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EDITORIAL NOTE

The idea for this Journal has come from the graduate students
at the Institute of Social Anthropology in Oxford. Papers given at
graduate seminars and ideas arising from work for diplomas and higher
degrees very often merit wider circulation and discussion without
necessarily being ready for formal publication in professional journals.
There obviously exists a need in social anthropology for serious
critical and theoretical discussion; JASO sees this as its main purpose.

The editors would like to express their thanks to John Hill,
Richard Heelas, and Stephen Heelas, who have helped with the production
of this issue.

FORMAT

The journal is published three times per year. Articles are
welcome from students of anthropology and from people in other
disciplines. It is preferred that the main emphasis should be on
analytical discussion rather than on description or ethnography. Papers
should be as short as is necessary to get the point over. As a general
rule they should not exceed 5,000 words. They should follow the conven-
tions for citations, notes and references used in the A.S.A. monographs.
Comments will also be welcome. Communications should be addressed to
the Journal Editors, Institute of Social Anthropology, 51, Banbury Road,
Oxford.

BACK ISSUES

We have a stock of back issues. Single issues are available at
35p. in the U.K. and $1 abroad. Complete volumes (I (1970), II (1971),
III (1972) and IV (1973)) are each available at the following rates:
U.K. - £1.00 to individuals, £1.25 to institutions; abroad - $3.00 to
individuals, $3.50 to institutions. The subscription for Vol. V (1974)
is the same. (All prices cover postage). Cheques should be made out
to the Journal of the Anthropological Society of Oxford, and sent to the
Journal Editors at 51 Banbury Road, Oxford.

Science and Sentiment:

an Exposition and Criticism

of the Writings of Pareto*

This paper is a section of an Histoire des Doctrines of Primitive
Mentality. It aims at showing what in Pareto's work is directly
relevant to the methods and observations of Social Anthropology. It
is further the chronicle of an attempt by Pareto to apply to documents
about civilized peoples the same comparative analysis as was applied
to documents about savages in the great classics of social
anthropology, Primitive Culture, The Golden Bough, Les Fonctions
Mentales, etc. When we realise that Pareto reached the same
conclusions about 'civilized' behaviour as Lévy-Bruhl reached about
'savage' behaviour it will readily be granted that his writings
are of concern to anthropologists and that if the rigid division of
social studies into those that deal with civilized peoples and those
that deal with primitive peoples is to be maintained it can only
be as a temporary convenience.

In Vilfredo Pareto's vast Trattato di Sociologia Generale[1] over
a million words are devoted to an analysis of feelings and ideas.
The treatise is always amusing and is born of wide reading and
bitter irony. But Pareto must be classed as political philosopher
rather than sociologist. His were the brilliance and shallowness
of the polemicist and the popularizer of scientific method. Like so
many Italian students Pareto was a quarter of a century behind the
rest of the scientific world so that his constant jibes and jeers
at phantom enemies become tedious. It is surely unnecessary to
spend two thousand pages in controverting the opinions of philo-
sophers, priests and politicians. Moreover, Pareto was a plagiarist,
and a very foolish one. One might think that he was unaware of
contemporaneous sociological literature and such may indeed have
been the case. He does not mention the works of Durkheim, Freud,
and Lévy-Bruhl, to cite only three savants, though they had dealt
with the same problems of sentiments, rationalizations, and non-
logical thought, that he was enquiring into. Even if he was ignorant
of these works he certainly took many of his ideas, without due
recognition, from earlier writers whom he often repaid with abuse.
Of these I will mention only Bentham, Marx, Nietzsche, Le Bon,
James, Sorel, Comte, and Frazer. Many authors are held up to
derision because they use metaphysical terms, for Pareto throughout
his prolix and ill-aranged arguments makes much ado about remaining
in the scientific (logico-experimental) field. Nevertheless, he is

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often as open to criticism on this score as those whom he ridicules.[1]

It may be asked why we trouble to digest a book which is so bad. Pareto tried to solve a number of genuine problems and we can learn as much from his failure as from his success about the nature of the problems themselves and the terminological and methodological difficulties involved in an enquiry into them. The data he cites and his treatment of it also provide an illuminating commentary on the theories of a number of writers, especially on Lévy-Bruhl's theory of primitive mentality because both men were trying to classify types of thought and to discover their interrelations.

The treatise contains five major propositions.

1. There are sentiments ('residues') making for social stability ('group persistences') and sentiments making for social change ('instinct for combinations'). The study of these sentiments, of their persistence, distribution, and inter-relations, in individuals and groups, is the whole subject-matter of sociology.

2. Sentiments are expressed not only in behaviour but also in ideologies ('derivations'). These are of very little social importance compared with sentiments and the only point in studying them is to discover the sentiments they both express and conceal.

3. Individuals are biologically heterogeneous. In any society a few are superior ('élites') to the rest and are the natural leaders of a community.

4. The form and durability of a society depends on (a) the distribution and mobility of these superior persons in the social hierarchy, and (b) the proportion of individuals in each class who are mainly motivated by sentiments that make for stability ('rentier' type) or by sentiments that make for change ('speculator' type).

5. There are alternating periods of change and stability due to variation in the number of biologically superior persons in the classes ('circulation of élites') and to the proportion of rentier and

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1. This is well noted by Dr. Franz Borkenau (_Pareto_, London 1936). He is easily the best critical commentator on the _Trattato_. A good critical account is Sorokin's section on Pareto in his _Contemporary Sociological Theories_. (New York and London, 1928). A useful precis and exposition is contained in G.C. Homans and C.P. Curtis, _An Introduction to Pareto. His Sociology_. New York, 1934, and G.H. Bousquet's _Précis de Sociologie d'après Vilfredo Pareto_ (Paris, 1925) and his _Vilfredo Pareto, Sa Vie et son Oeuvre_ (Paris 1928). L.J. Henderson's _Pareto's General Sociology. A Physiologist's Interpretation_ (Cambridge, U.S.A., 1935) has little merit. Some of the comments made by Arthur Livingston, editor and part translator of the English edition, _The Mind and Society_, will be found useful even though he speaks as if he were the exponent of a new religion. A fuller bibliography will be found in Sorokin's book and in Bousquet's biography.

speculator types in the governing class ('distribution of residues').
The first two propositions are more directly relevant to a study of
primitive mentality than the others.

There are six classes of residues: (1) Instinct for combinations
(2) Group-persistences (persistence of aggregates) (3) Need of
expressing sentiments by external acts (activity, self-expression)
(4) Residues connected with sociality (5) Integrity of the
individual and his appurtenances (6) The sex residue.

Most actions that are expressions of these residues are
non-logical in character and are rigidly distinguished by Pareto
from logical actions which derive from and are controlled by,
experience. Pareto includes thought (speech-reactions) as well
as behaviour in his concept of 'actions'. Logico-experimental
thought depends on facts and not the facts on it and its
principles are rejected as soon as it is found that they do not
square with the facts. They assert experimental uniformities.
Non-logico-experimental theories are accepted a priori and
dictate to experience. They do not depend on the facts but
the facts depend on them. If they clash with experience,
in which term Pareto includes both observation and experiment,
arguments are evoked to re-establish the accord. Logical actions
derive mainly from processes of reasoning while non-logical
actions derive mainly from sentiments. Logical actions are
found connected with arts, sciences, economics and in military,
legal and political operations. In other social processes
non-logical actions predominate.

The test between logical and non-logical actions is
whether their subjective purpose accords with their objective
results, i.e. whether means are adapted to ends. A logical
proposition is demonstrable by observation and experiment.
The sole judge of the logico-experimental value of a notion of
action is modern science.

Pareto quotes Hesiod "Do not make water at the mouth of
a river emptying into the sea, nor into a spring. You must
avoid that. Do not lighten your bowels there, for it is not
good to do so."[1] Both of these injunctions are non-logical
actions. The precept not to befoul drinking water has an objective
result, probably unknown to Hesiod, but no subjective purpose.
The precept not to befoul rivers at their mouths has neither
objective result nor subjective purpose. The precepts belong
to Class II Genus 3 and Class II Genus 1 in Pareto's synoptic scheme
of classification.[2]

GENERA AND SPECIES,   HAVE THE ACTIONS LOGICAL ENDS AND PURPOSES.

Objectively?   Subjectively?

Class 1. Logical Actions.

(The objective and the subjective purpose are identical)

Yes   Yes

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1.   The Mind and Society, p.79.

2.   Idem, p.78.

Class 2. Non-Logical Actions.

(The objective end differs from the subjective Purpose).

Genus 1	No	No
Genus 2	No	Yes
Genus 3	Yes	No
Genus 4	Yes	Yes

SPECIES OF THE GENERA 3 AND 4

3a, 4a, The objective end would be accepted by the subject if he knew it.

3b, 4b, The objective end would be rejected by the subject if he knew it.

"The ends and purposes in question are immediate ends and purposes. We choose to disregard the indirect. The objective end is a real one, located within the field of observation and experience, and not an imaginary end, located outside that field. An imaginary end may, on the other hand, constitute a subjective purpose[1]".

If there is no real end then an action or proposition cannot very well be judged by reference to scientific values because it lies outside the logico-experimental field where alone science can operate, e.g. "When St. Thomas (Aquinas) asserts that angel speaks to angel, he sets up a relation betweenthings about which the person keeping strictly to experience can say nothing. The case is the same when the argument is elaborated logically and one or more inferences are drawn. St Thomas is not content with his mere assertion; he is eager to prove it, and says: 'Since one angel can express to another angel the concept in his mind, and since the person who has a concept in his mind can express it to another at will, it follows that one angel may speak to another'. Experimental science can find no fault with the argument. It lies altogether outside its province[2]".

Pareto is aware of the fact that from the standpoint of formal logic the validity of premises is irrelevant, all that is required being sound reasoning from the premises. However, he chooses to speak of thought and action as logical when they are in accord with reality and are adapted to the end at which they aim and as non-logical when they are not, from the point of view of science, in such accord nor so adapted.

"Every social phenomenon may be considered under two aspects: as it is in reality, and as it presents itself to the mind of this or that human being. The first aspect we shall call objective, the second subjective. Such a division is necessary for we cannot put

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1. Idem, p. 78.

2. Idem, p. 289.

in one same class the operations performed by a chemist in his
laboratory and the operations performed by a person practising
magic; the conduct of Greek sailors in plying their oars to drive
their ship over the water and the sacrifices they offered to Poseidon
to make sure of a safe and rapid voyage. In Rome the Laws of
the XII Tables punished anyone casting a spell on a harvest. We
choose to distinguish such an act from the act of burning a field
of grain.

