ANTHROPOLOGY AND THE PHILOSOPHY OF SCIENCE.

Naivety and Exposure

No one will seriously contend that it is possible today to be a 'Renaissance man'. At the same time social anthropology gains nothing by its practitioners being uneducated. But if some regard the specialization which has accompanied the tremendous increase in knowledge as a necessary evil, 'Closed Systems and Open Minds' suggests another view (see Gluckman ed: 1964). Although associated particularly with the Manchester school, the naivety thesis advocated in this book represents the outlook of many of the older generation in our discipline, and it is a view which has had severe consequences.

For Gluckman, genuine understanding stems from specialization. This requires that we delimit a field for an academic discipline. Social anthropology has its problems and its way of dealing with them. We are to be naive about other fields of academic endeavour; that is, we can work with simple assumptions about the nature of other disciplines. The premise of naivety, then, erects ignorance to the status of a methodological virtue. Now, firstly, to say that a discipline has its problems is to presume a great deal of agreement on the part of its practitioners as to what it is they ought to be doing. Not only is this not required for a healthy branch of scholarship, it may be positively harmful. If anthropology is what anthropologists do, it is clear that the interests of its individual scholars will form at most, family likenesses in which there need be no constants. In this sense the idea that a subject has an essential nature would not be true. As to the harmful effects of being over-eager to define a discipline, one need only quote from Fortes' inaugural lecture at Cambridge. He suggests that with the functional theory we now have a sound empirical science having eliminated conjecture and history. He rejects as no more than stumbling blocks to clear thinking the approaches of an older and far more scholarly tradition than that of which he is a part. Social anthropology, he goes on, is now 'able to recognize itself, once for all, as a separate discipline concerned with 'mechanism and function''(Fortes 1953: 24).

No comment on this is surely required. He would have us, it seems, work for ever with one model of society and confine our attention only to the problems which this functional view generates. Whilst those problems may be worth attention, the functional framework fails even to formulate satisfactorily others which are equally our province and perhaps more important. This is not to say there need be no shared assumptions as to the general territory of a discipline, but a definition of the type that Fortes suggests which effectively freezes thought can only have a negative value.

What is of special significance here, however, is the idea that anthropology is now a separate discipline. I suggest the pedagogic-institutional position of our subject is quite unimportant. To take it seriously produces a sterile concern for questions of relations between disciplines. Presumably individuals will read wherever their interests take them. To suggest that the relationship between sociology and anthropology is, or ought to be, such and such would seem to lack meaning.

On this issue, it is the contention of Gluckman that we must close off a conceptual system and work with simplified versions of other disciplines. (Murdock in 1951 picked out precisely this indifference to other fields of scholarship as a basic weakness in British social anthropology-and it is clearly evident, among other places, in a whole list of works on substantive economics stemming

from Malinowski). But the naivety thesis would virtually eliminate
one of the chief sources of theoretical advance which might be called
convergence phenomena. Advances in knowledge seem toproceed sideways
as often as forewards, as can be seen from the construction of new
sciences precisely from the borderline areas between existing
disciplines. Gluckman suggests we taboo thought at these pedagogic
margins, effectively ruling out the possiblity of this type of
progress.

To demonstrate the consequence of such an attitude, the ASA
conference of 1964 which resulted in The Structural Study of Myth
and Totemism (Leach ed: 1967) is rather valuable. It shows that we
must not remain ignorant of developments in other discipline or
cut ourselves off from the insights they afford, that there are
hazards invol. d in conceiving anthropology as a separate discipline
with its own problems and its own special approach. Agreement on
the nature of a discipline, when it is combined with naivety leads
to intellectual inbreeding and a degeneration in thought. 'Only
when there is sufficient variety (in a population) it is ensured
that there are always individuals available with characteristics
suitable to meet the changes that occur in the environment'.
(Young: 960; 147). The population in this example is British social
anthropologists and the changing environment is the realisation
in French anthropology of the tremendous value of structural
linguistics in providing a method of tackling our own material.

Linguistics had virtually disappeared in British anthropology,
although learning a language of course survived as a necessary part
of fieldwork. Its value, then, was seen only as pragmatic, and in 1960
there was no ASA member (see Ardener E&S: 1965) whose declared main
interest was in language as such. Thus when Lévi-Strauss demonstrated
the value of the structural approach to myth, we were, for the most
part, at a loss intelligently to evaluate the analysis. Leach was
quite right in his introduction to the ASA volume to point out that
its main value was an exposure of the prejudices of the contributors
towards this French sage.) b · Strauss had first published his approach
as early as 1955 in the Journal of American Folklore, yet in 1964 a
discussion of his work is strikingly lacking in competence.

Such a community has produced its critics, but those who have
been most noisily appalled, for instance Jarvis, have not distinguished
themselves in their critiques. There was so much to be attacked in
current anthropological practise, but in 'The Revolution in Anthro-
pology' (Jarvie: 1964) we are offered ill-informed comments from which
our discipline can derive no benefit. For instance, blindly to follow
Popper in rejecting what is assumed to be a baconian view of science,
providing no evidence, either textual of bibliographical, of having
read any Bacon, is exactly the lack of education which harms anthro-
pology.

Enough has been said for the moment on social anthropology. If
it is accepted that naivety is harmful, there would seem to be no
reason why our exposure should be confined to other social sciences.
All these disciplines have, since their origins, been influenced by
an image of the natural sciences and in view of this it is not un-
reasonable to endeavour to acquire some familiarity with the history
and actual practice in these exact disciplines. In the general issue
of the applicability of natural science techniques and methods to
social phenomena it would seem to be helpful if we were more informed
than at present about the natural sciences. It is, in fact, crucial
for all the social sciences to substitute for the present gross
misconceptions of the natural sciences a sympathetic and informed
view from which we might be able to construct a better type of
humane discipline.

