A Science of Mind
The Quest for Psychological Reality

Peter du Preez

Department of Psychology
University of Cape Town
7700 Rondebosch
South Africa

ACADEMIC PRESS
Harcourt Brace Jovanovich, Publishers
London Boston San Diego New York
Sydney Tokyo Toronto
Problem-solving and the evolution of theory

Problem-solving is the characteristic and essential feature of science, as we learn from Popper, Kuhn, Lakatos, Feyerabend and Laudan; and this view is shared by active scientists. Medawar (1967) describes science in the phrase ‘the art of the soluble’ and says that scientists who tackle problems they can’t solve can only hope for ‘the kindly contempt earned by the utopian politician’ (1967: 7). Not only must problems be solved, though; they must be solved in competition with others. All credit goes to the winner of the race. Like athletes, scientists try to be first, though the arena of competition is different in the two cases. This implies that there is no fixed or stationary standard by which the adequacy of performance can be judged, since the standard is continually revised by the latest achievement. Latour and Woolgar (1979) tell us in their interesting study of laboratory life how the standard can be raised to eliminate the opposition, who may withdraw because they don’t have the money, the equipment, the talent or the ‘guts’. Schally, one of the protagonists in the ‘laboratory life’ drama, is cited: ‘The key factor is not the money, it’s the will... The brutal force of putting in 60 hours a week for a year to get one million fragments’ (1979: 118). His team had to collect millions of hypothalamics to obtain enough of the hormone, TRF, in which they were interested. This was made necessary by their own announcement that the goal of their team was not merely to isolate TRF but to collect enough of it to determine its molecular structure. The effect of this was to reduce the field of competition, since the risk of failure and the sheer arduousness of the task were vastly increased. Latour and Woolgar speculate that it is probably relevant that the main competitors (Schally and Guillemin) were immigrants (1979: 119). Schally dismissed a possible rival as ‘Establishment... he never had to do anything’ (1979: 119).

These accounts alert us to the complexity of any account of the ways in which problems are defined and focused. Our stories could equally well be about the personalities of the competitors, social demands, crises, the intellectual climate, or clashes between research traditions. How should we decide which factors are important – the ‘internal’ historical of the tradition, concentrating on the ways in which ideas, practices and problems are modified within a particular scientific community pursuing truth as it sees it and resisting worldly temptation; or the ‘external’ history of the tradition, which shows temptations, demands and political forces impinging upon the work of science? Wouldn’t it be rather foolish to decide that the whole account was to be either one or the other? As a matter of convenience, one might focus on one set of factors, rather than another, but it cannot be a decision of principle, unless one has some quite foolish principles to assert.

How should one investigate the factors which influence the evolution of a scientific tradition? One way, if one is that fortunate, is to find a theory or two to test against the facts of evolution. This is the way which satisfies us best, because it seems more rigorous. Another way, for those who are Low Church Investigators (without the incense or the splendid ceremonial appendances of science at its most inspiring) is to draw up a shopping list of factors which should be considered in examining the history of a tradition. Sometimes the theories are merely shopping lists, and the shopping lists have the accents of theory, but we soon grow accustomed to pretentiousness. The value of good shopping lists should not be underestimated, because they can be used to remind theorists who are about to cry that they have made all their purchases that there are still items which may be needed.

Let us look at these approaches to the evolution of traditions. On the one hand, we test theories against facts; on the other, we construct cases by going through our checklist of things to be borne in mind.

Theories and historical facts

Examples of theories of scientific change which might be tested against historical facts are those of Kuhn, Laudan and Lakatos. One of the first problems we face is knowing what they say and how what one says differs from what another says.

Theorists may differ sharply from each other even when they seem to be saying the same thing, and may be saying the same thing when they appear to be saying different things. Each theorist likes to wear a different vocabulary. There are many illustrations of this (and some will follow in the next three chapters), but Feyerabend’s (1981) remarks on Laudan may show what we mean. It is the old problem that what is new is not true and what is true is not new. There is nothing novel in Laudan’s claims to be focusing on the problem-solving capacities of science (Popper had done this), on the importance of conceptual
problems (virtually everyone had done this) or on the importance of traditions (Lakatos had done this, using the term programme). What Laudan had done was to invent some new vocabulary and to proclaim a difference. In this book, a fair amount of use of Kuhn, Laudan and Lakatos has been made, without respecting their differences overmuch. Kuhn's concept of a disciplinary matrix is treated as a set of resources in a tradition. The distinction between traditions (or programmes) and theories has been adopted from both Laudan and Lakatos. The focus on problem-solving is shared by all.

It is, therefore, interesting to examine an attempt to state each of these theories in such a way that they can offer competing accounts of historical facts. If they are indeed in substantive competition, then we have been careless in assimilating the one to the other. (This is not to deny that one vocabulary rather than another may be victorious in the end.)

The attempt I wish to consider. is one which addresses the facts of the history of learning theory from the perspectives of Kuhn, Lakatos and Laudan (Gholson and Barker, 1985). What they attempt to do is, first, to define each approach in such a way as to distinguish it from the others; and, second, to test each approach against the known historical facts. Their definitions of the approaches are as follows:

Kuhn's approach is said to rest on the assumptions that paradigms are incommensurable; research programmes defeat each other in scientific revolutions; and there is no continuity in content from one paradigm to the next. Since this is a caricature of Kuhn's views based on his early work, they call this approach 'Kuhnian'. The later concept of the disciplinary matrix makes it difficult to believe in hermetically sealed paradigms. However, it is valid to describe a position (whether we call it Kuhnian or something else) with sufficient clarity to make it a candidate for testing.

Lakatos's approach rests on the following assumptions, according to Gholson and Barker. Preference for one research programme rather than another is a rational choice; programmes are progressive or degenerative, depending on whether they solve critical new problems or accumulate anomalies; problems may be empirical or theoretical; each programme consists of core commitments and a protective belt of auxiliary hypotheses which may be sacrificed without abandoning the programme; and as theories mature, a common reason for replacing them is experimental failure. Has Lakatos been successfully distinguished from Kuhn? Certainly, from the approach called 'Kuhnian', with its incommensurable and sealed paradigms; but every feature of Lakatos's approach can be matched in Kuhn's approach, though the emphasis may be different. It

has been said repeatedly that showing up non-rational factors in changing theories or paradigms is hardly the same as saying that rational choices are not made. At the frontier, scientists do take risks in betting on competing theories. Presumably, if anyone knew a completely foolproof way to bet they would adopt it. If one waits till nearly the end of the race, one can be (almost) sure who is going to win, but it's too late to bet.