"We must not be misled by the names we give to the two
classes. In reality both are subjective, for all human knowledge is
subjective. They are to be distinguished not so much by any
difference in nature as in view of the greater or lesser fund of
factual knowledge that we ourselves have. We know, or think we
know, that sacrifices to Poseidon have no effect whatsoever upon
a voyage. We therefore distinguish them from other acts which
(to our best knowledge, at least) are capable of having such an
effect. If at some future time we were to discover that we have
been mistaken, that sacrifices to Poseidon are very influential in
securing a favourable voyage, we should have to reclassify them
with actions capable of such influence. All that of course is pleo-
nastic. It amounts to saying that when a person makes a
classification, he does so according to the knowledge he has. One
cannot imagine how things could be otherwise.

"There are actions that are means appropriate to ends and which
logically link means with ends. There are other actions in which
those traits are missing. The two sorts of conduct are very diff-
erent according as they are considered under their objective or
their subjective aspect. From the subjective point of view nearly
all human actions belong to the logical class. In the eyes of the
Greek mariners sacrifices to Poseidon and rowing with oars were
equally logical means of navigation. To avoid verbosities which
could only prove annoying, we had better give names to these
types of conduct. Suppose we apply the term logical actions to
actions that logically conjoin means to ends not only from the
standpoint of the subject performing them, but from the standpoint
of other persons who have a more extensive knowledge - in other
words, to actions that are logical both subjectively and objectively
in the sense just explained. Other actions we shall call non-logical
(by no means the same as 'illogical')¹".

Besides asking (1) whether a belief is scientifically valid
('objective aspect') we may also ask (2) why do certain individuals
assert the belief and others accept it ('subjective aspect') and (3)
what advantage or disadvantage has the belief for the person who-
states it, for the person who accepts it, and for society as a
whole ('aspect of utility'.) Like many other writers (Mill, James,
Vaihinger, Sorel, etc.), Pareto emphasizes that an objectively
valid belief may not be socially useful or have utility for the
individual who holds it. A doctrine which is absurd from the
logico-experimental standpoint may be socially beneficial and a
scientifically established doctrine may be detrimental to society.

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1. Idem, pp. 76-77.

Indeed Pareto states it as his aim to demonstrate "experimentally the individual and social utility of non-logical conduct[1]".

How does non-logical behaviour gain acceptance among people capable of logical behaviour? Why do people believe in foolish doctrines? Tylor and Frazer say it is because they reason erroneously from correct observations. Lévy-Bruhl says it is because they passively accept collective patterns of thought in the society into which they are born. According to Pareto the answer is found in their psychic states expressed in residues, the six classes of which have been ennumerated. As Pareto does not pay great attention to the last four classes of residues we will transcribe the subdivisions of the first two classes only.

CLASS I.

INSTINCT FOR COMBINATIONS.

1 a.   Generic combinations.

1 b.   Combinations of similars or opposites.

    1 b (1)   Generic likeness or oppositeness.

    1 b (2)   Unusual things and exceptional occurrences.

    1 b (3)   Objects and occurrences inspiring awe or terror.

    1 b (4)   Felicitous state associated with good things; infelicitous state, with bad.

    1 b (5)   Assimilation: physical consumption of substances to get effects of associable, and more rarely of opposite, character.

    1 c   Mysterious workings of certain things; mysterious effects of certain acts.

    1 c (1)   Mysterious operations in general.

    1 c (2)   Mysterious linkings of names and things.

    1 d   Need for combining residues.

    1 e   Need for logical developments.

    1 f   Faith in the efficacy of combinations.

CLASS II.

GROUP - PERSISTENCES (PERSISTENCE OF AGGREGATES)

    11 a   Persistence of relations between a person and other persons and places.

    11 a (1)   Relationships of family and kindred groups.

    11 a (2)   Relations with places.

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1.   Idem, p. 35. See also Pareto's *Le mythe vertuiste et la littérature immorale*, Paris, 1911.

11 a (3)	Relationships of social class.
11 b	Persistence of relations between the living and the dead.
11 c	Persistence of relations between a dead person and the things that belonged to him in life.
11 d	Persistence of abstractions.
11 e	Persistence of uniformities.
11 f	Sentiments transformed into objective realities.
11 g	Personifications.
11 h	Need of new abstractions.[1]

This classification will strike the reader as being a number of arbitrary, haphazard, categories, but it is only fair to an author to try to discover the meaning behind his words so we will give one illustration of each sub-division of Class 1 and seek in these for an interpretation of 'residues'.

1 a

Generic Combinations. Example: Pliny gives as remedies for epilepsy "bears' testicles, wild boars' testicles, wild boars' urine (which is more effective when allowed to evaporate in the animals' bladder); hog's testicles dried, triturated, and beaten in sow's milk; hares' lungs taken with frankincense and white wine[2]". (This residue comprises those magical associations of which Tylor says that they either never had rational sense or if they once had rational sense it has been forgotten, i.e. we can perceive no ideal link between the diseases and the drugs intended to cure them).

1 b (1)

Combinations of similars or opposites; Generic likeness or oppositeness. These are the principles of similia similibus curantur and contraria contrariis. Example: The witch in Theocritus says "Delphis (her lover) has tormented me. A laurel-branch I burn upon Delphis. Even as this crackles aloud when it is kindled, and burns in a flash so that not even its ashes do we see, so may the flesh of Delphis be consumed by the fire.......Even as I melt the wax with the help of a God, so may Delphis the Myndian be likewise melted with love; and as I turn this rhomb of bronze, so may he (Delphis) be turned by Aphrodite towards my threshold[3]".

(This residue comprises associations of ideas in magic of which Tylor wrote and which are analysed at length by Frazer and classed by him as ('Homoephathic magic').

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1. Idem, pp. 516-517.

2. Idem, p. 522.

3. Idem, p. 533.

1 b (2).

Unusual things and exceptional occurrences. Example: Suetonicus records that, "Once upon a time a thunderbolt fell on the walls of Velitrae (Vellitri), and that incident was taken as a presage that a citizen of that city was to hold supreme power. Strong in that faith the Velitrians made war on the Romans, but with little success. 'Not till years later did it become manifest that the presage had foretold the advent of Augustus,' who came of a family of Velitrae[1]". (This residue includes omens and portents).

1 b (3).

Objects and occurrences inspiring awe and terror. Example: "This residue appears almost always by itself in certain situations of which the following is typical. Speaking of the Cataline affair, Sallust relates, Bellum Catilinae, XXII: 'There were those at the time who said that after Catiline had finished his address he pressed his comrades in crime to take an oath, and passed around bowls of human blood mixed with wine, whereof after they all had tasted, with imprecations upon traitors, as is the custom in solemn sacrifices, he made known his design to them, saying that he had done as he had to the end that each having such a great-crime to the charge of the other, they would be the less likely to betray one another. Some hold that these and many other stories were invented by certain individuals who thought to mitigate the unpopularity that later arose against Cicero by stressing the enormity of the crime of the men who had been punished'.

"Whether this story be true or a fabrication, the fact of the association of two terrible things remains: a drinking of human blood and a conspiracy to destroy the Roman Republic[2]".

1 b (4).

Felicitous state associated with good things; infelicitous state, with bad. Example: "The ancient Romans credited the gods with the successes of their republic. Modern peoples attribute their economic betterment to corrupt, ignorant, altogether contemptible parliaments. Under the old monarchy in France the king partook of the divine. When something bad occurred, people said: 'if the King only knew'. Now the republic and universal suffrage are the divinities, 'Universal suffrage, the master of us all'. Such the slogan of our Deputies and Senators who are elected by the votes of people who believe in the dogma, "Ni Dieu, Ni Maître!"[3]

1 b (5).

Assimilation. Physical consumption of substances to get effects of associable, and more rarely of opposite, character. Example: "In view of and considering the strength, courage, and fleetness of foot of Achilles, some were pleased to assume that in his childhood he had been fed on marrow from the bones of lions and others specified bear's marrow and the viscera of lions and of wild boars[4]". (This residue

1. Idem, p.541.
2. Idem, pp. 552-553.
3. Idem, p. 558.
4. Idem, p. 561.

corresponds to Frazer's category of "Contagious magic").

1 c.

Mysterious workings of certain things: mysterious effects of certain acts. This residue figures in amulets, oaths, ordeals, and taboos.

1 c. (1).

Mysterious operations in general. Example:"According to Tertullian "Among the heathen there is a dreadful thing called the fascinum",'the spell', which comes as the unfortunate result of excessive praise and glory. This we sometimes believe to be the work of the Devil, because he hates whatever is good, sometimes the work of God, for of Him comes judgement on pride in an exalting of the lowly and a humbling of the haughty[1]".

1 c. (2).

Mysterious linkings of names and things. Example: St. Augustine says "In a perfect number of days, to wit, in six, did God finish his handiwork[2]".

1 d.

Need for combining residues. Example: "The human being is loth to dissever faith from experience; he wants a completed whole free from discordant notes. For long centuries Christians believed that their scriptures contained nothing at variance with historical or scientific experiences. Some of them have now abandoned that opinion as regards the natural sciences but cling to it as regards history. Others are willing to drop the Bible as science and history, but insist on keeping at least its morality. Still others will have a much-desired accord, if not literally, at least allegorically, by dint of ingenious interpretations. The Moslems are convinced that all mankind can know is contained in the Koran. The authority of Homer was sovereign for the ancient Greeks. For certain Socialists the authority of Marx is, or at least was, just as supreme. No end of felicitous sentiments are harmonised in a melodious whole in the Holy Progress and the Holy Democracy of modern peoples[3]".

1 e.

Need for logical developments. "The demand for logic is satisfied by pseudo-logic as well as by rigorous logic. At bottom what people want is to think-it matters little whether the thinking be sound or fallacious. ...We should not forget that if this insistence on having causes at all costs, be they real or imaginary, has been responsible for many imaginary causes, it has also led

1. Idem, pp. 572-573.

2. Idem, p.586.

3. Idem, p.588-589.

to the discovery of real ones. As regards residues, experimental
science, theology, metaphysics, fatuous speculations as to the ori-
gins and the purposes of things, have a common point of depar-
ture: a resolve, namely, not to stop with the last known cause of
the known fact, but to go beyond it, argue from it, find or imagine
something beyond that limit. Savage peoples have no use for the
metaphysical speculations of civilized countries, but they are also
strangers to civilized scientific activity; and if one were to assert
that but for theology, and metaphysics experimental science would
not even exist, one could not easily be confuted. Those three
kinds of activity are probably manifestations of one same psychic
state, on the extinction of which they would vanish simultane-
ously[1]". Example: None is specially cited but the entire collection
of derivations afterwards enumerated by Pareto exemplify this
residue.