Having received no training in either philosophy or science,

I have been able to take to the literature an anthropological
sensitivity rather any type of expertise, so the problems which
I shall pick out and the manner in which I shall discuss them will
stem very much from an anthropological education. And the intention,
moreover, is by no means to become a philosopher, but simply to allow
the insights gained in the exposure to feedback and improve the practise
of anthropology itself.

'We ourselves come into the process.'(Young: 1960: 103).

Meaning belongs to language; language comes in systems; therefore
meaning comes in systems. Language is a human creation so meaning is
not external. If we accept this we have the problem of defining what
science is about, for if we suggests that sience is about the world
we must yet concede that in some sense the world is of our own making.
What dowe contribute when we suppose we are talking about an external
reality - we enter the process, but where? This problem perhaps
naturally suggests itself to an anthropologist, so I shall begin by
discussing the literature that brings it to the fore.

Human being live in a symbolic universe, a fabric of meaning.
Can we therefore accept A E.Housman's lines-'I a stanger and afraid,
in a world I never made.' There is an obvious sense in which we
think about the world as an independent reality, yet, at the same
time the world for us is the meaning, we give it. So, do we talk
about an external world or do we find in Cassirer's words that
instead of dealing with the things in themselves 'man is in a sense
constantly convereing with himself'.(1944: 25). All the sciences,
says Hume in his Treatise on Human Nature 'have a relation to human
nature; however wide any of them may seem to run from it; they
return back by one passage or another'. ln this section I shall look
at this relation. The problem- that our science is about the world
but that meaning derives from us - is not solved; but the opposition
is weakened somewhat.

German metaphysics represents an extreme form of philosophical
speculation. Santayana discusses it in terms of egotism, an attitude
which assumes that 'nothing should control the mind except the mind
itself. Egotism is subjectivism become proud of itself and proclaiming
itself absolute'.(1939.151). The egoist 'grasps only himself and in
that sense his egotism turns out true'.(Ibid: 71). But the omnipotence
of thought is neither a primitive nor a metaphysical peculiarity.
Do we ever assume that our meaning and language (that is our thought)
do not define the world in which we live. But, do we ever get any
nearer to reality than meaning? Do we in science have a dialogue of
the mind with nature or only of one mind with another?

Langer in her Philosophy in a New Key (1942) suggests that
man lives in an essentially human world. The symbolic universe is
constructed by us, the fundamental process of the human mind is
symbolic transformation. (This has absolutely fundamental importance
for the task of theory construction in the social sciences which
an ignorant form of scientism has managed to obscure). And we must
recognise here, besides the creative aspect of our thought, the
essentially social (because linguistic) nature of our ideas.
Feuerbach has said that 'two beings are as necessary for the
generation of the human mind as they are for the generation of the
human body'. The essential point is that we live in a shared conceptual
world. We are not in the realm of private meaning but of collective
representations, but as we are dealing with meaning, we can sensibly
oppose to subjectivity not objectivity but only inter-subjectivity.
Meaning here is not individual but its supra-individual social character
consists in its shared quality not in any externality. Now it is
clear that in characterising social facts as external Durkheim meant
nothing metaphysical, and we must remember that when he suggested
there existed an independent realm of social facts he was endeavouring

to create a discipline. Nevertheless to substitute inter-subjectivity for externality does rather require that we redefine the distinction between individual and social facts, for clearly psychology and sociology cannot here be seen as referring to two completely independent types of phenomena. Thus despite the marked failure up till now of integrating psychological insights into the social sciences , this stance does compell us to continue to search for ways in which the two disciplines can be meaningfully integrated.

To return to the relation between science, the world and ourselves, two important areas in which it can be said that we make a significant contribution are these. Firstly, in sensory experience, we always knew, but modern experimental psychology supports common sense, that sensations simply are not accurate or complete reports about an external world. The physical constitution of our organs and the brain work together to select and organise from a chaotic flow of impressions. Our merest sense experience is a process of formulation. An object is, then, not a datum but a form construed by an intelligent organism. As N.R.Hanson says in <u>Patterns of Discovery</u> (1958) seeing is a photo-chemical event but perceiving is interpretative and creative. So ideas do not derive from sensations rather experience gets its sense from conception. Sensation, then, is theory-laden, influenced, for example, by expectation, so there must be an intimate connection between 'perceiving as' and 'perceiving that'.

Secondly, there is a close relationship between science and language. I shall not here concern myself with whether there is a metaphysics concealed in the structure of a language, but as science is essentially communicated knowledge, it is <u>in language</u> It uses symbols and we cannot assume that for literature language is central but that for science it is merely a neutral means of expressions (see Barthes in Lane ed.1970). No code is privileged and no language is innocent. For istance, we cannot assume we have eliminated ourselves from science simply because it uses impersonal grammatical constructions.

I can best start my discussion of the philosophy of science by dealing with the problem of externality. It is often said that Bacon offered an inductive theory of scientific method-science accumulates facts and from them generates general principles Now inductivism is a hopelessly erroneous description of, or prescription for, scientific activity, but we would be wrong to attribute this type of view to Bacon. He was far too much a product of a medieval education for this to be even possible. His desire was to break the hold of the Aristotelian system and to erect a new system of reliable knowledge. In this task he did not deny a creative role to the human intellect, but ideas were not simply to be conceived in 'the little cell of human wit', but tested 'with reverence in the greater world'. They were to be used to find out experimentally the most basic processes of nature by discovering which ideas were of the most wide applicability (see Harré:1964,Purver:1967). The new science was to be subject to a continuous and external control.