Laudan's approach is said to rest on the following assumptions. Research traditions consist of a family of theories with a common ontology and methodology, which both change as the tradition evolves; and conceptual factors are as important as experimental success or failure in appraising a theory. It is hard indeed to distinguish in a substantive way between research programmes, research traditions, and paradigms (Feyerabend, 1981). It is also hard to see that conceptual factors do not play an equally large role in all three approaches, but let us for the moment suspend judgement and simply take it that three approaches have been distinguished. How do these three approaches explain episodes in the history of psychology?

The strategy adopted by Gholson and Barker is to show that Lakatos's approach fits the facts of history, in both physics and psychology, better than Kuhn's approach. Having done this, they attempt to show that Laudan's approach is an improvement on Lakatos's. The conclusion, therefore, is that Laudan gives the best fit to the historical facts.

First, why do they believe that Lakatos is preferable to Kuhn? They argue that there was considerable 'experimental commensurability' between cognitive and conditioning programmes of research, as shown by conflict and interchange in the mid-1930s (Gholson and Barker, 1985: 761). Second, cognitive psychologists adopted several elements from mathematical learning theorists, such as Markov chains. However, though the Lakatosian account is better than the Kuhnian (if we assume that the approaches of learning theorists and cognitivists represent different paradigms which nevertheless interacted), there are still deficiencies, since the core commitments of learning theorists 'underwent a constant evolution between about 1950 and 1965' and learning theorists also adopted core commitments from the cognitive programme (1985: 761). Core commitments are not supposed to change or exchange, according to their version of the Lakatosian approach. As an example of a changing core commitment, they cite mediation theorists (such as Kendler, 1979) who surrendered the view that all learning is a form of conditioning in favour of the view that there are two processes — learning by conditioning and learning by hypothesis testing, the former being more common in younger children. (Nowadays, when we
postulate hypothesis testing and operating principles as essential for the acquisition of grammar in the first two years of infancy, even this would be greeted with scepticism (Slobin, 1979.) This shows, of course, what purists have always feared. Eclectics are everywhere, borrowing wherever they see a good idea, without any respect for purity of form or tradition.

These difficulties and the way in which Laudan's approach apparently solves them, lead Gholsen and Barker to prefer Laudan to Lakatos. Laudan does not postulate core processes, though he does postulate that traditions have a distinguishing ontology and a methodology. To cite him:

A research tradition is a set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain (1977: 81).

It is not clear what the difference is between Laudan's ontology and Lakatos's 'core'. One could regard a change from conditioning to hypothesis testing as a change in either ontology or core, since (referring to ontology), the change is a change in the very nature of the process of learning, or (referring to core) the change refers to the basic processes which are central to the theory, relatively immune to refutation, and yet are part of the theory used in making predictions.

Yet another reason for preferring Laudan's account to Lakatos's (according to Gholsen and Barker) is that 'conceptual factors' play a relatively important part in theory appraisal in Laudan but not in Lakatos. As examples of such conceptual factors they cite circularity in definitions of reinforcement and the lack of precision in Piaget's theory, in particular the vagueness of concepts such as equilibration, assimilation and organization (Gholsen and Barker, 1985: 765). Conceptual solutions may lead us to adopt a theory; conceptual failure may lead us to abandon it. This is hardly novel. All the approaches to science which are under discussion admit the significance of conceptual factors. After all, we are talking theory and not recipes for cooking eggs. Conceptual clarity, elegance, rigour, and other theoretical virtues are among the essential 'values' of science (in Kuhn's vocabulary). We may illustrate by citing instances in which theorists have maintained a theory against current empirical evidence, because of its virtues.

It seems that if one is working from the point of view of getting beauty into one's equations, and if one has a really sound insight, one is on a sure line of progress. If there is not complete agreement between the results of one's work and experiment, one should not allow oneself to be too discouraged (Dirac, cited in Brush, 1976).

Circular definitions and vague concepts on the one hand, and sound insight and beauty in one's equations on the other, must be part of every appraisal of theory. Experimental models, axioms, laws, metaphysical assumptions, ideal cognitive objects, and values are the conceptual factors in a theory and we don't need a separate category for the latter. Since science consists of both concepts and practices (or methods), it is hardly surprising that concepts and practices are important in the evaluation of a theory.

Nor does the view that theories are rationally compared distinguish any one approach from the other. Kuhn, Popper, Lakatos, Laudan and many others give instances of rational comparisons; yet all conclude that such comparisons are not simple. When should one trust 'facts' and when should one trust theory? Appraisal depends on where the facts come from – who did the experiments and what were they trying to show? – as well as the conviction that some things cohere to form a good picture or deep insight. As was said earlier, up to a certain stage, neither resistance to the evidence nor conversion are entirely rational. Young scientists, ambitious scientists, scientists with a contempt for the 'establishment' may challenge a theory or a tradition on relatively flimsy evidence. They may be prepared for risks. Needless to say, their gamble does not always pay off, and they may never be heard of again. This is not 'irrational'. It is a decision to back a possible winner before everyone else has spotted it. In art, entrepreneurial work, speculating on the stock exchange, and even in science, there are risks to be taken because the truth cannot be known in advance. There is, in all of them, a direct relationship between the level of risk and potential reward. Scientists are well aware of this, as we see from personal accounts (Evans, 1976; Siegel and Zeigler, 1976).

Suppose we look again at the data in Gholsen and Barker, but this time we treat each tradition as a matrix of resources – or disciplinary matrix. This enables us to discuss the kinds of change which occur in a complex way, without positing discontinuities. Even when a revolution has occurred, we shall often find that the matrix of resources has not changed completely. In any case, subtle comparisons are possible and we avoid oversimplification.

Thus, in looking at the changes in learning theory we find changes in all the dimensions of the matrix. Mathematical learning theory changes the symbolic generalizations of learning theory by introducing complex new concepts such as stimulus sampling and Markov chains. New metaphors are introduced as hypotheses come to supplement conditioning. The range of exemplary solutions is extended with the application of learning theory to clinical problems. Behaviour
modification introduces a wide range of models which are linked, by analogy, to laboratory models of operant and classical conditioning. Mathematical theorizing leads to changes in value. There is increasingly complex speculation in the work of Hull, Estes, Suppes, and others about intervening processes. The ‘ideal cognitive object’ of the learning theorist becomes an increasingly complex connection machine.

