human behaviour and ultimately to contribute towards the solution of social problems. Knowledge, and particularly social knowledge, is essential in combatting ignorance, prejudice and dogmatism. Stated differently, the primary aim of research in the human sciences is the acquisition of objective, reliable and valid knowledge of all facets of human existence.

The rationale for conducting research on methodological issues in the human sciences is to be found in the emphasis which is placed upon the scientific nature of research. The aim of research methodology, therefore, is to identify methods and strategies by means of which the scientific character and credibility of the human sciences may be enhanced.

In accordance with the above aims, the specific objectives of the HSRC Investigation into Research Methodology* are:

- to increase awareness in the South African research community of the importance of methodology in the research process;
- to encourage and initiate research on issues related to methodology in order to increase the level of proficiency in this field in South Africa;
- to publish reports, monographs and collected papers on research methodology.

In pursuing these objectives a **Research reports series** has been introduced. This series contains reports on research conducted by the Division for Research Methodology of the Institute for Research Development (HSRC), proceedings of relevant seminars and conferences as well as final reports on research supported by the Investigation into Research Methodology.

Johann Mouton
Co-ordinator

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PREFACE

With the exception of the introductory paper by Biesheuvel, the papers collected in this volume were originally drafted as contributions for an advanced one-day seminar presented under the auspices of the HSRC's Institute for Psychological and Edumetric Research in July 1985. The theme of the seminar was ***Research in cross-cultural psychology: current trends.*** The speakers were asked to concentrate on issues relating to cross-cultural research methodology. They had been invited because of their established interest in cross-cultural psychology, their active involvement in research in this area, their concern about methodological issues in the field, and the divergence of their approaches.

Biesheuvel, the doyen of psychologists and particularly of cross-cultural psychologists in South Africa, wrote the introductory paper on the historical context of cross-cultural psychological research in South Africa. In addition to some comments on the conceptual difficulties embedded in the label "cross-cultural psychology", Biesheuvel presents a lucid historical contextualization of the present-day situation of cross-cultural research, and makes a number of tantalizing suggestions regarding future research issues which ought to be addressed by this field of psychology.

The methodological emphasis envisaged for the seminar emerges clearly. It would, however, appear that there are two distinguishable perspectives that may be identified in the collection of papers. Without implying any form of value judgement in the use of the terms, these two perspectives can be termed the "disengaged", on the one hand, and the "committed", on the other. We shall try to make clear what we mean when using these terms.

The ***disengaged*** perspective is illustrated in three papers in the volume: those by Sechrest, Verster and Du Preez. In his paper Sechrest discusses the quantitative tradition in methodology, as well as the implications of such an approach for important theoretical issues. He stresses the point that qualitative methods have, as yet, not received much attention in cross-cultural psychology, and he adds the qualification that, in general, qualitative techniques need to be more explicitly specified if they are to attain general application and greater utility. Sechrest confines his argument to what one may conveniently label the relatively "pure" aspects of methodology. The criticism he offers is directly related to the methods currently in use in cross-cultural psychology, and is aimed at refining these methods in order to improve their precision, and to specify the preconditions for their use.

Verster advocates the use of the comparative method in cross-cultural psychology. He regards cross-cultural psychology as providing a stringent test for the validation of constructs in psychological theory. His paper can be viewed as embodying a

disengaged perspective in the sense that traditional scientific values are held to be important: the comparative method and psychometric testing are seen as valuable tools for assessing similarities and differences between groups in order to establish the universality of psychological constructs, and also for generating hypotheses about, and laws of, human cognition. (This, however, should also be situated against his more recent remarks (Verster, 1986) on the complementary goals of an indigenous and a universal cognitive psychology.) Verster provides a valuable overview of the developments in the field of testing in Africa, as well as an exposition of Berry's theoretical framework for contextualizing variables in cross-cultural research.

Du Preez confronts the question underlying the engaged-disengaged perspective in the introductory section of his paper, by adopting the point of view that culture and identity are contaminated constructs that have become reified and politicized to such an extent that their scientific utility may have become questionable. He argues in favour of a stance that appears to be situated somewhere towards the midpoint of the engaged-disengaged continuum. He favours the use of constructs such as culture and identity in the sense in which they are employed in theories of culture like Lumsden and Wilson's gene-culture co-evolution theory, the semiotic approach of Lotman, and, by implication, the general systems approach which is associated with his emphasis on process, the dynamic aspects of culture, and change over time. Theories of social science, such as those of Smuts and Alexander, can be

hanged into (cultural) theories of identity when they are ligned with the interests and practices of particular classes of eople. It would, therefore, appear that theorizing of this ature operates at a different level of discourse when it is used o serve the interests of a particular cultural group or ub-group. While this may result in some social advantages theory is used to justify the existence of a group or to negotiate etter social conditions for the existence of a particular group), t could as readily lead to social ills (the denial of the right of xistence or the subjugation of other groups).

According to Du Preez, however, the real business of social cientists is to describe and explain the process of maintaining dentity, which includes identifying the way in which social heories are used for this purpose. Implicit in the analysis which s presented by Du Preez appears to be a notion of levels of heoretical discourse, some of which are appropriate to the escription and explanation of culture and cultural processes uch as, for example, the maintenance of identity. If, however, heorizing were to move beyond this level, and if it were to be pplied in the service of a particular group, it may start to cquire a different explanatory focus, in the sense that social nds (theory as an instrument of social power) as well as xplanatory and descriptive ends (theory has explanatory or pistemic power) are served.

u Preez would appear to opt for a disengaged perspective when e maintains that social scientists should explain culture by neans of some of the theories which they have at their

disposal. This is preferable to using theories for ideological purposes. In addition to this, however, his contention is that by adopting an appropriate attitude for studying cultures-in-interaction (which also includes studying the way in which social theories are used to maintain cultural identity codes), three conclusions follow. First, our social theorizing becomes more relevant because it focusses on actual processes of cultural interaction; secondly, these processes require certain appropriate theoretical perspectives of an evolutionary and adaptive nature; and thirdly, there is a need to study processes that result in changes over time - identity, for example, is not static but is contested and maintained. It is in this sense that Du Preez's attitude may be seen as being situated somewhere between committed theorizing and a disengaged perspective. Although he wants social theory and cross-cultural enquiry to become more relevant to real-life issues by, for example, adapting methodology to the study of change, he also displays a preference for conceptualizing culture in terms of global evolutionary perspectives that do not serve sectional social interests, but that may be employed to describe and explain cultural processes in a relatively neutral fashion.

The authors of the last three papers in the volume all advance different arguments in favour of an engaged form of cross-cultural enquiry. Whereas Sechrest and Verster accept the validity of conventional methods for studying cross-cultural phenomena and offer internal criticisms aimed at revising and refining these methods, the last three contributors criticize

these methods on external grounds, questioning the validity of the assumptions upon which they are based.

Bhana points out the dangers associated with conventional methods in the sense that structured tests (for example, factorially-based questionnaires) may result in the imposition of a predetermined interpretative structure on the culture in question, thereby militating against the achievement of a good *emic* picture. She advocates the use of more open-ended qualitative questionnaires by means of which good emic descriptions or constructs may be achieved, and which may, in turn, serve as the basis for extrapolating to more general entities. In essence, her argument is in favour of adapting existing theories and methods to the study of an acculturating group (South African Indians) in which certain traditional behavioural patterns and values of the group co-exist with those of Western culture. She contends that by following this approach we may find that other variables, such as education and socio-economic status, are of greater explanatory importance than what she sees as the value-loaded construct of culture.

According to Miller, the central problem confronting social scientists in South Africa is the issue of change, and how to set about studying it, whether this be in a disengaged (aloof) or in an engaged manner (committed to some vision of the future). Miller's answer to the methodological aspects of the problematics of change amounts to an external critique of the methods conventionally employed in the social sciences in

general, and in cross-cultural psychology in particular. Miller portrays the experimental methods which abound in cross-cultural psychology as still firmly rooted in positivism. The consequence of this situation, in Miller's view, is that cultural experience becomes dissected and compartmentalized in terms of variables that are isolated and quantified for research purposes, when, in actual fact, experience of this nature is qualitatively embedded in a cultural context.

According to Miller, we should seriously consider the possibility that culture may be no more amenable to analysis by means of the experimental method than history or evolution. He would like to see a shift in our thinking towards an awareness of the embeddedness of behaviour: in history, in culture, and in a changing context. According to his view this may be accomplished by drawing upon the scientific realism of Harré and Secord, and by combining it with Vygotsky's social and developmental psychology. This approach results in a scientific and methodological framework that accommodates:

(a) a conception of cause that is more appropriate to studying cultural experience (generative mechanisms are taken to cause observed behaviour, in line with the precepts of Harré and Secord);

(b) a conception of change over time in which behaviour is seen as contextually, historically, and culturally embedded (as proposed by Vygotsky and Geertz); and

(c) a view of cross-cultural psychology as a developmental or historically sensitive analysis of the processes that generate change. In this sense, cross-cultural psychology is seen as theoretically and methodologically engaged in, or committed to, the process of studying change.

In a similar vein, Gilbert also adopts an anti-positivistic stance in which he points to the discrepancies that exists between first world and third world (or local) frameworks. He regards traditional cross-cultural psychology as predominantly positivist, and he maintains that, in the main, criticism may be brought against it from both reformist and radical perspectives. Reformists argue in favour of an adaptation of conventional methods and modes of research, while the radicals reject these out of hand. Gilbert's preference is for a stance in which Morgan's arguments in favour of a more committed and qualitative approach to research are taken as a fundamental point of departure, and which ultimately leads to a position explicity in defense of an engaged social science. The research process is viewed as a form of human action that stands in a dialogical relationship to research participants, and this mode of operation enables the research participants in the third world environment to participate actively in the research process. By these means avenues for the development of a critical psychology engaged in change are opened up.

The seminar was characterized by a tension between researchers who, for different reasons, advocated an engaged, committed

version of cross-cultural psychology, and researchers who espouse a more disengaged, "international" perspective.

The observed differences appear to be associated with an acceptance, albeit a critical one, of quantitative methods, as opposed to an external criticism of these methods by the engaged group. This is coupled with a tendency towards viewing more qualitative methods as appropriate means of conducting research into the dynamic process of cultural change. While researchers of the first type compare cultures according to what they see as relatively neutral indices appropriate to, and derived from, the large-scale comparison of cultures, the burning issue in third-world contexts is often that associated with rapid social and cultural change and even upheaval. The concomitant social and political pressures exerted upon social scientists is to produce some form of theorizing and research that is relevant not only to the accommodation of the process of social change, but also to influencing its nature and direction.

We believe that the pressures referred to in the preceding paragraph, are admirably reflected in the papers that have been assembled in this volume. In this sense, the spectrum covered by the contributors also represents a cross-section of life in South Africa during a specific historical period (1985-1987) which is characterized by violent social and cultural upheaval. Inevitably, the social sciences are progressively being drawn into this spiral, and the local contributors to the seminar appear to have tried to account for their roles in this process. It is, therefore, clear that the issues raised in this volume have implications that go well beyond the methodological. They

represent different views in the academic sub-culture of South African society, as it is related to a broader national and third-world background, and they situate psychology in the South African context.

In conclusion, we would like to thank the following persons for their assistance:

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1.

CROSS-CULTURAL PSYCHOLOGY: ITS RELEVANCE TO SOUTH AFRICA

S. Biesheuvel

A curious fact about cross-cultural psychology is that the two terms indicating its field of specialization - culture and behavioural comparisons across cultural groups - have remained undefined and rather vague. Despite this fact - or perhaps by virtue of it - cross-cultural psychology has become one of the most, if not the most vital discipline in the psychological domain. Definitions sometimes have a way of restraining innovative or radical thinking about the problems of a subject.

THE "CULTURE" CONCEPT

In a symposium article entitled *Do we need a concept of culture?*, Jahoda remarked that *culture is arguably the most elusive term in the generally rather fluid vocabulary of the social sciences. The number of books devoted to the topic would fill many library shelves* (1984:140).

Rohner, in the lead-in contribution to the same symposium, concluded that *within the framework [of his analysis, which did attempt to formulate an operational definition] it seems that relatively little research within cross-cultural psychology has much to do with "culture" per se. In any case, few researchers give more than lip service to the question "What's cultural about my cross-cultural research?* (1984:133). Segall, the most radical of the three symposiasts, doubted *that it is worth the effort to try to enhance the concept's clarity or to struggle to articulate a universally acceptable definition* (1984:153). In another paper he argued *that "culture" is simply a concept that is gross, abstract, and nothing more than a superordinate name for its many component parts* (Segall, 1983:137). He does not reject the concept of culture, but thinks it does not matter that we cannot pin it down. If the attempt to do so were to succeed, we might end up with a definition suggesting that cross-cultural psychology is studying the wrong things, which he thought is certainly not so.

The debate really has its origin in the fact that culture is the central concept in social anthropology, where it cannot be treated merely as a given, but has to serve as the starting point for basic research and the fountainhead of the major theorems and constructs of that discipline. Social anthropologists have run into major problems in attempting to define their subject matter. Kroeber and Kluckhohn (1952), both cultural anthropologists, reviewed 160 different definitions which they classified in six categories: (1) historical; (2) enumeratively descriptive; (3) normative; (4) psychological; (5) structural;

(6) genetic. These clearly reflect the different disciplines and objectives of those who have occasion to use or study the culture concept.

It appears that cross-cultural psychologists can manage quite well without definition. Cronbach and Drenth (1972) in their foreword to ***Mental tests and cultural adaptation***, which published the papers read at the Istanbul Conference of the NATO Advisory Group on Human Factors, stated that ***"Culture" emerged as the ubiquitous undefined term of the meeting.*** Within the superordinate cultural framework, and starting from a phenomenon they are interested in or that presents a problem, psychologists can select the environmental variables they believe could be relevant. These could be drawn from the way we subsist, are housed, or protect our security. They could concern the material and symbolic objects we make, devise, and use for our daily needs and pleasure; the institutions we have created to regulate our family and communal life; the numerous arts we practise; the language, customs, manners, values, philosophies which mediate our social relations. Proceeding thus on a situational basis, with an input-output approach, meaningful and useful relationships can be established; but they will throw very little light on the larger problems of the still only very partially discovered species-wide potentialities and characteristics of human behaviour. There is more to the cultural context than a row or column of loadings on variables whose interrelatedness has not been studied in depth. What is an independent variable in one investigation may have to be treated as dependent in another. This may be good enough for studies

with limited scope and mainly in the applied field; but it will not do for researchers with theoretical interests in holistic relationships and in common features in the behaviour of man. LeVine has mentioned *four different types of universal (or quasi-universal) structures as observational frames: The bodily symptoms of affective reactions; the circadian (daily) rhythms of activity and inactivity; developmental phases in the course of individual life from birth to death; and bureaucratic institutional structures of Western origin* (1973:226). The areas chosen have a clinical bias and with the exception of the last, they refer more to behaviour than to culture. The term "quasi-universal" as used by LeVine refers to types of situations, not to behaviour patterns. His plan offers no suggestion on how such patterns can be identified and how their interrelationships and causalities can be studied.

A model proposed by Berry (1983) does meet these requirements. He argues *that the assessment of the context of behaviour has to be accomplished with the same degree of precision as the assessment of behaviour itself. For if we do not, our search for relationships between our two classes of data ("culture" and "behaviour") will be lopsided and probably invalid* (1983:117). The model adopts a system approach in which environmental contexts, ranging from the holistic to the reductionistic, are related via organismic processes to effects ranging from long-standing, adaptive behaviour patterns, through abilities, traits, attitudes and more fleeting ad hoc responses of an adaptive kind, down to scores on adaptibility tests. In a discussion of the rationale of the model, Berry,

states that it can be used to investigate the existence of both universal and societal-specific patterns of cognitive functions, and that *The framework serves the dual purpose of outlining a strategy for comparative research and for evaluating the validity of each of the current conceptualisations [cognitive structure]* (1986:74).

THE MEANING OF "CROSS" IN CROSS-CULTURAL PSYCHOLOGY

Cross-cultural psychology is by intent comparative. In his foreword to "Human assessment and cultural factors", Cronbach (1983) stated that "Cross-cultural research is comparative or it is nothing". There is, however, no straight answer to the question what it does compare or should be comparing. In a definition of cross-cultural psychology, Brislin et al. refer to the empirical study of members of various culture groups *who have had different experiences that lead to predictable and significant differences in behaviour. In the majority of such studies, the groups under study speak different languages and are governed by different political units* (1973:5). (My italics.) The latter qualification is not essential, nor is it logical, even though it is a factually correct observation. The cultural difference (as that term is commonly understood) between the Americans and the British is vast, though they speak what passes for the same language. So is the difference between the Flemish and the Walloons, who speak different languages but are part of the same political unit. The cultural gap between professional people in Britain and Germany is probably a good

deal narrower than between the Sloane Rangers and the Punks, both British and using slang-dialects of English. In fact there are numerous subcultures composed of individuals who behave predictably differently in many social situations as a result of differences in their upbringing and experiences, but who share a common nationality or citizenship, and a common language. Cronbach and Drenth agree that *perhaps it has been sufficient in the past to identify cross-cultural research as research that collects data in two or more nations. But research is equally cross-cultural when it tests two distinct populations within the same nation. Even within a single community or district there may be different cultures at work, if different homes use different language patterns and teach different lifestyles* (1972:vi).

Acceptance of this view has vast implications, not just for the scope of cross-cultural psychology, but for the way we look upon, or attempt to define, what is commonly referred to as "mainstream" psychology. Evidence is forthcoming that its theorems and constructs are predominantly the outcome of Western philosophy, values, scientific tradition and, of course, more than a hundred years of research on and development of a science of mind and behaviour rooted in these traditions. Eurocentric psychology was the natural outcome, and there is nothing reprehensible about this. The questioning of this ideology is of relatively recent origin, partly no doubt owing to criticisms from Third World psychologists that mainstream psychology has had very little to say that is relevant to their societies, and much that is detrimental to the image of their

worth. The concept of the cultural relativity of social psychology (Berry, 1972, 1978) is fairly generally accepted to-day, particularly among cross-cultural psychologists, but it leads to the conclusion that mainline "general psychology" is but one among a number of "indigenous psychologies", albeit by far the most important one. The search for universals across cultures goes on with some encouraging results (Verster, 1986a); but we have a very long way to go before we shall arrive at a universal cognitive psychology, leave alone a universal general psychology covering the personality domain as well.

The search is not without its critics (Cronbach, 1983; Berland, 1983). To allay fears that although it will lead to a scientifically satisfying framework, it will move attention away from the central role of cultural factors and from what goes on within the frame, it must be pointed out that ***pari passu*** with the search, there will be an equally strong move towards the establishment of societal psychologies which will highlight what is unique in the numerous social aggregations of man, as well as reflect the style of their cultures. The Berry multilevel arc model can accommodate this feature.

To return to the question of what is "cross" in cross-cultural psychology, we can say that investigations only undertaken within a homogeneous White population sample are not cross-cultural, (e.g. determining the structure of psychomotor ability among White athletes). If a control group matched for age and sex, but otherwise randomly selected in respect of athleticism is used, the study does not thereby become

cross-cultural, as the object of this procedure is only to obtain a broader range of individual differences of the ability in question and thereby to facilitate, by means of factorial study, the definition of the dimensions of psychomotor abilities and their causal relationships. If now we were to add yet another sample to our design, one drawn, say, from members of literary and similar cultural societies, such a study could rank as cross-cultural because the researchers are unlikely to have selected their particular sample if they had not been interested in testing the etic status of their psychomotor construct from its basically similar structure in a culturally quite different population. Consider also the case of a black psychologist studying work motivation in a Third World black country in which a large proportion of the population still practised a rural subsistence economy. If the design included the measurement of a notion such as need for achievement defined in terms of Western criteria, the study could be described as cross-cultural, particularly if urban and traditional rural population samples were included. If, on the other hand, the study was designed to discover an emic construct, derived from indigenous criteria and value judgements, it would not be cross-cultural, even though the outcome could feature in a cross-cultural work motivation study by a mainline psychologist.

It looks as if the "cross" term is about as difficult to define as the "culture" term, which from a definitional point of view makes cross-cultural psychology an elusive discipline indeed. I do not think that this indeterminacy matters much. As Segall remarked on the futility of attempting to conceptualise

"culture" definitively: ***We should turn to the real business at hand. That business ... is to intensify the search for whatever ecological, sociological and cultural variables might link with established variations in human behaviour*** (1984:154). What it boils down to is that researchers should be able to identify the scientific or societal problems that are important, relevant to the "state of the art" or the social needs of the times; that they should know the right questions to ask about these problems; and that appropriate, scientifically valid methods should be applied to their solution.

WHAT PROGRESS HAS BEEN MADE WITH CROSS-CULTURAL RESEARCH IN SOUTH AFRICA?

South Africa has remarkable achievements in Social Anthropology to its credit. Many illustrious names of men and women who achieved international status could be mentioned. Three South Africans - Schapera, Fortes and Gluckman - occupied important Chairs of Social Anthropology in the United Kingdom. When descriptive accounts of the social systems of the principal nations and tribes had been completed, attention turned to urban ethnic communities, an ongoing pursuit for social anthropologists and sociologists because of acculturation, urbanization and economic/political change.

The record of Psychology is modest in comparison. A number of comprehensive bibliographies dealing with the behavioural sciences (including some Sociology) in Africa South of the Sahara have been published, of which the most notable is

Aptitudes and abilities of the Black man in Sub-Saharan Africa, 1784 – 1963 (Andor, 1983). My probably one-sided impression of the South African contribution is that in the earlier years it was largely concerned with the measurement of ability differences between black and white, in which use was made of inappropriate Western-style tests and constructs. An exception was a comprehensive study of the attitudes of educated Africans towards Western customs, values, laws and administration of justice. This work was carried out during the late 1930's, but owing to interruption by the Second World War, only part of the material was analyzed and published (Biesheuvel, 1955, 1957). The publication of *African Intelligence* (Biesheuvel, 1943) was a signifant event in cross-cultural research in that for the first time in South Africa a comprehensive ecological approach was adopted in the study of blackwhite IQ differences. Within the context of the nature/nurture controversy that was at its height in the earlier part of this century (and that has not ceased since then) the book not only considered the cultural appropriateness of the tests in common use at the time, but also took the influence of cultural milieu, home environment in rural and urban groups, scholastic education, nutrition, attitudes, temperamental factors and control group methodology into account. Another major South African contribution was made at NIPR in the development, selection and classification tests for black mineworkers. These tests not only measured trainability but also work leadership in a predominantly tribal, preliteration, population speaking numerous African languages and dialects. The noteworthy features of this work were not only that highly reliable measurement techniques were devised despite

extremely difficult testing circumstances, but also that a new construct emerged: "Adaptability" which played a significant role in cross-cultural theorizing for quite a number of years (Biesheuvel, 1950, 1952a, 1972). It needs to be stressed, of course, that this work did not pretend to provide a measure of indigenous cognitive competence and that its validity was relative to the demands imposed by mining tasks and their organization at the time in South African goldmines. Their success also opened the door to a variety of ability, attitudinal and environmental studies, for example a series of cross-cultural studies of three-dimensional perception (Hudson, 1967) which stimulated a vast amount of research and publications internationally; investigations into the motives to seek work on the mine; the periodicity of recruitment, and the attitudes of migrants towards other employment opportunities. These disclosed some interesting inter-tribal and inter-acculturated group differences (Confidential NIPR reports to the Chamber of Mines). Similar work involving social anthropology, sociology and psychology was also conducted on blacks in various stages of urbanization (Hellman, 1948, 1956; Mayer & Mayer, 1974; Reader, 1961).

There has been criticism from certain quarters that psychology in South Africa tends to be technocratic, dominated by psychometric methodology, supportive of apartheid, Big Business, and more concerned with the latter's profitability than with poverty and the plight of the oppressed. NIPR and HSRC are held largely accountable for this situation as seen by these critics (Cloete, ***et al.*** 1986). (One could ignore these criticisms

as the views of a small but militant group carrying little weight scientifically; but they are not without influence in the universities and they could also influence the disposal of funds made available by South African subsidiaries of American corporations for research and development programmes on behalf of disadvantaged ethnic groups.)

I submitted an article to *Die Suid-Afrikaan* (Biesheuvel, 1986) to correct a number of mistakes in a paper ***Neutrale navorsing in diens van die politiek?*** (1986), in which I gave details of the vast amount of basic research carried out by NIPR since its formation in 1946. This work was neither technocratic nor could it be construed as having any bearing on business profits. My article was published as a letter and reduced in length; but unfortunately the cuts eliminated my references to NIPR's basic research. My rejection of the contention that the Institute's work was "onbeskaamd tegnisisties" (unashamedly technicist) thus lost some of its effectiveness.

It is true that NIPR made extensive use of psychometric techniques; but this practice was influenced by the phenomenal success achieved by means of this methodology in the psychological services of the Allied military forces during World War II. Managements, both in the public and in the private sectors, believed that square pegs in square holes would solve most of their labour efficiency problems, and so tests is what they asked for. NIPR was well aware of the limitations of testing programmes and of the need to adopt a holistic approach which would take the internal and external environmental

circumstances under which the work would be performed into account. A major contract to install selection tests for black women to be employed in a new textile industry in the Ciskei was lost because NIPR insisted on the concurrent development of a township and conditions of service which would facilitate the transition of women used to a rural domestic life to industrial employment and urban living. This happened in the 1940's. Managements are now more familiar with the principles of organizational behaviour and more responsive to situational solutions. NIPR was recently involved in a homeland project in which environmental planning, particularly the preferred type of housing, received due consideration.