1 f.

   Faith in the efficacy of combinations. Example: "Speaking in
general, the ignorant man is guided by faith in the efficacy of com-
binations, a faith which is kept alive by the fact that many combina-
tions are really effective, but which none the less arises spontaneously
within him, as may be seen in the child that amuses itself by trying
the strangest combinations. The ignorant person distinguishes little
if at all between effective and ineffective combinations. He bets
on lottery numbers according to his dreams just as confidently as he
goes to the railroad station at the time designated in the time-table.
He thinks it quite as natural to consult the faith-curer or the quack
as to consult the most expert physician. Cato the Elder hands out
magical remedies and directions for farming with the same assurance[2]".

   Pareto does not consider that logical actions are to be distingui-
shed from non-logical actions on psychological grounds. "If a per-
son is convinced that to be sure of a good voyage he must sacrifice
to Poseidon and sail in a ship that does not leak, he will perform the
sacrifice and caulk his seams in exactly the same spirit[3]".

   It is not entirely clear what Pareto means by residues. Evidently
he knew very little psychology and preferred to be as vague as possible
at this end of his study. His critics and disciples do not enlighten
us about residues. Borkenau says that the concept has the qualities
of being unchangeable, meaningless, and unintelligible.[4] Sorokin says
that they are relatively constant "drives" which are neither instincts
nor sentiments. He compares them, among other things, to 'dispositions'
and 'complexes'[5]. Bousquet says that they are certain tendencies, certain
sentiments[6].

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1. Idem. pp. 530-591.

2. Idem. pp. 593-594.

3. Idem. p. 210.

4. Borkenau, op. cit., p. 48.

5. Sorokin, op. cit., p. 48.

6. Bousquet, op. cit., p. 135.

I interpret his writings like Homans and Curtis who describe
residues as the common element in "certain utterances and wri-
tings", as an abstraction from "the observed sayings of men". Ne-
vertheless, in their exposition they prefer to apply the term also
to certain hypothetical sentiments. They say "Strictly, they (resi-
dues) are not parts of a conceptual scheme, but uniformities ab-
stracted from the observed sayings of men. Common-sense, however,
has set up a conceptual scheme which in our habits of thought is
so closely joined with observations that it is inconvenient to
separate them. We all observe that we say and do certain things,
but we all feel as well that we have sentiments connected with
these sayings and doings. Therefore the word 'residues' will be
used to mean 'sentiments'. For it is not worth while to sacrifice
the directness of the language of common-sense for the sake of a
consistent rigour[1]".

Pareto himself often speaks of 'sentiment' instead of 'residue'.
In an address at Lausanne he said, "L'activité humaine a deux bran-
ches principales: celle du sentiment et celle des recherches expéri-
mentales. On ne saurait exagérer l'importance de la première. C'est
le sentiment qui pousse à l'action, qui donne la vie aux règles de
la morale, au dévouement, aux religions, sous toutes leurs formes
si complexes et si variées. C'est par l'aspiration à l'idéal que
subsistent et progressent les sociétés humaines. Mais la second branche
est aussi essentielle pour ces sociétés; elle fournit la matière que
met en oeuvre la première; nous lui devons les connaissances qui ren-
dent efficaces l'action et d'utiles modifications du sentiment, grâce
auxquelles il s'adapte peu à peu très lentement, il est vrai, aux
conditions de l'ambiant.

Toutes les sciences, les naturelles comme les sociales, ont eu, à
leur origine, un mélange de sentiments et d'expériences. Il a fallu
des siècles pour opérer une séparation de ces éléments, laquelle, à
notre époque, est presque entièrement accomplie pour les sciences
naturelles et qui a commencé et se poursuit pour les sciences so-
ciales[2]".

But Pareto uses the word 'sentiment' only as a useful concept
and not as something which can be observed. Though he often
speaks of sentiments and residues as though they were interchan-
geable terms in his scheme they strictly refer to quite distinct
things. We observe that men act in certain ways in certain situa-
tions and we find that there is a common factor in their beha-
viour. This constant element in the behaviour-patterns is the resi-
due and is the important variable in a complex of real behaviour.
What is inconstant are the derivations which are the unimportant variable
in the complex. The residues and the derivations are therefore
observed facts and the sentiment is a conceptualization of the facts,
i.e. is the facts translated into a system of ideas.

We can best understand Pareto's scheme by quoting examples.
We see that certain insects (Eumenes and Cerops) prepare a food
supply for their worms and that all members of these species pre-
pare it in very much the same way. What is variable in their beha-
viour is a derivation.[3] What is common to all insects of the species

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1. Homans and Curtis, _An Introduction to Pareto. His Sociology_.pp.87-89.

2. Journal d'Economie Politique, 1917, pages 426 seq. (Quoted as appendix
   to Homans and Curtis, An Introduction to Pareto. His Sociology.

3. To Conform to Paretran usage one should say it is analogous to derivations
   because he does not consider derivations to occur in animal behaviour.

is a residue; i.e. what remains after all variations have been abstra-
cted (it is the purest type of non-logical action and belongs to Genus
3 in Pareto's synoptic scheme). We conceptualize this common
behaviour and call it instinct. We do this for our own convenience.
Similarly it is convenient to speak of an instinct for nestbuilding
in birds since it obstructs thought if we have always to describe
the whole range of like behaviour for which the word 'instinct'
stands in a conceptual scheme. Those who do not like the word
may substitute for it the behaviour. When we sepak of instinct we
make no statement about the psychophysiological action that may
accompany, or even cause, behaviour, but we speak of observed
behaviour alone.

The word 'sentiment' is used in the same manner. A residue
is what is constant in a range of behaviour, i.e. it is a constant
uniformity. An observer notes that in England people react in
certain situations to certain symbols such as 'King' and 'Union
Jack'. He abstracts from their behaviour what is constant in
individuals and ceremonies. This is the residue. It is a pure
abstraction because it will not be observed except in combination
with the variable elements in real behaviour but it is observable
behaviour none the less. For sake of convenience we refer to the
residue as the 'sentiment of patriotism' and we say that the beha-
viour both expresses and strengthens the sentiment. This hypo-
thetical entity denotes a psychological state and therefore may not
refer to anything observable and describable but it is useful because
it enables us to relate a great number of facts to one another in
the same way as the notion of gravitation enabled people to relate
falling apples, the motion of the planets, and many other observa-
tions, to one another.

Pareto finds in his survey of literature that in many countries
and times when a storm arises at sea people do something to
quell it. They may make magic, or pray to the Gods, or do some-
thing else. Exactly what they do is, from his point of view, irre-
levant. That they feel something can be done to quiet the storm
and that they do this something, are the important facts. Men have
always feasted but many different reasons are given for their banquets.
"Banquets in honour of the dead become banquets in honour of the Gods, and
then again banquets in honour of saints; and then finally they go back and
become merely commemorative banquets again. Forms can be changed, but it
is much more difficult to suppress the banquets. Briefly (and therefore not
very exactly) one might say that a religious custom or a custom of that
general character offers a less resistence to change, the farther
removed it stands from its residues in simple associations of ideas and
acts, and the larger proportion it contains of theological, metaphysical,
or logical concepts[1]"The banquet is the residue; the reason for holding
it is the derivation. But it is no special kind of banquet but simply
the act of banqueting at all times and in all places that is Pareto's
residue.

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1.   Pareto, _The Mind and Society_, p. 607.

I will give two final examples to illustrate Pareto's use of 'residue' and 'sentiment'.

CONCEPTUAL PLANE   OBSERVATIONAL PLANE.

   Sentiment   Residue   Real Behaviour.

                                          abc.

   A _____ a _____ ade.

                                          afg.

                                        ahi.

Let us take a hypothetical African people who in drought perform a ceremony to make rain. Their rites are abc. Christian missionaries convert them and now when they want to make rain they go into a church and the minister prays for rain. These Christian rites = ade. The people, however, become converted to Islam and adopt new rites to obtain rain, namely afg. Later they relapse into paganism again but having forgotten their ancient rites of rain-making borrow those of a neighbouring people, namely ahi. When we compare all these rites we find they have a common element, a, in that in the situation of drought a ceremony is held to obtain rain, and there may be common elements in the rites themselves e.g. prayers to a Divinity and so forth. However, in real situations these common elements are always found with the other and variable elements. The residue is an abstraction from these real situations. Those who find that it helps them to understand the facts better by saying that this African people have a social sentiment, A, in regard to rain and account for the constant behaviour they observe by attributing it to the sentiment are in no danger so long as they realise what they are doing, i.e. that they are merely conceptualizing the residue.

We need not have taken an hypothetical African tribe. Let us take abc = Christianity, ade = Islam, afg = Hinduism, ahi = Christian science. The theologies and rites of these religions are very different. Let us consider only one element in the complex, namely, moral conduct. All these religions condemn adultery, theft, murder, incest, etc., and the peoples in those societies where the religions hold sway express horror at the breach of the moral code; the great majority observing it and punishing those who break it. Conduct is constant and uniform. Only the reasons given for the conduct and the sanctions which are associated with it differ in many particulars. This is an observable fact. Those who like to conceptualize it by referring to religious sentiments are at liberty to do so.

From what has been said about 'residues' the meaning Pareto attaches to 'derivations' will be apparent. Strictly derivations are relatively inconstant elements in a range of behaviour. In the above diagram they are b, c, d, e, f, g, h, i. But Pareto generally uses the term to denote what are often called ideologies or speech-reactions, i.e. the reasons men give for doing things. He thus contrasts the sentiment and the action which expresses it with the explanation men advance to justify their action. He recognizes, however, that sentiments are expressed in both action and ideologies

because men not only have a need for action but also a need for
intellectualizing their actions, though whether by sound or absurd
arguments matters little. What is done and said need have no di-
rect relation to a sentiment but they satisfy needs for action and
intellectual justification.

Pareto saw the sentiment, the behaviour, and the ideology as exi-
sting in a functional relationship. The behaviour and the ideology
are derivatives of the sentiment and of these two the relatively con-
stant behaviour is the more important variable.

Above all Pareto objected, like Durkheim, Lévy-Bruhl, and many
other writers, to theories that interpret behaviour by reference to
the reasons that men give to explain it. He severely criticises Spen-
cer and Tylor for suggesting that primitive peoples argued logically
from observation of phenomena that souls and ghosts must
exist and that they instituted a cult of the dead in consequence of
their logical conclusions. Likewise he criticised Fustel de Coulan-
ges for saying that from the religion of the hearth human beings
learned to appropriate the soil and on their religion based their
title to the soil. Pareto remarks that religion and ownership of land
are likely to have developed side by side. Coulanges further said
that the family, which by religion and duty remained grouped around
its altar became fixed to the soil like the altar itself. Pareto com-
ments that what obviously happened was that certain people came
to live in separate families fixed to the soil and one of the mani-
festations of this mode of life was a certain kind of religion which
in its turn reacted on the mode of life and contributed towards
keeping the families separate and fixed to the soil. The relation-
ship is not a simple cause-effect relationship but one of reciprocal
interdependence. Family.life, cult, and system of beliefs, interact
on one another and strengthen one another.