This is not inductivism, but we have still to explain the idea of external control. Science as a 'second Scripture' is possibly the solution—for Bacon, God reveals himself in the world. As Heisenberg states:'This new activity was in its beginnings certainly not meant as a deviation from the traditional Christian religion. On the contrary one spoke of two kinds of revelation of God. The one was written in the Bible and the other was to be found in the book of nature '(1958:16). Thus meaning is derived not imposed and is external in the sense that it belongs to God. But this view and with it the notion of a purely external control becomes unacceptable the moment we focus our atheistic attention on our contribution, the models we build up—the morphology of significance of which we are the creators. If nature really is a book to be read, in which

language is it written? We cannot escape Whewell's dictum:
'There is a mask of theory over the whole face of nature', and
of this theory we are the authors - this is where we enter the
process.

This view is of some signigicance, for a whole set of terms
that are still used in the philosophy of science, for instance
empirical, fact, etc. are semantically kin to this idea of externality.
If we are unable to find a useful meaning for this concept then
these others belonging to the same epistemological standpoint can
only be a source of confusion. All activities in science are
theory-dependent, so how could we use the term empirical to which
the term theoretical is opposed? In the O.E.D. we find that the
concept of datum and fact are related to the notion of givenness,
which on psychological grounds, we know to be untenable. Now the
philosophy of science uses for the most part the language of ordinary
discourse, and natural languages are simply not in order. They are
the anonymous creations of unconscious generations of amateurs and
can be improved upon. Their capacity to carry meaning is, of course,
rooted in their stability, but if we equate meaning with use and
then conclude they are in order we put ourselves at the mercy of
the theoretical prejudices of our predecessors in the use of
language (see Gellner:1959). When we are aware that words simply
do not express what we mean they can only be substitutes for
thought. It is no advance if we feel uneasy speaking of reality
to use the concept 'reality' instead. Philosophy is concerned with
evaluating the use of concepts, that is, not simply with the use of
words but rather with what it makes sense to say. Fully conscious
of our contribution in science, thinking in terms of models deriving
from ourselves, the terms in which we talk about the activity of
science are most unsatisfactory. (At the same time as making this
remark about philosophy, it ought to be added that if anthropology
is basically about a fabric of meaning and language, then it will
be the natural language of the culture in question that in part
supplies the structure of the phenomenon which is being investigated.
Here, therefore, the natural language must be treated with great
respect, and those logical deficiences and ambiguities which one
would wish to remove from a philosophical language which has a
precise task to achieve may be precisely the more important aspects
of the language user's situation).

Moving from Bacon to classical and modern physics, one must
discuss the Cartesian distinction between res cognitans (self)
and res extensa (world), which was so significant in the evolution
of the natural sciences. Its implication was that one could talk
about the world without reference to oneself; a position which
came to seem a necessary condition for all natural science. In
the C17th science looked away from man towards machines for
explanatory purposes (with several dire effects on the social
sciences, which were founded upon a slavish and unscientific
imitation of them) but by 'a curious revenge(this) is now found
to be also its chief theoretical deficiency'.(Young:1960:107).
It has been found in modern theoretical physics that we cannot
eliminate ourselvss; in certain circumstances knowledge is
essentially a relationship and the scientist has theoretically to
reenter himself into his science. In Young's words: '—our physical
science is simply not a set of reports about an external world.
It is also a report about ourselves and our relations to that world—'.
(1960;108). Heisenberg in a similar way: 'what we observe is not
nature in itself but nature exposed to our method of questioning'.
(1958:57). But perhaps Jeans in his address to the British
Association in 1934 sums up this general trend in thought most
efficiently: 'The nature we study does not consist so much
of some thing we perceive as of our perceptions. It is not the
object of the subject-object relation but the relation itself.
There is, in fact, no clear cut division between the subject

and object , they form an indivisible whole—.'

Now if this weakening of the Cartesian position is to be welcomed, that is, we become more conscious of our part in science, Jeans' conclusion is wrong. If in some sense, science is about reality for us, it does not follow that it speaks about our perceptions rather than about the world. This suggestion and the type of science to which it leads, are unacceptable. How can I maintain this when all along my emphasis has been on our contribution? I have endeavoured to humanize science and now suggest that science is about the real world. No ultimate solution to this problem that science is about the world but that meaning is human is offered save to suggest that science does refer to the world but that it never stands alone; it is always part of a larger system of thought. Science has not suddenly become philosophical in the C. 20th, it has never been independent on philosophy. And here the underlabourer conception is clearly wrong. The suggestion that it clears up some preliminary confusions and then positive science can get along on its own, is simply untrue (see Winch: 1958). Philosophy is a permanent part of the structure of science; its foundations are metaphysical and its method is always intimately related to an epistemological position. To deny science freedom in this way allows us to include ourselves in our thought and to suggest that science is about the world. This is the more so when that ethos of the scientific community - rational criticism - provides, as Popper has stressed, a tough environment in which our thoughts about reality have to compete to survive. This factor for Popper (see Conjectures and Refutations: 1963) resolves the problem of how knowledge may be a human affair but yet not arbitrary. Whilst this view cannot simply be left as it is, it contains a great deal of truth.