NIPR's psychometric work has, however, had important implications for cross-cultural methodology. Problems of test comparability and equivalence, the selectivity of the constructs involved in them, the possibility of generalising across cultures, all received attention. The notion of "culture-fair" tests came to be rejected because it overlooked the incompatibility of Western constructs involved in the structure of such tests and in the tasks to be performed, with traditional African cognitive modalities and perceptions of causal relations (Jahn, 1958; Biesheuvel, 1966). South Africa's competence in this field was recognised in the appointment of a South African as chairman of a working party in London, set up by the International Biological Programme (IBP), to draw up a handbook for the measurement of psychological performance for use in IBP field projects (Biesheuvel, 1969).[1]

nother major cross-cultural study area concerned the lower edian IQ of Afrikaans speaking groups in comparison with nglish speaking population samples (Biesheuvel, 1952c). Ideas bout the constancy of the IQ and its independence of cultural ircumstances which prevailed at the time led to pejorative dgements about average Afrikaner intelligence. The problem nd research work related to it were politicized to the point hat it was considered safer to leave the matter alone. With the ifferent understanding we now have of the nature of telligence and its measurement, and because of the growth of frikaner self-confidence, the subject is no longer taboo. esearch has now shown that whereas a difference in est-intelligence still prevails for the older generations, it iminishes down the age ladder and has now vanished for ontemporary school children (Verster & Prinsloo, 1986). The ontroversy does show up the harm that can be done both to cience as an objective source of truth and to harmonious group elations by unwarranted interpretations of psychometric test ata. It is a problem we are still facing in the interpretation of milar differences that have been observed between ability test cores of blacks and whites. It is most important for ross-cultural research that we continue the study of group and ndividual differences, as it is only by means of extending the ange and variety of environmental and cultural circumstances s widely as possible that complete insight can be gained into he determinants and course of human development.

o get some impression of individual psychologists' current terests in cross-cultural psychology the contents of 20 issues

of the S.A. Journal of Psychology between 1981 and 1986 were analysed. In terms of the criteria which have been discussed 20 out of a total of 112 articles were cross-cultural, of which two were borderline as they merely referred to literature trends on group differences for comparative purposes. Two papers were by outsiders, 2 were theoretical, or dealt with intra-white, 5 with intra-black, 2 with intra-Indian, 4 with black-white and 4 with multi-ethnic comparisons. The contents were mixed, revealing no particular trends, no overriding interests in psychometric findings on technology, and only one paper dealt with a political/discriminative question (desegregation of public facilities). A somewhat similar impression is congered by an analysis of 89 papers presented to the 1986 Congress of PASA. Twenty could be construed as cross-cultural, of which four were doubtful for the same the reason mentioned in the case of the journal articles. Most of the papers dealt with intra-sub-cultural comparisons, educational and developmental themes being the most frequent (5), followed by theoretical discussions. There was only one that dealt with group conflict (intra-Afrikaner).

Living as we do in a plural society currently in a state of serious upheaval, with more group conflicts, interface problems, and cultural and political dilemmas than one can shake a stick at, it is remarkable that cross-cultural psychologists find so little to research about and that if they do, they avoid the sensitive problems. There may be many reasons for this. There may be reluctance to get involved in political questions, which did sometimes have unfortunate consequences for the research

worker in the past and may do so again in the future but from a different quarter. There are problems of access to material, as co-operation, especially from disadvantaged minority or ethnic groups is not easily forthcoming and it is therefore more convenient to choose research topics which can be investigated using students as subjects. Criticisms from colleagues about alleged ideological bias may also be expected. These are the principal hazards of what is commonly referred to as "relevant research" and which lead the research worker to seek safer scientific havens.

I do not think that the indeterminacy of the cross-cultural domain is a significant factor. It does not seem to trouble research workers who, by and large, follow Segall's advocacy to select environmental variables that interest them, that have plausible causal implications for behaviour, to subject these to empirical research and ***winnow away competing hypotheses that don't do as well as others*** (Segall, 1983:132). There are indications, however, that some research workers are being attracted to the compelling problems that have been raised about the appropriateness to cross-cultural psychology of the constructs and theorems that are the common coin of Western psychology. There is a growing recognition of the importance of finding the unique elements that apply to specific cultures. The search is also on for universals across cultures that could serve as the building blocks for a general theory of human behaviour. To complete the circle, the usefulness and validity of the constructs that have been used in the analysis of the behavioural characteristics of our Western societies is being re-examined in

the light of these cross-cultural findings. A new and exciting chapter is thus being opened up by cross-cultural psychology in the theory-building of psychological science.

South African research in this direction has been in progress for only about five years, the principal contributions being made by Verster. Reference to some of his recent papers (Verster, 1983, 1986a, 1986b) will give a good indication of the problems involved, and of the contact made with overseas researchers. This involvement illustrates the importance to South African science of active membership of a group where ideas and concepts are generated and the results of research can be thoroughly debated. From the review of past work that has been presented in this paper it is evident that South Africa was once upon a time in a dominant position in African psychological research. We have gradually lost this position through the isolation that is being forced upon us through political circumstances. Every effort should be made to keep the channel with IACCP and NATO open, for through it we remain informed about some of the most significant developments that are currently taking place in theory and research. In this regard Verster's contribution to the NATO Advanced Research Workshop on ***Indigenous cognition and models of information processing***, held at Queen's University in Canada during June 1986, presents a synoptic statement on the outcome of information processing to date in cognitive research (Verster, 1986b). The identification of value- and culture-free processes is a primary objective; but at the same time the need is stressed ***to begin within particular societal or ecocultural***

contexts, using indigenous conceptions of the criteria of competence and everyday knowledge and task skills as the starting point for experimentation and measurement (1986b:38). Verster's contribution on problems and prospects of cross-cultural cognitive research in this volume, gives some details of the progress that has been made with the definition of universals, whilst Gilbert's and Bhana's presentations illustrate the need to recognize the ethnic concepts and operating characteristics of a group as well as the merits, problems and disadvantages of developing indigenous psychologies.

Verster's criticism of psychometric theory that it has led to constructs such as intelligence and abilities which have no real world referents that could be linkened to natural acts, such as walking and thinking whereas cognitive performance and processes do have such referents, deserves careful consideration. One should be careful, however, not to fall into semantic traps. Verster argues that contemporary psychometric practice can have no claim to a basis in science, that it is no more than a sophisticated technology with the same sort of pseudo-scientific pretentions as alchemy in its day, and that it can be wrongly manipulated in the service of ideology (1986b:38).

This is too harsh a criticism. Measurement will have to continue and so will the use of tests, although test contents and methodologies will change. Psychometric technical knowledge will remain scientifically valid if properly used (Sechrest, this volume.) Misuse and misinterpretation should be laid at the door

of the user, and not blamed on the technology itself.

SUGGESTIONS FOR FUTURE RESEARCH

Apart from the suggestions made by Verster in this volume on further research arising from the current state of cross-cultural methodology, there are a number of topics that commend themselves. The temperament and personality aspects of behaviour are as important to cross-cultural psychology as cognition. Personality attributes are even more obviously the product of upbringing in a particular cultural milieu than the competence aspects of behaviour. The problem in this case is further complicated by ideological controversies about personality formation which go far beyond the complexity of the naturenurture problem in cognition. There is even less clarity about the constitutional constructs underlying the diversity of personality manifestations. Some "terra firma" from which to start on the perilous journey of tracing developments would be useful, particularly as there is some evidence that cognitive and non-cognitive processes are intertwined, as one would expect from the adoption of a holistic point of view (Verster, In press, a;b). I believe that a useful starting point could be found in the Heymans and Wiersma (Biesheuvel, 1952b; Heymans, 1929; Wiersma, 1906) model, particularly from its developments in South Africa. This theme cannot be further pursued here. Suffice it to say that there are a number of psychologists in South Africa, both at NIPR and at some universities, who have the necessary background knowledge of the model to embark on this research.

IFFERENTIAL BIRTHRATES

opulation growth is the most serious social problem that faces outh Africa. It is a potential source of future unrest and a reat to the stability of society, be it free enterprise, socialist, Marxist. Within 35 years, at present growth rates, South frica will have reached its maximal capacity of 80 million eople, although it is difficult to attach specific meaning to uch a statement in view of the unpredictable nature of the rection, rate of change and the course of world events. I oubt whether we shall be able to solve the natural resource, mployment, housing and social organization problems involved. is said that birthrates correct themselves with improvement standard of living, but it is difficult to see how even in the onger run the life circumstances of blacks will change fficiently to have a significant effect. The point to remember that what is happening now already commits the situation 20 ears ahead when generations being born now reach adulthood. I elieve this problem, because of a number of unknowns, lends self particularly well to multi-disciplinary cross-cultural esearch, provided of course that a sufficient number of ndividuals in the population groups concerned can be persuaded o cooperate. Some of the questions that arise are the ollowing: What part is played by traditional indigenous cultural actors in the black population? What part is played by values in ll ethnic groups? Is poverty a major cause for high birthrates, nd what kind of attitudinal factors operate to produce this overty effect? Can the effects of urbanization and education e precisely determined? What political factors have come into

play and how do these influence attitudes? Are there significant differences in attitudes towards birth control and family planning between the sexes, ages, ethnic groups and individuals according to education, and personality make-up? What is the *ecology of population growth*?

IS THE POLITICAL ORGANIZATION OF OUR SOCIETY ALONG GROUP LINES INEVITABLE?

It is a cornerstone of government policy and the rationale for the Group Areas Act and the Population Registration Act that any constitutional dispensation for South Africa must follow group lines. We are seen as a plural society of minorities whose cultural identities and self-determination rights must be protected by law. Involved in this policy are some questions that could be answered by cross-cultural psychology. What groups are we talking about? Do tribal differences within the black population still count? Intellectuals, business people and the liberation-politically minded among blacks deny that tribalism is still alive as a political force. According to them, historical factors, language and cultural style differences would be no obstacle to the formation of a unitary state. They see the tribal argument as a device used by whites to justify separate development and separate political organization. This view gains some support from the fact that separate political structures are not deemed to be necessary for Afrikaans and English speaking groups, who are linguistically and culturally as different as the Nguni and Sotho speaking groups. Can cross-cultural research determine whether tribal antagonisms,

owever latent they may be now, could become a source of onflict and division when blacks gain political supremacy? Can ve find out what black people really want, and for what reasons?

THE CASE FOR OR AGAINST A UNITARY EDUCATIONAL YSTEM

he educational sphere has unexpectedly turned out to be as reat a conflict generator as the constitutional question. Some f the causes are political, or organizational, not affected by sychological questions. Some aspects could, however, be andled by means of cross-cultural research. If there are value ifferences between different ethnic groups, and if education is o be a preparation for life in a particular society, should these ifferences not be taken into account in education at the cholastic levels? (The group difference question obviously cuts cross this issue.) Has the differentiation into English and frikaans medium schools proved a boon or an unnecessary urden to our society? Has it contributed to harmony or to isunion? What of the babel of tongues in an urban black school ystem, not to mention getting English and Afrikaans mixed up vith this? Are there any differences in talents, cognitive trengths, or maturation rates between the ethnic groups, which ould be better handled for the full development of children's otentialities in separate rather than in ethnically integrated chools? Such differences have proved no obstacle in a unitary ystem for whites. Once again, what do the people want, when olitics are left out of account, and why?

METHODOLOGIES FOR CROSS-CULTURAL ATTITUDE STUDIES

From the above questions it follows that the expressed wants, opinions, attitudes of majorities in various cultural and ethnic groups will or should play a decisive part in policy determinations and actions to be taken. What do we know about how to measure attitudes in a plural society? How far can we get with existing methodologies? Or do we have to develop entirely new ones? How do we get co-operation in a society riven by suspicions about motivation, fears of reprisals for unpopular opinions, intimidation or plain lack of insight on the part of responding parties as to what it is all about? One thing is certain. The usual opinion polls are, for a number of reasons, virtually valueless. So are responses to telephone or mail surveys. I do not think it is possible to get meaningful answers without probing responses in depth, particularly in the case of respondents from a different culture than our own.

A unique study of attitudes towards social and political questions of public importance in sub-cultural samples of the White community was conducted during the 1960's. It started off with a conventional opinion poll to identify the situations and statutory matters on which attitudes were divided or strongly biased towards one or other ethnic group. By means of a lengthy tape recording of a simulated in-depth interview, a number of volunteers were trained to conduct such interviews. Interviewers also had a list of 50 social and personal problems they could use to get their informants to talk about their beliefs

nd opinions in a free discussion about their future in South frica. Each person was encouraged to talk about his or her xperiences, life circumstances, ambitions, fears and world iew. Interviews could last from two to three hours, and ranscriptions of tape recordings could be from 30 to 40 pages in ength. A questionnaire containing 142 items dealing with social nd political questions was administered as well as a thematic pperception test. It took two years to collect, analyse and repare the material. The outcome has proved to be remarkably redictive of the actual course of events (Hudson *et al.*, 1966). he multi-method measurement procedure has since been urther developed by the inclusion of group discussions, small roup in-depth interviews, participant observation in conflict ituations, interactions between informants and interviewers, nd feed-back group discussions. Good rapport for this approach ould be established in a situation as sensitive as the unrest in lack schools (Lazarus, 1985).

ARE THE RESEARCH PROPOSALS REALISTIC AND EASIBLE?

o be fully effective, cross-cultural research cannot confine tself to theoretical and methodological problem solving. It nust get close to the problems of real life. It must provide nswers that are not only of interest to science, but also useful o the people who have to grapple with the problems: the ducators, businessmen, administrators, politicians, the nan-in-the-street who needs facts against which to test his alues or to determine which way to vote. That said, can what

is needed actually be done? The research projects will be onerous, lengthy, elaborate, costly and the outcomes will be uncertain. They involve considerable risks to reputations and careers. They will have to be interdisciplinary and experience has shown that it is notoriously difficult to achieve that kind of cooperation, even on projects such as the International Biological Programme where value judgements did not enter to a significant extent as they will do in ideologically and politically sensitive subjects. These research projects will require independent, enterprising and rather heroic minds and sponsors. Whether the grave situation in which the country finds itself will call forth such spirits from the ranks of the scientists, and not only from those temperamentally inclined towards activism, politics and security pursuits, remains to be seen.

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(1) Another international cross-cultural involvement was the direction of a pan-African study of labour productivity set up by the Commission for Technical Cooperation in Africa South of the Sahara (known by its French acronym as CCTA) and its Scientific Council for Africa (CSA). At a meeting in Salisbury in 1956 of experts from the participating countries - France, Belgium, Portugal, the United Kingdom, Southern Rhodesia and South Africa - to draw up a Technical Plan, it was agreed to adopt concepts and methodology developed in South Africa. NIPR landed in some political difficulties about the contents of the report on the South African data. I was prevented from presenting this report at a CCTA conference in Dakar to consider the findings of the project. NIPR eventually was permitted to issue a modified report (Glass, 1960).

Political trouble also arose over another project commissioned by the Government and directed by NIPR, on *The Bantu Civil Servant.* One paper on *The Bantu Clerk: A Study of Role Expectations* was published, (Sherwood, 1958) but the final report was restricted and never saw the public light of day. A doctoral dissertation was held up for many years on the grounds that its submission was unauthorized, although use of the scientific material for degree purposes had been sanctioned in the contract.

A multi-disciplinary longitudinal study of the effects of supplementary feeding, child-rearing practices and home circumstances on physical and mental development had to be discontinued when the Ford Foundation withdrew its promised financial support after Sharpeville. The study did, however, yield an infant testing procedure and a complete set of norms for the first year of life. This work attracted considerable attention internationally. Research workers from Ghana, Nigeria,Uganda and Belgium received instruction at NIPR on test administration or joined in the project. Dr Nelson took an EEG team and equipment to Mulago Hospital in Kampala, where a British Medical Research Council unit under Dr Dearn was studying kwashiorkor and its mental concomitants. Dr Dean and Dr Marcelle Géber, the French Child psychologist who carried out the developmental testing in Uganda, spent some time with the South African team in Johannesburg to agree on the symptomatology of kwashiorkor and to ensure that similar conditions were being studied and related to mental development in South Africa and Uganda. A number of publications resulted from this international co-operation, of which the most important cross-cultural papers reported comparisons between black and white infants in Johannesburg and Brussels (Falmagne, 1962) and in Kampala, Johannesburg and Dakar (Géber, 1956, 1958). No mention has so far been made of cross-cultural studies within the white population. The outstanding one historically is the Carnegie Commission study of the Poor White Problem in South Africa. It was truly cross-cultural in that the Poor Whites as a group were contrasted in respect of way of life, attitudes, values, mental and physical health, education and intellectual competence with

general white South African norms. The project produced the first South African Group Intelligence Test, IQ comparisons and an assessment of the ecological and historical circumstances that led to poor whitism and its social ills. It has been argued that this study set the fashion for the empiricist and psychometric manner in which we are said to deal with psychological and sociological problems at the national level. A study of the five volumes produced by the Commission shows that the psychometric enquiries were by no means the dominant element in the survey's observations and analysis. I believe that a reading of the joint findings and recommendations of the Commission (1932) is still important for an understanding of the current attitudes towards reform of an appreciable number of Afrikaans-speaking people.

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2.

CRITICAL ISSUES IN THE DEVELOPMENT OF CROSS-CULTURAL RESEARCH METHODOLOGY

L. Sechrest

When interrelating method and theory in the explanation of the world, and also the cultural world around us, the result should be a feasible and fruitful account of the processes that actually transpire in order to make the knowledge gained useful or relevant in some way. In psychology and in cross-cultural psychology a very important half of this equation is of course the methodological, in the sense that methods can serve to systematize our theoretical insights and can strengthen the feasibility of the conclusions that are drawn. If a study is methodologically suspect, doubt is usually cast upon the way that hypotheses are tested and thus also upon the validity of the conclusions reached.

Method is of course also simply another way of understanding the relationship between culture and behaviour. For example: at one level, a relatively pure theoretical level, it may not be critical that the differences in outcomes associated with some

cultural variation are of any particular magnitude. If one expects that the manner in which infants are carried by their mothers and other caretakers will be related to cultural preferences for rhythm patterns, even a small difference in the latter preferences may be satisfying. Still, if we are to understand the phenomena of culture and behaviour and have some sense of the importance of culture for behaviour, we must have ways of expressing effects in metrics that are directly and intuitively meaningful. If we have interest in how effects of culture may be changed, either enhanced or mitigated, knowing the strengths of those effects is imperative.

SAMPLE SIZE AND UNITS OF ANALYSIS

Sample size is often given scant consideration in cross-cultural research, but sample size is a critical determinant of what we find in our studies. That is because sample size is a critical factor in the statistical power (Cohen, 1977) of our studies to detect effects or differences. Even a fairly large difference between cultures may not reach statistical significance if sample size is small, and a very small difference may be significant if the sample size is large. If one were comparing two cultures in terms of the proportions of informants in them who reported believing in ghosts, a difference of 65 per cent versus 35 percent would not be significant with 20 informants per culture; a difference of 55 percent versus 45 percent would be significant with 100 informants from each culture. Or, to take another example, to be significant at the .05 level, a

correlation of about .60 is required with only 10 cases, but a correlation of only about .06 is required for 1000 cases.

Sample size does make a difference in cross-cultural as well as in other research. Cross-cultural investigators should perform power analyses (Cohen, 1977) in order to decide on the number of cases to be included in their studies. Letting sample size be determined by what is customary in the field or by what is convenient or practical is not satisfactory. At the very least, when results are reported, researchers should report power analyses done after the fact. If results are not statistically significant and if analyses show power to detect differences to have been low, readers are cautioned to treat such findings with care. In fact, however, even if the results are significant and power is low, results should be accepted with caution, for the implication is that the findings may not be dependable. That is particularly the case if the significant findings are but a few among many that were tested.

In order to do power analyses, one must be able to say something about the effect size that is expected or that would be considered noteworthy. Cohen (1977) presents a generalized solution to power analysis that does not require prior estimates of variability within samples and that is, hence, of great utility. Cohen also suggests that as a rule of thumb, based on effects actually reported in the literature, a difference between means amounting to about .2 standard deviations (corresponding to a correlation of about .10) should be considered a small effect, and a difference of about .8 standard deviations (corresponding

to a correlation of about .30) should be considered a large effect. Whether that rule of thumb makes sense in cross-cultural research must be a matter of a gradually achieved consensus.

A caveat of some importance is provided by Malpass and Poortinga (in press), who note that a risk entailed by large sample is that wrong findings may come to seem of even more unwarranted importance. If an inappropriate measure is applied to two cultures so that a spurious difference is produced, the day is not saved--in fact, it is made worse--by a large sample that increases confidence in the spurious finding.

Fundamental to the logic of statistical tests is the notion of independence of observations, or, more specifically, that errors of observation should not be correlated. In classical experimental design, this required independence is achieved by assigning subjects randomly to the experimental conditions so that whatever "error" exists in the score achieved by one subject is unrelated to the error in any other score. We do not, for example, assign all the persons seated on one side of a room to the experimental condition and all the persons on the other side to the control group because we suspect, or are at least willing to entertain the possibility, that there are systematic differences between persons according to where they sit.

Independence of errors can rarely be assumed in cross-cultural research. In comparisons between cultures, persons within cultures are subject to a great many common influences other

than the one of interest, and those common influences will be different from those in other cultures. For example, if one is studying a perceptual phenomenon that is influenced by physical environment but also by nutritional status, then nutritional status is a source of error in observations but a source possibly common to many members of one culture and not to those of another. Moreover, the problem is often exacerbated by the fact that samples are chosen in "clusters", as when representatives of a culture are all from one village or one clan.

There is no solution to the problem of non-independence of observations except, perhaps, the purist stance of refusing to have anything to do with such data, let alone analyzing it. What cross-cultural researchers must assume is that problems that might be caused by correlated errors are negligible in relation to real differences between cultures. That may sometimes be a rather large assumption, and it would be reassuring to find more often that the problem is at least recognized and to find it discussed explicitly.

STATISTICAL CONCLUSION VALIDITY

An important type of validity to be assured in any study is what Cook and Campbell (1979) call ***statistical conclusion validity.*** That refers to the legitimacy of the statistical conclusions that are reached in any experiment or other study. Obviously, for example, errors of computation may result in wrong statistical conclusions, however well or poorly done an experiment may

have been. There are many potential problems bearing on the validity of statistical conclusions, some of which are particularly likely to affect cross-cultural studies.

Major problems grow out of the widespread bias in cross-cultural research toward the finding of differences. Probably the conclusion that two cultures do not really differ in some respect is inherently less interesting and satisfying than the conclusion that they do differ. In any case, perusal of the literature suggests that the bias in favor of finding differences is strong.

One of the most likely threats to validity of statistical conclusions is that the bias toward finding differences leads to multiple statistical tests, usually without allowance for multiple testing. If one looks at enough outcome measures, e.g., examine tests item-by-item, one is almost bound to find some significant differences. Or, if one tests data in enough different ways, one increases the likelihood that one or the other of the tests will cross the threshold of statistical significance. If all analyses are actually reported, readers, at least sophisticated ones, can make allowances for multiple tests and decide how much reliance to put on reported findings. Probably most researchers will report any analysis involving multiple items since to do otherwise would be to invite scepticism. If the number of items in a set is of reasonable size and if the content is diverse, it is highly likely that two cultural groups will differ in their responses. It is even likely that they will differ on ***more items than would be expected by chance.*** The problem is, though, that if, let us say,

differences could have been expected on ten items by chance, differences are actually found on 20 items, it is difficult to know on which items the observed differences are dependable. In most studies I have examined, investigators tend to treat all observed differences as if they were dependable. The net result is that the responses of two cultures appear to be much more different than they probably are. More serious problems occur if investigators do not report all their analyses; failure to do so should be regarded as an ethical problem. Unfortunately, however, editorial policies may inhibit complete reporting.

One step that should be routinely taken to reduce problems with statistical conclusions stemming from multiple tests is to do overall tests of significance before doing univariate tests as on individual items or outcome measures. A second step is for a researcher to state detailed hypotheses ahead of time about the measures on which they expect to find differences. If investigators are able to identify ahead of time the 20 out of 50 items on which they expect to find differences and then show that it is precisely on those 20 items that they find differences, concerns about chance differences are much reduced.

Three characteristics of studies have the potential of reducing the probability that true differences will be detected and hence operate against the bias toward finding differences between cultures: low statistical power, unreliable measures and large error variance. I have already mentioned the problem of low statistical power for detecting differences. The response of some investigators to the problem may be to increase sample

ze beyond reason. Large samples enable the detection of small ifferences, but sometimes that increased sensitivity is not ithout other costs. If access to a population is limited or therwise difficult, attempts to increase sample size may lead o relaxing criteria for membership in the population or to nclusion of marginally representative persons. If there are only 0 adult male Bnobes available and power analyses suggest that 5 are really needed, it may be tempting to redefine "adult" so s to include boys down to the age of 14. Or it may be tempting o include some adult males from the adjacent Knobes, who are, fter all, a very similar group. Such practices can result in nvalid conclusions, either because they change the mean value the group in relation to that of the comparison group or ecause they increase the variability within the group and ecrease the sensitivity of the statistical test of differences.

nreliable measures will, for obvious reasons, jeopardize the egitimacy of statistical conclusions about cultures. Hence, in nterpreting cross-cultural data, it is important to have stimates of the reliability of measures within each culture. It easy to see that if a measure being used is less reliable in the ross-cultural context than it is usually found to be, differences etween cultures may go undetected. What is not so apparent is nat a measure may actually be more reliable than usual when sed in cross-cultural studies, and, therefore, differences may e determined to be significant that would not be significant in ost psychological studies. Cross-cultural psychologists may on ccasion go to unusual lengths to assure reliability of measures,

e.g., by adding more trials, and so bias chances of finding differences in their favour.

Related to the problem of reliable measures is the problem of reducing error variance. For example, if one simply introduces a measure to potential subjects by saying, ***Here, fill this out, please***, subjects will approach the task with widely different sets or intentions, and a great deal of variability in performances or scores will result. If, on the other hand, instructions are quite precise and focused, extraneous variability will be minimized. In the latter case, chances of finding differences between cultures will be enhanced. To the extent that cross-cultural investigators may be more precise than other researchers, their chances of finding differences will be increased, but so also will be the idiosyncracy of the results.

A second major factor disposing to a concentration on cultural differences is that we lack statistics for similarity. We can readily test for the occurrence of differences between cultures; we find it difficult to test for similarities between them. Similarity must ordinarily be inferred from a failure to find interesting differences. The discovery that two cultures differ in the frequency with which misbehaviour of children is met with withdrawal of love, may tend to obscure the fact that both cultures use that form of discipline infrequently. We do have statistics for profile similarity and factor similarity, and other aspects of similarity should receive greater consideration (see Gregson, 1975).