Nevertheless, although ideologies may react on sentiments it is the
sentiments that are basic and durable. A particular ideology may
change but the sentiment that gave rise to it will remain and an
entirely different ideology may take the place of the previous one.
In fact the same residue may give rise to apposed derivations, e.g. the
sex residue may be expressed in a violent hatred of all sex manifestations.
Therefore the derivations are always dependent on the residue and
not it on them. It is a one-sided functional relationship.

Hospitality is universal so that when the Greeks said that a man
must be hospitable to strangers because "Strangess and beggars
come from Zeus" we can leave Zeus out of consideration. The
Greeks "were merely voicing their inclination to be hospitable to
visitors, and Zeus was dragged in to give a logical colouring to
the custom, by implying that the hospitality was offered either in
reverence for Zeus, or to avoid the punishment that Zeus held in
store for violators of the precept"[1]. Other peoples give different
reasons for hospitality but all insist on the hospitality. The giving
of hospitality is the residue; the reason for giving it is the deriva-
tion. The feelings and the behaviour to which they give rise are

1. Op. cit. p. 215.

the important things. The reasons for the behaviour do not matter. Almost any reason will serve the purpose equally well, and, therefore even if a man can be convinced that his reasons for doing something he is very desirous of doing are erroneous he is unlikely to cease his action but will rather look for a new set of reasons to justify his conduct. Hence Pareto, unexpectedly, quotes Herbert Spencer with approval when he says that not ideas but feelings, to which ideas serve only as a guide, govern the world.

"Logically", Pareto wrote, "one ought first to believe in a given religion and then in the efficacy of its rites, the efficacy logically, being the consequence of the belief. Logically, it is absurd to offer a prayer unless there is someone to hearken to it. But nonlogical conduct is derived by a precisely reverse process. There is first an instinctive belief in the efficacy of a rite, then an 'explanation' of the belief is desired, then it is found in religion"[1].

In fact there are certain elementary types of behaviour, found in all societies, in similar situations, and directed towards similar objects. These, the residues, are relatively constant since they spring from strong sentiments. The exact manner in which the sentiments are expressed and the ideologies that accompany their expression are variable. Men in each society express them in the particular idiom of their culture. Logical interpretations especially "assume the forms that are most generally prevalent in the ages in which they are ›volved. These are comparable to the styles of costume worn by people in the periods corresponding"[2]. If we want to understand human beings therefore we must always get behind their ideas and study their behaviour and once we have understood that sentiments control behaviour it is not difficult for us to understand the actions of men in remote times because residues change little through centuries, even milleniums. How could we still enjoy the poems of Homer and the elegies, tragedies, and comedies of the Greeks and Latins if we did not find them expressing sentiments that, in great part at least, we share?[3] Pareto's conculsion may be summariaed in the dictum 'Human nature does not change', or, in his own words "Derivations vary, the residue endures"[4].

I will now note some comments on, and criticisms of, Pareto's theories about residues and derivations. In harmony with the diffuse and disjointed structure of the book I will not attempt general criticism but will isolate a number of points for remark. I select particularly those problems that are relevant to a study of primitive mentality.

(1) Pareto like Tylor, Fraaer, and Lévy-Bruhl, employed a faulty comparative method. He took beliefs from here, there and everywhere, and fitted them into his theoretical mosaic. What I have said elsewhere[5] in criticism of this way of writing applies also to Pareto's treatise.

1. Op. cit. p. 569.

2. Op. cit. p. 143.

3. Op. cit. p.1183.

4. Op. cit. p.660.

5. "The Intellectualist Interpretation of Magic" and "Lévy-Bruhl's theory of Primitive Mentality", Bulletin of the Faculty of Arts, Egyptian University 1933, 1934. /Reprinted in J.A.S.O. Vol.1V., No.3, Vol.1 No.2._7

(2) Pareto had this advantage over contemporary anthropologists: he did not have to rely on what travellers and missionaries said about savage superstitions. By restricting his field to classical and post-classical times in European countries he was able to use what the natives themselves said about their beliefs. His main texts were Greek and Roman and mediaeval books.

(3) He had a further advantage in that he was in a sense a fieldworker. It is true that he took all his data from books and newspapers and also that he made a study of ideologies rather than behaviour. But his life was not spent in the study and the walls of a college were not the limits of his experience. In his early life he had been a practical man of affairs and had learnt by observation that there are wide differences between what men say are their aims and what they really want to do. Nevertheless, he could, as a rule, only indirectly apply his observatins of human behaviour to interpret his data. Hesiod, Plato, Suetonious, and Aristotle, cannot be cross-examined and we cannot do field-work among the ancient Greeks.

(4) I have criticized Frazer, for comparing the scientist in modern Europe with the magician and priest in savage and barbarous societies, and Lévy-Bruhl for comparing the modes of thought of an educated European in the 20th century with the beliefs of primitive peoples. Pareto does not make this mistake. He intends to study the part played by logical, and the part played by non-logical, thought and behaviour side by side, and in interaction, in the same culture. His intention was excellent. In fact, however, he does not adhere to this plan. He writes at great length about fallacious beliefs and irrational behaviour but he tells us very little about common-sense beliefs and empirical behaviour. Therefore just as Lévy-Bruhl leaves us with the impression of savages who are continuously engaged in ritual and under the dominance of mystical beliefs so Pareto gives us a picture of Europeans at all periods of their history at the mercy of sentiments expressed in a vast variety of absurd notions and actions. If Pareto for civilized peoples, and Lévy-Bruhl for savages, had given us a detailed account of their real life during an ordinary day we would be able to judge whether their non-logical behaviour is as qualitatively and quantitatively important as the writers' selective methods would lead usto suppose. Actually, I would contend, non-logical conduct plays a relatively minor part in the behaviour of either primitive or civilized men and is relatively of minor importance.

(5) Pareto's work is an amusing commentary on Lévy-Bruhl books. Lévy-Bruhl has written several volumes to prove that savages are pre-logical in contrast to Europeans who are logical. Pareto has written several volumes to prove that Europeans are non-logical. It would therefore seem that no one is mainly controlled by reason anywhere or at any epoch. The situation is yet more amusing when we remember that Lévy-Bruhl excused himself from describing the characteristics of civilized mentality on the grounds that ancient and modern savants have adequately defined them. For Pareto bases his contention that civilized thought is primarily non-logical on the writings of these same savants.[1]

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1. Cp. J. L. Myres "The Methods of Magic and of Science", Folklore, vol. 36 1925. For his magical data Prof. Myres does not find it necessary to go further than the writings of his anthropological colleagues.

(6) Indeed one of the reasons why I have chosen to analyse Pareto's treatise is to bring out the fact that a study of unscientific thought and ritual behaviour cannot be restriced to primitive societies but must be extended to civilized societies also. He allows to common-sense notions and empirical behaviour about as much place in Greek, Roman, and modern European communities as Lévy-Bruhl allows them in central African, Chinese, and North American Indian communities. He admits that perhaps people are a little more reasonable than they used to be, but so little more that it is scarcely to be observed. Our diagram to illustrate Pareto's viewsis therefore thus:-

Later periods of history.
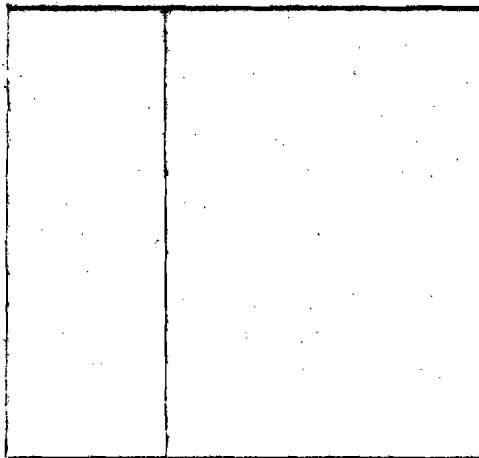
Earlier periods of history.

Logico-   Non-logico-
experimental.   experimental.

The relation of the logico-experimental field to the non-logico-experimental field is fairly constant throughout history. Also by his analysis of human behaviour and his classification of it into residual categories Pareto establishes sociological uniformities that may serve as units of comparison. If his analysis is correct it would seem disadvantageous to maintain studies of primitive societies and of civilized societies as separate disciplines as is the present scientific policy.

(7) Another reason why I devote so much space to a consideration of Pareto's writings is because he emphasizes the need for a clear distinction between logico-experimental thought and behaviour and other forms of thought and behaviour and in doing so raises questions of terminology which, had they been earlier considered, would have prevented much confusion in social anthropology. Pareto's division of thought into two categories, the logico-experimental and the non-logico-experimental is excellent and is necessary if we are going to investigate the part played by logico-experimental thought in society.

But it must be remembered, firstly, that our classificati n is never absolute since it is always relative to present-day knowledge, and, secondly, that it tells us nothing about the psychological and sociological qualities of the facts under investigation. It tells us only whether a proposition is valid, whether an inference from it is sound, and whether behaviour based upon it is adapted to the end towards which it is directed. It is possible that from the logico-

experimental view-point two propositions, A and B, may be placed
in opposite categories whereas from the psychological or socio-
logical view-points they may be placed in the same category.
Pareto understands that facts must be classified according to the
point of view of the observer and that the classification of one
observer will therefore be different from the classification of another
observer. Thus he points out that the logico-experimental and the
non-logico-experimental actions of Greek sailors are psychologically
the same.

But Pareto's terminology is not acceptable because his non-logi-
co-experimental category does not really tell us about the validity
of inferences from propositions but only about the validity of the
propositions themselves. Lévy-Bruhl saw that primitive thought is
coherent and that savages make valid inferences from propositions
even though the propositions are not in accord with experience
but are dictated by culture and are contained in beliefs that are
demonstrably false from a logico-experimental standpoint. It is
unfortunate, therefore, that he chose to speak of primitive notions
as prelogical because we then have to talk about prelogical logic
which is very inconvenient. Pareto, more clearly than Lévy-Bruhl,
has stated that human thought and actions are in logical accord
with propositions but when the propositions are invalid he calls
them non-logical. This creates an even worse terminological mud-
dle for we have then to speak of non-logical logic.