System and Meaning in Science

Pouillon in Les Temps Modernes (Vol. XII: 1956) rightly points out that the originality of Lévi-Strauss does not lie in his emphasis on structure; it consists in taking this characteristic seriously and 'd'en tirer imperturbablement toutes les conséquences'. Here I shall make the idea of system central and try to draw all consequences from it. But an anthropological note is in order first. The achievement of Malinowski was to emphasise, against an earlier tradition, the systematic nature of culture. Now the atomism of the Victorian approach coexisted with an interest in belief; and for the gain of system in functional theory we suffered the loss of interest in meaning. This interest returned in Evans-Pritchard's superb (1937) monograph on Zande thought where the ideas of system and sense are central. (But so anannounced was this shift in attention that it seems many became aware that it had happened rather belatedly). And perhaps it is one of the more important aspects of structuralism to look firmly together these ideas of meaning and system (see Douglas: 1966 on Judaic classification). Nor should it appear strange to combine Evans-Pritchard and structuralism at this point, for while he is in no sense a structuralist, it must be recognised that his interest in the Année school brings him into that tradition of French sociology of which Lévi-Strauss is also a product.

I shall deal with system and meaning in science primarily with respect to one historical example. Let me start with two quotes from Harré's excellent Matter and Method (I). He sees Newtonian dynamics as the final adoption of the Corpuscularian philosophy-the mechanical world- picture, a general conceptual system 'the acceptance of which determines the direction in which the analysis of phenomena should proceed and the content which must be included to make an explanation acceptable' (1964: 105). Elsewhere that: ' Acceptance of the doctrine that matter is that which is defined by the primary properties not only determine the

details of a g.c.s. and hence the acceptable form of explanation, but also the details of acceptable scientific method'.(Ibid:114).

It was poss ble for a Victorian positivist such as Pearson in his 'Grammar of Science' (1892) to see science as ideally free of philosophical impediments. But the Newtonian system, one of the greatest achievements of the natural sciences both rested upon and was intelligible only in terms of essentially philosophical assumptions. And this must be so of all scientific systems. If we now accept as natural the idea of a corpuscularian world, its essentially modern and philosophical character must be stressed, for it requires we admit the void into our universe. And we may recall, for instance, that Parmenides among others was unable to accept the reality of nothingness on logical grounds and then to deny the possibility of motion. Newtonian theory rests ultimately on the discontinuity of matter - we must first accept the possibility of empty space before we can conceive of motion as rearrangement in space. Historically it was the philosophy in Gassondi's Syntagmata which by separating the notions of space and matter made this idea acceptable.

But no less important than this foundation was the intimate dependence of Newtonian science on the type of philosophy which finds expression in the writings of Locke. The Newtonian model results from a selection from sensory experience: it gives a differential existential status to its various components. The key distinction here is between primary qualities(such as mass) which are judged to correspond to real properties in the world, and secondary qualities (such as colour) which belong to our perceptions but do not exist in the world. Newtonian mechanics is possible only with such a distinction - a different epistemological stance, for instance Berkeley's esse est percipi would have produced an entirely different kind of science. It is no exaggeration for instance to see the Copenhagen interpretation of quantum mechanics as a direct philosophical heir to this Berkeley view. In some sense, then, science talks about the world, but its relationship to metaphysics and epistemology define for it the type of world about which it is to speak, and constrains both what it is permitted to say and what method it can employ.(It need hardly be emphasised here that the social sciences must be in the same position. Thus a metaphysical assumption regarding the nature of man must be the basis of theory construction in those disciplines. The problem has been that in real ignorance of the natural sciences, an outdated and misunderstood paradigm has been used in the social disciplines derived from the exact sciences without real attention to the problem of what constitutes an adequate explanation. It is perhaps worth entertaining the idea that the social sciences may not yet even have stumbled on the right type of language in terms of which to explain their subject matter).
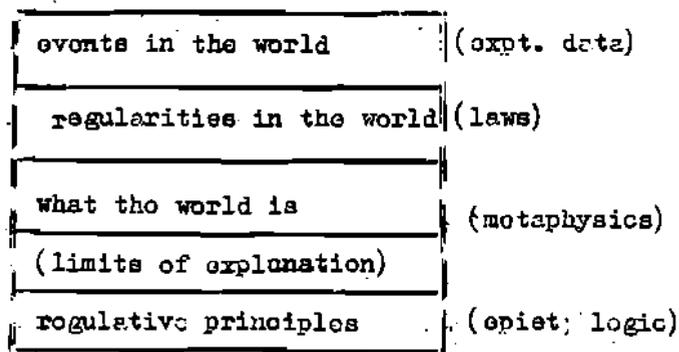
Diagram I.represents the outline of a general conceptual scheme. By regulative principles I mean epistemological assumptions and what are accepted as the correct rules of thought. These are not parts of a conceptual scheme, but obviously underlie all the propositions it contains . By metaphysics I refer to those basic concepts which tell us what there is in the world. This level is ontological, and because it is basic to a system of thought at any time it also constitutes the limits of explanations of that system. A system is based upon those concepts and since they refer to the fundamental processes in the world they are not themselves to be explained by the science that is constructed in their terms. The foundation of any system can never be justified by the system itself, only, if at all, by another system. Taken together these regulative principles and ontological propositions form what Polanyi (1957) might mean by the fiduciary basis of belief. With the same metaphore James in his lectures on

pragmatism suggested that all thought rests on a credit system.

Moving in my diagram from the general to the particular, laws refer to regularities in the world, or in semantic terms the relationships of the terms(and their derivatives) in the theoretical framework.Lastly, I come to events where the notion of system is still absolutely central. To accept that theory determines 'the kinds of things, proportion and processes we are prepared to admit' (Harré: 1964:50) requires we accept that events are largely theoretical and involved in a whole set of concepts which supplies them with meaning. As Neitzsche said: 'There are no facts in themselves - for a fact to exist we must first introduce meaning.' This view renders it difficult to use the term empiricism without confusion and at the same time demonstrates why inductivism is not possible. Induction is a passage from the particular to the general. Were meaning external we could perhaps start from observations and end up with generalisations, but the meaning and existence of particular events are created by a whole theoretical structure; we can understand particular occurrences only in terms of some model of the world as a whole, so significance reaches the events level from the ontological level. Propositions here underwrite our interpretation of particular events. The facts are not basic, semantically they derive from a theoretical structure in terms of which the world is conceived. It is this framework as a whole which is basic. As meaning proceeds form the general to the particular, science cannot go in the opposite direction, which is what inductivism would require.