Our philosophy of science tells us, of course, that no two things are ever likely to be quite the same, and even if they were, we would have no way of knowing that the conclusion of identity were not in error. Or, as we say, one cannot prove the null hypothesis. We tend, grudgingly, to accept the null hypothesis only when we cannot conclude the alternative. Still, we want sometimes to accept the null hypothesis, but we lack rules for doing so. (In South Africa, the white government want to conclude that ***black tribal groups are different from each other***, and some black groups want to conclude that ***black tribal groups are the same***). Ideally, researchers should specify in advance the findings or evidence that would lead them to conclude that the null would be acceptable, e.g., that two groups would be considered the same if they did not differ by at least so much on so many variables, or that a correlation would be considered to be zero if it did not reach such and such a value. After the data are in, the conclusion is more problematic since so many variables can contribute to a null finding. Berry's (1966) finding that male and female children among the Temne do not differ in field independence because the processes of socialization are the same would have been more persuasive had the no difference finding been predicted in advance.

The nonequivalent comparison group design (Reichardt, 1979), which is the implicit "design" of most cross-cultural studies, presents particular problems with respect to the null, or no difference, conclusion. In effect, what we want to be able to do in a nonequivalent comparison group design is assert that the groups to be compared are not different prior to the

intervention to be tested. Obviously, that assertion is of the null hypothesis, and we are on weak grounds in asserting it if we can adduce no better evidence than two groups are not significantly different. In most cross-cultural studies, we assume that the phenomena of interest would not have been different had the populations in the two cultures been in some manner interchanged at birth. The temptation in instances in which samples from two populations are obviously different at the beginning is to try to correct statistically for that difference, e.g., by using a covariate and analysis of covariance. Such procedures are never satisfactory since they are almost always undercorrect for the initial difference but by an unknown amount. Cross-cultural researchers would do well to examine Reichardt's (1979) work carefully.

ALTERNATIVE RESEARCH DESIGNS AND STRATEGIES

In my opinion, cross-cultural researchers have been too narrow in their approaches to design and analysis of their studies. Most studies have employed some version of the nonequivalent comparison group design analyzed as if it were a true experiment. The interesting problems in cross-cultural psychology are more diverse than that design narrowness would imply, and there are a number of alternative strategies that should be explored. Without trying at all to be exhaustive, I will describe some illustrative possibilities here.

The concomitant time series analysis (Mark, 1979) can be interesting and even powerful when matched to the right

questions. For example, if one had data from two cultures over a number of years, one might determine whether the cultures displayed similar histories with respect to the phenomena involved. The actual years studied would not even have to be the same if one were interested in equating the two cultures for stages in development. If one were interested in occurrence of family violence in relation to "modernization," for instance, one might assemble data on within family homicides for two cultures in the same stages of modernization but actually widely separated in time.

Similarly, the interrupted time series (McCleary and Hay, 1980) can be useful in studying the responses of cultures to relatively discrete events. One might show that changes in rates of marital dissolution occur in relation to the same event across cultures even though the particular cultures are afflicted with those events at quite disparate times. Or, one might use data from two cultures differing with respect to an event to show the specific effects of the event when it is introduced into one culture and not into the other, e.g., effects of the introduction of television on crime rates.

Causal modeling should also be considered as a strategy and design alternative. Causal modeling permits more sophisticated conceptual structures to be examined, and culture can be included as a causal variable in the models tested. Path analysis and covariance structure analysis are alternative approaches with the measurement requirements of the former being less demanding, although with a compensatory decrement

in sophistication of the model. Causal modeling would permit the investigator to enter known characteristics of cultures into the analysis and still preserve culture as a variable to subsume all unmeasured attributes. To do causal modeling does, however, require that one have a reasonably good model or theory to begin with; one cannot simply dump all one's data into a computer to see what comes out.

In the long run, our salvation in cross-cultural studies will lie not in different, or even better, designs and statistics, but in better theory, better thinking in the first place. One of the advantages of causal modeling is that it requires sharper thinking as a beginning point. We must, in any case, if we are to progress, identify the characteristics of cultures that are critical to our conceptions and analyze specifically for those effects; global "culture" is not a sufficiently informative variable.

Lykken (1968) has discussed the need in psychology for more replications of a "constructive" kind. By that he means replications that triangulate in on the variable(s) of critical interest. Exact replications are less than maximally informative since any errors, biases, or misspecifications are simply repeated in the second study. A preferable strategy is to allow presumably irrelevant design features to vary across replications, to achieve what Campbell and Stanley (1966) call ***heterogeneity of irrelevancies.*** That sort of replication is much needed in cross-cultural psychology and would be manifested in multicultural studies, whether serial or simultaneous, in which

cultures are selected for study because they have in common the attribute of interest and vary in many others. The conduct of such studies presents formidable obstacles, but that should not deter us absolutely.

SAMPLING PROBLEMS

Social scientists know by now, as do many other educated people, that if one wants to say something about a population on the basis of data from a sample, the sample must be representative of the population. Ordinarily, the best way to maximize representativeness is to ensure that the sample is drawn randomly from the population. Strictly speaking, though, we almost never have random samples of any population of interest. Aside from the not inconsiderable problems of getting samples that are random, there is the problem that populations of real interest are rarely enumerable and confined by space and time. We might be able to get a nearly random sample of South African students enrolled in institutions of higher education on December 1, 1985, so as to answer questions about the relationship between certain political and social attitudes. But the population to which we really would want to generalize would not be to that finite population existing on that day and in that space. We would be disappointed in the extreme if our findings were rejected on January 15, 1986, on the grounds that students enrolled at that time would be a different population from those enrolled on the earlier date. In fact, we would probably be hoping to make some general statements about attitudes and students that we would suppose should have held

during the previous year before some of our December 1985 college student population would ever have become students. What we must do, of course, is make the assumption that attitudes and their structures remain fairly constant in student populations, that they do not change greatly as some students drop out and others enter the population. As long as the changes in the population are small, the assumption is warranted, but we would need to pay attention to the possibility of drift over time even if changes during any brief period were small in size.

We all recognize the need for representative samples if we are to generalize to populations. That makes our casual approach to sampling in the conduct of experiments (and other studies) quite paradoxical. We would reject out of hand any proposition about politics in South Africa if it were based on the responses of students in one university, let alone if it were based on the responses of students in psychology classes in one university. Yet we accept without question propositions about how the mind works based exactly on such limited, unrepresentative samples. But note that the same sort of thing characterizes research with animals. Who ever had a random sample of rats or monkeys? Yet even our most rigorous scientists generalize freely about the behaviours of rats and monkeys based on the few they happened to have available in their laboratories when they got around to doing their studies.

All of our work depends on the important assumptions of homogeneity of structure and function in the organisms with which we deal, whether animal or human. No anatomist has

ver examined a random sample of humans (or rats), and yet we eadily accept the generalizations of anatomists, including euroanatomists, about the arrangements of the organs and tructures of the body. We would be quite shocked if someday a ribe were found in which most persons were left-handed and in hich the right hemisphere of the brain were dominant. It just ould not happen. Normal body temperature is 37°C. lyelinated nerve fibers conduct impulses more rapidly in veryone. We know these things inductively, but we also know hem at some deeper, intuitive level. We have a sense of how hings must be.

imilarly, we assume homogeneity of psychological processes cross persons and populations of persons. We also know that uch processes are homogeneous for both inductive and intuitive easons. Many years ago, Skinner and his students discovered he partial reinforcement effect. That effect has been widely emonstrated and has been found in every species in which it as testable. I do not ever expect to hear of a population in hich it does not hold. I am confident that whatever ultimately urns out to be true of differences between short-term and ong-term memory will be true in all humans everywhere. I do ot expect at all to hear of a subset of humans in which haracteristics of memory are quite different from those of the ollege sophomores, American at that, with whom most memory esearch is done.

s in other research, we assume homogeneity of structure and unction in cross-cultural research. That assumption may not be

justified for all phenomena, but the closer we get to fundamental psychological processes, the more likely the assumption is to hold. We might not want to trust a small, convenience sample of residents of a community as a basis for deriving estimates about how often parents punish their children, but we might have a great deal of confidence in generalizing about how people in that community go about solving subtraction problems or about how those people react to a particular perceptual illusion. We should, however, be more aware of our assumptions of homogeneity so that we can reach consensus about when they are reasonable.

REPRESENTATIVE AND EQUIVALENT RESPONDENTS

When we want to generalize to a population, we want a sample that is as representative as possible from that population, an aim that is often best achieved by drawing a sample randomly from the population. There are times, however, when a conflict may occur between our interests in having a sample that is representative within a culture and our interests in having samples that are really comparable or equivalent across cultures (Osgood, May and Miron, 1975). Random samples of white and black residents of the Johannesburg area, for example, would be representative of each culture, but comparisons between them on many variables might not make sense. For many reasons, it might be preferable to have samples of white and black college graduates or white and black students, or white and black small business owners so as to have samples of persons occupying equivalent positions within each group. In our study of attitudes

toward mental disorders in three cultures, Sechrest, Fay, Zaidi and Flores (1973) elected to focus on college students to provide for better equivalence in terms of social position at the sacrifice of representativeness of each group within each culture. Often a tradeoff must be made between representativeness and equivalence, and it may not always be clear in just which direction the trade should be.

In much cross-cultural research in psychology, the sampling procedure is haphazard at best. I will give one example here but without a reference since, although I dislike the procedure, it is so common that the study in question should not be singled out for opprobrium:

> *...subjects consisted of 850 adults, 50 years and older, drawn from urban areas in 7 countries...Potential volunteers were contacted individually, through churches, employment settings such as offices, public settings such as markets and parks, and educational institutions... Initial attempts to obtain only subjects 65 years of age were abandoned in favor of a more reasonable criteria (sic)—age 50.*

The research question in the study had to do with the frequency and nature of sleep problems among the elderly in various countries. Whether sleep problems are sufficiently homogeneous within elderly populations that sampling issues are moot is, one supposes, in need of consideration, but I would be dubious of invoking the homogeneity assumption in such a case. The major problem is that no sampling plan of any kind is evident in the report of the study. There was no justification of sample size so that one does not know whether 850 total subjects were

sufficient or not. One does not know whether the seemingly haphazard procedure produced equivalence at all. The study does not report the number of subjects within each country that were obtained by the various methods followed.

A more serious problem with the study is that it was so completely atheoretical that there was no rationale for any of the specific features of the study. The seven countries studied were not chosen because of specific characteristics that would make them interesting in relation to sleep problems. There was no basis on which to anticipate findings so that sample size could not very well have been established in advance. In fact, probably nothing so well illustrates the opportunistic nature of the study than the fact that the plan to confine the sample to persons over the age of 65 was quickly abandoned out of expediency.

One more point to be made about the study is that it was not really cross-cultural but cross-national. A fundamental rule of cross-cultural research, particularly research that is addressed to the testing of theory, is that ***nation does not equal culture.*** If cross-cultural researchers were forced to delineate the cultural features of interest in their studies, they would not be able so easily to slide into the assumption that samples obtained from two nations necessarily differ in any way(s) of interest.

ALTERNATIVE APPROACHES ARE NEEDED

In essence every cross-cultural study is a quasi-experiment

rected at discovering the effects of culture as a virtual (in the nse of computer jargon) independent variable. Test items, for kample, are counterparts of experimental stimuli, and our terest is in how the independent variable (culture) affects sponses to the stimuli. I would argue, though, for extending to oss-cultural psychology the benefits of contemporary xperimentation and basic theory with (often) simpler stimuli, etter defined and under better control. Memory, cognition, cial cognition, information processing, natural language ocessing, and many other topics of current interest could enefit from systematic testing in a cross-cultural context.

ne usefulness of cross-cultural data would be increased bstantially if more effort and resources were devoted to epeated assessments and longitudinal studies. Interesting uestions arise about changes in cultures in relation to changes persons living in those cultures. For example, with increasing xposure to western culture, we expect to find changes in sceptibility to certain perceptual illusions, but do those anges occur in the same degree in all persons or do they, erhaps, occur only in younger persons? In order to do useful ngitudinal cross-cultural research, we need to develop easures of cultural as well as individual change. Quantifying lture is not a simple matter.

ualitative methods should not be abandoned; they are likely to e of great value if systematically and carefully employed. We adly need better specification of our qualitative methods, mewhat similar to the specifications for conduct of

experiments. I do believe in that regard that the methodological book being produced by Lonner and Berry (in press) will be quite helpful. Nonetheless, there is very little agreement on how to go about in most qualitative studies.

Consider the case study, for example. Suppose one wanted to do a case study relevant to some hypothesis about social position in a culture and a psychological variable, e.g., depression. The first problem to be considered would be how to select the case. For obvious reasons, cases to be studied are almost always selected after the fact, i.e., after the events of critical interest have already happened. That leads inevitably to serious problems with respect to any causal interpretation. Moreover, it is likely that cases are selected for study and presentation not because they are representative but because they are unusual. Specifications should also be available for how measures are selected for study in particular cases. In particular, case studies should be carried out in such a way as to permit examination of plausible rival hypotheses (Campbell and Stanley, 1966), i.e., explanations alternative to the one favoured by the investigator. That means that measures must be obtained that would shed light on alternatives.

Case studies, as do many other more qualitative methodologies, depend very critically upon the acumen and biases of the persons doing them. It is usually easy to raise the question whether another person doing a case study would arrive at the same conclusions. If we are to take results of qualitative studies seriously, then we must take seriously the task of implementing

qualitative methodologies. It is sometimes argued that only a case study can show the richness or complexity of some phenomenon or even that, in some circumstances, a case study is the only method possible. Such arguments do not, however, rule out the possibility of replicated case studies, e.g., by having two or more persons doing independent case studies. In such a manner case studies may be converted from N = 1 to N = 1, N-at-a-time studies. I note that multiple case studies run counter to the usual ethnographic tradition, but that is not a reason to resist them. If multiple case studies cannot be done, then one certainly may be able to assemble the data in such a way that it can be assessed by multiple interpreters. By using several different persons to interpret the data, one could at least eliminate biases at that stage in the process.

We also need to begin to appreciate and apply methods for data synthesis in cross-cultural research. It is rare that any important scientific question is answerable within the context of a single study. Most questions require many studies before we begin to focus on an answer if, indeed, we are ever able to do so. More often than not, however, a body of research studies within a given area is not univocal, with various studies producing different and sometimes conflicting findings. When that is the case, a method for synthesizing the literature is required. Most data syntheses until recently have been impressionistic literature reviews, but more systematic methods are being developed and used in other areas. Meta-analysis (Glass, McGaw, and Smith, 1981) is one such method, one that is quantitative and objective, although not without problems.

Another quantitative method, but one less satisfactory than meta-analysis is "box score"; counting up of findings for and against a hypothesis, usually without much attention to the size of differences and the sample size for each study. Another method that may sometimes be worthy of consideration is the use of a consensus panel of experts (Vinokur, et al., 1985) in a position to give an opinion that may be authoritative. Finally, mention should also be made of the need for secondary analysis of data sets by investigators other than the one involved in the original analysis. Secondary analysis permits investigators with fresh points of view or with different approaches to the analysis to examine data with the aim of reinterpretation. Cross-cultural research has a tradition for such analysis in the Human Relations Area Files (Murdock, 1965), and that tradition should be encouraged. Cross-cultural psychologists should lead the way in offering to make their data available to any other investigator with a legitimate interest in them..

DO WE NEED MORE AND BETTER METHODS?

An old story tells of a young agricultural expert who went to visit a farmer to examine his methods of farming. Toward the end of the visit, the youthful expert told the farmer that if he would just listen to some advice, he could double the output of his farm. The farmer declined the advice with the reply, ***I'm only farming half as well as I know how already.***

I have a sense that when it comes to research methodology, that comes close to being an analogy to cross-cultural psychology.

Malpass and Poortinga (in press) believe that the impact of methodology on cross-cultural research has been limited. That rings true to me, and I suspect that if we are to see much improvement, it will have to come from raising our sights, our aspirations, rather than from the achievement of some new methodological insights. Improvement will have to come about because individual investigators resolve to do better research and not settle for what is convenient and easy and because journal editors and referees determine that research must meet higher standards if it is to be published. If cross-cultural psychological research is deficient and disappointing, it is we who are to blame.

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3.

CROSS-CULTURAL COGNITIVE RESEARCH: SOME METHODOLOGICAL PROBLEMS AND PROSPECTS

J.M. Verster

INTRODUCTION

The aims in this paper are to provide a brief overview of some central methodological problems in cross-cultural cognitive research and to review the progress being made in coming to grips with them. The focus is primarily on measurement studies, while experimental, developmental, or other research themes are implicated only indirectly. This in no way signifies an assumption that measurement studies are somehow more important or even more desirable than other types of cognitive research. Regrettably, no original methodological breakthroughs or exciting new empirical results from my own research programme are offered. Rather, I draw upon advances being made by leading exponents of the discipline internationally in the hope that South African researchers might find in these contributions something of value to help improve the prospects for success in their own research endeavours.

The discussion begins with a brief reconsideration of the goals in cross-cultural cognitive research. Next, the historical roots of present methodological concerns are briefly recalled. Special attention is paid to contributions from research conducted in Africa. More detailed attention is then devoted to three of the most important methodological problems in contemporary cross-cultural cognitive research. These are the problem of inferring meaning from dependent variable measures, the problem of defining relevant independent variables, and the problem of measurement comparability across divergent cultures. To conclude, some remarks are offered on likely future developments in cross-cultural cognitive research.

A RECONSIDERATION OF GOALS IN CROSS-CULTURAL COGNITIVE RESEARCH

Cross-cultural research in psychology is directed at the systematic study of human behavioural variations under the influence of different cultural conditions. There are three key elements in this assertion. First, that variations in human **behaviour** constitute the focal point of interest. Second, that differences in **cultural context** constitute a source of explanatory variables. And third, that the enterprise is a **comparative** one, to be carried out in a systematic fashion.

This brief formulation is consonant with the views of many earlier writers who have addressed themselves to the question of aims in cross-cultural psychology (Biesheuvel, 1958; Poortinga, 1971; Eckensberger, 1972; Triandis, Malpass and Davidson,

1972; Brislin, Lonner and Thorndike, 1973; Berry and Dasen, 1974). Lijphart (1971) has emphasized the point that cross-cultural psychology, unlike other branches of the parent discipline, is defined by its **method**, which is comparative, rather than by its content or the type of population it addresses. For this reason Berry (1980) argues that a more appropriate label for the enterprise we call cross-cultural psychology would have been **comparative psychology**. Regrettably, through an accident of history the latter label has come to be associated exclusively with phylogenetic, or cross-species comparisons in psychology. Variations in psychological functions related to culture, language, age, sex, or other factors have been excluded from the domain of comparative psychology, as this label has come to be used. An explicit identity thus needs to be made between the term "cross-cultural" in psychology and the term "comparative" in other disciplines such as sociology, anthropology, economics and political studies.

In keeping with this view the scope of cross-cultural psychology has been proclaimed by several prominent advocates as encompassing the full spectrum of ecological, social and biological variables, as well as cultural variables, in the comparative study of behavioural variation (Berry, 1966; 1969; 1976; Dawson, 1969; 1971; Jahoda, 1970). The goals of this enterprise have been summarized by Berry and Dasen (1974) as follows:

1. To understand the **range, variability** and qualititative **differences** in human behaviour as a function of cultural

(including biological, ecological and social) variables;

2. To understand the **uniformities**, or cross-cultural **consistencies** in human behaviour with a view to **generalization** and the development of pan-human psychological theory.

The first goal emphasises a **within** culture search for the **causes** of behavioural variation, while the second stresses the need to look **across** cultural systems for those behavioural characteristics that may be considered **universal** for all mankind, or at least generalizable to more than one society.

In essence there is no difference between the goals of cross-cultural psychology in general and cross-cultural cognitive research in particular. The distinction between cognition and other themes in psychology is a traditional, more than a scientifically justified one. Bateson (1936) used the terms "ethos" and "eidos" to distinguish between two historical orientations in cross-cultural studies of behaviour. "Ethos" refers to the socio-emotional aspects of behaviour, which formed the focus of attention in the now discredited "old style" culture-and-personality studies (Lonner and Triandis, 1980) while "eidos" refers to purely cognitive behavioural aspects which have a much shorter, if less flamboyant history in cross-cultural research.

Berry and Dasen (1974) specify three goals for a comparative cognitive psychology from the point of view of Western

psychology. Since these are appropriate to the concerns that are relevant to the present discussion, they are reproduced here for convenient referral:

1. To **transport** our present hypotheses and laws to other cultural settings to **test** their applicability or generalizability;

2. To **explore** new cultural systems to discover cognitive **variations** and differences we have not experienced within our own cultural context;

3. To **compare** our prior understanding with our newer knowledge within diverse cultures to **generate** more universal descriptions, hypotheses and laws of human cognitive functioning.

Hence the comparative method in cognitive research is not employed for the empty purpose of weighing the cognitive capabilities in one culture against norms and standards established in another. Nor is it limited to a mere testing of indigenous hypotheses and techniques in exotic contexts. It is directed at the wider possibility of expanding present knowledge with a view to achieving a universally applicable understanding of the causal mechanisms underlying variations in cognitive processes. In this sense the last of the three aims is acknowledged as the most important (Frijda and Jahoda, 1966; Berry, 1969).

LEGACY OF EARLY CULTURE- AND -COGNITION STUDIES

Among the most widely recognised and prominent pioneers of research into culture and cognition are Rivers (1901, 1905), Wundt (1916), Boas (1911) and Lèvy-Bruhl (1910; 1926). The first mentioned two approached their research chiefly from a psychological perspective, while the latter two leaned towards an anthropological perspective. It is important to recall this as cross-cultural research has its roots in both intellectual traditions and is claimed by some, such as Barry (1979) to have succeeded in combining some of the best, but also some of the weakest features of both parent disciplines in its conceptual and methodological repertoire.

Of special interest when considering the legacy inherited from these early pioneers of culture and cognition is the fact that their major conclusions do not lend themselves to a classification on the basis of the disciplinary perspective from which their observations were made. The essential conclusion arrived at by Rivers on the basis of comparative psychological measurement was that cognitive behaviour is culturally conditioned. He argued that the context in so-called primitive cultures predisposes the inhabitants towards a **concrete** mode of perception. This was considered to inhibit the development of higher cognitive processes such as **abstract** thinking which he believed to constitute the essence of Western cognitive behaviour. Wundt, working within the framework of his folk psychology, arrived at the very different conclusion that ***the intellectual endowment of primitive man is approximately equal***

to that of civilized man. He qualified this only in stating that *primitive man merely exercises his ability in a more restricted field.* This conclusion is echoed in work of Boas, based on anthropological field observations. Boas deduced from his comparative study of "primitive" and "civilized" man that "in reality the fundamental traits of mind are the same" leading him to formulate his celebrated notion of the "psychic unity of mankind". For Boas the mind of "primitive" man differed from that of "civilized" man only in terms of content, derived from differing cultural contexts and not in terms of process. Also proceeding from an anthropological perspective, Lèvy-Bruhl saw "primitive" men as mental infants whose thought processes were "prelogical".

From this very brief summary it can be seen that in this early work we have the genesis of a debate on cultural relativism versus universality in human cognition. We see too, that the methods of research available to the early investigators were not capable of resolving the dispute, nor did methodology lead to consistency in interpretation across researchers or studies. We see as well the pervasive influence of prejudice and ethnocentrism colouring the inferences made from measurement and observation, and we see shortcomings in the attempts to ascribe meaning to behavioural variables, to define context variables, and to achieve a scientifically acceptable basis for comparison across cultures. It was some time after the contributions of these early forerunners that questions of method came to be explicitly recognised as crucial to the further advancement of cross-cultural cognitive research.

CONTRIBUTIONS FROM SUB-SAHARAN AFRICA

Cross-cultural research on the cognitive aptitudes, abilities and processes of the peoples of Africa has a history of over seventy years. During most of this period the impetus has come from pragmatic considerations arising from the need to classify or select individuals for purposes of educational or occupational advancement. By far the greater part of this work has been carried out by expatriates from Western Europe or North America, or by their cultural descendants. Andor's (1966) first annotated bibliography, representing the published literature up until about twenty years ago, reveals that many of the problems cited in the preceding section were embodied in this early African work. Hence examples of ethnocentric prejudice, involving characterizations of African intellect as simple, concrete, or prelogical abound, as do uncritical applications of tests and other assessment procedures transported from abroad. Little or no attention was given in the early literature to questions of construct validity or measurement equivalence in comparative studies.

Fick's (1929; 1939) work, which seemed to provide scientific justification for the prevailing view that Africans were inherently inferior to whites, represents the culmination of the early colonial research tradition in Africa. This type of work, in which African abilities were assessed against Western norms on tests developed for application in Western cultural contexts was decisively terminated by the critical assessments of Biesheuvel (1943) and others. Biesheuvel stressed the need for a closer look

at the influence of environmental context variables, including culture, home life, education and instruction on African intellectual development. His central methodological argument was that the scores of Africans and Europeans on Western intelligence tests were not comparable. This was due primarily to a differential familiarity with the content and materials employed in such tests.

There followed a period of more cautious attention to methodological detail in African cross-cultural research. In particular, great care was taken to adapt the item content of Western tests to suit local conditions. MacDonald's (1945) adaptation of aptitude tests for the selection of African soldiers best exemplifies this type of study. But it soon became clear (Biesheuvel, 1949) that even careful adaptations of this nature carried no guarantee that test scores could be considered comparable. For example with regard to the translation of pictorial material in so-called non-verbal test items, Biesheuvel (1949:8) argued: ***To make the object pictured culturally meaningful is of little avail, if pictorial representation itself is unfamiliar, and if it does not evoke an attitude of interpretation which a European group automatically assumes.*** The point clearly made is that in order to test cognitive abilities in a given culture, it is necessary to have a good understanding of the skills and knowledge base of the target population so that culturally appropriate tasks can be set in the test items.