All of these are resources as well as problems. When we examine any real cluster of theories in a tradition, we see that few of them match in all features of the matrix, though all of them will have some of the identifying features. A cluster of theories is, in fact, a fuzzy concept which we continually attempt to grasp by identifying its prototypes. This leads to a stereotyped view of what a tradition is, but is an inevitable consequence of our tendency to understand complex arrays by forming ‘ideal cognitive models’ of them (Lakoff; 1987; Rosch, 1973; 1978).

We may attempt strict definition of behaviorism (let us say) as a system which reduces all explanations to s (stimulus) and r (response) elements (Fodor, 1968), but when we trace the evolution of behaviourism into cognitive behaviourism, there are many moments of indecision about the applicability of such a strict criterion. The evolution of species implies the abolition of species (as strictly demarcated entities). The alternative often seems to be extremely loose definitions of behaviourism. Danziger (1987), in an essay on ‘investigative practices’ suggests that there may be a strict use of the term (‘specific set of doctrines propagated by men like Watson and Skinner’) and a loose sense of the term (‘commitment to the general idea of psychology as a practically useful science of human performance’). One may then speak of the burgeoning of the ‘behaviourist perspective’.

When one turns to Skinner to see whether he (as a core behaviourist) agrees with Fodor’s definition, we find that he says: ‘I do not consider myself an S–R psychologist’ (Evans, 1976: 85). He continues the interview: ‘Behavior is very fluid, it isn’t made up of lots of little responses packed together.’ And, when you are trying to account for the rate of emission of behaviour, ‘some of the influences will reasonably be described as stimuli, and some will not’.

Prototypes are reference points when we are trying to identify a category and describe its ideal structure. What Kuhn’s concept of disciplinary matrix does is to give a general way of describing the features on which exemplars can vary to discover the ‘family resemblance’ of theories. The strategy for attacking a cluster of theories is to attack the prototype theory, but because of this matrix structure of the cluster, we should not expect a neat ending.

Harré (1986: 201) adds to our understanding of the ways in which traditions are structured by the ideal cognitive model (or ideal cognitive ‘object’, as he calls it). He also uses the term ‘theory-family’ rather than tradition, but we may disregard these superficial differences and concentrate on the following: a tradition is recognized by the ideal cognitive model which it develops and exploits in understanding the world. Each ideal cognitive model is the union of (a) an ‘analytical analogue or model’ which is used to give order to perceptual experiences, and (b) a ‘source analogue or model’ which is used to explain how these perceptual experiences are produced. These analogues correspond to metaphors in Kuhn’s approach, though Harré argues that metaphors are not up to the task of explanation, since they collapse into similarity, whereas what is needed is the logic of analogy. In other words, what is needed is a similarity of relations between elements rather than a simple similarity of elements. An example of a successful analogy is the analogue of breeding which Darwin used to develop the theory of evolution by natural selection. As Harré tells us, Darwin might have looked at the English countryside with religion or aesthetics in mind, and picked up evidence of God’s bounty or of the picturesque. (Nature imitating Art, perhaps?) Instead, he looked at it as a country gentleman, and picked up evidence of breeding and bloodlines. With the analogue of breeding in mind, he constructs the following pair of relations (Harré, 1986: 204):

Domestic variation acted on by domestic selection leads to domestic novelty (e.g. new breeds)
Natural variation acted on by [...] leads to natural novelty (e.g. new species)

Harré’s point about the fruitfulness of analogies is well taken, but there is little difference between them and metaphors, unless one wished to confine metaphors to a comparison between isolated elements rather than between structures of relations. It is clear from our earlier discussion that successful metaphors create conceptual structure and that this conceptual structure has a logic (including analogy) which can enable us to understand the problems we are studying. There is a danger in the multiplication of terms. ‘Tradition’ and ‘metaphor’ are by now familiar; what we should do is add to our understanding of these rather than add fresh terms. What Harré does, in his vigorous discussion, is to increase our understanding of the structure of the implications of successful metaphor.

The Darwinian analogy has also been used to create an ideal cognitive model to account for the selection of behaviour. Consider the following:
Domestic variation acted on by domestic selection leads to domestic variety. Behavioural variation acted upon by reinforcements leads to novel forms of behaviour.

Skinner has exploited the power of this analogy to create a science of behaviour. He has created experimental models of behavioural selection which have been persuasive for many decades, in spite of major difficulties of the kind discussed in an earlier chapter ('The fulcrum of reason'). These difficulties have concerned the circularity of the definition of reinforcement and the relation between ontogeny and phylogeny; in particular, the question arose whether there could be general laws of reinforcement or laws of learning, given that different species of animals are prepared by their evolutionary history to learn different things about the world. (This will also be discussed later in the present chapter, when considering the impact of Garcia on learning theory.) The problem here arose from an over-ambitious attempt to go beyond the demonstration of behavioural selection by consequence to the conclusions that (a) all behavioural patterns arise by such selection; and (b) all selections occur in the same way and exhibit the same responsiveness to schedules of reinforcement. This would be rather like proceeding from the general argument that evolution occurs by natural selection to the conclusion that all forms of natural selection have a common property, just as all behavioural selectors have a common property – that of being 'reinforcers'. This led us to search for what it is that they all do to the organism. Natural selection occurs in an immense variety of ways; it simply refers to various processes by which some variants are eliminated and others favoured. Similarly, behavioural selection refers to an undefined set of factors which eliminate some behaviours and favour others. Some selectors may be information which confirms or disconfirms hypotheses; other selectors may be foods which satisfy hunger; other selectors may be pain or relief from pain. Each may be an event in a different functional system, with little in common. We will learn much by using them to understand the ways in which different functional systems operate, rather than by attempting to discover a common property of reinforcement.

What I intend to do now is to extend the Darwinian analogy to the evolution of theory, bearing in mind the dangers of looking for a common factor in the selection process. I shall accept that selectors operate in a variety of ways. All that they have in common is their effect on the survival of theory variants. They do not account for the ways in which these variants arise, nor are they normative in some ideal way; they simply act on variants once these have arisen. Norms may be established and may operate in leading us to prefer one theory to another. One such normative description of the way science ought to work is Popper's (1963) conjecture and refutation, though as a description of what actually happens, it is not always correct.

When we look at the factors which select theories, norms of scientific practice can be possible candidates but no more. We may accept or reject a theory because it meets norms, but just as often we change our norms when a theory solves interesting problems. An example of the survival of theory against the norms of evidence and argument is the case of psychoanalysis, which has been subjected to severe critiques and yet continues to flourish and adapt. (For a recent and telling assault see Eysenck's (1986) Decline and Fall of the Freudian Empire.)

For the present, let us revert to the shopping list (or does 'checklist' sound better?) approach to historical influences on theory.