Diagram 1.

G. C. S.

events in the world	(expt. data)
regularities in the world	(laws)
what the world is	(metaphysics)
(limits of explanation)	
regulative principles	(epist; logic)

To use a cartographic analogy, we construct reality in terms of a set of rules of what constitutes a permissible map, and having decided upon the language for a model as a whole, we have determined in advance the type of event which can occur by making available only a limited kind of semantic label. Thus it must be that evidence is of essence theoretical. Yet we find F.A.Hanson, discussing African thought, making the following comment: '..general propositions seem seldom to be evaluated in the light of contrary empirical evidence'. (1970:61). There is no such thing as empirical evidence; his problem, which is a genuine one, is better expressed in these terms- why do primitives operate with only one model? Hanson's empirical evidence is actually an alternative interpretation that would itself belong to another (in this case our own) system of thought. There are ontological implications involved in the choice of a certain language for building a map of reality. For a primitive to accept the applicability of the language of social relations as a map for the whole of reality (which is what anthropomorphism is) determines for him his experience of that world and the type of concept he may use to explain any particular occurrence within it. It is the symbolic framework, especially when only one is available, that decides the meaning of evidence. It may be objected here that this map analogy leads to an extreme form of relativism. No discipline may rest content upon such a foundation, and it is therefore right

that anthropological interest should again be directed to a search
for universals. Clearly our task is to produce maps, which embody
other maps, that is to arrive at some system of terms which may
serve as a mode of discourse for bringing into meaningful relations
more idiosyncratic maps. I do not regard the relativism implict
in the map idea as leading in the other direction, but rather as
a brake in this search. Lest we too readily seize upon features
that immediately and intuitively suggest themselves as constants,
this map notion at least ought enable us to suspend judgment —
until we are better equiped to vouch for the authenticity of the
universals we believe we have found.

If experience is interpreted in terms of a system, what is it
that determines the choice of the root metaphore used in the
construction of a model? That is, what factors are responsible
for the decision that a certain language is the right one in terms
of which to map reality? A suggestion from Stark's 'Sociology of
Knowledge' may be helpful: the 'basic picture of the world is ___
constituated under the guidance of the axiological system under
which society lives and acts–' (1958:278). Would it possible, then,
to relate, the anthropomorphic models typical of primitive societies
to a point Lévi-Strauss makes in the Entretiens (Charbonnier:1961).
There he distinguishes society and culture; the former is the
relationships between men, the latter the relationship between
men and the natural world. Now civilized society, he says, values
mastery of the environment, and technological progress involves
dislocations in the social order. For the primitive, on the other
hand, social harmony is a prime value. Perhaps there is more than a
causal relationship between that technical progress in the West
and the stripping away of anthropomorphism by which Turgot
characterised the rise of the natural sciences. Could we see in
the anthropomorphism of the primitive an expression of the value
he places on social as opposed to cultural goals; it may be that
the choice of root metaphores expresses social values. Thus, the
primitive sees the language of social relations as a good map for
what we would call the natural world, and by contrast in the
technical West the social sciences have endeavoured to explain man
and society in terms of analogies deriving from the natural sciences.
We are reminded by Horton (1967) that society, because orderly, is
a good model, but anthropomorphism is perhaps deeper than this.
And of course, science which studies nature is a product of culture,
so in searching for those social conditions which made possible the
rise of natural science we may have some clue as to the reason for
the existence of other types of explanatory model under other social
circumstances.

An interesting problem in this area is the stability of those
primitive models as compared with the restructuring that is a feature
of the natural sciences. Is there something in the nature of
anthropomorphic models which lies at the root of this stability;
are such premisses so able to absord and define evidence that —
fundamental rethinking is never required? Are primitive models
simply less rigourous logically, or is the difference to be
located in the content of the concepts themselves? Our own social
sciences seem to have enjoyed a rather prolonged youth, so is the
problem the complexity of social phenomena so that here also we
cannot expect the same type of theoretical progress that has
occured in the natural sciences? Or might it be that a vulgar form
of scientism has prevented the social disciplines from being
anthropomorphic enough so that they have been forced to construct
theory with the wrong set of terms rather than with those types of
concept (rule-following, for instance) by which we ordinarily under-
stand human action? Perhaps there are some conceptual niches (like
ecological niches) the acquisition of which allows the ascent of a
theoretical ladder. It is certain, for instance that a Berkeley view

would not have permitted the theoretical progress which occured in the natural sciences. We shall have to see whether all anthropomorphic models essentially lack this evolutionary potential or whether the stability of primitive models has another explanation.

Before I leave this area I would like briefly to mention the idea of epistemic communities (see Holzner: 1968). Such communities share a set of assumptions about the world and accept a certain system of rules as governing their activities. Now there are different modes of reality construction; different cultures build different maps. But all maps have a coherent cognitive style and are searches for dependable knowledge; but the criteria for reliability will be internal to a map and so will differ between communities. Natural science, for instance accepts epistemological empiricism, by which I mean the equivalence of observers. In such a community there will be a competitive critical ethos, and, as such science will likely be antagonistic towards tradition. For a mystical religious community, on the other hand, we may expect as central the non-equivalence of observers, that is, certain individuals are presumed to possess special insights into the nature of reality. The ethos will likely be hierarchic, the community based upon authority (inegalitarian) rather than competition. But both communities will have a consistent cognitive aspect and in understanding the modes of discourse in these two communities and their respective sociologies, we must remember the different epistemologies upon which they are based.