While attention shifted from attempts to compare African and European performance directly on cognitive ability tests, there

emained during the period following the Second World War a preoccupation with efforts to test the validity of Western ability theory and constructs in an African context. Simultaneously, increased effort was directed at the need for techniques with which to assess African candidates for placement in Western-orientated educational and occupational institutions. Hence the development and application of tests of adaptability (Biesheuvel, 1949; 1952a; 1952b) and educability (Ombredane *et al.*, 1956; Laroche 1959) dominated interest during the nineteen fifties and sixties. Regrettably, however, little basic research effort was directed at the same time to the study of African cognitive competence within the context of its own culturally derived meaning systems, using culturally relevant criteria against which to evaluate performance. Work by Cole and associates (Cole, Gay, Glick, & Sharp, 1971) in the field of anthropological cognition provides a good example of the type of basic research that was needed on a wider scale.

Nonetheless, within the constraints set by the need for applied, problem-oriented research, several important basic issues did come under the research spotlight, causing an increasing shadow of doubt to be cast over the validity and applicability of Western psychometric theories and constructs in the African cultural context. Experience with tests in the U.S.A., U.K. and elsewhere in the developed world had generated a comforting body of well replicated findings. Among these were, for example, the view that psychometric tests, particularly of the so-called non-verbal, culture-fair, or fluid intelligence variety were fairly resistant to the effects of practice, coaching,

retesting and the like; that both means and correlation patterns remained stable under a variety of experimentally induced interventions; that females typically outperformed males on verbal tests; and that environmental variables such as social class, family size, sibling order and socio-economic status consistently and predictably correlated with scores on intelligence tests. As the evidence from test applications in the African context came in, these pillars of Western psychometric theory progressively crumbled.

First to go was the belief in the robustness of test scores in the face of coaching and practice effects. McFie (1961), Lloyd and Pidgeon (1961), Silvey (1963) and others demonstrated large and significant test score gains for African subjects following training or coaching. Even the correlation patterns of so-called culture-fair, or fluid "g" measures, with a non-verbal or figural item content were shown to change substantially following practice. This work, which has been well replicated and amplified in subsequent studies, including in the South African context (Pons, 1974; Crawford-Nutt, 1976; M.A. Verster, 1973; 1978; J.M. Verster, Muller and Kendall, 1976; 1977;) has given rise to a serious questioning of the meaning of test scores, particularly when administered to non-Western subjects. The conventional wisdom that females outperform males on verbal tests, giving rise to theories of biologically-based female precocity in verbal development has been strongly contradicted in the African testing literature (Irvine, 1966; 1969; Drenth, Van der Flier and Omari, 1979). These findings are echoed in cross-cultural studies on verbal abilities conducted in other

parts of the world, including Germany (Preston, 1962) and Japan (Butcher, 1969) where males generally outperform females on verbal tests. Finally, the stable pattern of correlations expected between test scores and environmental variables such as socio-economic status or social class, education, sibling order and family size could not be replicated in African test applications (Irvine, 1969).

The cumulative weight of these and other contradictory findings in the testing literature of sub-Saharan Africa (see Kendall, M.A. Verster and Von Mollendorf, in press) represent a serious challenge to conventional, or classical test theory as developed primarily in North America and Europe. They point to the need for a systematic reappraisal of psychological theory in general, from the viewpoint of cross-cultural validity.

THE MEANING OF DEPENDENT VARIABLES

We turn now to what is certainly one of the most important issues facing cross-cultural researchers in any context, namely the problem of interpreting the **meaning** of dependent variable measures. As we have seen cross-cultural research essentially involves the study of **behaviour** in relation to **culture.** Behaviours therefore constitute the **dependent** variables in research designs, while the cultural context in its widest sense represents a source of **independent** variables.

Irvine (1983) has outlined a valuable framework for dealing with the problem of inferring meaning from test scores in

cross-cultural cognitive research. This amounts to a study of the issue of cross-cultural construct validation. On the basis of established knowledge about tests derived from research in North America and other Western contexts, he argues that inferences about test score meanings have come from three types of empirical relationships: correlations with criteria, correlations with other tests and correlations with extraneous, non-test variables. Inferences from these three classes of relationship jointly help to establish the meaning of the construct assessed by the test. Findings from the three types of correlation are considered briefly below.

1. **Correlations with criteria**

Criterion correlations provide an operational definition of tests. In the case of cognitive tests, criteria have come mostly from the real world performance domains of education and occupational life. Applied Western psychometric research has produced an impressive body of data testifying to the stable relationships between scores on ability or intelligence tests and criterion measures representing academic achievement or success in vocational training and subsequent occupational advancement. Indeed, the history of mental ability testing is closely tied to the need to predict success in these domains with a view to classification, selection, or placement.

Criterion correlations are said to indicate the test's

predictive validity when performance subsequent to testing constitutes the criterion. An example would be a pass/fail measure at the end of a study or training programme. When test scores are correlated with simultaneously obtained measures of criterion performance, the relationship denotes the test's concurrent validity. An example here would be a correlation between test scores and concurrently obtained teacher's or supervisor's ratings of the subjects tested.

Although test-criterion correlations in the African context offer many parallels to results from the Western World, as testified in Ord's (1972) well-known monograph and the accumulated data collected by NIPR in prediction studies spanning three decades, this does not constitute sufficient grounds for assuming that the operational meaning of our tests has been securely established. There are important anomalies or differences in the pattern of results that cannot be explained exclusively in terms of known differences in the criteria.

Despite Irvine's (1983) claim that the operational meaning of tests in Africa, as in North America, can be classified into broad categories of behaviour such as verbal skills, reasoning, mechanical-spatial skills, numerical facility, speed of encoding on perception, or dexterity and psychomotor quickness, particular tests do

not always provide the same kinds of criterion correlations in the two cultural contexts. Moreover as Cronbach (1983) has noted, some writers claim that almost any cognitive test has validity for almost any performance criterion, giving rise to the disconcerting doctrine of "validity generalization". Transposing this to a cross-cultural setting, Cronbach raises the point that if test correlations with performance criteria were indeed much the same everywhere, generalizations about universal relationships would be strong, but by the same token, interest in the causal role in behaviour played by cultural factors would be eliminated. We know too well, from attempts at performance predictions among black and white South Africans that the issue of differential validity, based on cultural and other differences, cannot be dismissed.

2. Correlations with other tests

The establishment of correlations between a test of interest and other known tests has long been the method of choice among test constructors wishing to demonstrate the construct validity of their instruments. Indeed, it is probably fair to say that many test makers regard this practice as a sufficient means of establishing construct validity. Inter-test correlations are generally studied by means of factor analysis and the test's loading on a predicted factor is taken as conclusive evidence of its construct identity. Yet from the

accumulated heritage of literally thousands of factor studies of ability tests in the published literature we now know that factor analyses alone is not enough.

There is a large literature on the methodological shortcomings of factor analysis as a means of establishing construct meaning. Problems commonly mentioned include the non-falsifiability of factor-based theories, the indeterminacy problem in factor rotation, the problem of sampling arbitrariness with regard to both the universe of potential tests and the universe of potential subjects, the individual differences problem, based on a recognition that factor analysis proceeds from inter-individual rather than intra-individual variations in performance and the failure of factor analysis to penetrate to the level of specifying underlying processes in the domains of measurement. As Irvine rightly points out: ***Certainty about what a test score means is hard to come by through factor analysis, since an infinite number of tests and an infinite number of subjects has never been available to exhaust all possibilities*** (1983: 46).

Nonetheless, factor analysis can be accepted as a useful means of achieving a gross classification of categories of behaviour. Irvine reminds us that this is the role initially intended for factor analysis by its originator, Charles Spearman, who in his early writings (Spearman, 1927) stressed that factor analysis offers us no more

than a rough indication of where to look for the causes of behavioural variation. We should not make the mistake of seeing factors as entities with dispositional, if not causal properties. Neither should they be ascribed status as structural elements describing the intra-individual organization of mental life.

Notwithstanding, an invariance of factor structures across populations would provide strong support for claims of equivalence of meaning with regard to underlying cognitive dimensions or processes. Faced with the reality of cross-cultural variations in factor structures both within (Grant, 1971) and across populations (Irvine and Carroll, 1980; Verster, in press (a)) however, we are left with the unresolved problem of inferring the meaning of our dependent variable measures.

3. Correlations with non-test variables

The third class of correlation to be considered in cross-cultural construct validation, according to Irvine (1983) has to do with relationships between test scores and extraneous, non-test variables. These may be either attributes of the individual, termed "disposition variables", or attributes of the surrounding ecocultural context, termed "environmental variables". Examples of disposition variables commonly studied in Western contexts include age, sex, hormonal influences, degree

of genetic similarity and rate of physical maturation. Environmental variables of interest would include socio-economic status (SES), ethnic group classification, ecological press, family size and birth order. A third source of extraneous variables recognized only implicitly by Irvine includes experimental, or artificially manipulated variables.

Non-test variables involving only a small inferential step in causal attribution with regard to performance variations on tests are defined by Irvine (1983) as "low inference variables". These include prior practice or coaching on tests, as well as streaming and placement in schools, or even school quality. In contrast, "high inference variables", including most disposition and environmental variables mentioned above, involve a large inferential leap in causal attribution, assuming they correlate with test scores at all.

Irvine (1983) points out that general conclusions from North American research indicate that low inference variables, such as practice and coaching, have weak or neglible effects on test scores, whereas high inference variables, including SES, ethnicity, birth order and the like, have shown robust and substantial effects. These results have encouraged the view that tests are resistant to experimental manipulation and measure stable dispositional qualities. Yet in the African testing data, a rather contrary pattern of results has been obtained,

as summarized in Table 1.

In Table 1 it can be seen that it is precisely those low inference variables with a small, inconsistent effect in America, that account for large and consistent score effects in Africa. Conversely high inference variables which maintain stable and pronounced score effects in America, contribute only to small or inconsistent correlations with test scores in Africa. These contradictory results constitute a serious challenge to the validity and cross-cultural generalizability of Western notions about test score meanings.

Hence cross-cultural research can be seen to provide a severe test for construct validation in psychological theory building. Only when patterns of correlations in all three classes of relationship identified above are shown to be the same can we infer equivalence of meaning, or construct validity for tests intended for use in cross-cultural comparative research.

TABLE 1

TEST SCORE CORRELATIONS WITH NON-TEST VARIABLES. (After Irvine, 1983).

	SCORE EFFECTS	
	America	Africa
LOW INFERENCE		
Prior practice Coaching Streaming Placement School quality	small, transient inconsistent	large, significant consistent
HIGH INFERENCE		
Socio-Economic Status Ethnicity Family size Birth order Ecological press	consistent, pronounced	small, transient inconsistent
Age Sex Genetic similarity	large consistent consistent	inconsistent inconsistent inconsistent

DEFINING THE INDEPENDENT VARIABLES

In this section the problem of defining the independent variables in cross-cultural research is considered. As we have noted already, cross-cultural psychology entails an attempt to comprehend systematic relationships between **culture** (understood in its broadest sense to include ecological and constitutional, as well as specifically cultural variables) and the **behaviour** of individuals. Not surprisingly, psychologists in the cross-cultural field to date have devoted a great deal of attention to the problem of defining and measuring dependent behavioural variables, while the study of independent variables has been seriously neglected. It has become increasingly clear, however, that to explain variations in behaviour across different societies as simply due to "culture" is not good enough. This simplistic ***Gee whiz! Look what I found!*** approach to in-group comparisons has not helped to advance cross-cultural psychology at all.

Recognition of the need for a more thoroughgoing approach to the study of **independent context variables** in cross-cultural psychology comes at a time when many other social sciences are giving greater prominence to the importance of understanding human behaviour in its naturalistic context. The search for independent variables within psychology has led to an upsurge of literature on the topic with a concomitant increase in confusion. This is well illustrated in a recent special issue devoted to the problem in the ***Journal of Cross-Cultural Psychology*** (Rohner, 1984; Jahoda, 1984; Segall, 1984).

On one extreme in this debate is Ronald Rohner, who presents a very careful analysis of the concept "culture", distinguishing it from related terms such as "social system" and "society" on the one hand while on the other hand attempting to separate it conceptually from the notions of human attributes and human artefacts. Having done this, Rohner is left with the rather narrow and not very easily operationalized view of culture as a ***system of shared (learned) symbolic meanings in a society.*** For Rohner culture is therefore an abstraction which, he stresses, should not be reified and then treated as if a causal entity. To do this, he observes, would be committing a double fallacy. Opposed to this view on the other extreme is Marshall Segall, who adopts the anarchic position that "culture" as a concept for study in cross-cultural research has no value and can be profitably dropped from further consideration. He motivates this position in saying that as it has come to be used, the term "culture" is too all-pervasive and diffuse. It is a generic taxonomic label that cannot be easily interpreted and hence is of no utility to the cross-cultural analyst. In its stead, he proposes that we should simply recognise a multitude of independent variables in the environment of individuals and that any or several of these might be adopted as a start in cross-cultural work in an attempt to understand context effects on behaviour. Gustav Jahoda, writing in the same issue, adopts a more moderate, intermediate position. Taking issue with both Rohner and Segall, he concludes his assessment by asserting that we do, indeed, need a fresh conception of culture, but that it should embody more than simply what is in the minds of the psychologists, which is what Rohner's concept amounts to. He

suggests tentatively that a useful concept of culture would probably have to have the property of a system, rather than of a variable. He puts forward the notions of a "cultural complexity" continuum (Whiting & Whiting, 1975) or "developmental niche" (Super and Harkness, 1981; 1982) as possibilities in this regard.

Several other prominent authors in the cross-cultural field concur that what is needed is a systemic concept of culture, preferably one that transcends Rohner's preoccupation with semantics and stipulative definitions about the meaning of words such as "culture" and "society". Systemic models of context variables for use in comparative research have been proposed by Barker (1968), Bronfenbrenner (1977), Brunswik (1955) and Berry (1975; 1979; 1980; 1983) among others. In the remainder of this section, consideration is given to the main elements in Berry's emerging multilevel arc model as a promising framework for coming to terms with independent context variables in cross-cultural research.

The multilevel arc model is a developmental outgrowth of Berry's (1975) ecological approach to cross-cultural work. This approach stresses the need to consider the behavioural phenomena under investigation within a naturalistic, total environmental framework. Berry (1980) acknowledges the dependence of his arc model on the earlier work of Brunswik (1955) and Barker (1968) both of whom argued that psychology has developed with a lopsided emphasis on reductionist, experimental settings at the molecular level, with too little emphasis begin given to functional, real-life settings at the

molar level. Both forerunners of Berry agreed that the environment is patterned, or "textured" as Barker termed it. By this they meant that environmental variables are "tied" together in a "textural ecology" in much the way Whiting (1976) saw many of our independent variables to be "packaged".

Working from this conceptual base, Berry (1980) has proceeded to define the key analytic terms needed to produce his model. First, the terms "ecology" and "environment" are carefully distinguished to overcome earlier inconsistencies in their use. "Environment" is seen to constitute a context for an organism (and its behaviour), whereas "ecology" is used to refer to **relationships** between the organism (and its behaviour) and the environmental contexts in which it operates. Hence any ecological analysis would need to contain three categories: **environments, organisms** (and their behaviours) and **relationships.**

A fourth category, "behaviour", is added in recognition of the fact that behaviours generally attain conceptual status separate from the organism, as "output" necessary to balance the environmental "input". Ecological relationships are considered to be conceptually dynamic, in the sense that both culture and behaviour are considered adaptive, while environmental settings are considered static.

A second analytic distinction is made between the physical or objective environment and the psychological environment, or "life space", following Lewin (1936). The psychological world is seen to consist of those features that are experienced and which

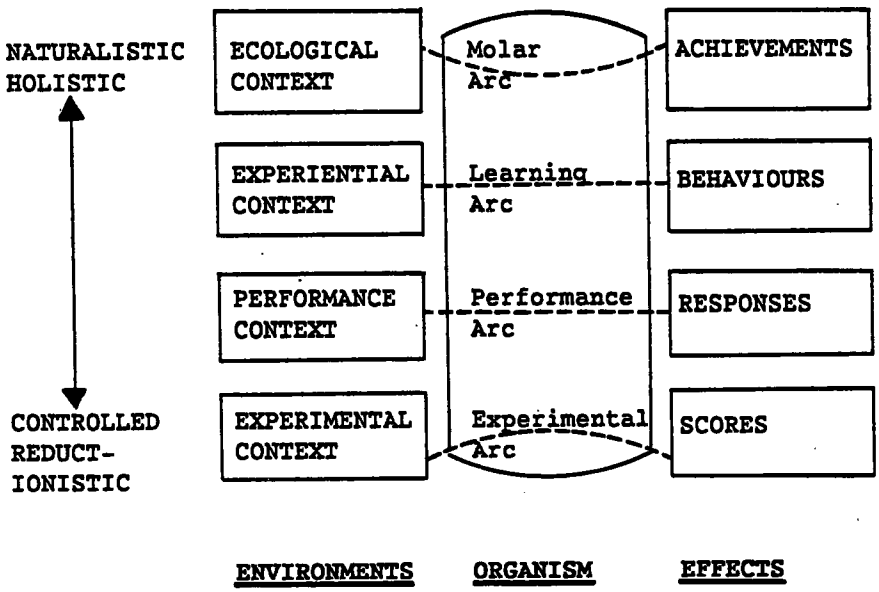
have behavioural consequences. The psychological world of the individual is considered just as real as the physical world, for, as Lewin (1936) saw it, what is real is what has effects.

Berry makes a third distinction between the ***recurrent or regular features of the life space*** and ***current or immediate*** experiences. The former are seen as contexts in which learning takes place, while the latter are considered to give rise only to responses.

A fourth and final analytic distinction is made, following Brunswik, between ***controlled stimulation*** as in an experimental context and an overarching functionalist approach embodying the more ***naturalistic, holistic, ecological context.***

Using these analytic concepts, Berry (1980) proposes a multilevel arc model for the systematic study of behaviour in relation to its context. The model, in schematic form, is reproduced here in Figure 1.

Figure 1. Berry's (1980) multilevel arc model of relationships between environmental contexts and behavioural effects.



In the diagram four levels of environmental context are identified on the left, with corresponding classes of behavioural effects on the right. On each level contexts and effects are related through a human organism. A vertical dimension traces a continuum from natural and holistic at the top to more controlled and reductionistic at the bottom.

Considering the environmental contexts on the left, from top to bottom, the ecological context, natural physical world, or "life

space", after Lewin (1936) consists of all the relatively permanent characteristics which provide the context for human action and achievements. Nested within this overall ecological system are various subordinate situations. The **experiential** context is defined as that pattern of recurrent experiences which provide a basis for learning. Berry sees this as the set of independent variables which cross-cultural psychology tries to identify as being operative in particular habitats, influencing the development of particular behavioural characteristics. The **performance** context is the more limited set of enivironmental circumstances which may be observed in immediate relation to particular behaviours. The **experimental** context represents those environmental characteristics that are deliberately designed by the psychologist to elicit a particular class of responses, or test scores. Berry points out that the experimental context may or may not be nested in the first three contexts. The degree to which it is nested represents the **ecological validity** of the experiment or task.

On the right in the diagram are four levels of effects, corresponding to the four contexts. **Achievements** include those complex behaviour patterns which develop over a long period of time as an adaptive response to the ecological context, as a whole. Behaviours at this level may be discovered in an individual, but also exist as shared behavioural patterns in a cultural group. **Behaviours**, on the next level, are molar or emergent skills, traits and attitudes which have been learned, over time, in response to particular roles, or to education whether formal or informal. **Responses** are those performance

ffects which are produced by immediate stimulation or xperience. The fourth class of effects, **scores**, comprise those pecific behaviours measured or recorded during psychological xperimentation or testing. If the experiment has ecological alidity, then the scores will have **behavioural validity.**

Relationships between contexts and effects in the model are raced across linkage **arcs** on the four levels. The **molar** arc is oncerned with the total life situation of an organism and its ccomplishments. The **learning** arc is concerned with elationships between recurrent independent variables in the xperience of an individual and his characteristic behaviours. The **performance** arc concerns more specific behavioural acts as a function of immediate and current experience. Finally, the **experimental** arc is limited to the laboratory or other controlled experimental situations in which scores are studied in response to systematic variations in task demands.

Berry (1983) points out that whereas general experimental psychology fails to ascend the vertical dimension to make valid inferences about causal relationships at the middle two levels, cross-cultural psychology has failed to descend the vertical dimension from the top to achieve a specification of experiential, performance and experimental context variables which are responsible for task performance and behavioural variation across habitats. Hence, experimental psychology may be said to lack "external" validity, while cross-cultural psychology generally lacks "internal" validity.

Berry's multilevel arc model illustrates the important point that the terms "context" or "environment" cannot be limited to any particular level of analysis. Both broad systems and specific situations are elements of context and any throughgoing study of cross-cultural variations in behaviour will need to be pursued in a systemic fashion to accommodate relationships between behavioural effects and environmental contexts at all levels of analysis.

COMPARABILITY, RELATIVITY, AND CROSS-CULTURAL GENERALIZATION

The availability of conceptual frameworks for dealing with dependent and independent variables in cross-cultural cognitive research leaves unaddressed the vexed issue of whether and under what conditions valid comparisons of constructs are indeed possible. Is all behaviour relative only to its specific cultural context, or are generalizations across cultures possible with a view to achieving universal laws of human experience and conduct? These are the questions underlying the present section. The topic is complex and multifaceted, hence only a cursory treatment can be attempted here. The aim is to contribute to a conceptual clarification of issued and terminology, rather than to evaluate technical aspects of a mathematical or statistical nature.

The central problem of the comparability of data in cross-cultural research has not been rendered less complex by

the unfortunate proliferation of terminology, often poorly or only partly defined. Parallel developments in the broad field of psychological assessment, in general, have spawned a profusion of related, though different concepts, each with its own varied and often overlapping terminology. Hence the literature is replete with such terms as "test bias", "fairness", "culture-loadedness" and "equivalence", to mention a few, all of which, on occasion, have been used as approximate synonyms for comparability, as it is to be used here. Since the American testing literature, in particular, seems plagued by terminological inconsistency in this sphere, the more standard terminology of the internationally influential "Dutch School" of cross-cultural methodologists (Poortinga, 1983; in press; Mellenbergh, 1983; in press; Van de Vijver, in press; Van de Vijver and Poortinga, 1982; Van der Flier, 1982, Van der Flier and Drenth, 1980) is preferred here.

At the outset, it would seem useful to clarify what the term "comparability" does not refer to, by contrasting it with related concepts with which it is often confused. First among these is the notion that a test may contain within it certain features which give rise to a systematic, unwanted discrimination against one or more groups. The term "bias" is most often used to refer to this condition, although many American authors, as reviewed by Jensen (1980) use "bias" differently, albeit inconsistently (Flaugher, 1978). Sophisticated psychometric procedures are available to assist in the detection of both test bias and item bias, although these need not concern us here. It is also possible to detect bias in a test or its items by purely conceptual means,

but this method, associated with the tradition of culture-fair testing, is generally considered insufficient or unreliable.

Another concept often confused with both bias and comparability, but for which a distinct meaning should be recognised is that most often referred to as "fairness" in selection testing. As used here "fairness" has to do with the set of circumstances under which decisions are made about individuals on the basis of test scores, particularly in the applied context of selection for education or employment. "Fairness", in this sense, has to do with the characteristics of the decision rule or model adopted in choosing one candidate above another, whereas "bias" has to do with properties inherent in a test or testing situation in relation to the groups tested.

"Comparability", by contrast, is used to refer to the set of statistical conditions that must be met **after** test data have been collected, to determine whether valid inferences can be made about such intergroup differences as there may be in the test scores. The term "equivalence", implying measurement along the same underlying scale is often used as a synonym for "comparability". "Comparability" serves as a generic term for the state of affairs where, on the basis of test data, valid inferences can be made about intergroup differences in terms of a common scale (Poortinga, 1983). Both bias and fairness models may set conditions for measurement comparability although they are addressed to rather different issues. The key question in the analysis of comparability is essentially whether the **same** construct or dimension is being assessed in different

groups. Note that this is not the same as asking which construct is being assessed, which is the question in construct validation as discussed in a preceding section.

With this understanding of comparability in mind, we now turn to the problem of whether equivalent data, enabling valid interpretation of intergroup differences can be expected when the cultural context of behaviour is known to differ across groups. This issue lies at the heart of the debate in cross-cultural psychology on cultural relativism versus universality. This debate, as noted in an earlier section, is as old as cross-cultural psychology itself. Indeed, it has a much longer history in philosophy, which can be traced to classical times.

A potentially useful resolution to this debate has been forthcoming only very recently, in the work of Van de Vijver and Poortinga (1982). These authors argue that the contrast between universality and cultural relativity amounts to a false dichotomy; one of many in psychology including the closely related false dichotomy between emics and etics, which Jahoda (1980; 1983) has recently exposed. Van de Vijver and Poortinga demonstrate that the apparent discontinuity between cultural specifics and universals can be easily reconciled within a unified view if these are conceived as extremes defining opposite ends of a single continuum of generalizability.

On this view, dependent behavioural variables in cross-cultural research can be characterized as varying along a continuum of

generalizability from those that are unique to a particular culture, through those that generalize to two or more cultures, to those that are common to all human subgroups and hence may be considered universal. The more similar the cultures under investigation the more likely it is that construct generalization will be found; conversely, the more dissimilar or highly contrasted the cultures, the less likely it is that comparable constructs will be found and the greater the likelihood that variables will prove to be culturally specific. Note that in this sense the degree of generalization itself is relative to the sample of cultures under study and is not an invariant property of the dependent variable constructs.

With regard to the concept of "universality", Van de Vijver and Poortinga (1982) point out, that as in the case of comparability, there is a lack of precise definition. They argue that the various existing definitions of universality differ in the extent to which they lend themselves to empirical evaluation. Within the methodological provisions for assessing degree of generalizability, four categories of universals can be identified along a dimension of experimental rigour or strictness. These are termed conceptual universals, functionally equivalent (weak) universals, metrically equivalent (strong) universals, and scalar equivalent (strict) universals. The properties of these four categories of universals are summarized briefly below.