Lévy-Bruhl and Pareto both wanted to make the same point and
both used the same cumbrous terminology. In science the validity
of premises and the logical co-ordination of propositions are
everything and the scientist aims always and above all to test his
thought by observation and experiment and to avoid contradiction
between his propositions. Outside the field of science a man does
not trouble himself whether thought is based on observation and
experiment and is not seriously inconvenienced by contradictions be-
tween his propositions. He aims always and above all to ensure
that his notions and conduct shall be in accord with sentiments
and if he can achieve that end their scientific value, and to some
extent their logical value, are of little importance. A savage sees
an ill-omened bird and abandons his journey to avoid misfortune.
His conduct is in accord with a socially determined proposition.
He does not consider whether it is experimentally sound because
for him the experimental proof is contained in the proposition. A
train is wrecked. Some people at once say that communists have
wrecked it. That communists could not have been responsible
and that it would have been entirely against their interests to have
wrecked the train, are irrelevant to such people. They hate com-
munists. A train has been wrecked. Therefore the communists are
responsible.

Sentiments are superior to observation and experiment and dic-
tate to them everywhere save in the laboratories of science.
What have logico-experimental methods to do with the feelings of
a lover, a patriot, a father, a devout Christian, and a communist?
A lover is notoriously blind to what is evident to everyone else.
What is sense to a communist is nonsense to other people. For
in these realms our judgements are made to accord with sentiments
and not with observations.

Feelings cannot be logical or otherwise and sentiments are outside the domain of science. But when they are expressed in words the propositions can be classed from the point of view of formal logic into logical and illogical statements, and from the point of view of science into valid and invalid statements.

Logical reasoning may be unscientific since it is based on invalid premises. It is therefore desirable to distinguish between science and logic. Science is understood in the sense given to the word by most scientific writers on the subject, e.g. Mach, Pearson, and Poincaré. Scientific notions are those which accord with objective reality both with regard to the validity of their premises and to the inferences drawn from their propositions. Unscientific notions are those which are invalid either in their premises or in the inferences drawn from them. Logical notions are those in which according to the rules of thought inferences would be true were the premises true, the truth of the premises being irrelevant. Illogical notions are those in which inferences would not be true even were the premises true, the truth of the premises again being irrelevant.

Much confusion that has arisen by use of such terms as non-logical and pre-logical will be avoided by maintaining a distinction between logical and scientific. In making pots all grit must be removed from the clay or the pots will break. A pot has broken during firing. This is probably due to grit. Let us examine the pot and see if this is the cause. That is logical and scientific thought. Sickness is due to witchcraft. A man is sick. Let us consult the oracles to discover who is the witch responsible. That is logical and unscientific thought.

(8) Pareto makes his writing unnecessarily difficult to follow by speaking of actions as well as speech as logical and non-logical. What he means is that actions can be based on scientifically valid propositions or scientifically invalid propositions. If a man shoots another through the heart it stops beating and dies. Acting on this proposition A shoots B through the heart. This is what Pareto calls a logical action. If a man makes magic against another he dies. Acting on this proposition A makes lethal magic against B. This is what Pareto calls a non-logical action. It will be more convenient to call the one an experimental, and the other a non-experimental, action since the one is from the viewpoint of observer well adapted to achieve the end aimed at whereas the other is ill-adapted.

Problems of terminology become more difficult when we leave the technological plane and begin to discuss behaviour on the moral plane. In this review, however, we may use Pareto's device of contrasting experience with sentiment, science with morals, for I attempt only to expound Pareto's scheme and not to propound a scheme of my own. Like Lévy-Bruhl he defined scientific thought and moral (mystical, non-logico-experimental) thought in the rough and showed that there is a real sociological task to perform in unravelling, and in tracing the development of, their interrelations. Like Lévy-Bruhl, he left detailed analysis to others.

(9) Pareto's reference to sentiment was dangerous. Too often we see him falling into the pit prepared for those who seek to explain behaviour in psychological terms by attributing it to sentiments, needs, dispositions, and so forth. They observe a range of behaviour with a common objective and say that there is a sentiment or instinct that produces the behaviour. They then explain the behaviour by reference to the sentiment or instinct they have hypothesized from the behaviour. Men act in a certain way towards their country's flag. It is assumed from this that there is a sentiment of patriotism and the behaviour is then explained by saying that it springs from a sentiment of patriotism.

Nevertheless in fairness to Pareto it must be admitted that he perceived a basic, perhaps the basic, problem in sociology, and realised that only inductive methods of research will solve it. If different societies are to be compared then it is essential to strip behaviour of its variable characters and to reveal its uniformities, i.e. to reduce observed behaviour to abstractions which will serve as units of comparison. And who would deny that in all societies there is a range of simple and uniform modes of behaviour, call them sentiments or residues, or participations, or merely X, for else how could we, as Pareto asks, so easily understand the speech and behaviour of savages and men of earlier times?

(10) There is a great similarity between Lévy-Bruhl's collective representations and Pareto's derivations, and between Lévy-Bruhl's mystical participations and Pareto's residues. The main theoretical difference between them is that Lévy-Bruhl regarded the facts as socially determined and thus accounted for acceptance of belief by generality, transmission, and compulsion, whereas Pareto regarded them as psychologically determined and explained them by sentiments and other somewhat mysterious psychological drives. In any society we find a large number of collective representations (derivations) organized into a system. When we analyse them by comparison and remove what is not common to all societies we find a residue of simple modes of behaviour powerfully charged with emotion, e.g those classified by Pareto as group-persistences: relations of family and kin, relations with places, relations between the living and the dead, and so forth. These relations are what Lévy-Bruhl calls mystical participations. Any occurence is at once, as Lévy-Bruhl puts it, interpreted in terms of the collective representations, and as Pareto puts it in terms of the derivations. The thought of men is organized not so much by the logic of science as by the logic of collective representations or the logic of sentiments, and an action or statement must accord with the representations, or sentiments, rather than with experience. It is only in the technological field that science has gained ground from sentiment in modern societies. Hence our difficulty in understanding much of   primitive magic while we readily appreciate most of their other notions since they accord with sentiments we ourselves possess for "Derivations vary, the residue endures".

(11) Another cardinal problem perceived by Pareto is the relation between individual psychology and culture. Indeed the treatment of this problem is perhaps the best part of his thesis. There are in all individuals certain psychological traits and in any society there are psychological types and these traits and types will manifest themselves in culture regardless of its particular forms.

The sex instinct manifests itself in every society and if it is prohibited in one mode of expression it will manifest itself in another. A dominant and ambitious man will seek power by all means and at all costs whether he is born in China or Peru; whether he enters the army, the church, the law, or academic life; whether for the moment he expresses his ambitions in the idiom of socialism or conservatism. For individuals are not entirely conditioned by culture but only limited by it and always seek to exploit it in their own interests. Thus a moral ideology may be acknowledged by all men but often they twist it till it serves their interests even though it is contradicted in the process and one man quotes as authority for his actions what another quotes as authority for condemning them. In any situation a man will select from social doctrine what is of advantage to him and will exclude the rest, or will interpret a doctrine in the manner which suits his interests best. Christian teachings are supposed to determine human behaviour but what often happens in fact is that men control Christian dogma selecting from its doctrines what pays them and excluding the rest, or interpreting what conflicts with their actions so that it seems to support them.

(12) Finally, I will draw attention to Pareto's methodology which was sound even if his employment of it was often unsatisfactory. It may be summed up in two statements, (a) In a real situation we have to consider certain factors and neglect others if we are going to obtain scientific results. Science deals always with abstractions in this manner and allows for distortion until it can be corrected by further study of the neglected factors. Thus Pareto decided to pay no attention to environmental, historical, racial, and other, factors that condition social life but to study only the interrelations of psychological facts with one another and, to some extent, with economic changes and biological variations, (b) He tried to make a functional study of these facts by noting uniformities and interdependencies between them. He expressed contempt for people who seek to discover the origin of things both in terms of development and in terms of diffusion. Indeed one of the chief weaknesses of his book is that his exclusive interest in functional relationships of a psychological kind led him to neglect a study of cultural development and cultural variations which alone enable functional relationships to be established.

(13) There are many points in Pareto's rambling account about which criticism might be levelled against him. I have mentioned only a few and have rather sought to emphasize his contributions than his shortcomings and to remark upon those ideas which can be compared with the ideas of writers about primitive peoples and those which lead to important sociological problems. I would make it clear that I do not consider Pareto's contributions to sociology of great importance. What is valuable in his writings is commonplace in comparative sociology. None the less he is a useful subject for treatment in an history of theories of primitive mentality.

E.E. Evans-Pritchard.

'Nor No Witchcraft Charm Thee! '

In his article, 'Two Styles in the Study of Witchcraft' (1973),
Crick mentioned three recent publications (ed. Gluckman 1972; ed-
Douglas 1970; Mair 1969) as examples of the style he condemned.
Even Douglas, suggesting that 'As far as witchcraft studies are
concerned, the field is open to anyone who cares to enter it' (1970:
xxxvi), seems to imply that'witchcraft' should continue as an
isolable subject for comparative study. After making some pertinent
and down to earth temarks about this point of view, Crick proceeded
to lay out a series of ideas which were indicative of the direction he
would like to see anthropology advance. With regards to his alternative
style, Crick might be correct in predicting that some will find his
paper wholly unsavoury and metaphysical. It is to be regretted if his
image of an articulated moral space is considered by few, but this may
be because he kept his discussion to the abstract and offered no
evidence to substantiate his claim that witchcraft can be lost in a
'shared conceptual and moral space'. No doubt he envisages a wider
application of his scheme than the dissolution of the theoretical concept
of witchcraft and the real purpose of the paper was as a vector of certain
critical ideas. But to move on, he would chance leaving witchcraft bobbing
in his wake. First, he should relate his work to ethnography, thereby
attending to a point implicit in his paper, that the conceptual categories
of another culture must be exhaustively examined before our distinctions
are imposed on them. For as he says, "We can never be sure exactly how
odd our own categories of thought are" (1973:21).

Sharing this sentiment, I have selected the ethnography of a particular
society, the Konkomba, for close examination. This should exemplify the
risks of entering fieldwork with certain anthropological notions which have
the power to predetermine what will be found to a remarkable degree. Whereas
Crick's concern was to dissolve witchcraft in a larger conceptual framework,
my more pragmatic approach is to show how witchcraft can beguile the
ethnographer.