Checklists and the evolution of theory

The more we look at the activity of science, in which hypothesis competes against hypothesis and theory against theory, the more we begin to suspect that an evolutionary account of the growth of knowledge is necessary. We take the source analogy of Darwinian evolution and extend it yet again.

Theoretical variation acted on by social selection leads to novel theories.

A general schema for an evolutionary epistemology (or theory of knowledge) contains (a) mechanisms for introducing variation; (b) consistent selection processes; and (c) mechanisms for preserving and propagating the selected variations (Campbell, 1974: 421). From this point of view, a research tradition is a system biased by its history to solve certain problems and not others.

The reason for the rapid advance of the problem-solving capacity of natural sciences is that scientists are trained to introduce theoretical variations, to test them empirically, and to preserve and propagate those innovations which survive whatever tests have been proposed. This is distinct from belief systems (often disguised as knowledge systems) in which propositions are tested against our wishes and hopes. They may become better and better at fulfilling these as long as we can keep the real world at bay. The form of rationality in science – conjecture and empirical selection – has itself been subjected to selection in competition
with other forms of rationality, among them: demonstrative rationality, in which propositions are demonstrated by deduction from a priori truths; revealed truths; textual exegesis; truth by consensus or conformity; truth by wish-fulfillment, aesthetic preference; or waiting for patterns of nature to reveal themselves as opposed to experimental intervention to speed up the process of selection. Since those societies which have adopted scientific rationality have, on the whole, been able to produce knowledge-based technologies which have increased their power, wealth, comfort, and understanding of the world relative to those which have not, these methods of solving problems have spread rapidly. Furthermore, as with any complex human practice which has utility, the qualities essential to its pursuit (such as openness to new ideas, rigour, curiosity, willingness to question what is accepted, willingness to submit ideas to the test of experiment and observation, and honesty) are cultivated as virtues by active practitioners.

We might also spend time on taxonomy and classify traditions and theories according to their ideal cognitive objects (as Harré suggests) and models of rationality (of the kinds given above). This taxonomic exercise would be comparable in many ways to the great biological taxonomies, indicating both lines of descent and similarity. This confusion of purposes is, incidentally, a feature of biological taxonomies, where different classifications can result from attempts to group by line of descent or by current similarities in form and function. The former mode of classification is advanced by cladists and the latter by pheneticians. The cladistic approach apparently results in some peculiar reclassifications, such as the abolition of the categories of zebra and fish. Gould (1983) tells us that there is no such thing as a fish, since some (such as the lungfish) are more closely related to terrestrial animals than they are to other fish.

Why should we want a taxonomy – or several taxonomies – of theory? For the same reason that biologists do. When one writes evolutionary history, one attempts to discover both line of descent and similarities in form and function.

What I wish to concentrate on here, though, are the processes of selection which might eliminate some theories and enable others to spread rapidly. 'Internal' models of these processes pay attention to the rational selective processes and norms within each tradition. But one thing we have learnt from the study of evolution is that selection may occur in many surprising ways. This is where the shopping list is useful, though not exhaustive. One way to become aware of outside influences on selection is to think of theories as methods of investigation; then we immediately realize how offensive they may be to others who share different views. The result is a long list of 'external' influences on theory change. What would our shopping list look like? One way of organizing it is as follows, going from external selectors to internal selectors, from those which are 'historical accidents' to those which seem to be truly part of the very practice of science.

Selection factors in the evolution of theories

Political movements
Climate of thought; zeitgeist
Professional opportunities
Demonstrations of legitimacy
Institutional power structures
Psychodynamics of leadership
Strategies of competition
Methods of inquiry; disciplinary norms

From a theoretical perspective this must seem to be a truly laughable list, but then a study of the ecology of theories must seem to be a laughable undertaking to the purist, interested only in ideas. What I wish to emphasize, though, is that it is not enough for systems which survive (whether they be theories or organisms) to meet some fixed standard of excellence or fitness. They survive in competition with other systems in a particular social environment, and this competition can take the most diverse forms. This does not slight tests of scientific virtue imposed within the scientific community. It is these tests which make scientific theories so apt to survive and to serve both our need to understand the world and to solve practical problems.

Let us now return to some of the evidence which each of the entries in our list might lead us to pick off the shelf.

Political movements

It is difficult to estimate the effects of political suppression (or promotion) on the long-term development of science. Overt suppression would seem to be less effective than ideological conversion, which makes scientists willing and even eager to serve the prevailing political system. Thus, overt suppression of Vygotsky's ideas in the Soviet Union has not prevented their reappearance and popularity half a century later; whereas Vygotsky's conversion to Marxism and his attempts to create a truly Marxist psychology have had a persistent effect (see Wertsch, 1985 for recent testimony to this). As Zinchenko and Davydov tell us in their preface to Wertsch, Vygotsky announced 'I don't want to discover the
nature of mind by patching together a lot of quotations. I want to find out how science has to be built, to approach the study of mind having learnt the whole of Marx’s method’ (1985: ix). The synthesis appears to have been fruitful and we can probably look forward to many years of interesting work in this tradition. It seems, therefore, that political suppression can have little long-term effect on the history of science. This is the optimistic view, since we should not take it for granted that political conditions always change in such a way that the continuation of work is possible. At the very least, work may be slowed down.

An equally notorious example of political suppression dates from the same time, when the publication of Luria’s researches in Uzbekistan (1931 and 1932) was largely prevented. He was interested in studying changes in the thought processes of people being introduced to ‘higher and more complex forms of economic life and the raising of the general cultural level’ (Luria, 1979: 213). Possibly the ranking of cultures was, at the time, against official interpretations of Marxist doctrine; possibly there was some bureaucratic anxiety under Stalin. Was it or was it not acceptable to attribute improvements in psychological functioning (abstract classifications, self-criticism, better reasoning) to greater literacy? The result was the almost complete suppression of this work until 1968, when the publication of a brief article revealed that the political climate had begun to change. A monograph followed on Cognitive Development (1976) in which his views on the historical formation of mental processes was set forth. One of the key findings was that traditional Uzbekis were not susceptible to classical visual illusions, such as the Muller-Lyer, and Luria interpreted this as support for his view that gestalten are not permanent characteristics of the mind but are culturally and historically variable.

Once more, the ending of the story is a happy one, but the effects on young scientists attempting to learn about psychology were probably considerable. There were many warnings in Luria’s career, culminating in his dismissal from the Institute of Neurosurgery in 1950. Pavlov’s work became the model and Luria ‘had to state that his work on aphasia and the restoration of brain function was deeply flawed because of his failure to apply Pavlovian teaching’ (Cole, 1979: 219–20). Luria learnt to write his papers in Pavlovian language, perhaps hoping that readers would penetrate the disguise. ‘Sadly, in the 1950s many young Soviet psychologists could not make the translation, nor could I’ (Cole, 1979: 221).