Conceptual universals are molar, theoretical concepts at a high level of abstraction and lacking in empirical referents. Since their meaning is not specified in operational terms,

the universality of such concepts cannot be refuted experimentally. Examples of concepts in this class include the "psychic unity of mankind" put forward by Boas (1911) or the notion of "sensotypes" of Wober (1966). Concepts such as "intelligence", or "adaptability" (Biesheuvel, 1972) are also cited as examples belonging to this class provided their meaning is not further specified in operational terms. By the same token, constructs generated by means of non-quantitative observational methods could at best be claimed to achieve status as conceptual universals. Van de Vijver and Poortinga question the scientific usefulness of conceptual universals owing to their non-falsifiability and consequent weak explanatory value.

Functionally equivalent or weak universals are concepts for which empirical referents have been specified and for which construct validity has been demonstrated in each culture. Although weak universals are experimentally falsifiable, it is not a requirement for functional equivalence that identical empirical referents, or measuring instruments should be used in all groups. For this reason concepts with functional equivalence can be claimed to be universal in a qualitative, but not necessarily in a quantitative sense. The three dimensional structure of affective meaning of words as investigated with the semantic differential technique (Osgood, May & Miron, 1975) is cited as an example.

Metrically equivalent or strong universals are concepts assessed in the same unit of measurement (metric) across cultures,

although the scales may have a different origin in each culture. The Celsius scale and the Kelvin scale for measuring temperature are cited by way of an analogy, indicating that relative rather than absolute differences are at stake. Studies by Cole, Gay, & Glick (1968), Poortinga (1971), Irvine and Reuning (1981) and Verster (1983; in press (b)) provide examples of metrically equivalent data in cross-cultural psychology.

Scalar equivalent or strict universals are concepts with empirical referents that would have to have an equal metric and equal scale of origin in each culture. This will nearly always imply distributional identity across cultures as well. Differences in means in the performance of culturally different samples can be taken as evidence against claims for the construct as a strict universal. Van de Vijver and Poortinga note that only for very few concepts can strict equivalence be claimed on the basis of available research evidence. Possibilities include speed of processing of very simple visual and auditory stimuli in reaction time experiments of the type employed by Jensen (1980).

The work of cross-cultural methodologists of the "Dutch School", led by Poortinga, has offered us a useful framework in terms of which to deal with the false dichotomy implied between cultural relativism and universality. The unifying principles embodied in Generalizability theory, and adapted for cross-cultural psychology from classical test theory (Cronbach, Rajaratnam & Gleser, 1963) provide a promising means for dealing with the problem of equivalence in intergroup

comparative research. The close correspondence between the four levels of equivalence identified by the Dutch methodologists and the four levels of analysis in Berry's multilevel arc model should not pass unnoticed. The even closer agreement between the psychometric requirements for conceptual, weak, strong and strict universals, and the four levels of measurement associated with nominal, ordinal, interval and ratio scales (Stevens, 1951) is clearly apparent. Such relationships suggest the possibility of bringing the difficult issue of evaluating data comparability in cross-cultural research an important step closer to the relatively firm ground of measurement theory in general.

CONCLUDING REMARKS

In this paper I have sided with the view that cross-cultural research, in general, should recognise two contrasting goals. First, to describe and explain the range, variability and differences in human behaviour that occur as a function of cultural (including ecological and social) variables. Second, to seek out the similarities, or pan-human uniformities to be found in behaviour and experience across cultures. These goals address the dual probabilities of cultural relativism and universality in human psychological function. With regard to cognitive research in particular, and especially as pursued from the perspective of Western psychology, I have stressed that the emphasis should be on comparative studies in as many diverse cultural contexts as possible with a view to generating more universal descriptions, hypotheses and laws of human cognition.

To achieve this goal, I have argued that greater attention needs to be given to the central methodological problems of cross-cultural construct validation, context measurement and comparability analysis. Drawing on the work of international leaders in these fields, I have presented conceptual frameworks that seem to offer good prospects for dealing with these problems.

To conclude, I would like to offer some general remarks on what I see as important challenges and prospects facing cross-cultural cognitive research in the future. Firstly, lest the impression be gained that cross-cultural research amounts to no more than an exercise in abstruse methodology, I would like to refer to some imminent challenges I foresee from the context of real world affairs, here in South Africa.

In the field of psychometric testing for employment selection a major challenge is looming. It will become increasingly necessary for test producers and users to demonstrate that their techniques are valid for purposes of intergroup comparative assessment. Employers, employees, organized labour and legal authorities will need convincing evidence that the tests are unbiased, yield comparable measures across cultural groups and can lead to fair decisions in personnel selection and placement.

In the context of education the challenge goes beyond mere selection testing. Cognitive assessments are needed to define more precisely the educational needs of different subgroups. This involves more than just assessing levels of developed

bilities. It requires diagnostic procedures to determine more recisely the types of cognitive skills, processes and knowledge ases available to members of different groups in relation to oth curriculum and external environmental context demands. here is an urgent challenge awaiting cognitive research with a ew to developing cognitive skills and efficiency. This implies ot only a need to help individuals adapt more effectively to heir environment, but, more importantly, to adapt nvironmental conditions to meet their own needs. Many rominent theorists consider this to be the essence of telligence (e.g., Sternberg, 1984).

the broad and complex sphere of human relations, cognitive esearch has a rôle to play in elucidating intergroup differences the mental processes and strategies employed in decision aking, negotiation, conflict resolution, coalition formation, lanning and judgement.

urning finally to the rôle of cross-cultural cognitive research the wider context of theory development in psychology, it is ncouraging to note that an increasingly important impact is eing made. For example, one of the leading contemporary heorists in mainstream intelligence research, Robert Sternberg, as recently seen the need to extend his earlier Componential heory of Intelligence (Sternberg, 1977) into a new Triarchic heory of Intelligence (Sternberg, 1984) in order to give greater ecognition to the importance of cross-cultural context ariables in intellectual development and functioning. A leading ournal, the *International Journal of Psychology*, recently

devoted an entire issue (1984, volume 19) to the topic of the influence of cultural context variables on intellectual development. Finally, the increasing prominence being given to cross-cultural considerations can be seen in the proceedings of international conferences and symposia on human cognition and assessment (e.g., Friedman, Das and O'Connor, 1979; Irvine and Berry, 1983; Newstead, Irvine & Dann, in press).

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4.

CROSS-CULTURAL PSYCHOLOGY OF IDENTITY: METHOD AND THEORY

P. du Preez

DIRTY WORDS

The words "culture" and "identity" are dirty words to many social scientists in South Africa for several reasons. The first is that both culture and identity are often reified as possessions and categories; the second is that these categories may be used to justify segregation; and the third is that culture, race and nationality are often confounded in the journals, both here and internationally. Paul Rich says that even liberal anthropologists have served the cause of segregation because their work in the thirties ***emphasised the separateness of African culture from Western civilisation*** (1984: 61). Swartz and Foster tell us that cross-cultural psychiatrists generally reify ***black culture*** as an organic, archaic essence which is contrasted with the fragmented, alienated "Western Culture" (1984: 17). This may seem like high praise, but idealisation is a well-known move for keeping people ineffective, as we know from the history of

"ladies". A conclusion from this view is that ***Westerners and blacks live in two different worlds*** and that passage from the traditional world ***inevitably leads to the loss of holism, security, and an integrated universe*** (idem: 23). This is exactly what might happen to a lady attempting to become a person. The two world view is congruent with respectful segregation, from the very best motives. This is not new, as we see when we read Smuts's Rhodes Memorial Lecture at Oxford in 1929 (cited later).

If the contribution of the liberal anthropologists is supposed to be a subtle poison – crippling by idealisation – the work of "Volkekundiges" is said to be the real crude stuff, with instructions on the label – Sharp (1980); Skalnik (1984).

In the social sciences, words have a way of becoming contaminated since almost all of them have been put to bad use (or are said to have been put to bad use) by someone or other. Should we abandon "culture" and "identity" and try to find substitutes? We might, but we should remember that there are functions which contaminate all the words referring to them. I suggest that we need both culture and identity, but that they both require a good scrubbing with the finest theory we can get. We need to be more explicit about what they do, rather than more genteel and evasive, and this means that we shall have to stop seeing them as treasured possessions and start seeing them as weapons in the business of living – which means politics and economics. In this way, we shall take our cross-cultural psychology out of the museum and into the ...townships? Well, wherever it is that people are supposed to be living their lives.

Firstly, let us see how culture, politics and economics are connected, and how identity is a resource which can be mobilised for cultural, political and economic purposes.

SOCIAL SYSTEMS

Suppose we were to insist that culture and identity are the systematic activities of human beings in relations with each other and their natural environment. How might we then proceed? First, we note that every system must have three components. These are:

1. a composition;
2. an environment; and
3. bonds holding the system together. (Bunge, 1981).

How can we apply this to the description of a particular social subsystem in which people identify themselves and others? We define it as follows:

1. the **composition** M_i is the set of people who identify themselves in a common way in relation to others in their social environment;

2. the **environment** M_e is the set of people with either complementary or conflicting identification to M_i, as well as the output of other social subsystems, viz., the political E_p, the economic E_e, the cultural E_c, and the kinship E_k. These outputs will be relative to absolute

wealth (E_e), relative power (E_p), ideas and ideologies (E_c), and relative populations (E_k);

3. the bonds S of the system of identification are the sets of economic S_e, political S_p, cultural S_c, and kinship S_k practices which maintain or change the identity system M_i. These practices are commonly referred to as "real" social relations (e.g. Bhaskar, 1979).

Before we continue, I should state that each subsystem has a different focus. The focus of the economic subsystem is the production and consumption of goods and services; the focus of the political subsystem is administration and power; the focus of the cultural subsystem is the production and transformation of signs; and the focus of the kinship subsystem is solidarity and biological reproduction. The output of each of these subsystems can be input to the others. They are, in other words, joined in various ways.

Of course, we might study each of these systems in isolation and we often do. Economic theories focus on the production and consumption of goods and services and may attempt to exclude all political, cultural or kinship influences; or cultural theories might focus on the transformation of signs and information and the search for culture codes (e.g. Lotman, 1979). Yet many of the most interesting questions concern the linkage of these

sytems. We might ask how cultural resources, such as theories of identity, are mobilised for political purposes and how economic resources might be mobilised to maintain a system of identification. We are no longer in a museum or a folkish utopia, but in an arena of action.

How should we study systems of collective identification? From a purely abstract point of view, we might study any and all of the elements in the composition, environment and bonding of the system. In practice, we are more modest (at least to commence with) and pose only a few of the possible questions. However, let us first consider potential causes of changes in the ways in which the set of people M_i identify themselves and others. Causes of change could be:

1. changes in competing identification by sets of people M_e in the environment;

2. changes in the input of other social subsystems, such as the political and economic; and

3. changes in social relations and practices.

This is a large set of questions, and here I shall be concerned with only a minute part of the set. The first question I shall ask is simply comparative and descriptive, viz., what is the content of processes of identification? What is the stock of signs which is utilised, transformed and applied to real problems of kinship, solidarity, politics and economics? The second question will be

about change, and it will refer to the activities of particular persons attempting to promote various theories of identity for political purposes. We shall see that they take an existing code and work with it, twisting it this way and that to find a solution to their problems. These people are **identity entrepreneurs**, and we find them as active in selling political programmes as in marketing soap. This refers to the activities of the set of people M_i in the environment M_e. The third question will also be about change, and it will refer to the spread of theories of identity under the different social conditions. It will be suggested that we can understand the spread of particular identities in the same way that we understand the spread of particular species to fill an ecological niche. This refers to the spread of the activities M_i in S.

It should be clear from all that has been said that the argument here is concerned with collective identity rather than idiosyncratic variations. We should now turn to the first question: what is the stock of common signs which encode public or collective identity?

THE STOCK OF COMMON SIGNS

Each and every system of identification makes use of a stock of common signs, transmitted from generation to generation. The organization of this stock of signs is the **identity code** of a particular collective, and this identity code contains both **core and peripheral constructs**, ranked according to their degree of centrality in maintaining collective identity. The constructs of

the collective identity code are distributed polysemously throughout the membership of the collective, in the sense that all members will exhibit some of the constructs of collective identity, though few will exhibit all. It is also likely that in arousing situations, more and more constructs will be triggered in members of the group.

Constructs may take the form of **attributions**, or predicates arranged more or less systematically in a coherent theory or belief system, and **myths**, which encode images of relations between collectives. When a myth is particularly central (a core myth) in defining relations between collectives, we call it a **primal myth**. An ideology consists of both predicates and primal myths. To give an example: a core construct of Black Consciousness appears to be that **blacks are oppressed and exploited people** (we shall refer to others a little later), and its primal myth originated in the dehumanization of Apartheid, and events such as Sharpeville and the Soweto riots on June 16, 1976.

We should note that constructs are often asymmetrical, in the sense that the myths and beliefs of one group may not be those of the other group or groups involved.

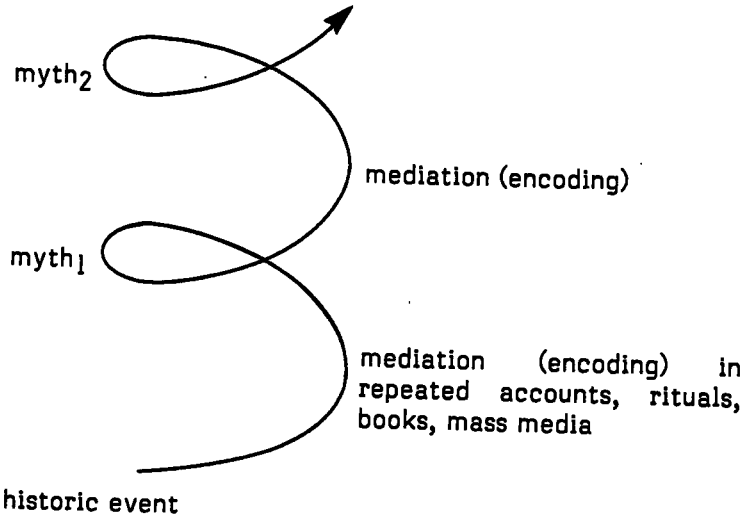
Even when two or more groups attach weight to an event, their myths may be asymmetrical in the sense that the images, beliefs and implications of the event are different.

The relationship between an historic event and a myth is not linear, of the kind:

historic event ⟶ primal myth

but a spiral, of the kind:

Figure 1



Conversion of an event into a myth – an activating and directing structure of images and beliefs – may occur extremely rapidly and is then often consolidated in official rites and ceremonies, when a group is established in power. Again, I refer to the speed with which the Soweto myth has spread and become an unequivocal symbol of rebellion and martyrdom. We can safely conjecture that this primal myth will become a member of a family of myths representing the relation between white oppression and black resistance.

Herman's (1973) study of Jewish identity reveals the same structure of primal myths, as well as core and peripheral constructs. The primal myths of Jewish identity are (among others) Exodus, the Fall of Jerusalem, the Babylonian Exile, the Holocaust, the Foundation of the State of Israel. The core constructs refer to religious beliefs and observances. In South African history, the primal myths of Afrikaner identity are the Great Trek, the Boer Wars, the Republic, and Blood River.

How should we study primal myths? One obvious way is to examine the use to which they are put in politics or in official and semi-official ceremonies. We may collect series in which we can study their contextual transformation in the media. In the context of the 1950s, we find S.P. le Roux, Minister of Agriculture at the time, using the Blood River myth to warn, metaphorically, that *we must beware of entering strange and unknown kraals. Piet Retief was careless and did not sense the treachery. Look out for unknown kraals. Beware of strange ideas and ideologies.* (Cape Times, 18 December 1950). In the context of the 1980s, we hear P W Botha using the same myth to make the point that *Blood River was not a victory of white over black, but the truimph of God's omnipotence over all. The millions of Christians in South Africa are proof that God and Christianity were the victors at Blood River .... The striving of the Afrikaners to make contact with other groups in South Africa must have the power of the symbolic 1938 Ossewatrek* (Cape Times, 17 December 1983).

These are illustrations of the transformation of a primal myth. How deep are the changes? How significant is a particular primal myth – or a collection of such myths – in determining action? The hypothesis I would like to state here, as a basis for further investigation, is the product of many discussions with Peter Collins of the University of Cape Town. The hypothesis is that many political interpretations and reactions can be understood only by referring to the ways in which events are filtered through the primal myths which they trigger. For many Americans, we might conjecture, the American civil war is about to be fought again in South Africa, and the emotions which are aroused are those of the war against slavery. On the other hand, the wars against the American Indians do not feature in American liberation mythology – for obvious reasons. I think it is important to understand the historical schema to which events are being assimilated in understanding the reactions of nations to other nations.

It seems to me that detailed case studies are necessary to understand and evaluate the role of primal myths in collective identification. These have still to be undertaken. What I can offer here is a much simpler study of the extent to which nominal groups share identity prototypes and primal myths.

As a preliminary step, we may ask respondents whether any historical events are particularly significant to them and whether there are any historic figures they would particularly wish to resemble. In this, we follow the example of Marisa Pavolloni and C Louis-Guérin (1984) in Montreal, who studied

the identity of French and English Canadians. Here, with the generous help of Elizabeth Nel at the University of Stellenbosch and Eugene van Niekerk at the University of Fort Hare, I was able to put the above two questions to White Afrikaners (N = 198), White English speakers (N = 171) and to Black students (N = 153) of several different language groups.

Even in such a preliminary investigation, one is able to test the following hypotheses:

1. that the collective identity of English speakers is more diffuse than that of either Afrikaners or Blacks; and

2. that English speakers produce a lower proportion of local prototypes (figures and events) than do either Afrikaners or Blacks.

How might we test the first hypothesis? One way of addressing the question is to calculate a diffusion score to show the extent to which respondents focus or fail to focus on common prototypes. The less commonality, the greater the diffusion of collective identity. We calculate this as follows:

$$\text{Diffusion of collective identity} = \frac{\text{number of prototypes produced}}{\text{potential number of prototypes}} \times 100$$

The higher the score, the greater the diffusion of collective identity and the less the definition, in terms of reference to common prototypes.

The diffusion scores are as follows:

Heroes:

UCT 53	Fort Hare 29	Stellenbosch 31

Events:

UCT 27	Fort Hare 29	Stellenbosch 20

In both cases, Stellenbosch students concentrate on fewer prototypes that UCT students do, thus confirming the hypothesis that Afrikaner identity is more defined than English speaking identity. Black students resemble Afrikaners in their concentration on relatively few heroes, but resemble English speakers in the relative diffusion on their history prototypes.

Now let us look at the local content of prototypes.

This is calculated by the ratio

$$\text{local content} = \frac{\text{number of local prototypes actually produced}}{\text{total number of prototypes actually produced}} \times 100$$

The higher the score here, the greater the local content of identity prototypes of images. The local content scores are

Heroes: UCT 1	Fort Hare 43	Stellenbosch 28
Events: UCT 14	Fort Hare 59	Stellenbosch 33

Here, we see that identity prototypes have very little local content for English speakers, intermediate local content for Afrikaners, and highest local content for Blacks. Who are the most frequently cited heroes and what are the most frequently cited events?

Heroes

UCT	Fort Hare	Stellenbosch
Churchill	Mandela	Hitler
Jesus	Thatcher	Verwoerd
Ghandi	Marx	Vorster
Kennedy	Ghandi	Thatcher
Einstein	Martin Luther King	Paul Kruger

Events

UCT	Fort Hare	Stellenbosch
World wars	Soweto 16 June	World wars
Jesus' life	World wars	Republic and Union
Industrial Revolution	Jesus' life	Arrival Jan van Riebeeck
Nats came to power	Sharpeville	Jesus' life
French Revolution	French Revolution	Blood River

Apart from the events and people common to all groups (and the astonishing prominence of Hitler), certain things are striking. The local content of history is much greater for Afrikaners than for any other group, both in terms of heroes and in terms of events.

What would the next step be? First, we should probe more deeply, wherever possible, by asking about the implications of these events and heroes. Secondly, we should attempt to construct historical series by examining public utterances about heroes and events. To what extent is the primal myth of black/white relations seen as Soweto 16 June 1976 from the Black point of view? To what extent is the primal myth of white/black relations Blood River, as seen from the Afrikaner point of view? And what images will the English commonly refer to in relating to various other groups?

An analysis of primal myths and the fantasies associated with them will enable us to understand the political unconscious, which is the key to the relations between groups as lived at the level of fantasy.

There is something missing from our study. Though we have asked people about their heroes, we have not asked them about their dark images, or those they fear and hate. Any future probe should refer to negative as well as positive prototypes.

Many further questions could be raised here about the way in which image, myth and code are to be understood as the source

of intergroup relations, but I think enough has been said to show the direction in which one should proceed. A socioanalysis of collective identity will investigate:

1. images which people have of themselves and other groups – particularly as encoded in primal myths;

2. the circumstances which trigger these images and their primal myths;

3. the processes by which primal myths are consolidated; and

4. the processes, if any, by which primal myths may be eliminated.

In the next section, we shall examine the active transformation of core constructs and primal myths by people who act as identity entrepreneurs.

IDENTITY ENTREPRENEURS

Identity is received and transformed by people. There is a stock of ready-to-wear kits, but there are also the tailors to fit us out for new expeditions. Some of these work in the economy, tinkering away with products and consumers, offering models along with the soap, or soap along with the models. Others work squarely in the cultural field, defining new forms of consciousness and new types of person. And, of course, products

can be transferred from one system to the next. Images designed in the cultural system may be used for gaining political power; political power may be used to direct image-making in the cultural system.

The entrepreneurial spirit thrives in the most unexpected corners of social existence, and certainly it thrives in the marketing of identities, because it is by virtue of our accepting various identifications that we can fit into various social transactions. Identity is the relationship of one category of person to another; to change identity is to attempt to change this relationship. The tactics are interesting. The elements of identity are assembled and reassembled with the limits of a code until they fit a given purpose. What are the common codes? In South Africa, the major political codes are racism, ethnicity, black consciousness (or inverted ethnicity), Marxian class analysis, and liberalism. Events are assimilated to codes and mythologised within them. This is the process of mediation. Contrast two codes and picture the ways in which an event might be encoded as myth in them.

Code 1	**Black consciousness**
	african roots; organic culture; wholeness; a common and exclusive black consciousness; ancestry; spiritual values.
Code 2	**Class analysis**
	racial capitalism; relations of production; class conflict; petty-bourgeoisie; dialectical materialism; Marx and Lenin.

The former mobilises people on the basis of the wholeness of black culture and the assertion that blacks understand blacks. Soweto 1976 is exactly what it seems – a clash of Black and White; an ethnic encounter, though between unevenly matched opponents. The latter mobilises people for a class struggle in which the majority of the workers happen to be black. Soweto 1976 is not exactly what it seems to be. When we penetrate beneath the surface we see the effects of capitalism and class exploitation.

The remedy in the first code is to substitute black rule for white, relying on a unity of black consciousness to transcend any internal differences. Where class is considered by black consciousness movements such as AZAPO, it is seen as ***an attitude of mind*** (Sole, 1983: 59). The remedy in the second code is a substitution of socialism and the rule of the working class for capitalism and the rule of the bourgeoisie.

We can now understand the spiral of mythologising a little better than we did at first. We conjecture that the process involves an ideological encoding or mediation of an actual historical event. In the process of spiralling, other events contribute to the final form of the myth. These events include the reactions of the target group and inputs from the economic and political systems.

Let us now return to the theme of entrepreneurship, or the activities of individuals attempting to encode identity for different audiences. The entrepreneurs who interest us

ere take an existing situation – the South African political cene and its many events – and attempt to give it a rational or heoretical structure which will make action coherent and egitimate rather than accidental.

n terms of the systems model advanced earlier, I am looking at ne ways in which people maintain and innovate identification in ne context of yet other attempts to maintain and innovate ompeting systems of identification. These identifications are esigned as inputs to the political system; they serve individuals eeking to control power.

Ny first example is Smuts, speaking from a position of power; ny second is Neville Alexander, speaking from a position of elative powerlessness. Both are "organic intellectuals" who lace their talents at the service of a class. Smuts served mperialism and racial capitalism. Alexander serves the working lass.

et us first listen to Smuts, giving his Rhodes Memorial Lecture t Oxford in 1929. He speaks of the distinctiveness of the frican and of the constraints which this imposes on policy.

> *For there is much that is good in the African and which ought to be preserved and developed. The negro and the negroid Bantu form a distinct human type which the world would be the poorer without.*
>
> *Here in this vast continent, with its wide geographical variety and its great climatic difference, this unique human type has been fixing itself for thousands of years.*

He then goes on to state some of the characteristics of this human type. It is "child-like", "good-tempered and care-free", "absorbed in the present", and incapable of creating an indigenous literature, art or architecture. Now the conclusion follows:

> *It is clear that a race so unique, and so different in its mentality and its culture from those of Europe, requires a policy very unlike that which suits Europeans. Nothing could be worse for Africa than the application of a policy, the object or tendency of which would be to destroy the basis of this African type, to de-Africanize the African and turn him either into a beast of the field or into a pseudo-European* (Smuts, 1940: 38).

The intellectual framework of Smuts' discourse is the racial anthropology of his day. The key words and phrases of this code are:

Some anthropologists hold ....
Cradle of mankind ....
Child psychology ....
negro and negroid Bantu ....
distinct human type ....
South African petroglyphist ....

Reading the speech as a whole we see how the code of virtue is combined with the code of scientific rationality in advocating separate political institutions for African and European. First, there is the contrast between the child-like African and the grownup European, with its implication that Christian guardianship is proper. Second, there is the sacred trust under

the Covenant of the League of Nations which obliges civilised nations to act as guardians in the interests of indigenous peoples. These combine to make it essential to preserve tradition and allow *the fullest, freest development of people along their own specific lines* (1940: 39).

The functions of careful code-selection are obvious. Action is rationalised for a particular audience. We should understand successful code-selection, therefore, as a product of a system consisting of articulators and other persons in a particular environment and bonded in particular ways. Smuts is in fact articulating a theory which was widely accepted by many of his audience - a theory which formed one of the bases of colonialism.

In triggering the constructs he shares with the audience, he has

- encoded a series of images and myths
- defined collective identities
- enhanced the identity of the in-group by contrasting it with "indigenous" or "native" identity
- revived and elaborated a common world of understanding
- justified action.