In fact it is sorcery that has been reported among the Konkomba, but,
as Turner emphasised ten years ago (1964), we can now assume that there is
no useful distinction to be made between sorcery and witchcraft to any
other than the ethnographic level. Even there, its appearance must make
us suspect the diffusion of Zande ideas, (by routes that we can plot with
the accuracy of aeroplane schedules). In terms of Saussure's chess
analogy, attempts to discriminate between witches and sorcerers seem like
exacting comparisons, sometimes even oppositions,between bishops and knights
(and in some cases, between king's pawn and queens). What we really need
is knowledge of all the pieces on the board, what Crick calls the 'person
field'.

The Konkomba are a Ghanaian people who speak a language belonging to
the Gurma group of the Voltaic family. Their segmentary lineage system
has been described by Tait in Tribes Without Rulers (eds. Middleton & Tait 1958:
168-202). For our purposes, it is sufficient to know that they are made up
of a number of distinct tribes, each consisting of several clans, the largest
autonomous units of social control. Clans of one tribe are linked by ritual
and other relationships and will come together in the event of inter-tribal
fighting. Tribes never unite.

In his paper on Konkomba sorcery, Tait enumerates the possible ways a sorcerer (osuo) can attack. The two main forms he distinguishes are the use of medicines (suoanjog); and transvection, whereby the sorcerer flies by night to his victim in the form of a moving light called suonmi, 'sorcerer-fire' (1967: 156). In addition, a sorcerer may send snakes to lie in the path of the victim or send his shadow to eat that of the victim, but these techniques are rarely employed. From another, though less detailed, source, there is general corroboration of these features (Froelich et al 1963: 157); but whilst Froelich talks loosely of sorcerers who kill by eating the souls of their victims (1949: 163), Tait emphasises that attack by suoanjog is far more feared and that greater precautions are taken against its administration. I shall be returning to suoanjog later.

Tait isolated three general beliefs about sorcerers:

    i. That anyone can be a sorcerer, sorcery is not related to descent;

    ii. "....that sorcerers attack anyone. They are evil and attack for the sheer joy of destruction.

    iii. On the other hand, it is also believed that sorcerers may kill in order to inherit from an older person and that men may even kill in order to inherit wives, goods, and status, and sisters kill their older sisters in order to inherit goods," (1956a:338).

The first two general beliefs made Tate suppose that patterns of sorcery-accusation would be random, the third, that accusations would fall within the family (ibid: 340). But, on analysing the twenty cases known to him, he discovered, much to his consternation, that

"....accusations seem to be made by the older unmarried men either against young women married into a minor lineage other than the accuser's but of the same major lineage, or against the husbands of such women" (ibid :339).

Taking into account that for a Konkomba, status and authority are acquired only upon marriage and that, as polygyny is practised, the men marry rather late, Tait explains his findings by concluding that,

"The accusations made by men against the young women married into their major lineage can be regarded as an expression of hostility between the in-group and the out-group. The accusations by young men against their seniors can be regarded as an expression of hostility towards men who exercise some authority, are possibly wealthy in cattle, and who, at the same time, are not yet senior enough to enjoy the privileges and ritual protection of elderhood" (1967: 167).

So what have we really learnt about sorcery? Sorcery itself has been discarded in favour of the readily observable, the quantifiable, sorcery-accusations; and the apparently inconsistent beliefs about sorcerers have been skipped over. By this reductive process, the problem has become that of eliciting the patterns of sorcery-accusation (in terms of the social structure), then relating these patterns to the social structure. Inevitably, a relationship is found and this is then explained in crude functional terms. This procedure is in accordance with Tait's maxim that,

"It is now well established that accusations of witchcraft
and sorcery tend to be directed within the framework of
the social structure" (1956a: 337).

This tautologous point of view is characteristic of the approach of the
British structural-functionalists of the 1950's and early 1960's and still
prevails. Its appearance suggests that Tait's perception of Konkomba
sorcery and the way he directed his research might have been influenced
more by the prevailing theories of witchcraft, derived from a misreading
of Evan-Pritchard's Azande book and Kluckhohn, than by his own experiences
in the field. Fortunately, it may be possible to test the validity of
this thought as, though he subscribed to such a mechanistic type of
approach, Tait seems to have sensed its limitations and, in his writings,
he has supplied rich and varied details, both ethnographic and linguistic.
With these, it may be possible to attempt a construction of a clearer
picture of the location of 'sorcery' in Konkomba thought.

The following can be split into three sections. The first seeks to
show that Tait wrongly translates osûo as 'sorcerer', and that this leads
to his inability to make sense of sorcery beliefs. By examining linguistic
material, it can be shown that osûo is a generic term which refers to those
who make a conscious choice to utilize the powers of 'evil', specified by
a central moral distinction which contraposes what may be best translated
as 'good' and 'evil'. With the elimination of the false category of
sorcery, it is possible to see this moral distinction at work. We can then
discern relationships between semantic systems, built upon a moral dimension,
which has previously been obscured. To exemplify this, the two following
sections explore the semantic fields of beer and kola and their interpen-
etration through their common reference to the moral dimension. Beer and
kola have been chosen because they are the principal media in which sûoanjog
are secreted.

So I shall first examine the available linguistic data concerning the
word 'osûo'. Though Tait has made some extremely valuable notes about this
word (1955a) he invariably translates it as 'sorcerer'. However, osûo is
an unusual word in several respects. Here are two points concerning its
morphology. First, it ends in a nasalized diphthong and this is rare
among Konkomba nouns. Those that do so have a ritual or magical significance.
Second, there are two plural forms, besûom and isûo. Besûom indicates that
it belongs to the concord class that has 'o-' and 'be-' as the singular
and plural prefixes, and in particular, it belongs to the subclass which
comprises solely nouns applicable to human beings (e.g. 'man', 'woman',
'chief', 'diviner', etc). On the other hand, the class with 'o-' and 'i-' as
the singular and plural prefixes consists of animals. Included in this
class are 'onamu, inamu', an animal which is dangerous in life and death,
and 'otuwe, ituwe', a one-legged, one-armed spirit of the bush. Another
feature of osuo is that were-animals, were-plants, inamu, ituwe and benekpib
(dangerous spirits of the bush) can all be called osûo.

Already, here is enough evidence to discredit the unqualified translation
of 'osûo' as 'sorcerer'. Moreover, Tait admits that

"There is no noun that can properly be translated as 'sorcery'" (1947
1967: 155).

It would appear that this category of beliefs and actions labelled 'Konkomba Sorcery' was indeed an import and may have misled Tait into directing his research along the narrowing lines of sorcery-accusation. But, to continue,

> "...there is a word kesůo, which refers to a class
> of phenomena that are evil, a class to which the
> activities of sorcerers belong" (Ibid.).

Again, by a change in prefix, the word has shifted to another concord class, and again its meanings are several, ranging from sůoanjog, sorcerer's medicine, to, more generally, 'something bad' or 'evil'. In this class, the word has a more abstract quality than the other two forms which seem to refer to what may be crudely glossed as 'user of evil'.

There are two words, kenjaa and onjaa, which appear to be conceptual counterparts of kesůo and osůo. Onjaa can be used to describe a good or worthy person and kenjaa is best translated as a 'good thing' or just 'good'. The crucial distinction between kesůo and osůo, and also between kenjaa and onjaa, is, I believe, the element of choice. For instance, kesůo is frequently used as a synonym of sůoanjog, and sůoanjog is made from the exuvia of dead bodies - therefore it is inherently bad. On the other hand, a man eats sůoanjog "... in order to gain power as a sorcerer ⎾osůo ⏌" (Ibid: 166), and he does this of his own free will. Moreover, whereas kesůo and kenjaa are mutually exclusive and fixed, the transformations of osůo into onjaa, and vice versa, are possible. What we are dealing with is thus a moral dimension. The forces of 'good' (kenjaa) and 'evil' (kesůo) are absolute; and to achieve one's objectives a conscious choice is made as to which shall be drawn upon.

This can be further illustrated by the word 'benekpib'. Tait gives three meanings for this word.

> i. "Senior persons or lineage elders"; ii. "ancestors
> or ancestor spirits"; iii. "spirits of the bush
> (probably spirits of evil ancestors)" (1953).

Recalling that Tait mentioned 'spirits of the bush' as being referred to as osůo, we can see that the benekpib of Tait's gloss 'spirits of ancestors' become osůo by the simple addition of the adjective 'evil'. However, if certain beliefs about benekpib (third meaning) are examined, further elaborations are uncovered.

Spirits of the bush cause mental stress in adolescents. To overcome them, it is necessary to perform rites called the 'catching of the spirits'. These may last 2-3 years before suffering is alleviated. By invoking the forces of good, the victim is able to control the benekpib who are now friendly and act in a sort of advisory capacity towards their former victim. Tait actually says that the spirits are now kenjaa as opposed to kesůo (ibid: 16), and it is certainly true that from the viewpoint of the adolescents, they now represent 'good' instead of the 'bad'. As for the benekpib themselves, they have been transformed from besůom into benjaa and can take their place with the 'good' ancestors and the living elders.

In passing, it seems worth mentioning that there is no evidence of a plural form of onjaa, injaa, which like isůo, would refer

to animals. This might simply signify that it is only by
resorting to the forces of evil that the human status of a man
can come into question.

Tait tells us that the two principal ways in which the sorcerer's
medicine, or rather sйoanjog, can be transmitted are in beer and in
a kola-nut (1967: 156-7). To understand why this should be so, the
place of beer and kola in the Konkomba world must be examined. It
should become apparent that the substitution of the idea of
Konkomba sorcery by the kesйo-kenjaa moral dimension not only
facilitates this task, but is crucial if we are to comprehend the
fit between parts of a system which otherwise would seem arbitrary
and fortuitous.

Let us begin by thinking about beer. In reading the principal
sources on Konkomba material - Cornevin, Cardinall, Froelich and Tait
- one is struck by the frequency with which they refer to beer. However,
it is not an everyday drink, but one that is brewed for particular,
though numerous, occasions. Work parties are provided with beer, it
is an essential component of funerals and Konkomba ritual, and most
people at Konkomba markets are there to share beer with friends
rather than to trade. In every instance, beer emerges as a symbol
of friendship or solidarity; it is kenjaa, a 'good thing',
par excellence.

In discussing interpersonal relationships, Tait (1961: Ch XI) stresses
the importance of, and the value which is placed upon, voluntary
friendship relations which cut across the structure of the lineage
system. Konkomba are hostile towards strangers and this category
includes all those who are not members of the clan, or clans,
putatively linked by agnation. Friendship ties transcend these
limits and help obviate recourse to violence between clans.
Friendships and the mutual obligations they entail are not entered
into lightly:

> "... in any lengthy rite that requires a heavy
> expenditure in foodstuffs and beer, material help
> is given between friends. Any man who is celebrating,
> for example, the Second Burial of his father, receives
> perhaps the bulk of the beer he distributes from his
> friends rather than from his agnates" (ibid: 215).