I think it is clear that studies of political influence on scientific development would be valuable. The allocations of funds altering the reward structure, ideological conversion, and direct terror or suppression are among the means the state can use.

Climate of thought

Each period has its set of assumptions about what is possible, decent or sane. In the nineteenth century, progressive theories could rank people and societies from savage to civilized. A climate of racist thought accompanied and probably facilitated European domination of the globe. In the late twentieth century, such theories would be regarded as not merely wrong but immoral. Cross-cultural theorists work against a background of relativism (Stein, 1986), or at least of anti-relativism (Geertz, 1984). Decorum hangs heavy over indications of possible indecency. At its most banal, relativism may be parodied as the view that ‘People do what they do ... pretty much because that’s what they do’ (Hippier, 1984: 434). The West has to compensate, though, for its past by idealizing the Third World (even as its economic policies continue to crush it).

Among relativists, guilt feelings are transformed via projection into accusation of others who are ethnocentric. At a more primitive level, the relativist symbolizes his inner splits through a dualistic system that portrays the modern West as evil and the primitive as innocent (Stein, 1986: 169).

This, of course, is relativism at its worst. At its best, it is a moral injunction against ethnocentrism and arrogance. The world is indeed a complex place! Relativism may be used to justify keeping the Third World as it is (underdeveloped – a zoo and an ethnology museum) or to exorcise Western arrogance. On the whole, though, decentration is a good thing. It leads scientists to question the authority and validity of their own points of view.

This process of decentration (against a background of relativism) has led to fierce criticism of tests linking intelligence and race in particular, and intelligence and heredity in general, focusing on the work of Jensen (1969, 1980) in recent times. The nature of these links is discussed in Scarr (1980) and dramatically debated in Eysenck and Kamin (1981). The relevant point here is that whereas the climate of opinion was hospitable to demonstrations of links between race and intelligence in the early years of the century, it has now changed in such a way that most scientists either avoid such studies or interpret results in such a way as to reject previous findings of a strong relationship. Racial discrimination and racist science are no longer acceptable (though racist practices may still be common enough). The study of race may flourish under certain political imperatives and cultural assumptions, but under others would be strongly discouraged.

In other fields, social interests have also selected certain scientific
achievements for pre-eminence. Latour (1988), in an essay on the 'pasteurization of France', describes the way in which the hygienist movement took up Pasteur's discoveries. His investigation of the invisible agents of illness strengthened their campaign against dirt, leading them to dramatize and magnify his achievements (which were great) in comparison with those of Koch and Jenner. One should note, though, that Pasteur was not only an exemplary scientist but also a national hero at the time of the Franco-Prussian war. This might have had more to do with his national pre-eminence than the efforts of the hygienists.

A remarkable attempt to impose scientific conformity is the Seville Declaration on Violence adopted at the business meeting of the American Anthropological Association in 1986. The Declaration solemnly lists five 'scientifically incorrect' propositions in its condemnation of 'a number of alleged biological findings that have been used even by some in our disciplines, to justify violence and war' (Fox, 1989: 58). Among the dangerous scientific theories is the theory of evolution. The Declaration has a deadly, inquisitorial rhythm, with each paragraph beginning: 'It is scientifically incorrect to say that...'. Certainly, many of those who voted for it appear to have been terrified out of their wits. How else to explain their support for such propositions as:

It is scientifically incorrect to say that in the course of human evolution there has been a selection for aggressive behaviour more than for other kinds of behaviour (How to quantify 'more'?).

It is scientifically incorrect to say that humans have a 'violent brain' (straw man!).

It is scientifically incorrect to say that war is caused by 'instinct' or any other single motivation.

What the Declaration does is to make psychobiological research more difficult by promoting a new consensus. There is an uneasy divide between 'explaining' and 'justifying' war. If one advances the hypothesis that there are biological factors which contribute to violence, is one justifying violence? Apparently one is, by contributing to 'an atmosphere of pessimism'.

The most important objection to the Declaration is that 'no position can be declared correct or incorrect by fiat... One dreads to think what would have happened to the sciences if a list of "incorrect" positions had been drawn up in, say, 1910 and subsequent research had been guided by that list' (Fox, 1989: 63).

And think of the awful pessimism caused by Darwin in 1859. Should he have been allowed to publish?

Professional opportunities

It is hardly surprising that those scientific movements which meet the requirements of administrators, managers, or others in power will flourish. We see this most strikingly in war or in preparation for war, but there are many other examples.

In many countries and especially in the European colonies, managers wished to select the most productive workers in a largely illiterate workforce. Obviously, those who made a profession of helping them to do this would enjoy their support. In South Africa, to give one example, psychologists (particularly at the National Institute for Personnel Research) were able to make a significant contribution to the selection of 'bossboys' on the mines (Louw, 1986), and to the composition of teams for improved productivity (Mauer and Lawrence, 1988). Many of these tests were exported and used in Commonwealth countries. As far back as the 1st Carnegie Commission to investigate the 'poor white' problem, psychologists had established their professional claims, particularly in the field of vocational guidance. They were recognized as experts to be consulted in education, industry, and clinical practice. By 1974, this recognition had led to the establishment of a professional register for psychologists, the effect of which was to grant them a new monopoly in the application of psychological knowledge to practical problems. However, the direction of influence is never one way. Professional demands begin to shape the knowledge base from the earliest times.

This has been demonstrated fairly clearly by Danziger's (1987) study of the modification of investigative practice to meet professional demands in the USA and Germany. In order to meet the needs of educational administrators and the military, group tests and testing techniques were developed to a high level of sophistication. Professional prestige was purchased at the cost of a severe constriction of disciplinary aims. The developmental psychology of men like Baldwin and Hull, which could have a prescriptive significance for education, was now replaced by practices that limited themselves to the sorting of individuals (1987: 21). The prestige of group investigations led to the downgrading of individual case studies as unscientific; and this downgrading became part of the attack on theories which were founded on an intensive study of single individuals.

There is a potential conflict between those who sort large numbers of individuals on the basis of group tests and those who attempt to understand particular persons in order to help them as individuals. This conflict is a product of methodological imperialism: each develops its own investigative practices and attempts to impose these on the other.
The symptom of this is the debate about nomothetic versus idiographic science, or clinical versus statistical methods (Allport, 1937, 1965; Kelly, 1955; Meehl, 1954).