Can we obtain any insight into some of the images which Smuts was attempting to encode in this way? I think we can, by carefully examining his writing and speeches. Here is one occasion on which he gives us a glimpse of what it is that has to be preserved, speaking at the Savoy Hotel in 1917, on the occasion of a dinner in his honour. He "smiles" at the point of

view of early missionaries who promoted intermarriage between white and black and states that we now accept the axiom: ***No inter-mixture of blood between the two colours.***

> ***It is probably true that earlier civilisations have largely failed because that principle was never recognised, civilising races being rapidly submerged in the quicksands of African blood*** (1940: 16).

Here he gives a biological form to "the heart of darkness", to the oblivion that may await the European who forgets his position of domination. How much better to act according to the principles of scientific rationality and preserve one's distance and one's dominion!

Now let us turn to Alexander's attempt to dissolve the codes of racial and ethnic identity and substitute the code of class in his address to the Association for Sociology in Southern Africa in Johannesburg in 1984. My reason for selecting Alexander is that he is prominent in the politics of the National Forum.

He argues that intellectuals should serve the interests of the working class – "in South Africa, the black working class" (p. 3). The argument is that ideologies of race and ethnicity have served capitalism and that it is essential to destroy both racial and ethnic identities as a basis for political action. The fact that people may identify themselves by race or identity at any moment in time is a fact to be explained, not an argument in favour of accepting race and ethnicity as basic social categories. The social scientist should unmask them as false

eliefs, like the belief that ghosts exist. We may remark in assing that this is a common move in the history of science. omething which is phenomenally obvious, such as the movement f the sun round the earth, is shown to be untrue at a deeper evel of explanation. In the same way, Alexander (as well as ohnstone (1978), O'Meara (1983) and many others), argues that he subjectively real experience of ethnic or racial identity is ot essential or fundamental. We have to explain this subjective xperience by referring to discourse, to the institutionalisation nd administration of identity, to the interests which are served y particular identification, and to processes of socialisation. Our reference point must, in the end, be our class position, since

> *... we can survey the map of possible identities scientifically. We can make our decision in accordance with our understanding of or feeling for the class interests with which we identify, bearing in mind that our options are not unlimited, that we have to be guided by the real social, political and economic conditions in which we operate* (Alexander, 1984: 21).

he task of the intellectual is to show that the experience of dentity is a function of these real social relations and to create discourse into which it can be mapped. The committed ntellectual will (among other tasks) contribute to the unmasking f ethnic identities through the nation-building process nderstood as a class struggle (Alexander, 1984: 23). This ctivity directly opposes that of the committed intellectuals of ne ruling class who have provided a theoretical and ideological ationale for ethnic identity and the fragmentation of pposition. We note how a theory of identity becomes "common

sense" and subjectively real to the extent that it accords with interests, practices and theoretical horizons of classes of people.

Why should one spend so much time on the entrepreneurs of identity? The answer is, I think, that in cross-cultural psychology we tend to examine things statically and to take identity as a received thing. It is not. Identity is fiercely contested and actively maintained. We both produce and reproduce our social existence, as the duality axiom states (Bhaskar, 1979; Shotter, 1983); and even this does not capture the intensity, the risk and the violence of the activity. Attempts to challenge a ruling system of identification, invested as it is in real social processes and material gains and losses, may lead from the public platform to the state goal.

THE ECOLOGY OF IDENTITY

The more one becomes aware of the intense struggle surrounding identification, the more one is reminded of the contest of different species for survival in an ecological niche. One might be content with current practice in the social sciences and say that one system of identification is "right", in an absolute sense, or one might ask why systems of identification succeed each other in the way they do. When one asks about change, one turns to the most successful theory of all – evolution by natural selection. In this case, one would argue that identity is a form of pseudospeciation (to borrow Erikson's term) and that the selection is social. What does a theory of social evolution require? There has to be (i) variation in elements; (ii)

transmission; and (iii) a process of selection.

The social process of identification has all of these features. We have seen that identity is composed of a stock of elements and that variations in this stock may be produced by the transformational efforts or identity entrepreneurs. In fact, these efforts are merely the most striking and public examples of variations which are introduced by everyone attempting to work out a personal position. Mutations are also introduced by borrowing new elements from others and by incorporating new historical events and heroes into the identity model. We can see this quite strikingly in the formation of black identity, which is in the process of incorporating Langa 1985 into the common matrix.

Here we must be clear about what is varying and being transmitted. The elements being combined and recombined are constructs or, to borrow a term from Dawkins (1978), memes. That is, they are units of imitation which are carried by persons. Other terms, such as mentifacts and culturgens are also used (e.g. Lumsden and Wilson, 1981). These "units of imitation" are not only ideas, in the sense of logical propositions; they are customs, practices, images, bonds, stories and so forth.

How are memes transmitted? Dawkins refers to imitation, but we realise that the process of socialisation is more complicated than that. Persons are constituted, as we like to say these days, in a complex discourse. Some impression of this discourse (but

only an impression of its verbal shape) has been given in analysing some features of what Smuts and Alexander had to say to their audiences. But, as we all know, there is more to it than that. Models are presented and shown to be powerful (or powerless), rich in significance and ideas (or insignificant), and moral (or immoral). Identity is embedded in culture – texts (Lotman, 1979) which we learn to read, both formally and informally, directly and indirectly. A full account of transmission will involve a discussion of ontogenetic sequences in learning, modes of learning and the forms of culture text and discourse.

Now, what about the third feature in any account of the social evolution of identity, viz. selection? When we note the spread of one form of identification – such as race or ethnicity – and the decline of another form – such as the nobility/commoner distinction, we are apt to think of them in terms of moral superiority or inferiority. Of course, we do and must make such judgements, but then judgements do not explain why a particular mode of social identification should spread. An evolutionary account will attempt to understand these phenomena in terms of the advantages or disadvantages which they confer upon the persons who are identified in this particular way in a particular social context.

The simplest way to look at a system of identification which is spreading rapidly is to ask: What advantages does it confer upon the persons who are to be identified? And, equally important, what is the ecological niche in which such advantages are

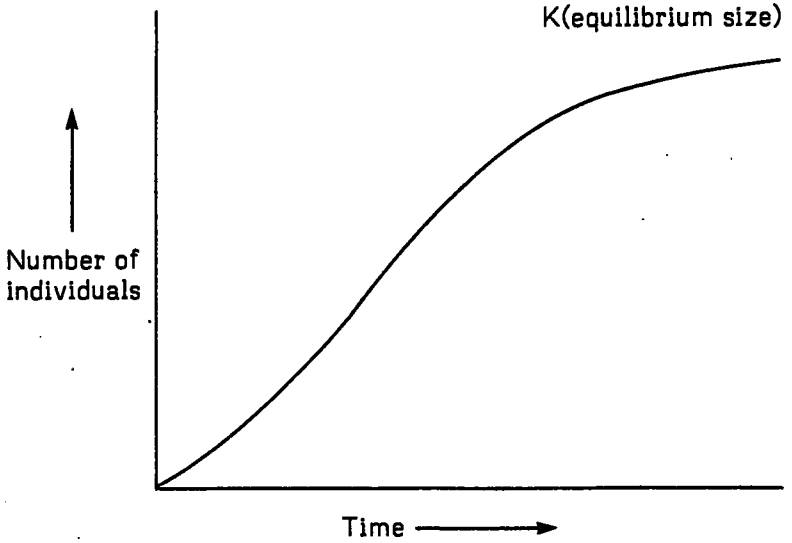
obtained?

Consider racism as a mode of identification. When did it flourish? What advantages did it secure for those who promoted it? Racist theories of a particular type divided the world into European and Native and superimposed this distinction on a variety of more detailed classifications (Indian, Negro, Javanese, etc.) Racist theories spread rapidly in Europe in the 19th and early 20th century, during the period of colonial expansion. Skills were measured, tests were conducted, anecdotes were exchanged, history was rewritten, and all tended to demonstrate an irrefutable fact, that Europeans are both different from, and superior to, the native races of the world. It is not difficult to see the advantages of such an identification to Europeans during a period of rapid colonisation. Accordingly, this form of identification spread rapidly in the ecological niche of the colonial period of European history.

The simplest model for the spread of a particular species of identity is provided by the Verhulst logistic equation for self-limiting population growth in a closed environment, in which one assumes that there is a certain equilibrium population size. The equation

$$\frac{dN}{dt} = rN\left(1 - \frac{N}{k}\right)$$

yields a sigmoid growth curve of the following kind



In the above equation, dN/dt is the rate of change of population size N, k is the equilibrium population size, and r is the growth rate per individual without any self-limiting condition. (Collier, Cox, Johnson and Miller, 1973: 216-217).

However, the environment of interest to us is not a stable one and the conditions of growth are those of competition rather than of self-limitation. Firstly, let us consider the environment once more. After the period of territorial incorporation, subject peoples were socially incorporated, though in subordinate positions, into the economic system, as workers and consumers, into the administrative system, as persons to be administered and also as minor administrators, and into the cultural system, by schooling and mass media. This incorporation transforms the "natives" by making them more skilled, more powerful and more articulate in the dominant culture.

The consequence of this is that they become receptive to competing forms of pseudospeciation or identification. What are the possibilities? The elements commonly available are:

- racism (and its invérsion)
- ethnic pluralism (and relativism)
- Marxian theory (and class struggle).

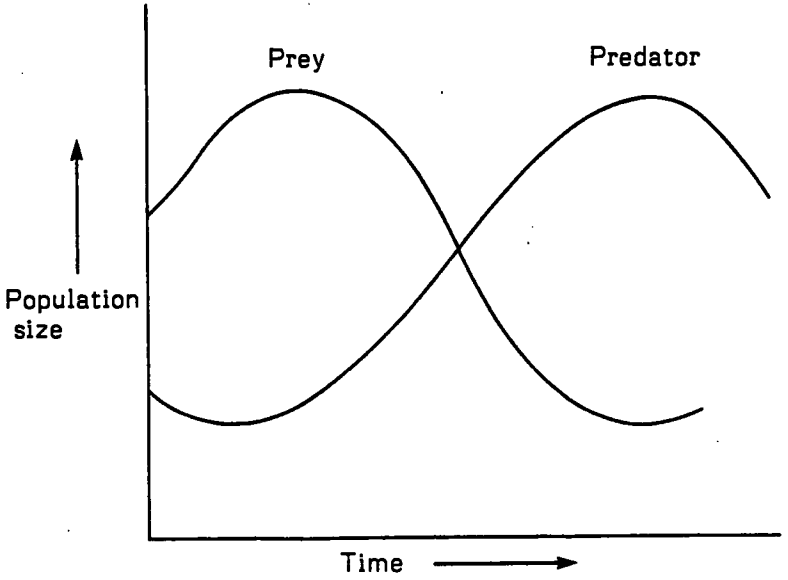
In Muslim countries, people have drawn on entirely different resources, but this will not concern us here.

The present situation in South Africa is this. The ecological niche which favoured the spread of racial identification has disappeared. "Natives" have been incorporated as skilled and semi-skilled workers, as increasingly fluent participants in the dominant cultural system, and as administrators. What form of identification will be to whose advantage? All protagonists can be expected to propose their own form of identification as a universal solution. Most whites have abandoned racism and have opted for an ethnic identification. Some blacks have opted for a racial/cultural identity, seeing the major problem as the replacement of white power by black power. Others have adopted an explicitly Marxian approach, in which the struggle of classes terminates in a common Azanian nationality in a socialist state. The nation has to be structured by and in the interests of the working class, largely black.

What is clear from this, though, is that a successful model of identification will have to describe the spread of identities

under competition. In fact, the relation of identification is commonly one of predation, in which one pseudospecies will prey on the other. White racists preyed on natives in the ecological niche of colonial expansion; and workers and the bourgeoisie prey on each other in situations of class conflict.

The appropriate model for the spread of species under these conditions is the Lottke–Volterra predator–prey equations. This is a pair of simultaneous equations[2], one representing the rate of change of the prey population, the other representing the rate of change of the population it is preying on. This kind of relationship is represented graphically as follows:



Cesare Marchetti has applied these equations to the growth of different technologies and energy systems (such as wood, coal, oil, gas, nuclear energy) and has been able to treat them as species competing for a niche. He has found that the energy system behaves as though it had ***a program, a will, and a clock .... (which) implied a potential for hard modelling human affairs*** (1985: 13).

It has been pointed out that the Lottke-Volterra equations are too simple even for biological predator/prey relations and they are certain to be too simple to give more than a sketchy account of identity pseudospeciation in different social niches. However, the significant thing is that they suggest a new methodology and a departure from merely ideological accounts of systems of identification. The task of science is to grope towards a system of explanation; moral judgement must occur in a different context. This is as true of modern sociology as it was of the attempts to formulate the helio-centric theory of the planetary system, the theory of evolution by natural selection, or the theory of the psychological unconscious.

Where should we look for evidence of the spread of different forms of identification? One place to start is in the pronouncements of politicians. We can count the references they make to different classifications of population. An equally interesting task would be to extend this work to the actual instruments and practices of administration – to regulations, acts, proclamations, and the very organization of the administrative apparatus. Where else might we search? In

popular fiction, in newspapers, in scientific theories, in conference papers. Whereas "race" was once a major subject of research and the word could be uttered without apology, ethnicity and class are now in competition. In fact, the radicals prey on the liberals at conferences and in journals. Will the Lottke-Volterra equation describe their relative growth and decline?

GENE-CULTURE CO-EVOLUTION

One of the most striking things about human beings is their tendency to pseudospeciation by identification with one or other group. The content of this pseudospeciation will almost certainly be culturally specific, but the tendency to identify probably has a genetic basis. In other words, we have evolved as a species peculiarly apt for group formation. For most of our evolutionary history, this tendency has probably had great advantages, but the cost-benefit ratio of the process may be changing.

As Lumsden and Wilson (1981, 1983) suggest, the way in which to investigate a genetic bias in the formation of mind is to examine cross-cultural universals and the preferences which children exhibit during development. They suggest the following inbuilt biases:

- a preference for mating outside the immediate family circle

a bias towards learning colour vocabularies based on the four basic colours perceived by the human brain

that infants pay more attention to certain patterns

a preference for certain kinds of sugar

anxiety in the presence of strangers at about 6–8 months of age

phobias tend to cluster round the greatest dangers of early human environment – closed spaces, heights, thunderstorms, running water, snakes, spiders.

re there any inbuilt biases in the process of identification? I elieve there are, and I would like to refer to work which has roduced puzzles which are resolved by coevolutionary theory.

has been consistently found that whereas virtually no white hildren misidentify, in the sense that they indicate that they esemble black rather than white dolls or photographs, a large umber of black children do misidentify, pointing to the white timuli as more closely resembling them. (A good review of the outh African material here is in Foster, 1984). Even where uch misidentification is absent, a large number of black hildren would prefer to be white rather than black, whereas bout 90 per cent of white children prefer their own group Davey, 1983, cited in Foster).

One way to account for these findings is to postulate that children are biased towards an identification with socially powerful figures and from an evolutionary point of view this makes good sense. If we have a tendency to identify with models who are more powerful, who are resourceful and able to cope, this may enable us to cope and be resourceful ourselves. Freudian theory, whether it is discussing anaclitic (defensive) identification or developmental identification, draws attention to the advantages which the child gains by identifying with some model. These advantages are gained in fantasy at first, but to the extent that the model is a good one, they are likely to be gained in social life at a later stage.

Our hypothesis is this:

> Children are biased towards identification with resourceful and coping models.

What evidence do we need? We need to show that there is an ethnographic curve, or a bias across a variety of cultures, towards such an identification. Which figures do children imitate? What kinds of figures do they identify with in stories? What we need are frequencies of choice, to indicate the strength of the bias, and not merely typical choices.

There may also be a sequence of identifications to be studied, bearing in mind the sequence of pattern preferences found by Fantz, Fagan and Miranda (1975). We should not assume that children will make the same kinds of choices at all ages.

Biases do not condemn us to internecine war and do not imply biological determination of our destiny. People may identify with universal heroes as well as tribal ones. What do we offer our children? Do we offer them human models or tribal ones? If we hope to lay the psychological foundations of humanity, it seems to me that figures who embody universal values are essential. They will wear local dress, use the local language, and know local customs, but they will speak to us all. Identification in childhood is merely the foundation and cannot be the whole structure, but without foundation the structure cannot be securely built.

CONCLUSIONS

After a prolonged diet of ideology, social scientists should sit down to the job of explaining social processes. The methodological implications are:

1. that we study societies as the activities of persons in systematic relation to each other;

2. that we study the ways in which social systems are connected by persons in the course of action;

3. that we study the process of identification in a variety of ways, by examining its culture texts, the activities of identity entrepreneurs, and its dynamic ecology;

4. that we investigate changing systems of identification

both in historical and evolutionary time; and

5. that we examine the developmental biases of children towards identification with certain kinds of models.

If we do these things, I am confident that we shall discover much that is of interest.

Note (1)

I wish to acknowledge the help of Professor Elizabeth Nel and Mr Eugene van Niekerk in collecting data on prototypes.

Note (2)

The Lottke–Volterra equations consist of a pair of simultaneous equations, one representing the rate of change of the predator population, the other representing the rate of change of the population it is preying on.

$$\frac{dN_1}{dt} = (\alpha_1 - \beta_1 N_2)N_1$$

$$\frac{dN_2}{dt} = (-\alpha_2 + \beta_2 N_1)N_2$$

N_1 = prey population size

N_2 = predator population

α_1 = growth rate per individual of the prey population in the absence of the predator

$-\alpha_2$ = rate of decline per individual of the predator population in the absence of its prey

β_1 = decrease in prey population growth rate per individual predator present

β_2 = increase in predator population per individual prey present.

(Collier, Cox, Johnson, Miller, 1973: 218)

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5.

PSYCHOLOGICAL RESEARCH WITH INDIAN SOUTH AFRICANS: EXPLORATIONS IN METHOD

K. Bhana

There is an embarrassing paucity of psychological research on Indian South Africans. For this paper psychological research will be defined very broadly – all the research that looks at problems and issues traditionally studied by psychologists will be interpreted as psychological research.

The Indian group first came to South Africa as indentured labourers from various parts of India in 1860. By 1910 there were a total of 149 791 Indians of which the Cape had 6 606, the Transvaal 10 048, the Orange Free State 106 and Natal 133 031. The 1980 census placed the figure at 794 639 and the 1983 official estimate was 870 000. Many members of the original indentured labour community elected to stay after the expiration of their labour contracts. They were joined by other Indians referred to as passenger Indians who came to South Africa on their own. These arrivals continued till 1914 when immigration of Indians was officially stopped. Thereafter only a very small percentage came in as brides.

There is an increasing amount of literature documenting the political and economic history of this community but very little on the sociological and psychological aspects (Kuper, 1960; Meer, 1964). Pederson (1979) has identified a number of philosophical and theoretical dimensions which could represent possible alternatives to the Western dimensions along which the Asian population at large could be operating. However, whether these are relevant to Indian South African persons is questionable.

TABLE 1: POSSIBLE ALTERNATIVES TO THE WESTERN CONCEPTUALIZATION OF REALITY

	WESTERN	NON-WESTERN
1.	Scientific method of knowledge acquisition	Other forms of knowledge acquisition e.g. intuition
2.	Emphasis on individualism	Collective unity
3.	Individual role in society	Relationship role in society
4.	Guilt culture	Shame culture
5.	Separation of the spiritual and secular	Unity of spiritual and secular

Firstly, this community is an acculturating community where the acculturation appears to be taking place in isolation, influenced more perhaps by the perception of social reality as portrayed in the media and the economic environment than by the actual reality.

Secondly, the Indian group is a politically defined group and within this politically defined reality are the three major religious groupings, the various language groupings, and groupings in terms of the places of origin. Thus the selection of culture as an independent variable becomes exceedingly difficult if one wants to undertake cross-cultural research. Often the answer seems to depend upon the knowledge base of the researcher. Sometimes the political grouping has been used as the independent variable and at other times the different religious groups or language groups have been used.

It might be useful at this point to examine the available psychological literature on Indians. The tracking of this was problematic: when one does not find research studies one wonders whether there really is no material or whether the tracking procedure is inadequate. The analyses that follow are based on the studies extracted from the bibliography on Indian South Africans compiled by Greyling and Miskin (1976), computer printouts from the HSRC/NIPR on all the psychological research on Indians, the issues spanning the last 20 years of major overseas psychology journals, the relevant South African journals, the thesis research of the Natal universities and by following up wordof-mouth leads. A total of 211 studies

were thus found. If one allows for a five percent margin of error the basic features of the following analyses should not change much.

The results of the analyses are tabulated below. Since in South Africa the HSRC/NIPR are major research institutions, it was decided to examine their contributions separately and in relation to the other contributors.

TABLE 2: MAJOR CATEGORIES OF PSYCHOLOGICAL RESEARCH

	NUMBER	PERCENTAGE
Thesis	87	41
Non-thesis	71	34
Publications	53	25

The major category of research on Indians is in the form of theses for higher degrees with relatively few publications. The figure of 211 studies may thus be an overestimation as publications could be based upon thesis research.

TABLE 3: RESEARCH BY MAJOR INSTITUTIONS

	NUMBER	PERCENTAGE
Universities	87	41
HSRC/NIPR	62	29
Publications	53	25
Other	9	4

The universities are the major institutions of research on Indians. The HSRC/ NIPR involvement is also relatively large in terms of non-thesis research.

TABLE 4: RACE OF RESEARCHERS

NON-THESIS			THESIS		
	NUMBER	PERCENTAGE		NUMBER	PERCENTAGE
White	103	49	White	28	13
Indian	21	10	Indian	59	28

The major portion of thesis research has been undertaken by Indian students while the non-thesis research has been conducted by white researchers. Overall, 62 % of the research on Indians appears to be of an imposed etic nature. The following table will indicate this to be so for the Indian students' thesis research too.

TABLE 5: CONTENT OF RESEARCH EFFORT

	Thesis		Non-thesis		Publication		Total	
	No.	%	No.	%	No.	%	No.	%
Construction and administration of local tests	18	9	60	28	1	0	79	37
Mental health	24	11	2	1	6	3	32	15
Attitudes, values/social aspects	17	8	4	2	8	4	29	14
Profiles of university students	4	2	1	0	19	9	24	11
Cognitive development	7	3	2	1	7	3	16	8
Standardization of overseas tests	4	2	1	0	7	3	12	6
Education/ Miscellaneous	7	3	0	0	3	1	10	5
ndustrial	6	3	1	0	2	1	9	4

The most striking feature of the above analyses is the tremendous research effort expended on testing procedures. A total of 43 percent (91) of the studies were primarily involved with this. There is, in contrast very little research in many of the essential areas of human development.

An attempt was next made to identify the methodological procedures used to obtain the above information. For this, the studies aimed primarily at the development and standardization of assessment instruments for Indians were excluded (N = 79). This left a total of 132, and from these 78 were selected at random and examined further to identify the data gathering procedure. The results are tabulated below.

TABLE 6: THE MEASUREMENT OF THE DEPENDENT VARIABLE

	NUMBER	PERCENTAGE
Standard tests	51	65
Questionnaire only	15	19
Non-standard tests	9	12
Combination	3	4

The major procedure is the application of tests. If one adds to the above those studies that were aimed at the development of tests for Indians (N = 79) then 82 % of the research on Indians involves test construction and administration and suggests that

the research is of the imposed etic type. Clearly, other methods need to be explored.

Moll (1983) identified three periods of cross-cultural research in South Africa. The first was concerned with demonstrating differences between the Black and White groups where Blacks were frequently found to perform more poorly than Whites. This culminated in institutional separation in 1948. The period thereafter attempted to develop further tests to rationalize the separatism. Then followed the post-1958 period where capitalist needs were assumed to influence the type of research which centered largely around sociological and psychological factors motivating Black workers. It would appear that research among Indians is largely in the second stage as the Indian does not feature predominantly in the South African industrial scene. Only nine studies were found on industry-related areas. This analysis could perhaps explain the tremendous expenditure on the construction of tests specifically for Indians. Or perhaps there is a simpler explanation. In the absence of social integration and knowledge of the community it may be easier to undertake test research. Then, perhaps, extensive knowledge on the emics of a culture are not required.

The "Indian" in these tests has been defined in terms of a political category and as such is clearly a non-homogeneous collective being made up of three different groupings representing three of the world's major religions (Hinduism, Islam and Christianity). If one wants to maintain the tradition of creating separate tests then one must also go ahead and

create tests for Indian Hindus, Muslims and Christians. One could also extend this to the language groupings, places of origin in India, and so on. Clearly, this becomes an absurd exercise!

Lest somebody argues that these separate tests are necessary to predict success in the Indian matriculation examination, the question is: how is this information useful? The universities are opening to all population groups – will it predict success there? If, eventually, everybody has to succeed in the same economic arena, do they predict success there? Why not develop a test for all South Africans?

But ideology and ignorance may not be the only factors influencing the type of research. The question arises as to why the more basic descriptive and emic type of research has not been undertaken by Indian scholars themselves. A few possibilities can be suggested.

The majority of Indians were in Natal but the University of Natal only opened its doors to Indians to enrol in the Arts in 1936 on a separate and part-time basis with relatively small enrollment figures. In 1961 the University of Durban-Westville was established with more Indian students pursuing higher degrees. But the majority of the staff were trained within the Western philosophical, theoretical and conceptual structures. Perhaps this made it difficult for persons to break away from their conceptual modes and develop new structures. There were signs that this may have been happening but the emerging movement has been curtailed by the professionalization of

psychology in South Africa. With the increased emphasis on the development of professional psychological skills the universities may be forced to sacrifice the development of conceptual thinkers who would be adequately equipped to develop the discipline analytically and conceptually for the training of practitioners of psychology.

Clearly, with respect to research on Indian South Africans there is a need to do research on the socialization practices, on conceptions of health and illness, on cognitive activities of children etc. One needs to abstract emics of behaviour and perhaps then aim towards the true etics of behaviour as applicable to all South Africans. In this way the procedure we eventually follow would explain the full range of a particular variable under study. Ideally, this would mean true cross-cultural research which at present is not very easy to conduct, given the prevailing ideologies (Rex, 1983).