This passage continues

> "Since all the Second Burial rites of a clan are
> carried out simultaneously, clearly clansmen cannot
> help each other. The material help comes therefore
> from matrilateral kin and from friends."

Tait is implying that friends supply beer because clansmen are unable
to. However, the interest lies not in the genesis of this custom; it
is sufficient to note the identification of beer with friendship.
Friends are beer-givers, and vice-versa.

At another level of the social structure, an analagous
relationship to personal friendship can be perceived between clans
of the same tribe. There are two major links between clans, the
parent/filial relation and the reciprocal relationship of being
ritual partners (mantotib). The latter is formed at a rite to
end feuding between two clans of the same tribe. Called
Bi sub kedza, 'They bury the fight', or Bi sub tibwar, 'They bury
the words', it was first recorded by Sir Alan Cardinall in 1918.
The essential details he gives are in accord with those observed
by Tait in the 1950's (ibid: 147). In brief, the elders of the
two feuding clans meet with those of a third. Libations and
sacrifices are made, words are uttered, then

> "The ceremony is completed and piteau /‾beer_7 is freely
> drunk, both villages fraternising and drinking out of the same
> calabash at the same time" (Cardinall 1918: 50-1).

In all ritual, beer appears as both libation and in communal
drinking; for instance, the New Food Rites,which take the following
form. They open with the ritual drinking of beer, the elders
drinking two at a time from one calabash. Libations are then made
while the names of ancestors are called. Next, sacrifices are
performed, followed by further libations, then general sharing
of beer. Tait notes that libations occur at Van Gennep's état de
séparation' and 'état d'agrégation'. Froelich has transcribed in
French the following, which was chanted during the 'état de marge'
of a New Food Rite. Significantly, he reports that such a rite is
called 'La fête de la nouvelle bière'.

> "Woumbôr /‾God_7, protect us, give us abundant harvests,
> keep away fever and sicknesses of the head,[1] enable us
> to drink beer without any hitch, make our seeds good
> and fertile" (1954: 221, my trans.).

In this passage, we find a clue to the importance of beer in Konkomba
thought and the suggestion that there is no real difference between
the use of beer by men, lineages or clans. For to share beer is to
make oneself vulnerable. It places a person in a liminal situation
and is an expression of trust. By entering a relationship that
entails the sharing of beer, one is exposing oneself to constant
danger from that quarter. There is the ever-present risk of drinking
beer that has been spiked with suoanjog and thus transformed from
the category of kenjaa to that of kesuo. Furthermore, beer is
invariably drunk from a calabash. Not that this is surprising, but
it so happens that calabashes symbolize ungwin, a word which may be
translated as the spirit or soul of an individual as it is described
as "that which God gives a man" (Tait 1961: 137). To drink from one
calabash, therefore, expresses the bond between the drinkers. The
calabash signifies the shared soul or spirit of the participants, a
soul filled with kenjaa, beer. Yet the beer might be contaminated or
the sould devoured by an osuo. The danger is great and the meaning of
the act of sharing the beer enhanced.

It is now possible to view libation, not as another category
of events in which beer is used, but as an extension of these same
ideas to the relationship between men/lineages/clans and ancestors/
spirits/God. Libation only appears different to the anthropologist
because one party is incorporeal.

Kola is a nut which is chewed extensively in West Africa for its stimulative properties. Like tea and coffee, kola contains caffeine.

Apart from kola in the context of sorcery, there is little mention of its use among the Konkomba in the literature. That it was bought from traders and chewed by Konkomba can be inferred from a few references to it in Tait's work. We may suppose that the paucity of data concerning kola reflects its exclusion from their ritual, but, as I hope to show, this fact may be of importance in itself. To the visiting anthropologist observing its everyday use, it would not appear to have much significance, no more than the drinking of Coca-Cola in his own society. In fact Tait recommends that "... if you are tired and thirsty, stick to the excellent kola-nut" (1956b: 77). As a carrier for súoanjog, he says this of kola:

> "The kola-nut splits down the middle. Sorcerer's
> medicine can be put into this split and so passed
> to the victim. The Konkomba do not eat kola-nuts
> given to them by strangers. They accept the nut,
> thank the giver, and, later, throw it away" (1967: 157).

In order to appreciate the association between kola and súoanjog in Konkomba thought, it is necessary to understand what kola means to them. This is nowhere expressly stated, but certain insights may be gained by looking at their relationship with their neighbours, the Dagomba.

The Konkomba have suffered two series of invasions from the mounted Dagomba, first in the 13th-14th centuries and later in the 16th century. These were part of the general spread of the Mole-Dagbane-speaking peoples. The Konkomba eventually found refuge in their present homeland which was unsuitable for the Dagomba cavalry. However, there was no clear boundary between them, and the Dagomba made frequent raids to obtain slaves for their annual tribute to the Ashanti (Tait 1955b; Froelich et al 1963).

Serious fighting between the two peoples has now ceased, but the mutual hostility has remained and the distinction between them is still strong. Indirect Rule enabled Dagomba chiefs to appoint Konkomba sub-chiefs in the Konkomba area but their influence has been minimal. The traditional elders have retained their authority, and the Konkomba still despise the Dagomba and guard against their intrusion. This is not to say that there is no contact between them. The Konkomba markets have for long attracted Yoruba and Hausa traders, and Mossi butchers and weavers. Konkombaland lies on the overland trade-routes which lead from the kola-producing areas of Ashanti to Kano. Of late, the markets have grown in importance and the traders who now come to buy in bulk are mainly Dagomba. Nevertheless, it is significant that, according to their genealogies and myths of origin, time began for the Konkomba with the 16th century Dagomba invasions. For them, the Dagomba have remained their archetypal enemies, and they have always resisted the Dagomba way of life as they have Dagomba rule. Though the threat is no longer of violent invasion, the Konkomba now have to guard against moral subversion and loss of identity through the incursion of Dagomba values.

An important difference is that the Dagomba are Moslems, whereas the Konkomba never have been. Trimingham selects their insusceptibility to Islam as a distinguishing feature of Voltaic peoples (Gur language group) (1959: 15). Interestingly, he has also noted an identification commonly observed between Islam and kola (ibid: 198 -9, 156n). Kola has an important ceremonial function in West African life and frequently replaces beer in ritual, as is the case among the Dagomba (Rattray 1932: 463). It also forms part of Dagomba marriage payments. In addition, kola nuts were formally presented to officials and lesser chiefs by the Nas (Paramount Chiefs). At his death, a similar distribution of kola would be made on behalf of the Na and some would be received by mallams for reading verses from the Qur'an. When the new Na was enstooled, he would give kola to the chiefs who would serve him (ibid: 460, 570, 573, 580, 586). We can thus infer that among any other meanings kola might have for the Dagomba, to accept kola was a sign of subservience in certain contexts, and kola was also symbolic of the Dagomba Islamic beliefs as opposed to beer and paganism.

That kola does not appear in Konkomba ritual probably reflects the fact that it is not indigenous. It could not have entered the Konkomba universe until foreign traders had begun to visit the markets. As Dagomba started to dominate the market trade, so Konkomba were faced with a problem of how to maintain this desirable trade whilst continuing to resist the Dagomba threat. In resolving this quandary, they accept kola from a stranger so as not to insult him, thereby establishing a trading relationship. Later, they throw it away as a revocation of the possible connotations of their acceptance, i.e. subservience.

The Konkomba rationalize this action by terming kola a possible carrier of suoanjog, in other words, badness, and in particular, the badness they associate with Dagomba. Yet they accept kola from Konkomba friends and relations. I don't think that this is contradictory. It is only in a context where the giving of kola could be taken as a symbol of superiority that it is thrown away. Among the Konkomba, power and authority are prescribed and inevitable and do not have to be asserted. The threat of suoanjog remains though, but as Tait's patterns of sorcery-accusation have shown, it is those, or the wives of those, who enjoy more wealth, power and status than they should in the eyes of the accuser, who are the candidates for sorcery-accusation. Moreover, the accusations come from the men who feel they are lacking the status they deserve, i.e. the older, unmarried men.

What I hope this exegesis has achieved is to dissolve the category of Konkomba sorcery. The particular beliefs and practices which approximate to the anthropological concept of sorcery are not a group at all, but have been severed from an expressive system, built around and reaching out from a moral dimension which contraposes the forces of good and evil in Konkomba society. The extensions of the metaphorical use of kola and the metaphorical use of beer over a range of phenomena are facilitated by the grouping of the phenomena through their mutual association with

this moral dimension. Kola enters as a nexus between two semantic
systems, Dagomba and Konkomba; beer, as a metonym for special kinds
of relationships which are entered into out of choice and are kenjaa.
However, it should not be thought that suoanjog necessarily conveys
the same meaning in beer and kola. In kola, it tends to emphasise
the dangers from the outside, epitomized by the Dagomba; whereas in
beer, it is the dangers from the inside, from those in whom trust
has been placed, though the extent to which this is so depends chiefly
on context, and the possible conceptual range in each case would
appear to be the same.

Ethnography should aim at expressing native conceptual structures
as accurately as possible, for it is the foundation of our theory.
Before theory is fed back into fieldwork, it must be exhaustively
tested in order to seek out those a priori assumptions which may
distort interpretations of the field material. Witchcraft is such a
notion and its limitations as an interpretative framework are apparent
in Tait's writings. By labelling events that have a dim family
resemblance to our image of witchcraft, derived from our own cultural
experience, we run the risk of obscuring, or even excluding, those
connections between different realities, which, being outside or
out of phase with our own conceptual boundaries, are the ones that
should be concerning us. An approach like that advocated by Crick,
broader in scope and more systematic in application, would seem to
offer greater potential. Certainly, Konkomba sorcery can be lost
in a 'shared conceptual and moral space' and a system of person
catagories, one of Crick's primary articulations of moral space';
emerges from the linguistic evidence. Osuo takes its place with 'man',
'woman', 'diviner','chief', etc. and also with a group of supernatural
animals, though the material is insufficient to attempt a more complete
construction. As for his other articulation of moral space, that of
'action and evaluation concepts', we have uncovered a central moral
distinction in Konkomba conception which serves as an important
parameter in the structuring of their universe and from which certain
actions - for instance, libation or the sharing of kola - derive
their meaning.

David Price.

Note:

1. 'Sicknesses of the head', (orig. les maux de tête) refers to
the mental stress in adolescents caused by the spirits of the bush
mentioned earlier.