A good case study of this conflict is given by Van Strien and Dehue (1985). Some of pressures on psychological practice and theory may be summarized.

1. Practitioners (in the Netherlands and elsewhere) responded differently to different clients. To industry, they offered psychometrics; to individuals in search of cure or enlightenment they offered holistic understanding of the person.

2. The large influx of students and the growth of the research industry after World War II changed the balance between clinical practitioners and academics. The academics had to train numbers of students, find publishable research topics, and gain scientific recognition, all of which tasks could be most easily undertaken in the dominant psychometric research tradition.

3. This was consolidated by major Dutch academic theorists who replaced the 'subjective and authoritarian style' of intuition by testable predictions of psychometric instruments, which appeared to be transparent and 'democratic'. This eclipsed the phenomenological-existential tradition (and other holistic clinical practices). Intuition was restricted to the 'context of discovery', but excluded from the 'context of demonstration' or proof.

4. However, the failure of the psychometric approach (for clinical practice) became increasingly evident. It could tell little about the complex dynamics and point of view of the individual. In spite of this, the view persisted that the psychometric approach was superior (though of little value) and the clinical approach was inferior (though more useful).

5. The result was a split between 'official' methodological rules as taught in the universities and the rules of practice.

It is hardly surprising, therefore, that clinicians even in the USA rate journal articles and books with empirical findings bottom of the list of useful information (Kupfersmid, 1988). The paradoxical situation arises that the methodology which is least useful to clinicians was approved by academics looking after their own research industry, and the methodology most useful to clinicians was dismissed by academics. As recently as 1980 the clinical practitioner was being advised in the Netherlands to wait for group testing methods to deliver the goods (Van Strien and Dehue, 1985: 15).

Taylor (1958) had vigorously attacked the shallow conceptualization of theoretical problems which led to this kind of science, in a paper aptly entitled 'experimental design: A cloak for intellectual sterility'. He asks the rhetorical question: Would Newton have discovered the laws of motion by correlating data from many solar systems? General laws are able to predict differences as well as similarities; merely lumping together different cases will conceal relations. Can we hope to discover general laws of human psychology by pooling data from many people who, as often as not, construe the situation differently? We simultaneously reject the view that people are identical and pool the results obtained from them. Why do we do this? The professional opportunities cited above suggest at least part of the answer. There was a demand for sorting techniques; those psychologists who supplied them became dominant in the profession and while their dominion lasted they imposed their views on others. The formidable techniques they developed became the true ideology of psychology, so that even when it did not answer questions, the dissatisfied felt that the fault must lie in themselves. Yet it was the usual imperial overreach. What was good for some was thought to be good for all.

Demonstrations of legitimacy

A practice is legitimate if it is positively valued in a particular 'reference system' (Van Strien and Dehue, 1985), or by a particular client or audience. Reference systems may be (1) other members of the discipline; (2) members of other academic disciplines; (3) specific clients; and (4) 'society at large'.

Attempts to show that a practice or theory is legitimate will depend very much on the demands which arise within these different reference systems. Those within a discipline will demand that a novel practice or theory satisfy disciplinary norms and standards of proof. Members of rival disciplines will be anxious not to be displaced. Specific clients will demand that their needs be met. And what of 'society at large'?

When large-scale changes in the balance of political power occur, new publics make their voices heard. It is then that we become most aware of attempts to establish the value or legitimacy of a discipline. These attempts may have significant effects on the way in which that discipline develops or withers away. We may illustrate by referring to the anxieties of psychologists in South Africa. The question which they cannot avoid asking is: How do their activities look to revolutionaries? Will they be supported after massive political change? Does psychology have any specific role to play in the fight against apartheid? In order to examine
these questions, a number of young psychologists in Durban, Cape Town and Johannesburg founded a journal, *Psychology in Society*, which 'aims to critically explore and present ideas on the nature of psychology in apartheid and capitalist society', according to the editorial statement on the cover of the journal. The struggle to establish legitimacy can become fierce, as we see from discussions in the journal. Who will play the leading role in the political struggle and who will dictate the content and methods of psychology in a period of transition? One response to the situation was to establish courses in community psychology which would reach out to 'the community' or 'the people'. But this has not been unchallenged, as we can see from a hostile article in *Psychology and Society*:

Community psychology is a 'red herring'. . . Certain foxy psychologists . . . know that the herring will enable them to appear relevant while simultaneously keeping open the 'passage' to Australia and the Americas. It is not only individuals who benefit from such a tactic; but also departments who continue to theorize and teach within the mainstream individual paradigm while allocating a few junior staff members to hoist the community flag' (Suedat et al., 1988: 51).

The authors believe that 'lifting one of the masks' of the pretenders is a start, though they lamely confess that it is more difficult 'to develop a psychology that will take cognisance of the psychological processes of oppression and liberation' (1988: 51) than to expose failure. The difficulty in this game is that once we appeal to hidden motives we call for a general confession. The critics should be made to confess with those they criticize. To lift the masks of others is also a way of ingratiating oneself with 'the oppressed' and of keeping the 'passage' open to 'Australia and the Americas'. Cycles of denunciation are a source of gratification; they carry the obligation, if they are to be taken seriously, to propose a psychology which can be subjected to the same criticism. The cycle of denunciation, disguised as methodology, is clearly described in Kozulin's (1984) *Psychology in Utopia*.

One attempt at liberation psychology uses Frantz Fanon as a role model. His psychology of liberation speak directly to students who are members of oppressed groups as well as to students who are among the oppressors. There is, first, the psychology of power. Many Blacks identify with their white oppressors just as the inmates of concentration camps identify with their guards (Bettelheim, 1943). One response to inferiority is to become more like one's oppressor; another is to move against them. In *Black Skin White Masks* (1970; orig. 1952) Fanon describes the process of identification with the oppressor, whereas in later works, such as *The Wretched of the Earth* (1967; orig. 1961), he describes the process of identification against the oppressor. The two books complement each other to form the nucleus of a psychology of liberation. In the former, 'the Negro is comparison . . . continually preoccupied with self-evaluation and with the ego-ideal' (Fanon, 1970: 149) – that is, with the image of white superiority. The child who identifies with white heroes wishes to be white. This inner whiteness is possible until he is in contact with whites; then he is black. Fanon cites a novel (*Je suis Martiniquaise*) in which the heroine submits completely to her white lover. Her passionate submission to whiteness, her desire to be filled with whiteness, is a futile attempt to magically transform herself. In another novel, *Jean Veneuse* pursues the life of intellect, accumulates 'an impressive reading list', and falls in love with a white woman in France. He is accepted because he is not 'really black'. He has a white mind! Of course, he cannot believe it; he needs repeated assurance. He is an Othello waiting to be betrayed. The magic of possession can never work because it remains external. 'When my restless hands caress these white breasts, they grasp white civilization and dignity and make them mine' (Fanon, 1970: 46). As long as he identifies with an aggressor who merely uses him, he is caught in a contradiction. The answer, Fanon believed, was revolution and the assertion of a new identity which would not be self-contradictory. Should there be an ethnic or racist revolution? Or, on the contrary, should it be a revolution based on Marxist theory, in which one class replaces another? What is the political and social significance of this revolution to be, if it is to give content to a new social identity which is free of internal contradictions? This is the theme of critical works on Fanon by Bulhan (1985) and especially McCullough (1985).