EXPLORATIONS IN METHOD

Clearly there is a need for developing alternative frameworks with respect to South African psychological research with Indians by developing universal principles according to which human behaviour can be understood if it is to contribute toward the ultimate goal of cross-cultural psychological inquiry. A useful starting point might be to describe and identify some of the emics of Indian behaviour, proceeding to the identification of the etics valid for all persons.

There are two areas that are of immediate concern, and they relate to the areas of mental health and the family. The professionalization of psychology has resulted in universities training students of all political groups in clinical psychology. For the Indian student this has meant the absorption of therapeutic systems sometimes based upon the acceptance of the medical model and philosophical systems which have their origins in American and European philosophies, theories and social systems. In the light of Pederson (1979) and Dillbeck's (1983) work on the actual life experiences of Indians this is clearly a very limited approach. The Indians have for years been consulting indigenous healers for problems relating to mental health. The diagnoses by these healers and their therapeutic procedures clearly support the existence of an alternative mental health framework. While this has been documented in some overseas cultures like Puerto Ricans and to a lesser extent on black South Africans no such information is available on the Indian. The peculiar position of the Indian community in South Africa and its unique acculturation processes demand that such information be obtained to become an integral part of the teaching situation.

The examination of all available instruments to measure attitudes and opinions toward mental illness (Rabkin, 1972) indicated that the items did not tap the unique dimensions and issues involved in indigenous healing practices. Various procedures were attempted in order to identify the relevant items. Of these the most useful was that of asking Indian

students in a predominantly Indian university to submit 10 items in a Likert format and which were intended to tap an Indian person's attitudes and conceptualizations on the etiology and treatment of mental illness. The students were asked to consult their mothers, aunts or other older members of the community in the compilation of the items. This was done every year for three years. At the end of this period the items were examined and the most frequently submitted ones relating to beliefs about the etiology, treatment and description of mental illness were abstracted. These were put into a standard format where necessary: double-barrelled items were for example modified and comprised the scale denoting the Eastern conceptualization of mental illness. To compare this Eastern conceptualization with the Western conceptualization, the Opinion towards Mental Illness Scale (OPI) developed by Cohen & Struening (1962) was examined. Analogous items were extracted or developed – for example, ***Mental illness can best be cured by prayer;*** (Eastern conceptualization) versus ***Mental illness can best be treated by psychologists,*** (Western conceptualization). In this manner a 5-point, 70 item Likert scale was constructed, consisting of 10 items representing each of Eastern and Western concepts of etiology, treatment and description plus 10 filler items. They were then applied to university students and their mothers in a series of studies. It was felt that the Indian university student population would be fairly representative of all sections of the community as previous researchers have indicated (Behr, 1972). In addition, the mother group can be regarded as fairly representative of Indian middleaged females. It was reasoned that if the items collected over three years had no relevance

then they would lack any discriminative power with the new set of students and their mothers.

With this scale a series of studies were undertaken to examine the attitudes and conceptualization of mental illness of Indian subjects. The results of the initial study (reported in Bhana & Bhana, 1985a) included the finding that the etiology of mental illness was conceptualized largely in terms of the Eastern orientation but both Eastern and Western orientations were equally acceptable with respect to treatment. The second study (reported in Bhana & Bhana, 1985b) established that an extended period of exposure to formal psychology courses tended to make the students more accepting of Western modes and, on this aspect, to increase their similarity to their mothers. The third study (Bhana, 1984) found that the adolescents' perception of their mothers' conceptualizations differed significantly from the mothers' actual conceptualizations. This was linked to the adolescents' perception of their mothers' level of religiosity and westernization. The fourth study found that the largest difference between elderly Indian women (mean age 61 years) and their middle-aged daughters (mean age 41 years) was with respect to the treatment of mental illness. Further, the understanding of mental illness was primarily in Eastern terms but there was an acceptance of both Eastern and Western conceptions of etiology and treatment.

The above studies, briefly mentioned, suggest one major point. The existing scales are not adequate for measuring Indian

subjects' conceptualizations of mental illness. A replacement of these with emic scales *per se* does not provide a solution either. Eastern and Western orientations feature prominently in the Indian subjects' conceptualization of mental illness. Any further research in this area will have to consider this basic feature in its design. Further experimentation with the scale is needed to eventually construct an instrument with the necessary range of variation. This must not be a scale for one group only but an expanded scale for all groups which would be equal to the task of identifying the true etic concerned.

Another method that is also of great potential value in cross-cultural research is the use of more loosely-structured open-ended questionnaires. Using fixed response questionnaires may be more "objective", "scientific" and may be regarded as "good research" because all the right statistics can be done and subjectivity in scoring can be eliminated. While this may be so, subjectivity in interpretation can certainly not be eliminated. These objective scales impose a particular theoretical structure upon the subject, unless of course they have been emically constructed. They generally exclude many possibly contentious but crucial features of a social, economic, political or ideological nature that determine and influence the behaviour of individuals. What is suggested is that more open-ended questionnaires be used to identify pertinent issues. An example of this was the attempt to identify information on Indian persons' belief in and consultation of indigenous healers for illnesses.

This research identified another important issue that can be operative in cross-cultural research: the cultural affiliation of the researcher. Some researchers aware of this fact have employed members of the group being studied to do the actual field work, but the studies themselves have been conceptualized within a "different" theoretical framework and have often been interpreted within that framework as well (Rex, 1983). My explorations in method suggest that if one wants to identify the psychological structures operative in a group then more open-ended questionnaires should be administered by persons linked with the group and these same persons should be allowed to interpret the results as well. The ideal situation would be one where such a person is given the opportunity for input at the design and measurement stage and, after the field work, is allowed the opportunity for independent interpretation. Thereafter the two interpretations and analyses could be discussed and meaningful structures identified. It serves no purpose simply to employ people to conduct the fieldwork only, and then to claim to have acquired meaningful data.

In a preliminary and exploratory study many dimensions about the consultation features of healers and doctors were identified (Bhana, 1985). The procedure involved the administration of an open-ended questionnaire in an interview format. The analyses revealed, among other things, that patients "shop" around with healers as they do with doctors. The healers were more likely to diagnose in terms of "being tricked" and "evil" and treat patients with amulets and holy water in a psychologically supportive atmosphere. The results also supported some of the conclusions

of the studies mentioned above. The patients accepted the diagnoses of the healers but when it came to treatment they were following the two sets of treatments (healers and doctors) concurrently, which suggests that there should be closer links between doctors and healers. Perhaps a more effective treatment format could be worked out if the patients are managed in a complementary fashion rather than in opposing directions.

Another area that could benefit from an open-ended approach is that of child rearing and development. As indicated in table 5 there is a virtual lack of any research in the area. One of the most promising and relevant conceptual approaches to this area has been proposed by Ogbu (1981) who believes that a general theory of child rearing and development could emerge only after the patterns of child rearing of different groups and societies have been mapped out in their respective contexts. Ogbu's (1981) model was formulated within the context of stable societies. It could be that in transitional societies the suggested congruence between adult competencies and child rearing patterns become weakened, leading to increased intra- and extrafamilial conflicts. Nevertheless, this model was thought to provide a good starting point for mapping patterns of child rearing in the Indian community.

A deliberate attempt was made to avoid the available instruments with their implicitly encoded dimensions. In the first of a planned series of studies an open-ended questionnaire

format was used to identify the competencies valued by the mothers in their children as well as in themselves (Naidoo, 1985).

The results included the findings that good and bad mothers were conceptualized primarily in terms of having positive and negative psychological characteristics. Good and bad children, however, were conceptualized in terms of behaviour. A well mannered, well behaved, obedient, respectful and a quiet child was more highly valued than a child possessing positive features like being independent and intelligent.

Another vitally important procedure is the use of the observational method in, preferably, an ecological context. The observations must be conducted over an extended time period. But the critical issue here relates to the identification and interpretation of the relevant scoring categories: it is very tempting when dealing with observation records to abstract categories in terms of the researcher's hypotheses which, again, have generally been influenced by the prevailing theorising (Western-based) in the area.

Thus at this stage, the utilization of non-standard procedures and bold explorations in perhaps even unconventional methods, might develop new insights and seems to be the most fruitful procedure of mapping important issues in understanding behaviour. It needs to be emphasised that the attempt here is not to develop an ethnocentric model but rather to provide a basis for meaningful cross-cultural or intergroup comparisons.

CONCLUSION

The above presentation may suggest to some that what is propounded is the development of Indian psychology as distinct from the traditional Western-oriented psychology. This issue is a very real one and before making any hasty decisions one should examine carefully a similar debate involving the American society (Jones, 1980). The major argument was, that since the Blacks have a unique world view and a distinctive ethos, mainstream psychology does not explain Black behaviour adequately and is thus irrelevant for Blacks. This then perhaps justifies the development of a separate Black psychology. The counter argument has been that American Blacks are part of the community and have to operate within the social and economic system and so, rather than to develop a separate psychology, attempts must be made to extend the traditional psychology to incorporate the Black dimensions. Similarly one could argue that because Indians have a definite culture and religion and are politically and socially isolated one should expend efforts to develop an Indian psychology catering for specific Indian concerns. This, however, is not the way this paper should be interpreted.

Rather, what is being suggested is that Indians, like other groups in this country, form a political minority group. Tension exists between the politically defined reality of this group and its cultural heritage. It further has to operate within the economic and technological systems of the political majority group which is Western-oriented. To operate successfully requires

competencies and values necessary for coping with that environment, and this can also create tension between those competencies regarded as being valuable in terms of the cultural heritage and those values required for success in the Western-oriented technological environment. Because of this symbolic relationship, investing research efforts in the creation of Indian tests or examining how Indians cope with the tests of other groups is not the way to be followed in developing local psychology (although it may be the way of serving specific group aims).

What is needed is research on the Indians and the other political minority groups that will map out the behaviours and constructs and the emics of these groups. This may reveal that what is happening is that persons, in addition to having the behaviours, values and competencies that reflect their heritage, may also be displaying the rudiments of alternative patterns of behaviour which they try to utilize in coping with economic demands. These patterns are not replacing the old ones but are co-existing with them. The obvious conclusion is that the individual is developing behavioural repertoires for specific situations, and any test either specifically for Indians or adapted for them may not be capable of revealing this.

Research is needed to identify the cultural patterns and the slowly emerging rudiments of alternative behaviour patterns. This means that in the tradition of true cross-cultural research one needs in this country to identify the different sets of behaviour patterns, to develop a list of the full range of

behaviours and utilize these when remediation in school and industry is required to develop alternative coping strategies without trying to eliminate the cultural ones. In this way we could then extend the scope of psychology in South Africa to cope with the behaviour of all its people and in so doing move away from a deficit hypothesis where attempts at remediation and treatment are made on the basis of the Western group as the standard.

The task before us is a challenging one. Bold explorations in method and procedures need to be developed and encouraged. The local journals will possibly need to re-examine their requirements for publications, and accept more research where the procedure does not involve only the administration of standard instruments. The use of more open-ended questionnaires, interviews and interpretations within non-standard frameworks can assist the ultimate aim of developing a psychology in South Africa which is unicultural. Perhaps we will discover that having "culture" as the independent variable is not the most useful way of doing research. We may rather find that the type of education, socio-economic status, and physical environment are the more critical independent variables which transcend "culture" as the independent variable. Perhaps the ultimate irony of good cross-cultural research would be the discovery that cross-cultural research is neither necessary or relevant. Peoples' behaviours are influenced by a conglomerate of many factors, and we should try to identify these patterns and in so doing move away from the value loaded culture construct.

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6.

METHODOLOGY: A FOCUS FOR CHANGE

R. Miller

It is a sobering exercise for social scientists to reflect on the views of other scientists whose methods and achievements they have tried so hard and for so long and with such meagre success to emulate. The eminent biologist, Sir Peter Medawar, in a series of lectures on the nature of scientific method, commented as follows:

> *Ask a scientist what he conceives the scientific method to be, and he will adopt an expression that is at once solemn and shifty-eyed: solemn, because he feels he ought to declare an opinion; shifty-eyed, because he is wondering how to conceal the fact that he had no opinion to declare. If taunted he would probably mumble something about "Induction" and "Establishing the Laws of Nature", but if anyone working in a laboratory professed to be trying to establish Laws of Nature by induction we should begin to think he was overdue for leave* (1969: 11).

Perhaps the time is due for psychology to take sabbatical leave from its laboratory and, in a frame of mind detached from the

ily routines and rituals of data gathering, analysis, and print, regenerate its energy and sharpen the intuitions that vigorate the progress of science.

he title of this contribution – Methodology: a focus for change is deliberately ambiguous. As implied in the opening omments, we could take time out to rethink and change our ethodological persuasions, perhaps updating some of the more ubious prescriptions of what constitutes respectability in cience: more in accordance with the actual methods of the dvanced sciences and less in the uncertain light of a certain iennese circle. But the title has a more direct and, in the ncertain climate of our time and place, a more salient eaning. The most urgent problem confronting social scientists this country is that of change; social change in all its forms nd dimensions. The issue is not whether one is for or against nange. The issue is how to study change whether as an aloof bserver in the manner of the astronomer, or as a dedicated unctionary committed to some vision of the future.

ith change as the object of our understanding, methodology ecomes the lens through which the object may be brought into ocus. No amount of grinding away at an opaque lens will roduce even a blurred outline of the process of change. Much what is called methodology may better be described as ethod. If methodology means a set of procedures then there is ttle justification for cluttering our already overburdened social cience jargon with another "ology". But there is more to ethodology than method. Perhaps a more explicit term would

be metamethod, or to use Vygotsky's (1978) term, theory–method.

Unlike methods, such as statistical procedures, that can be detached from their objects of application, theory–method or methodology is intimately part of the phenomenon of interest. A methodology of change must be shaped by the nature of change and, in turn, must illuminate that nature. Although he uses the term method in the following passage, Vygotsky is not referring to the procedures that one typically encounters under journal headings of method.

> *The search for method becomes one of the most important problems of the entire enterprise of understanding the uniquely human forms of psychological activity. In this case, the method is simultaneously prerequisite and product, the tool and the result of the study* (1978: 65).

According to Vygotsky, these contrary conditions can only be adequately satisfied by a developmental methodology. To penetrate the outward appearance of what he called "fossilized behaviour", psychological processes must be traced back to their source in order to reconstruct the transformations that occur in the process of development. Vygotsky's theory–method deserves careful consideration in any attempt to relate mind and culture or more generally psychological and social processes. This is because for Vygotsky the distinctly human higher mental functions are intrinsically social in the sense that their origins do not lie in the biology of individual actions but in the culture of social interactions.

Vygotsky explicitly rejected the standard experimental method, what he called the S-R method, in which a subject is confronted with a stimulus situation and produces a response that is analyzed by the experimenter. He argued that although this approach may yield information of a descriptive nature, it could not provide an explanation of higher mental processes. Vygotsky's arguments are remarkably similar to those of Harré and Secord; remarkable in the sense that they anticipated by some fifty years ideas that have shaken the foundations of much of the psychology that developed in the intervening half century. The thrust of Harré and Secord's critique is that early in its development, psychology adopted as the basis of its methodological and theoretical foundations a set of ideas that have *placed severe constraints on the possibilities of discovery in psychology* (1972: 51).

> *These three conceptions, the mechanistic model of man, a Humean and externalistic idea of cause, and science conceived in the logical positivist form, are, we believe, still the unconsidered foundation of a very great deal of modern psychology, particularly at the experimental level. They are so deeply rooted in the thinking of the majority of psychologists that is is as if we were wearing blinkers, or moving about in a two dimensional world* (1972: 33).

The blinkers are those imposed by the experimental method whose two dimensions are the independent and dependent variables as expressed by the functional relationship $y = f(x)$. In cross-cultural psychology, culture has the status of the independent variable while the dependent variable is some psychological phenomenon of interest such as personality characteristics and, more recently, cognitive performances of

various kinds. Before discussing in any more detail the implications of Vygotsky's theory-method, we need to explore further the reasons for rejecting an experimental methodology given the fact that, with some notable exceptions, it is one of the few issues about which there is general consensus among research psychologists. Rather than attempt a purely formal analysis, it is instructive to borrow some data, not hard data, but an example that involves a thought experiment and highlights a major, if not the major, weakness of the experimental approach in cross-cultural psychology. I am indebted to Lee Sechrest and his co-author Elizabeth Brown from whose chapter on *Experiments in Cross-Cultural Research* in the *Handbook of Cross-Cultural Psychology* I have borrowed the example. In their chapter, they discuss the familiar and sometimes intractable problem of construct validity. At the heart of cross-cultural experiments are the comparison groups that embody the cultures being compared. The authors comment as follows:

> *Comparison groups that are equivalent in pretreatment are necessary in any research, for pre-existing differences present a powerful obstacle to firm conclusions and interpretations of findings. The problem of pre-existing differences is inherent in cross-cultural research, for the cultural groups are, by definition, different to begin with. It is essential, however, that groups are as similar as possible with respect to all variables save that of cultural experience. Comparison of European college students with Eskimos would not produce data of an interpretable sort for most treatments. One might wonder if there is any way that Europeans and Eskimos could be compared at all, given the many irreducible differences that exist, certainly not easily and not with any assurance. However, some comparisons may be more meaningful than others* (1980: 308-309).

The point I would like to make is that the issue is not one of either firm conclusion, ease or assurance of comparison, or even whether some comparisons may be more meaningful than others. All of these suggestions imply that what is at issue is precision or accuracy and that with sufficient care, time, effort, and sophistication or training in experimental research methodology, meaningful comparisons of the Eskimo versus European kind are possible or obtainable. Contrary to these beliefs, it seems that the very opposite may in fact be true. The more and the better we apply the canons of experimental research the less meaningful the comparison may become. This assertion hinges around the following statement. ***It is essential, however, that groups are as similar as possible with respect to all variables save that of cultural experience*** (1980: 308). The difficulty is that it is seldom clear how cultural experience may be separated from the variables in question. The central question is whether variables can have meaning outside of or detached from cultural experience. The example that the authors provide is as follows:

> ***One could, for example, compare European and Eskimo political leaders on an experimental test of factors influencing decision making processes. The interpretation of findings would not be completely straightforward, but it would probably be enhanced if the groups were equated at least for age, size of community and for in-or-out-of party status. The most important task would be to identify the dimensions of greatest potential competitive power with respect to the cultural difference and try as closely as possible to equate for those*** (1980: 309).

These dimensions may include variables such as education,

socio-economic status, and personality factors. But what becomes of the concept cultural experience? What could it possibly mean if not some blend of the variables that must be equated? Even if we consider the most bland of the variables under consideration, that of age, it is difficult to understand how it can serve as a variable that is independent of cultural experience. Apart from physiological maturation, age is an index of experience and that experience is largely, if not entirely, cultural. But it is not only the quantitative aspect of age that is bound to cultural experience. Age is also qualitatively embedded in cultural meanings. In some societies, 16 year olds may be regarded as confused adolescents whereas in others they may be expected to function as independent adults who are ready for marriage and responsible for the provision of food and protection. Similarly, old age may be culturally defined in very different even opposite ways. In some societies, the aged may be the decision makers whereas in others their views may be disregarded or even derided. From a psychological perspective then, age is not a simple quantitative variable. To understand decision making, it may be necessary to control for age across cultures by ensuring that people of very different ages are compared. But this kind of control is not intended to isolate or partial out cultural experience. On the contrary, it is an attempt to saturate the concept of age with cultural experience. Of course the same analysis could be applied to any of the variables we would select such as gender, size of community, occupation, education, and so on. The point is that the better our experimental controls, the less meaningful the comparison becomes in terms of the logic of experimental

methodology. This is because culture is not a residue that is left over after all the variables that may account for any differences between groups have been neutralized. The difficulty of assigning culture the role of an independent variable or treatment is perhaps best illustrated if we consider what kind of control group we would need to ensure that any difference yielded by the dependent measure is in fact due to the treatment. Clearly, we would require a group with no cultural experience.

Now the absurdity of this idea must not obscure the source of the problem. The reason why we cannot produce a control group without cultural experience is not because such groups are rare or difficult to find, or because for ethical reason they cannot be constituted; the reason is that it is simply meaningless to speak of a group of humans without cultural experience. It is an empty concept.

Clearly, it is not the case that the notion of culture is a meaningless concept or that the experimental method is a meaningless way to acquire knowledge. It may, however, be that culture is not amenable to analysis by means of the experimental method any more than history or evolution. Decision-making is not a process that is dependent on a culture that in turn is independent of the effect it produces. Decision-making is culture and the decisions made by individual members of a society constitute the culture of that society. In other words, the dependent variable, decision making, constitutes an inextricable part of the independent variable,

cultural experience. This raises the possibility that what has been construed as an effect, outcome, or dependent variable could be, and perhaps should be, regarded as the independent variable, treatment, or cause. If there is any validity to this argument then the primacy of experimentation as a means of exploration to deeper understanding is undermined.

It is interesting that essentially the same problem is encountered if we shift a level and consider the problem of change from an anthropological perspective. Instead of mind and culture the focus shifts to culture and society or social structure. In discussing the limitations of functionalist theories in anthropology to account for change, Geertz locates the problem in the *failure to treat sociological and cultural processes on equal terms* (1973: 143) by regarding them as either in- or outgrowths of one another such that culture is derived from forms of social organization or the reverse. He argues that cultural processes and social structure must be treated *as independently variable yet mutually interdependent factors* (idem: 144). Clearly this relationship between cultural and social structure is of the same order as that suggested between mind and culture. In fact, Geertz's comments in the following passage concerning culture and social structure could equally well be applied to mind and culture:

> *Though separable only conceptually, culture and social structure will then be seen to be capable of a wide range of modes of integration with one another, of which the simple isomorphic mode is but a limiting case – a case common only in societies which have been stable over such an extended time as to make possible a close adjustment between social*

and cultural aspects. In most societies, where change is a characteristic rather than an abnormal occurence, we shall expect to find more or less radical discontinuities between the two. I would argue that it is in these very discontinuities that we shall find some of the primary driving forces in change (ibid).

Geertz continues his analysis and provides an ethnographic account of a funeral that turns into a drama in which the actors appear to be playing out a well rehearsed script on the wrong stage. Instead of a polished smooth performance, improvisations made by some of the actors catch the others off-guard with the result that an atmosphere of confusion prevails leaving the cast and the audience feeling distinctly uncomfortable and tense. In this situation the cultural script or learned behaviour consisted of a set of rituals that had been developed in the context of rural village life but were being enacted in the town having been transported there by "peasants-come-to-town":

Particularly in the kampongs, the off-the-street neighborhoods in which the common Javanese townsmen lived crowded together in a helterskelter profusion of little bamboo houses, one finds a transitional society in which the traditional forms of rural living are being steadily dissolved and new forms steadily reconstructed...What is occuring in the kampongs is not so much a destruction of traditional ways of life, as a construction of a new one; the sharp social conflict characteristic of these lower-class neighborhoods is not simply indicative of a loss of cultural consensus, but rather is indicative of a search, not yet entirely successful, for new, more generalized, and flexible patterns of belief and value (1975: 150).

To understand how new cultural forms or patterns of behaviour become established we must move back to the level of individual

action or mind in action where social change, to borrow a phrase from Medawar, moves closer to the ground. But in this shifting back and forth between levels of analysis, it is important not to lose sight of the common link between them. The link is culture but we must be careful that we do not strain our concept of culture to its breaking point by pulling at opposite ends of the chain that links individual minds and social structures. What Geertz called omnibus definitions of culture in the tradition of Tyler's complex whole that is "acquired by man as a member of society," (1958: 1) too much is said about too little. How people become members of society and how they acquire culture, seems an integral part of the knowledge, belief, art, and customs that constitute the complex whole and not an adjunct to it. The distinguishing feature of culture is not that it is acquired or learned but that it is constructed and reconstructed by people; and the distinguishing feature of people is not their ability to learn but their ability to learn from and through the constructions of other people. How this is achieved is the substance of Vygotsky's theory and in order to understand the processes involved he advocates a specific methodology.

The basis of Vygotsky's methodology lies in his distinction between descriptive analysis of manifest forms of behaviour and explanatory analysis of the processes that generate behaviour at any given point in development. This distinction is the same as that made by Harré and Secord. Drawing from the methodology of the advanced sciences, they argue that the core concept in scientific explanation is that of generative mechanisms; that is,

the processes that produce the non-random patterns that are identified in the course of description.

Although higher mental processes may be recognized and described in their outer appearance or manifest form, Vygotsky argued that they can only be understood in "the light of history" (1978: 64). But as he points out, to study something historically does not mean to study some past event. For Vygotsky, history is a method and in a psychological context, the historical method translates into the developmental method; not a conglomerate of regional psychologies of infancy, childhood, adolescence, and adulthood, but a lens with which to bring into focus the formations and transformations of mental processes in the attainment of their distinctly human stature.

What is distinctive about human thought is that it is grounded in culture, what Geertz has called webs of significance in which people are suspended by their actions of spinning (1975: 5). The task of Vygotsky's socio-psychological theory is to show how individual minds become entangled in socially mediated webs of significance.

> *From the very first days of the child's development his activities acquire a meaning of their own in a system of social behavior and, being directed towards a definite purpose, are reflected through the prism of the child's environment. The path from object to child and from child to object passes through another person. This complex human structure is the product of a developmental process deeply rooted in the links between individual and social history* (1978: 30).

Vygotsky thus lays the foundation for a psychology that is intrinsically social. Higher mental processes – those distinctly human features that set us apart from other animals, such as language and reflective thought – are not generated out of the commerce between isolated biological individuals and material objects, between subjects and objects. No amount of traffic between human subjects and material objects would yield even the most elementary but fundamental thread of significance, knowledge of one's personal name. For Vygotsky, the crucible of that complex whole that we refer to as culture is fashioned in a three dimensional mould in which subject and object do not collide head-on but are introduced through the agency of others. It is in this sense that Vygotsky shifts the emphasis away from a grotto conception of mind – to borrow a term from Ryle – towards the market-place of semiotic transactions. But there is also a deeper significance in Vygotsky's work. The chain of reasoning such that mind is located in the brain and the brain in the head and the head in particular biological individuals, and that therefore mind is located in or a property of singular people, rests on the validity of its major premiss. But the concept of mind as a property of individual heads may require revision. Part of the difficulty lies in the very duality of our concept of mind as a private part, and yet public expression of whatever our heads do. The problem is how to reconcile these two images or to phrase the question differently, to ask how it is possible for mind to assume its two-faced posture.