I can most easily approach this general area of the sociology of science by way of Popper's theory of knowledge as expounded in 'Conjectures and Refutations' (1963). Basically his view of science is, that it is in the words of Xenophanes, a 'woven web of guesses', and as a description of certain episodes in the history of science the idea of conjecture is not without value. If Popper emphasised this aspect, it will be consistent with my emphasis on system to follow up the textile metaphore and draw some implications from it.

For Popper, the truth is not manifest; we cannot know whether a theory is true as we can never completely verify it. All we can do is to make guesses so that all scientific propositions will have a permanently probationary status. But we move towards objective truth by falsification - we always know when a theory is false because we can empirically test the deduced consequences of our hypotheses. Thus we learn by our mistakes and science is a process of conjectures and refutations, or, in Medawar's terms: 'science begins as a story about a Possible World—a story which we invent and criticise and modify as we go along'.(1969).(I am not sure how Popper can be so confident of this movement towards objective truth. If motion is relative and the point to which it is relative (i.e. the Truth) is necessarily unrecognizeable in his own theory, how are we to judge or measure motion at all, let alone specify its direction). However we can accept that science comprises two types of episodes which an adequate methodology must distinguish: one of discovery which is artistic and creative, and one of justification and criticism which is very different. Induction is wrong, among other reasons, because it supposes we start with masses of independent facts whereas facts are never independent (if theory comes in systems then the world comes in systems too) but also because it fails to mention this human creative element. What I wish to criticise in Popper is that there is a sociological aspect in this context of justification also. Medawar (1961) describes Popper's view here as the asymmetry of proof and refutation. But in the second context science is far more than a decisive logical or empirical falsification; certainly far more is involved than rational criticism. We enter the process at this point also, and we do so precisely because of the systematic woven nature of scientific theory.

In primitive thought anomalies are marked off as dangerous; in science because they are regarded as theoretical they are challenging problems. And, in science, advance comprises the solution to problems, rendering explicable what was formerly anomalous by revising the theoretical framework. In primitive thought models are stable and predominantly events are absorbed into them. Now the history of science has seen, at times, dramatic theoretical movement. Normal science, however, is within a paradigm (see Kuhn: 1962) and experimentation and observation take place within a framework the basis of which is assumed to be true. All the propositions in this sense cannot be regarded as having probationary status, for at a certain time science works and must work, assuming a certain type of world to exist. Toulmin in his excellent Philosophy of Science (1953) has stressed this feature that we are not forever testing a whole system but rather accepting some of it and concentrating upon particular propositions which are meaningful only when the rest of the system is accepted. Now the problem is this: if science comes in systems and we have a phenomenon which is recalcitrant to explanation in its terms, where precisely is the failure of correspondence to be located? Where in the whole framework does the fault lie? At which level, therefore, must theoretical revision take place? We may therefore accept Toulmin's point, but in one sense each problem places the whole system in doubt. Now experimental evidence may be discounted, or minor adjustment may be adequate. But it may be decided that it is the basic framework itself (i.e. the conception of reality) that is wrong.

Now in all these decisions logic does not act alone. There are conflicting evaluations and interpretations and social factors may be of primary importance. It is easy to be naive in this matter of the sociology of science, but science is a social activity and we must be aware of its socio-historical context. After all it is scientists rather than theories that come into conflict. Generalisation about the role of social factors here would be foolish, we must go in each particular case to the relevant historical context, and in this sense scientific method can only be seen as the whole history of science. But social factors were undoubtedly responsible for the rise of the natural sciences and we must expect them to play a part in the rise and fall of particular theories also.

I shall now return to social anthropology by discussing the philosophy to be found in Pearson's 'Grammar of Science'. Pearson heavily influenced Radcliffe-Brown in certain ways, he belongs to the same age as Frazer, and therefore to read his work is of incomparable value in understanding the underlying philosophical assumptions of that age as well as the scientism of Radcliffe-Brown's.

Pearson does not accept that science is about the world or even that it should be a set of guesses at what there is in reality; assumptions most practising scientist make. For Pearson, science is about sensory experience. The term knowledge has meaning only in the realm of sensation and no sense, when applied to a realm beyond. Science he saw as gradually freeing itself of philosophy. Such a science is descriptive not explanatory in any real sense; it relates: 'solely to the special products of (men's) perceptive faculty -' (1892:19). The general concepts in science are associations of stored an immediate sense impressions and a law is no more than an economical resume of sensory experience, substituting for a more lengthy description. This is central to his and to all forms of positivism, the idea that there is no more content in a theoretical proposition than in a descriptive one. Thus to explain a chemical reaction in terms of atomic rearrangement says no more than an ordinary common sense description of what is observed in a test tube. Now this characterisation of science and this view of theoretical terms is simply false - and it must be so for if

metaphysics is declared nonsense we simply cannot give an adequate
account of the nature of scientific conceptual systems which are
always intimately bound up with philosophical foundations.