Perhaps this is enough to indicate why the work of Fanon is of particular interest in post-colonial society, and why psychologists have used it in attempts to construct a psychology which might be both illuminating and legitimate in Third World contexts. It confronts the facts of social identity and power in a way that few psychologies do. It grows out of a specific historical context, that of colonialism. It links psychological well-being to the act of liberation.

For this reason it is not surprising that psychologists at the University of Cape Town (and almost certainly at other universities in South Africa and elsewhere) found their students responsive to courses which discussed Fanon's contributions. Several South African postgraduates have gone to Boston to study under Bulhan, the noted Fanon scholar, and may well carry this work further.

A crisis in which a discipline must justify its value is an opportunity which may lead to new advances; or it may lead merely to recriminations
if there is no foundation on which to build. South African psychologists concerned with liberation have shown greater skill in 'unmasking' and criticizing each other than in building; but their efforts make one thing clear: the development of knowledge is not isolated from other social processes. Those who practise a profession must continually bear in mind the demands made by their different publics.

Institutional power structures

Scientific work occurs within institutional power structures of one sort or another. Universities make or do not make appointments; journals publish or do not publish articles; funding agencies allocate or do not allocate funds. Within each of these institutions, some individuals have more power than others.

Case studies may be revealing. One of the most complete is the study by Lubek and Apfelbaum (1987) on suppression of the 'Garcia Effect' in the main APA journals. Fortunately, there were other journals in which Garcia could publish his findings, but the case raises questions about the openness of journals to strikingly novel work which challenges the received assumptions of those who control publication.

First, what was the 'Garcia Effect'? Garcia and Koelling (1966) found that rats presented with a saccharin solution or 'bright noisy' water (when they licked a water tube a light flashed and a clicker clicked), followed after a long interval by stomach upset caused by radiation, would learn to avoid the taste but not the light-sound combination. Yet in a complementary experiment, when the saccharin flavour or the 'bright noisy' water were followed by electric shock, the rats learnt to avoid the 'bright noisy' water but not the flavour. The consequences of this experiment, if confirmed, were difficult to assimilate to established learning theory.

Thus, the occurrence of learning appeared to depend not upon what cues were used - that would have been understandable - or what consequences were used - that would have been understandable - but upon the specific relationship between cues and consequences. This was incomprehensible (Bolles, 1975: 249).

A well-established (or at least widely believed) principle of the learning theory challenged by Garcia was equipotentiality, or the principle that it should be more or less equally easy to link all discriminable cues to all effective reinforcers. What was being suggested here was that the process of learning depended on highly specialized functional systems.

The contrast between the traditional view of learning and Garcia's view is as follows:

From the evolutionary view, the rat is a biased learning machine designed by natural selection to form certain CS-US associations rapidly but not others. From a traditional learning viewpoint, the rat was an unbiased learner able to make any association in accordance with the general principles of contiguity, effect, and similarity (Garcia, cited in Lubek and Apfelbaum, 1987: 68).

Not only did the 'Garcia Effect' undermine the principle of equipotentiality, it also undermined the principle that reinforcement had to follow response immediately and frequently in order to establish a link. In Garcia's experiments rats had learnt to avoid flavours which were not followed an hour or more later by radiation sickness. From an evolutionary perspective, this makes perfect sense. Animals would not have survived if they had not learnt the most important things about the environment with great speed, while ignoring other relations. The S-R paradigm (like modern cognitive psychology) is biologically empty and therefore not informative about the behaviour of real organisms. The work of ethologists had already shown the value of linking learning to biology, but it took a long time for this to penetrate the American learning theory establishment. The important new principles are: (a) study the functional systems or modules of the nervous system; and (b) adopt an evolutionary approach to these systems.

The story of Garcia's exclusion from the major APA journals, dominated as they were by persons trained in traditional animal learning theory, may be summarized as follows: In the early part of his career, up to 1965, Garcia was able to place his articles in the mainstream APA journals. After this, they were consistently rejected and had to be published elsewhere. Even when he received the APA Distinguished Scientific Contribution Award, the published version of his address ('only the second to appear in an APA journal authored or co-authored by Garcia during the previous eighteen years' (1987: 60)) was edited to remove passages satirizing learning theory. In this period of exclusion he had made a significant impact on American science, being one of the few psychologists elected to the National Academy of Science. Rejections of his work included several ad hominem remarks. One reviewer suggested that a manuscript 'would not have been acceptable even as a term paper in his learning class' (1987: 79). Another wrote: 'Those findings are no more likely than bird shit in a cuckoo clock' (Seligman and Hager, 1972: 84).

Had the APA journals been the only ones in which to publish,
Garcia’s work might have been overlooked and forgotten; and given the competition for research grants and research positions, he might not have been able to continue his work. We do not know whether other researchers have yielded to the pressure to conform, but it is likely.

It is easy enough to understand why a group of scientists should attempt to suppress the work of those who undermine their position. There is, perhaps, a strong conviction that the underminers are simply wrong. Then, it is rational (within limits) to protect one’s investment in a particular theory. Loss of a theory may lead to loss of prestige and academic power in a field in which one has achieved eminence. It may mean surrendering the field to advocates of the new theory and accepting that one’s lifework is based on error.

Sometimes the position may be saved (for the established tradition) by assimilation. This is made difficult when the new work uses terms and concepts which have been explicitly rejected by the established tradition. Lubek and Apfelbaum describe this as an ‘epistemological breakaway’ (1987: 68). It was this that made Garcia difficult to assimilate after 1965.

In addition to conflicts within disciplines, there are also conflicts between disciplines searching for power. In the first chapter, I referred to Danziger’s (1979a, b) comparative studies of American and German psychology, in which he suggested that philosophy had a strong influence on psychology in Germany (due to its established position in the German universities) and a relatively weak influence in the United States of America. This relationship of domination was even stronger in France than in Germany. According to Piaget (1972: 27),

> the implicit permanent principles of the French university authorities are that psychology is part of philosophy, that every philosopher is fit to teach psychology, but that the converse is not true; that there is no question of an segregation in psychology, since the agreges in philosophy know everything.