Perhaps the least fruitful approach is the attempt to pin the mind down in a definition by location. To use an analogy at

least as old as Heraclitus, locating thoughts may be like locating a river. We may well ask of William James where the "stream of consciousness" flows. When we locate a river between its banks not only do we say something about rivers and banks but also, and perhaps more significantly, we say something about the nature of the term location. Rivers are not less substantial, let alone real, because by their action of flowing they locate themselves; and banks are not insubstantial metaphysical entities because without rivers to sustain them they cease to be banks. In these terms, location is not a place in the sense of fixed co-ordinates but a place where something happens; a place of no fixed abode like a Bedouin homestead. There are many lessons to be learned from rivers, but the most useful in the present context may be that we think through our heads, not with our heads. The thoughts "in" our minds are no more inside our minds than the water "in" a river is actually inside the river. With the concept of river to fall back on, the essentially social nature of thought becomes less puzzling when one is caught in the apparent contradiction of sitting in solitude wrestling with ideas that seem to rattle around the cranial cavity.

In a passage that could easily have been written by Vygotsky, Geertz comments that: *Human thought is consummately social: social in its origins, social in its function, social in its forms, social in its application* (1975: 360). But this does not mean that here is nothing more to say about thought. It is helpful to emind ourselves that the fact that river water is consummately et, does not remove or alter the fact that its flow is

constrained. These constraints are not homogeneous in their nature. Some are intrinsic to the composition of the river, others such as the terrain through which the river flows, are extrinsic. One way to understand a river is to study its flow; its source, tides, turbulence, and ultimate destination. Vygotsky expresses this as follows:

> *To study something historically means to study it in the process of change; that is the dialectical method's basic demand. To encompass in research the process of a given thing's development in all its phases and changes – from birth to death – fundamentally means to discover its nature, its essence, for "it is only in movement that a body shows what it is". Thus the historical study of behaviour is not an auxiliary aspect of theoretical study, but rather froms its very base* (1978: 65).

Vygotsky's adoption of an historical method was a deliberate attempt to apply not the substance but the method used by Marx. But what distinguishes Vygotsky is that he did not confuse Marx's Capital with his own; social change with individual change, history with ontogenesis. The territory that Vygotsky stakes out as the peculiar domain of psychology is not distinguished by its terrain – mind, culture, society – but by its location; not an analytic set of disciplinary co-ordinates but a place where mind and culture are constantly at play: the development of individual children in particular societies with distinctive cultures.

It has now become fashionable to oppose Vygotsky and Piaget, but it is important to recognize that a similar methodological strategy was adopted by Piaget with respect to epistemology.

Drawing on the work of another great historian, Piaget applied the developmental methodology of Darwin to explain how a distinctive aspect of culture, logico-mathematical reasoning, develops in children. But for Piaget, the development of mind, or those higher mental processes that constitute logico-mathematical reasoning, is primarily an affair between an active subject or agent and objects. Through the exploration of objects, the child learns not only about those properties that inhere in the objects, but also about properties that are a function of the child's actions on objects such as reversibility – what Piaget calls reflective knowledge. Culture and social life in general provide a developmental context for Piaget's epistemic subject but are not treated as formative factors in the developmental process. Language as a vehicle for symbolic communication is an emergent property of essentially biological mechanisms that are operative in the sensorimotor period prior to the onset of language. It is only after a long period of development that the child is able to shed an egocentric frame of reference and participate fully in a world of social knowledge. Leaving aside the details of Piagetian theory, the underlying conception is of a biological organism that grows into a culture much the way that children grow into the old clothes that their siblings have outgrown.

Vygotsky projects a very different view of the development of mind in which, to borrow again from Geertz, ***cultural resources are ingredient, not accessory to human thought*** (1975: 83). Higher mental functions are located between agents, that is in the exchange between active subjects with respect to objects.

Thought does not slowly become social bit by bit but is fertilized from its inception with the seeds of social life.

> *Every function in the child's cultural development appears twice: first on the social level, and later, on the individual level; first between people (interpsychological), and then inside the child (intrapsychological). This applies equally to voluntary attention, to logical memory, and to the formation of concepts. All the higher functions originate as actual relations between human individuals* (1978: 57).

When the developmental method is put to work to map out the transformations that occur in the ontogenesis of mind, the kinds of generative mechanisms that are implicit in the Piagetian and Vygotskian constructs of mind emerge as the private and public faces of thought. For change to be possible, for development to occur, the individual's actions must be regulated. The construction of Piaget's reflective knowledge by the child presupposes as a necessary condition a set of regulatory mechanisms. Organization, adaptation, assimilation and accommodation are all terms that denote regulations of one kind or another. In general, Piaget uses the term equilibration to describe the dynamic or regulatory mechanisms that generate change and they are located within the subject, inside the biology that constitutes each private individual. Clearly, for Vygotsky, the regulatory mechanisms that generate change from the inter-psychological or social level to the individual intra-psychological level must be extrinsic to the individual and located in "the relations between individuals". Parallel to Piaget's self-regulation or equilibration, Vygotsky proposes that change or development is other-regulated and that this

egulation of one subject by another constitutes what he terms ne zone of proximal development.

ygtosky defines the zone of proximal development as follows:

> *It is the distance between the actual developmental level as determined by independent problem solving and the level of potential development as determined through problem solving under adult guidance or in collaboration with more capable peers* (1978: 86).

his definition is similar to that of defining a river in terms of ts banks; the constraints that determine the course of its ctivity. The constraints that operate in the zone of proximal evelopment are set by the two agents, child and adult, or ninformed and informed, with respect to the activity of roblem solving. But Vygotsky also provides a second definition n response to the question, *What, then, is defined by the zone of roximal development?*, given the constraints that determine its peration.

> *The zone of proximal development defines those functions that have not yet matured but are in the process of maturation, functions that will mature tomorrow but are currently in an embryonic state. These functions could be termed the "buds" or "flowers" of development rather than the "fruits" of development. The actual developmental level characterizes mental development retrospectively, while the zone of proximal development characterizes mental development prospectively* (1978: 86).

his definition relates to what happens between the banks; the ynamics of the flow of the river from its source to its

destination. As Vygotsky points out, the zone of proximal development provides us with a conceptual tool with which to unravel the constructive processes that generate human thought. The two sets of constraints or generative mechanisms at work in the zone of proximal development, the uninitiated child and the initiated adult may be understood as mind and culture.

The value of experimentation lies in the power of a well designed experiment to identify which treatments or conditions produce the observed outcomes or effects. But if the effect is part of the treatment, or itself a cause, experimental methods may become empty rituals whose meaning is difficult to fathom. The crucial question, then, is whether there are conditions in terms of which it would be reasonable to regard human action, not as dependent on cultural experience, but as a determinant of that experience. There is at least one compelling set of conditions in which human action seems inescapably cast in the role of agent and this is under conditions of change. Whatever view of culture one may take, one fact seems plain. Culture cannot change itself. Change must be mediated by human action. Scientific discoveries or technological inventions are the products of human action as are works of art, architecture, and engineering. When we confront a building or scientific theory or collections of myths, all of which are concrete expressions of change at some point in time, we are witness to the transforming nature of human action on culture. This does not mean and should not be understood to imply that human action can occur in a cultural void. The point

is not to argue that mind or human action should replace culture as the independent variable, but merely to demonstrate that under conditions of change, relegating mind or action to the status of a dependent variable can only be achieved at the cost of reifying culture. If we allow the experimental method to dictate not only the procedures but also the very terms of our understanding, then it is elevated to a methodology. The consequence of this methodology is that mind or human action and culture are artificially allocated to different sides of an equation in which the underlying conception is that of colliding billiard balls. It is this conception of psychological analysis in terms of stable fixed objects that Vygotsky opposed and sought to replace by a developmental analysis of the processes that generate change.

The child's activity is regulated by the adult and in this way the "control mechanisms – plans, recipes, rules, instructions – for the governing of behaviour" (1975: 44) that Geertz identifies as the substance of culture, become tangible targets for assimilation into an active mind that eventually can assume the monitoring function of its own activity. In this way, the flow of thought is regenerated through the transmission of culture and its transformation through the heads, hands, and perhaps even hearts, of individual agents of thought and action.

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7.

BAOBABS, OR METHODOLOGY AND PSYCHOLOGY IN THE THIRD WORLD

A.J. Gilbert

Not far from Nikiema's concession, there is a baobab tree, which was already there when he was a child. The boabab contributes to the daily life of the village, with its bark for medicines, its delicious fruit for the children, and its precious leaves for the sauce made every day to go with the millet porridge called "to". A bit further on, there is a square of eucalyptus trees planted by the people from the Water and Forests Department nearly two seasons ago. There were some whites with them when they came to ask the villagers what they should plant. Nikiema said: "Neres, karites for their fruits, oil and butter, and acacias for the animals in the dry season, when there is no more grass". The whites wrote everything down in a notebook, but when they came back they brought eucalyptus trees. "The local trees take too long to grow", they said. "Ours grow faster"....So the women had to go and fetch a lot of water from the well, which is far away, to water those strange trees which did not remind Nikiema of the old forest. Those sickly trunks will not provide more than poles for the huts. Furthermore, the wood does not burn well and the leaves are good for nothing. The people from the Department asked a lot of questions but they did not listen....

Nikiema tried to tell them that their life in the village was a bit like the boabab – which, as tradition would have it,

> *was planted upside down, with its branches in the earth and its roots in the air. All the white man wanted to see was the part that was visible above the ground. The part hidden in the ground represents the custom and tradition, and the white man is not interested in the branches and the leaves of the boabab which are below the surface...* (Thébaud, 1984: 68-69)

This anecdote captures the constant difficulty facing "experts" who have been raised and educated in a particular environment and then use their skills and knowledge to understand the lives of others who do not share the same history or life-experiences. While the expert in this anecdote was an agriculturalist or forester, in principle the anecdote also applies to psychologists who step out of their cultural milieu and try to understand the behaviour of persons from other cultures. The dilemma, articulated in cross-cultural psychology in terms of the emic versus etic debate, is particularly accentuated when psychologists who are products of the First World apply their skills in the Third World context.

In order to overcome the problems of the failure to understand the baobab, cross-cultural psychologists have given considerable attention to improving their research methodology over the last two decades. There has been the hope that by modifying the methods of data collection and analysis to be more appropriate, relevant or equivalent to the contexts in which they are applied greater understanding will occur. This refinement and sophistication has resulted in the proliferation of techniques that can be used to help overcome the problems of cross-cultural research.

Thus, to give two examples, the back translation technique (Brislin, 1970) can be used to overcome the problem of the code equivalence of verbal material when questionnaires, interview schedules or tests are used across cultures. Similarly, a variety of sophisticated correlational techniques have been developed to test the item or construct equivalence of tests developed in the First World and applied in the Third (Perera & Eysenck, 1984).

While such techniques are useful and do generate insights, the focus on refinement and greater sophistication tends to produce a technicist approach to understanding. Such an approach rarely leads to an examination of the network of the underlying assumptions, the hidden agenda, that lies behind any methodology.

Elements of this approach to understanding can be found in Berry's introduction to the volume on methodology in the Handbook of Cross-Cultural Psychology. In this introduction he states that there are three goals in crosscultural psychology:

> *to comprehend the systematic covariation between cultural and behavioural variables....*
>
> *to bring the total range, the broad variability, and all the possible differences exhibited in human behaviour within the scope of psychological science....*
>
> *to check the generality of our existing psychological knowledge, theory, laws, and propositions* (1980: 5)

Such goals, worded in the above manner, present a number of problems. First, in all three goals no mention is made of

understanding the individual; somewhere along the line he or she has been lost in the concern for methodological exactitude. Second, the goal of bringing the totality of human behaviour under "the scope of psychological science" seems to be based on the assumption that it is only through scientific methodological control that understanding will be guaranteed. Such a goal places very strict restraints on what is regarded as valid and neglects the normative base of science and the influence this has on the production of knowledge. Third, the inclusion of "our" and "existing" in the final goal, gives precedence to the contemporary knowledge of the expert. Such a priority leads to the investigation of "our psychology" and not that of others and suggests a form of imperialism.

Feyerabend in talking about the various philosophical traditions that exist regarding the distinction between subject and object argues that ***physicists choose one of these traditions (without realizing that a choice is being made) and turn it into a boundary condition of research*** (1981: xii). (His emphases and parentheses). If there is a desire to understand the baobab, a wish to move psychology from the First to the Third World, perhaps there is a need to ask the question: does psychology set up boundary conditions for research which shut out the people psychologists try to understand?

In this paper I set out to show that the failure to understand the baobab is not simply a failure of methodological technique (although this may play a role) but rather a failure to recognize the network of assumptions that lie behind prevailing

methodology in cross-cultural psychology. The normative base upon which research methods in psychology are built will be identified through the examination of the critique of psychology provided by practitioners in the Third World. This will be supported by some insights provided by the philosophy of science.

While the focus of this argument will be on the positivist nature of contemporary methods in cross-cultural research, the principle that science is a normative endeavour, applies to other forms of science as well. If this is an accurate perception, then the replacement of one method for another may provide new insights but, because such a step also means substituting one set of values for another, it still does not guarantee that the baobab will be understood. I will argue that the only way around this dilemma is to change the conventional conception of the nature and role of methodology in cross-cultural research. In the final section I will propose an alternative view.

THE CRITIQUE FROM THE THIRD WORLD

Any methodology employed by a social scientist is likely to be influenced by a number of underlying factors: the philosophy of science or metatheory under which the methods are generated; the ethics set down by the profession to which the researcher belongs (which are often but not necessarily tied up with the metatheory); and the demands of the context or the people with whom the research is being conducted. Ideally the method finally employed results from a resolution of the tensions arising from all three sources; it emerges out of the debate between

these three often conflicting demands. In practice, however, this ideal is rarely met and individuals tend to take greater cognizance of certain pressures and either neglect the others or are unaware of them. A look at the critique of psychology from psychologists living in the Third World identifies the dominance of the first two factors in this context.

This critique has two forms: a reformist and a radical. The reformists argue that psychology has certain built-in assumptions which create problems when it is applied in the Third World. They argue, however, that once these are recognized, modifications to theory and methodology will result in a more applicable discipline. The radical critics are more strident and argue that psychology and the social sciences are part of First World hegemony (or the system of international capitalism) and reforms are purely cosmetic. To be relevant the discipline has to replace existing formulations and establish an independent identity rooted in the Third World experience. Although the reformist critique is more prominent in the cross-cultural psychology forum, the radical critique deserves attention for it does help expose the hidden agenda of conventional psychology.

Jahoda argues that the theories in psychology are *the product of particular social milieu, namely advanced industrial societies. Their critical attributes... are those of literacy, impersonality and a wide range of available beliefs, ideas and attitudes* (1983: 3). While these attributes go unquestioned in the First World, they do not necessarily apply in the Third World situation.

Furthermore, in the First World the nature of the social structure is generally regarded as a constant, and higher-order determinants of behaviour do not form a central part of psychology. The Third World milieu, on the other hand, is characterised by rapid social change and a constant shift in the status quo. There is a need, therefore, to treat the social structure as a **variable** if there is to be an understanding of psychological processes.

Sinha (1983, 1984), an international figure in cross-cultural psychology, expands on Jahoda's views and draws on his Indian experience. His views epitomize the reformist critique.

He argues that the positivist orientation in psychology engenders a "tyranny of methods" that has little significance for understanding the individual in the Third World. He asserts:

> ... *the West has certainly produced a vast output of neatly designed researches into various social processes. Reading these studies in prestigious foreign journals, one is impressed by their neatness and precision. But at the same time, one is disappointed by their artificiality, triviality, and lack of relevance to real-life psychological phenomena* (Sinha, 1984: 24).

Nandy presents a similar argument. He describes the psychological science employed in the Third World as having a certain "ornateness" or "baroqueness" about it which is manifest in

> *innumerable half-hearted replications, unending streams of adaptions and readaptations of Western scales and tests, absurd dependence on captive student respondents, ritual*

search for "highly significant" reliability and validity and norms, avoidance of subjects which do not permit the use of experimental designs and multivariate techniques and a pathetic faith in that morose jargonese which goes by the name of technical language (1974: 3).

The reasons for this lies in *the tendency to trivialise or fragment psychological research in the name of operationalism* and the *nomothetic approach of the psychological sciences* (ibid). The reformist and radical critics differ in their proposed solution to the bias of Western psychology. Despite the fundamental problems of applying Western psychology the reformists do not reject the assumptions that underlie it but argue for revisions. Thus Jahoda (1983) while suggesting that co-operative work with anthropologists may be of some benefit, suggests that a form of quasi-experimentation may be of utility and that theories should be revised to take into account the relationship between psychological processes and social systems. Sinha also argues for the refinement of existing methodologies. He talks of a need for greater innovativeness and *experimental tasks need to be made meaningful to the people one is experimenting on* (1984: 14). The reformist changes he suggests are also exemplified in his proposed solution to the problem of relevance. He recommends that greater co-operation between the consumer and the research worker is essential and *interacting with policy makers and administrators would enable him to locate problems for investigation that are not only of interest to him as a scientist but also appear important to others in terms of policy and programme* (idem: 18). The interests and concerns of the subjects in the research are not necessarily catered for by such a call for relevance.

The radical critique is less equivocal. The "decolonization" of psychology has many difficulties. Apart from the difficulty of overcoming the habitual ways of thinking that exist as a result of a long process of socialization, the leaders of Third World social science have links with Western metropolitan centres which brings them rewards. In addition, they have a privileged position in their own societies which alienate them from the majority of the people (Dube, 1982).

One way out of this dilemma has been the call for the "indigenization" of psychology. This process involves ***the rejection of the false universalism of Western social science... by investing historical and cultural specificity into social science education and research*** (Dube, 1982: 500). In this vein, Akbar (1984) has proposed an "Africentric" model of social science which replaces individualism in Western science with the view of the ***self as a collective phenomena.*** The implications for methodology of such a view, however, are not made explicit.

VALUES IN SCIENCE

These critics of psychology in the Third World highlight the impact of positivism in this context and the naivety of the view that scientific methodology is value-free.

The plight of psychology in the Third World, partially, has its origins in the positivist distinction between discovery and justification. While positivists recognize that in formulating a theory or a question for study all values can be legitimately

mployed, they see no place for values when deciding on the nerits of competing theoretical explanations. For the ositivist, observation and empirical evidence is the only bjective way of making such decisions. This view leads to the rimacy of the experimental method and the belief that the egree of rigour and detail with which data is collected ensures ood research.

: is widely accepted within the philosophy of science, following he critique of Kuhn (1970) and Feyerabend (1975), amongst thers, that the distinction between discovery and justification s artificial and that observations are not theory-free. Science s a human endeavour and operates within a social context. alues, therefore, form an integral part of scientific research.

he acceptance of this view has become so widespread that it oes not seem strange to see Howard (1985) calling for the xpansion of values in science as it concerns psychology.

his recognition that a network of assumptions underlie any cience, and therefore method, creates a problem. If values are n integral part of science, different methodologies are likely to e based on different value systems. If no recourse can be made o the objectivity of empirical observation, what criteria can be sed for determining the truth of the results of research enerated by different methods? How does one judge which ethodology facilitates a closer understanding of the baobab?

his problem is the same one as that incurred by the radical

critics of Third World psychology. In asserting that Western psychology is imperialist and psychology has to be "indigenized", the Marxist is simply substituting one set of values for another. This may have some political benefit and may assist in generating new insights but it does not provide any guarantee that it will get any closer to the truth. What is regarded as truth becomes relative to the assumptions which underlie the specific methodology.

TOWARDS A RESOLUTION

If research results are relative to the methodology that is employed, then Berry's view that cross-cultural psychology is a method which enables the researcher "to check the generality of our psychological knowledge, theory, laws, and propositions" (1980: 5), must be treated with caution.

On purely pragmatic grounds a persuasive argument may be made for adopting a particular set of assumptions and thereby employing a particular methodological approach. The truth status of the result, however, cannot be ultimately assessed by an appeal to the nature of the method.

How then is it possible to understand the baobab? How does one judge which method or methods produce results which come closer to the truth?

While it is unlikely that any absolute solution to this dilemma is possible, it is clear, following the realist critique (Keat &

Urry, 1982) and the conventionalist's (Feyerabend, 1975; Kuhn, 1970) that the positivist's faith in the objectivity of method does not provide a way out. A more hopeful alternative is to take up the suggestion made by Morgan (1983) and look "beyond method" for greater understanding.

Morgan presents an argument for the redefinition of the role of method in science. He sees method as a means of "engagement"..

> *Scientists engage a subject of study by interacting with it through the means of a frame of reference, and what is observed and discovered in the object (i.e. its objectivity) is as much product of this interaction and the protocol and technique through which it is operationalized as it is of the object itself* (1983: 13).

In other words, the methodology a scientist uses provides a framework within which he or she interacts with the subject of the research. This interaction generates a "possible knowledge" (idem: 369) that will differ from other possibilities according to the nature of the mode of engagement.

As it stands, this view does not resolve the dilemma and if it is taken to its extreme, it results in Feyerabend's (1975) anarchic view that *anything goes* in science. Morgan (1983) goes further, therefore, and puts forward an argument for a social science that is based on critical reflection. His assertion is that social scientists should move beyond method and create a scientific culture that replaces a foundational view of science, in which there is a search for certain and true knowledge, with a

concern for "reflective exploration" or "reflective conversation" (idem: 374).

He argues that the metaphor of conversation is a useful one for guiding research practice for the following reason:

> *First, it encourages us to recognize the research process itself as a form of social interaction in which the researcher "converses" with, and learns about, the phenomenon being studied...*
>
> *Second, we can view different research strategies as but different "voices" in a conversation about the nature and status of knowledge...*
>
> *Third, we can engage in reflective conversation about the nature and claims of different research strategies in a way that deliberately tries to minimize commitment to a favoured point of view* (Morgan, 1983: 374-375).

The consequences of such a perspective of a social science is that understanding is not confined to the knowledge generated by a particular methodology but also comes from the conversation between those scientists who employ different methods.

> *In creating the possibility of exchange based on differences of viewpoint, such conversation offers the promise of edifying dialogue that is not overly concerned with forging premature consensus or arriving at an end point that purports to establish or reveal some foundational truth. Rather, the point is to learn from the process itself, and to encourage the conversation to continue so long as disagreement lasts. In so doing, we are able to minimize the hegemony of a fixed evaluative stance or of any conventional wisdom that seeks to brush disagreement aside*

> .... *in favour of an edifying exhange that thrives on self-conscious criticism, challenge, and diversity* (Morgan, 1983: 375-376).

Adopting this view of social science does have distinct benefits for crosscultural psychology and does provide an alternative to prevailing approaches, thereby offering a better chance of understanding the baobab.

The view that method is a means of conversation operates on three levels. It provides the means by which researchers engage with other scientists, with the subject matter that forms the focus of the research project or programme, and with the respondents, "subjects" or participants in the research process.

A reflective social science, therefore, has a number of consequences for cross-cultural psychology.

First, the view that a method is a means for "engagement" and not and end in itself hinges on the perspective that the research process is a form of human action. Such a view encourages consideration of the moral, ethical and ideological underpinnings of research and produces a socially responsive science in which method loses its tyranny. Critical reflection and debate across methods encourages an exploration of the consequences and significance of research strategies which should act as a check on the imperialist nature of social science research in the Third World.

Second, a commitment to move beyond method casts the critique coming from the Third World in a new light. Recognizing that the critique forms part of the conversation that should be going on in cross-cultural psychology removes the threat or fear that this critique can generate and replace it with an acceptance of the role that psychologists from the Third World have as a force for change. These psychologists are the people who have to face the issues of the relevance and meaning of psychology in the Third World and they offer the hope for the development of unique methods born out of the need to resolve immediate problems in their local environments.

Third, the view of method as conversation at the level of interaction with those from whom researchers collect their data, changes their role away from subjects to participants. This enables the establishment of a dialogue with the participants and thereby an assessment of how they perceive the shortcomings of the method and the validity of the knowledge generated by the research.

Already a number of psychologists have responded to the challenge that the Third World offers and, whether it has been conscious or not, have begun to see method as a means of engagement. Thus there is a growing literature on indigenous psychology (Heelas & Lock, 1981) and a growing number of studies of conventional wisdom, using new methodologies, have emerged. These provide a check on the conceptual frames that have limited cross-cultural research. Goodnow (1984) and Dasen (1984) have begun to examine how individuals conceptualize

intelligence; Miller & Craig (1984) recognise the critical importance of mothers' conceptions of child development in the process of socialization and have developed new methods for investigating these concepts.

In addition to this there is a growing number of social scientists in the Third World who are pioneering new methods. One such development is the emergence of an action research strategy.

Based on the Marxist view of praxis the assertion is that understanding comes through direct involvement in action. Thus Fals-Borda (1981) argues that the distinction between researcher and subject should be eliminated and through joint participation in the mundane actions of life, all participants should become part of the process of generating knowledge. This approach is not the anthropological method of participant observation in a new guise. Being a rejection of the view that social scientists can be objective, it is a commitment to participating in the subjective world of others, through involvement in their actions, and jointly coming to an understanding of the world. One advantage of this strategy is that the validity of the knowledge generated can be judged by the participants themselves against the criteria of usefulness or relevance to their context. The work of Freire (1970), in the education field, exemplifies this approach.

It is valuable, therefore, to substitute the view that science is concerned with the generation of fixed unambiguous knowledge, with the view that it involves a "mode of engagement" by which

researchers can converse with their subject matter, respondents and other scientists. In doing this the role of the scientist is changed from that of the arrogant manipulator, wanting to predict and control, to that of the sensitive facilitator, wishing to improve understanding. Perhaps, if the challenge that is offered by such an approach is taken up in cross-cultural psychology, then we will begin to see the roots of the baobab and watch the buds on the branches burst open and bloom.

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