For us in social anthropology his idea, that knowledge derives
from experience is of central interest. For him, ideas were
associations of sense impressions and this was a part of that
psychology which underlay the intellectualism of our anthropological
forebears. Associationism we now know to be grossely inadequate,
but it explains why for the Victorian anthropologist the primitive
inhabited and experienced the same universe as himself but simply
reasoned incorrectly about it. Pearson says this: '-the physical
instruments of thought in two normal human beings are machines of
the same type, varying indeed in efficiency, but not in kind or
function. For — two normal human beings the organs of sense are
also machines of the same type and thus within limits only capable
of carrying the same sense impressions to the brain. Herein lies
the similarity of the universe for all-' (1892:57). And of course
we can also recognise in the vision of a pure science freeing
itself from philosophy exactly Frazor's notion of the progress of
the human mind from religion through metaphysics to science.
Perhaps for many of these Victorians this vision was intimately
connect.d with personal experience. Like so many, Frazor in his
own lifetime emerged a mature adult having abandoned the faith of
his childhood. The history of mankind was supposed to progress in
the same way. But rather from this religious crisis be plunged in
to doubt about all beliefs, these men made confident claims to
rationality (dogmatic perhaps because the notion of rationality may
not be rational) which explains also perhaps why they could so easily
and unreasonably attribute irrationality to others—both in their own
culture but especially in others.

Exposure and Social Anthropology.

What has gone before has profound implications for social
anthropology on its own account, but I shall end tis paper by
briefly discussing some specific topics in our discipline. I shall
not discuss functionalism as to criticise a theory at least implies
some respect, which functionalism does not deserve. Closely connected
with fieldwork, it seems to have been little more than a way of
transforming notebooks into monographs with a minimum of th ught.
At a formal level it is easily assailable, but I shall make only
one point. A theory of interdependence can only be tested by
evidence of concomitant variation over time. Yet the functional
theory was introduced precisely because it was contended that
historical data on primitive communities was lacking. Many theories
are difficult to verify but few have been introduced on the grounds
of the absence of the only type of evidence that could be used for
verification.

Instead I shall look at the work of Radcliffe-Brown since in
some form his ideas and approach are still acceptable to many.
Firstly, his idea of a natural science as expounded at his seminars
at Chicago in 1937. Science is essentially a method and according
to Pearson it comprises the study of groups of facts which are
classified and from which general principles are drawn by systematic
comparison. Now there are other opinions expressed in Radcliffe-
Brown's work but this taxonomic-inductive view is basic (see:1957)
He would have wholeheartedly agreed with Pearson that: ' the
classification of facts and the foundation of absolute judgments
upon the basis of the classification — is the scope of modern
science'.(1892:7). The more so as Pearson claimed this method as
applicable to social as well as to physical phenomena. So the
only way to kn wledge is the laborious study of sets of phenomena
among which sequences and coexistences are to be recognized. Now
this view of scientific method is erroneous (deriving indeed from

philosophers rather than from anyone with a working knowledge of actual science) and if we are to establish a natural science of society, such ignorance of Radcliffe-Brown's part is deplorable.

Radcliffe-Brown was reciving his own training at a time when the physical sciences wore undergoing profound changes. But he seems not to have been at all effected, and consequently was able to mislead a great number of those he trained. If we __must__ look to the natural sciences, why to a Newtonian system when even a casual acquintance with quantum mechanics, for instance, would suggest this as a much more useful source of ideas. His models, in fact, never seem to have been greatly modified. Let us take only the organic analogy which is explicit in his thought. I am not attacking the use of analogy; this type of comparison is basic in our thinking. Talking about the unknown in terms of the known at least provides a language, and of course, analogue models are passed between the exact sciences themselves. Now the organic model came to us from biology, but it came earlier to physiology itself as a model from classical physics and its associated technology. But these machines of the early industrial revolution have long been superceeded by ones to be understood in terms of information and organization. Are we still to think of societies in terms of structure and function when the original source of our model now provides ideas which would appear to be more appropriate? 'Biology, like physics has ceased to be matorialistic. Its basic unit is a non-material entity, namely organization'.(Young:1960:136). If we want to look for biological or mechanical analogies why with those which a little familiarity with the sciences themselves would tell us are outdated?

On another point, facts for Radcliffe-Brown are the starting points, and social structure, a network of actually existing social relations (Radcliffe-Brown:1940) is equally real. This is not an inconsequential standpoint; for instance, it makes for British social anthropologists raised in this positivist tradition an understanding of the alliance theory of marriage that much more difficult. Alliance theory concerns the exchange of women between the categories of an ideal model of the social order, and actual practise may be considerably different. But it is no criticism of the theory to point to, for instance, the statistical infrequency of that type of marriage in terms of which the social structure may be conceived. This distinction between normative exchange and actual behaviour must be difficult to grasp and its significance difficult to realize if it is suggested that social structure is 'real'. To have defined social structure as a network of behaviour rather than a system of rules influences the way ethnography is analysed, and though his work on kinship is generally praised, Radcliffe-Brown's attitude to structure which is vulgarly positivistic leads him, I feel, to a fundamental misconception of the nature of kinship.

Next I shall briefly consider social change. All theoretical frameworks generate certain problems and we must be aware of those issues with which a certain type of model cannot deal. At the same time it must be remembered that those problems a model does generate receive their definition from the theoretical framework and that they might be better approached in different terms. Now I do not deny that there is a phenomenon to which the label social change attaches itself, but it exists as a separate area of concern in our discipline simply as a problematical precipitate of the view of society as a functional-equilibrium system, and is no more real than that. Another view, for instance, that society is a historical process, makes it difficult to define what change is that processes over time in general are not; this tends to eliminate social change as a particular problem area. This is to say that functionalist social anthropologists have not been dealing with a phenomenon which exists in its own right but one which arises awkwardly from their own

theoretical assumptions. Change can be better dealt with in other
ways, or perhaps better eliminated altogether.