One result of this was that ‘during more than 50 years (up to the appointment of Fraisse, who has at last been given the opportunity), the psychological laboratory of the Sorbonne was a peripheral institution’ (1972: 27–8).

In their struggles with each other, persons working within one tradition may attempt to show that they are indispensable for solving the problems of another tradition. This is what Piaget has undertaken in his work on genetic epistemology, or the development of logic and knowledge in childhood. In his book *Insights and Illusions of Philosophy*, from which I have quoted above, Piaget continues his attempt to show that philosophers cannot do without psychological facts in formulating theories of emotion, motivation, or knowledge. (Strategies of competition will be discussed in greater detail in the following chapter.)

Psychodynamics of leadership

If a tradition is to be actively developed, followers must be recruited, trained, and given the means to work. Since some innovators are better at this than others, it will be a factor in the dissemination of their ideas. The formation of knowledge tribes is well-known in psychology and in all the social sciences, where there are Marxians, Freudians, Nietzscheans, Foucaultians, Lacanians and others. Followers regularly recite the founding father’s words and are angry with heretics.

Why this intense tribal life in social theory and criticism? Why the unending polemics and cries of misinterpretation? Perhaps one can suggest a rule. The smaller the probability of deciding an issue, the greater the need for a founding father to whose text an appeal can be made.

Donald Campbell examines a case in psychology – the case of the missing tribe. What, he asked, happened to Tolman’s tribe? When we compare two learning theorists, Tolman and Spence, we observe that Spence’s students worked on his theory whereas Tolman’s students did not. The puzzle is: ‘Why were Tolman’s students the least loyal when, of all the learning theories of the 1930s, Tolman’s can now be seen to have been clearly the best?’ (Campbell, 1979: 187). Tolman was a cognitive learning theorist at a time when learning theories were synonymous with conditioning. Also, Campbell cites data which show Tolman to have been a ‘charismatic’ leader and a ‘source of intellectual ferment’ on the Berkeley campus. He was the most ‘personally beloved’ of the various theorists compared. In the profession of psychology he was respected and his contributions were acknowledged, since he succeeded Hull as president of the American Psychological Association in 1937 and published in its principal journals.

When Spence and Tolman are systematically compared, we find that Spence was much more strongly convinced of the value of his own position, took himself much more seriously, expected a much stronger commitment to his approach, was more authoritarian, aggressive and strong-willed, was much less willing to accept criticism or to allow students autonomy of choice in research problems, and was much less open-minded or humorous.

Tolman’s style, on the other hand, was humorous and self-deprecatory.
In Campbell’s words, ‘he welcomed his students as equal-status fellow explorers. . . . Had it been preceded by the careful reading and examination of Purposive Behavior, it might have worked. But it was not, and was instead very poor pedagogy’ (1979: 190). The effect was that Tolman failed to convince his students to devote their research to his theory.

Concluding remarks

Knowledge tribes may form more readily under some conditions than under others. Campbell compares two rituals for testing beliefs. The first kind of ritual is one in which the ability of the participants to affect the outcome is reduced as far as possible, whereas the second kind of ritual depends largely on insight and interpretation. The more important interpretation becomes, the more prevalent will be tribal life constructed on the text of some oracle and set of disciples.

Consider the first kind of ritual, of which the scientific experiment is the model. By careful design the experiment is intended to remove the results ‘out of the control of one’s own hopes and wishes’ (Campbell, 1979: 198). This is the ambition of the objective experimenter; it is a procedure which distinguishes the empirical approach to knowledge. Yet its roots go deep into our intellectual history. Apparently caribou hunters would place a shoulder blade of the caribou in the fire and read the cracks according to strict rules in order to decide on the direction of the hunt. The rules were intended to eliminate interpretation as far as possible.

An even better example of the objective procedure in a non-scientific tradition is that in which the Azande consult the poison oracle. The original description is by Evans Pritchard, but I am following Jahoda’s (1982) account. When a prince commanded that the oracle be consulted, the result had the force of law, yet the manner of consultation apparently prevented interference by those who conducted the consultation. The poison, which was brought all the way from what was then the Belgian Congo, was carefully tested. Jahoda states that ‘it is possible to show that in certain respects the Azande thought about their oracles in much the same way as psychologists think about tests and questionnaires’ (1982: 178). First, the poison was tested to see whether it could discriminate, by being administered to a number of fowls. If it killed them all it was said to be ‘foolish’; if it killed none, it was said to be ‘weak’. The ideal poison would kill only some of the fowls. Jahoda compares this to the psychologist asking: Is this test discriminating? Second, questions were put to the oracle in pairs to see whether a consistent answer was given. Thus, if the death of a fowl indicated that the accused had committed adultery, a second question might be put in such a way that the survival of a fowl indicated guilt. In test jargon, this is the criterion of reliability. Third, impossible events are presented to the oracle to test false responses. Thus: ‘If I shall go up to the sky and spear the moon, or bring back the sun, poison oracle kill the fowl’ (1982: 179). If the oracle makes false responses, it also reveals that sorcery is being employed to interfere with the oracle. In other words, the test is biased.

One can read these precautions in consulting the oracle as an attempt to get at the truth, uninfluenced by the hopes and fears of the participants. The result is an empirical test, very similar to the tests employed by psychologists attempting to obtain the truth from subjects who may mislead them. As in all experiments, every precaution is of no avail if the experimenter is not scrupulously honest.

Then there is the second kind of ritual, which depends upon insight and interpretation, possibly by inspired persons. Shamans and priests are required to read the secret messages of sacred texts, auspices, sacrifices and other portents. Mystery is at the centre of such rites.

To the extent that knowledge is tested in the first way, personalities play a diminished part in convincing followers of the truth of a proposition. To the extent that knowledge is tested in the second way, by interpreting and reinterpreting texts, the personality of the founder will remain important. Rites of the first kind are intended to reduce both mystery and authority. Rites of the second kind increase them. As we have seen from the cases of Garcia and of Tolman and Spence, personal interest and authority can never be dismissed entirely, but rites of the first kind reduce their importance. There are no surviving tribes of Spence, Tolman or Garcia. Their writings are disappointingly barren of mystery and authority. On the other hand, the tribes of Jung and Freud, or Marx and Foucault, are likely to be with us for many a year. Rich in mystery, authority and inspiration, they are seers and priests for modern times.