On fieldwork I have only this to say. Jarvie (1964) argues
that fieldwork is part of a baconian inductive tradition, which
is an erroneous view of scientific method. But from advocating a
conjectural view of science he proceeds to bring into question
the necessity of fieldwork, suggesting, in Gellner's terms, that
it is merely a 'ritual'. This is irresponsible and as elsewhere in
this book he is content to allow cliché to be substituted for thought.
Jarvie is quite right to criticise traditional fieldwork but it was
never inductive in the sense of being theory-free; rather the
theory was of a poor kind. If fieldwork is theoretical it needs
to be consciously theoretical, problems have to be specified, (here
also Jarvie is correct). Anthropology is nothing without its field
tradition and we do not need its value to be called into question;
rather we need a new and more intelligent and sensitive type of
fieldwork.

I conclude with the type of problem with which I began; what is
anthropology about? Without suggesting any definition I shall simply
indicate one area in which the anthropologist can profitably engage
himself. We have seen the beginnings of a trend for anthropologists
to do research in complex societies, and now that the political
context of our discipline has changed the term primitive would seem
to be of no value. We may therefore reject the savage/civilised
opposition and see all forms of social life as being legitimate
objects of study. The time is right to introduce ourselves into our
subject. In this sense we may reverse a comment that Lévi-Strauss
made in his inaugural lecture at the College do France in 1960
(publ.1967). He suggests that only a study of primitive societies
can assign to human facts their true dimensions. The position for
an anthropologist now is surely this; the full dimensions of human
facts are realisable only when he includes in anthropology his own
culture. We are conscious of the consequences of this omission in
the past. Evans-Pritchard in his wonderful 1934 essay on Lévy-Bruhl
(reprinted 1970) quite rightly complains that though working with
such notions as primitive/civilized or pre-logical/logical in his
generalizations about thought, he nowhere stops to consider the
common sense of his own society. But now our attention has returned
to meaning this inclusion would seem to be essential. In talking
about the human mind we have an advantage over the philosopher,
our much wider comparative basis. The professional philosopher will
for the most be familiar only with the thought of a limited group
of linguistically and historically related cultures. But our advantage
is sacrificed if, despite our familiarity with the thought of so many
primitive peoples, we systematically exclude the thought of our own
scientific cultures from our competence. Science through technology
is intimately connected with the rest of the social system but that
apart, scientific thought has been one feature by which many have
attempted to distance ourselves from the savage. Can we really
make such pronouncements without embarrassment if we do nothing
to find out what science actually is, if we remain unconversant
with its contemporary practise and philosophy?

It may be argued that scientific thought is too close to us,
and that if anthropology deals with anything it deals with remoteness.
Now there are both geographical and historical distances. The
Victorian, in a sense, did not make any distinction for to travel
to an exotic culture was to travel through time also to meet one's
contemporary ancestors Rightly we no longer make this equation.
But the two types of remoteness separately constitute valid areas
for anthropological enquiry. We have contemporary cultures both
industrial and pre-industrial, but no less we have that distance
in our own culture that the time dimension provides. This is to say

that the alchemists are just as much in our field as are the Nuer.
Through this other dimension we have forms of society historically
related to our own, but we can also deal with that history of
scientific thought, which has evolved into our present world-view.
This new direction focuses our attention on conceptual systems to
which we can relate ourselves but from which we are also remote.
We would still be dealing with alien modes of discourse and the
sociology of other forms of cultural life - a legitimate province
for the social anthropologist.

Malcolm Crick.

(1) This essay is a revised version of a paper read at a seminar
in Oxford during the Michaelmas Term 1970. I should like to express
my gratitude to Mr.Harré, lecturer in the philosophy of science,
for reading through a preliminary draft and for a stimulating course
of lectures which did much to structure some of the views here
expressed.

Bibliography.

Ardener, E.&S.	1965	'A Directory Study of Social Anthropologists' B.J.S. XVI.
Cassirer	1944	An Essay on Man.
Charbonnier	1961	Entretiens avec Claude Lévi-Strauss.
Douglas	1966	Purity and Danger.
Evans-Pritchard	1937	Witchcraft, Oracles and Magic among the Azande.
"	1970	'Levy-Bruhl's Theory of Primitive Mentality'. Journ.of Anthrop.Soc.of Oxford,I.No.II.
Fortes	1953	'Social Anthropology at Cambridge since 1900'.
Gellner	1959	Words and Things.
Gluckman. ed.	1964	Closed Systems and Open Minds.
Hanson,F.A.	1970	'Understanding in Philosophical Anthropology'. J.A.S.O., I no.II.
Hanson,N.R.	1958	Patterns of Discovery.
Harré	1964	Matter and Method.
Heisenberg	1958	Physics and Philosophy.
Holzner	1968	Reality Construction in Society.
Horton	1967	'African Traditional Thought and Western Science'.,Africa, XXXVII.
Jarvie	1964	The Revolution in Anthropology.
Jeans	1934	'The New World-Picture of Modern Physics'.
Kuhn	1962	The Structure of Scientific Revolution.
Lane ed.	1970	Structuralism.
Langer	1944	Philosophy in a New Key.
Leach ed.	1967	The Structural Study of Myth and Totemism.
Lévi-Strauss	1967	The Scope of Anthropology.
Medawar	1961	The Art of the Soluble.
"	1969	Induction and Intuition in Scientific Thought.
Murdock	1951	'British Social Anthropology'.Amer.Anthr.LIII.
Pearson	1892	The Grammar of Science.
Polanyi	1957	Personal Knowledge.
Popper	1963	Conjectures and Refutations.
Purver	1967	The Royal Society: Concept and Creation.
Radcliffe-Brown	1940	'On Social Structure'.J.R.A.I. LXX.
"	1957	A Natural Science of Society.
Santayana	1916	Egotism in German Philosophy.
Stark	1958	The Sociology of Knowledge.
Toulmin	1953	The Philosophy of Science.
Winch	1958	The Idea of a Social Science and its Relation to Philosophy.
Young	1960	Doubt and Certainty in Science.