Bibliography

Cardinall, A.W.	1918	'Some Random Notes on the Customs of the Konkomba' J. of the African Soc.
Cornevin, R.	1954	'L'Enterrement d'un Chef Konkomba' Africa
Crick, M.	1973	'Two Styles in the Study of Witchcraft' JASO
Douglas, M. (ed)	1970	Witchcraft Confessions and Accusations ASA 9 Tavistock
Froelich, J.-C.	1949	'Les Konkomba du Nord-Togo' Bull de l'IFAN
"	1954	'Les Konkomba du Nord-Togo' Mem.de l'IFAN
Froelich, J.-C. Alexandre, P. et Cornevin, R.	1963	Les Populations du Nord-Togo IAI/PUF
Gluckman, M. (ed)	1972	The Allocation of Responsibility MUP
Mair, L.P.	1969	Witchcraft WUL/Weidenfeld & Nicolson
Middleton, J. & Tait, D (eds)	1958	Tribes Without Rulers Routledge & Kegan Paul Ltd
Rattray, Capt. R.S.	1932	Tribes of the Ashanti Hinterland 2 Vols. Clarendon Press
Tait, David	1953	'Spirits of the Buah; a note on personal religion among the Konkomba' Universitas I.i.
"	1955a	'Konkomba 'Osuo' ' Man
"	1955b	'History and Social Organization' Trans. of the Gold Coast and Togoland Historical Soc.
"	1956a	'The Family, Household, and Minor Lineages of the Konkomba' Parts I + II Africa
"	1956b	'Food in the Northern Territories' Universitas II, ii.
"	1961	The Konkomba of Northern Ghana (ed. Jack Goody) OUP
"	1963	'A Sorcery Hunt in Dagomba' Africa
"	1967	'Konkomba Sorcery' in Magic, Witchcraft and Curing (ed. J. Middleton) Rep'd from JRAI 1954
Trimingham, J.S.	1959	Islam in West Africa OUP
Turner, V.W.	1964	'Witchcraft and Sorcery: Taxonomy versus Dynamics' Africa.

Can there be an anthropology of children? A reply

I am spurred to action by the article in J.A.S.O., vol. IV,
No. 2, 1973 by Charlotte Hardman. She poses a question in her title,
and repeats it at the end in the form: "Should there not then be an
anthropology of children?" My short answer to both questions is "yes".
The long answer occupies the rest of this article, which is not intended
as a rebuttal of Hardman's thesis nor as the basis for a bitter
confrontation, as my agreement with her questions clearly indicates,
but rather as a modification and to some extent a critique of her position.

Let me begin with a brief description of the way I see the problems
she has raised. It will help if I start with a single-sentence autobio-
graphy. I studied social and physical anthropology (at U.C.L.) from
1956-59, rhesus monkeys and chimpanzees from 1959-68, and human school-
children from 1968-now; I intend to go on studying human schoolchildren
for at least the next three years. As a result of this hybrid background
there can, for me, never be a social anthropology of young children (in the
sense of a comprehensive explanation of why they do, say and think what
they do) along the same lines as there can be and is a social anthropology
of adults. Nor is the reason that children are 'immature' adults (in which
case one might expect a sort of social anthropology of immaturity); I fully
accept Hardman's view (derived mainly from her observations at St. Barnabas
School playground, Oxford and the work of the Opies) that there is a very
real sense in which one can talk of a 'children's subculture' or something
like that in which there are ideas, rules, values and so on that belong
strictly to the children and aren't part of or prior to (except in the
obvious temporal sense) the adult world of meaning, and so on.

The problem as I see it is another one, and is essentially a develop-
mental one. In a nutshell it is as follows:- the infant is born in an
extremely animal-like state, with a number of behavioural capacities known
in the medical literature as 'reflexes' (for a listing and description of
these see Illingworth 1972). It developes socially during the first year
in close association with the mother (or mother substitute) and its
interactions with her are non-verbal and based on a number of non-verbal
processes involving oral, tactile, visual, auditory and olfactory processes.
These processes have been studied by ethologists and others (e.g. Wolff
1963). As time goes by the infant both continues to interact with the
mother, father and peers on a non-verbal basis, and also to develop cul-
tural, meaning-laden actions, a consciousness of itself and of others,
pre-occupations with appropriateness and with the definition of social
situations, most of all this being linguistically mediated. What interests
me is how this set of transitions from animal-like beginnings to uniquely
human endings comes about in the early years. Thus the problem presented
by Charlotte Hardman's paper is that she has come into middle childhood
(the age of her children isn't specified but I understand it to have been
6-11 years) armed with an array of concepts from the history of social
anthropology and from the psychology of Piaget but has largely overlooked
the ethological literature on her subject, for instance two recent books,
Blurton-Jones (ed) (1972) and McGrew (1972), plus a goodly number of papers,
reference to which can be found in those books. Did she feel those works
were irrelevant? Can they possibly be irrelevant, dealing as they do with
the social interactions of young children?

Let me just give a few hints of the quality of the ethological writings, so that readers can judge for themselves the intellectual and linguistic distance between them and the kind of descriptions found in Hardman's article or the Opies' works. I'll start with a brief summary of some of Blurton-Jones' findings in his earlier (1967) study.

The social environment he studied was a rather loose, unstructured one, in a nursery school, not unlike that studied by Hardman, but his age group (3-5 year olds) was lower than hers (6-11 year olds). He found there were some friendships between children, some rather submissive children, but no clear 'hierarchy'. Children's behaviour to adults depended on who the adults were. In the case of the teacher, some children stayed near her, showed her paintings they had done, etc., and clung to her if they got hurt. Others (called 'little mothers' by McGrew 1972) led a child who was in need of help to the teacher, often with one hand behind the led child's back. Strangers were stared at, and shown things. Responses to parents took two forms: either the child smiled, ran to the parent and touched her (it was usually the mother), or the child walked to the parent and gave her an object such as a painting. A third, less frequent variety was to ignore the parent altogether.

In their relations with each other, Blurton-Jones described two types, agonistic, and rough-and-tumble play. Agonistic (i.e. fight/flight) behaviour occurred mostly over objects. A 'beating movement', bringing down the hand or fist on to a child, was common. Biting occurred, especially in the case of girls. A 'fierce' expression, with lower teeth bared and mouth corners down was interpreted as inhibited attack. A defeated child would scream, call for help, then weep with puckered brows and a reddened face, staying immobile in one spot. There was no wrestling and punching in real quarrels.

'Rough-and-tumble play', by contrast, did include wrestling and punching, and gave the appearance of violence and assault, especially to adults. Facially it was quite different, since it went with an 'open-mouthed smile with teeth covered', an expression similar to the expression described by Van Hooff (1967) and Loizos (1967) as the play face of chimpanzees and macaques. Alternation of roles, another feature of primate play, was common in rough-and-tumble play.

McGrew (1972) observed children aged 3-5 in two nursery schools, one in Oxford and one in Edinburgh. In his book he compares his findings with those hitherto published on behaviour in this age group, and with relevant comparative data on nonhuman primates.

He found that, overall, most child-child interactions were dyadic: 81% in the case of agonistic behaviour, 91% in the case of non-agonistic (friendly or neutral). Thus it seems at this age children interact nearly always with one other, and perhaps are not able to cope very often with the greater complexity of triadic or multiple interactions. The mean time of interactions was 12.9 seconds. Thus young pre-school children seem incapable for the most part of engaging in prolonged interaction, an ability that in most children would seem to develop at the primary school stage. Boys formed all-male groups with a frequency greater than could be attributed to chance, while this was not true of girls. 31% of all interactions involved the transmission or manipulation of an inanimate object.

McGrew, unlike Blurton-Jones, was able to rank his children into a dominance rank order, on the basis of predictable wins/losses in fights, especially over objects. The dominant boys were significantly older, heavier and more nursery-experienced than the subordinates, but were not taller or more intelligent.

Among the more valuable contributions of McGrew's study was his close analysis of the first 7 days' experience of one of his nursery schools by 8 children. They were observed from the moment they entered school, usually with their mothers; all of the newcomers were aged 3, and there were in addition 5 nursery-experienced children, aged 4, in the group. McGrew observed the whole group at once for this study.

At the very outset behaviour was characterised by crying, slow locomotion and an orientation towards the Teacher. Indications of 'social stress' or 'anxiety' such as digit sucking and automanipulation were greatest at the outset but declined during the 7-day period. In contrast, there was an increase in object struggles, and in aggressive acts such as 'push', the latter being more common in boys than girls.

Newcomers' behaviour after arrival was characteristically to suck objects or their fingers, to look away from other children and avoid eye contact, to move around with a sidling, shuffling, hesitant gait. They observed the activities of others intently, but declined offers to engage in social interaction or kept it brief. They avoided all boisterous activity and any kind of competition. In most cases the voice was quiet or silent but 3 children (all girls) were garrulous. In the case of the silent majority verbalisation increased subsequently, whereas in the case of the three noisy girls it declined.

Some resident girls displayed maternal attentiveness - a soothing tone of voice when talking to a newcomer, tactile comforting e.g. holding hands, or putting a hand on the back or an arm round the shoulders, or patting or kissing. These were the "little mothers", one of whom was aged 3, who made efforts to cheer up sad newcomers. Boys, by contrast, seemed for the most part indifferent to newcomers' tears or questions like "when's mummy coming back?" It has been found in studies of rhesus monkeys that juvenile females are more responsive to infants than juvenile males.

We can note as primary features of the ethological descriptions (a) their clear focus on observable, quantifiable and well-defined non-verbal actions, and (b) their zoological orientation, with an especial tendency to refer to non-human primates for comparative purposes.

Despite the fact that there is an age difference between the 3-5 year olds described by Blurton-Jones or McGrew and the 6-11 year olds described by Hardman, the fact is that 6-11 year olds can be described in an ethological way. But even were this not the case, the fact remains that 3-5 year olds do grow into 6-11 year olds and at that age a Hardman-type analysis shows us a completely different world, so different that we seem to be confronted by a different order of being. Yet I'm willing to bet that if Hardman had studied either Blurton-Jones' or McGrew's nursery school children she would have found them expressing ideas, thinking and talking, in a younger but essentially comparable way to that of her St. Barnabas children.

In other words we have at least in part to do with a severe contrast of methods of study.

And second we have to do with a real development, the development of the human organism.

Can we hope, even try, to synthesise the methods, in order the better to understand the development?

There are precedents, of a sort. Certainly Piaget has tried to build up an image of child development that starts from organic principles and builds outwards in a sort of dialectical spiral that moves between an organic development on the one hand and contact with a structuralist idea-world on the other over time. Piaget however says nothing about non-verbal interaction, even less than Charlotte Hardman who does at least tell us that "I was soon made aware that the bio-physical environment constituted the main equipment also for communication, as I later found out (p.95)". What did she later find out, exactly? It's not too clear, but it seems to be that certain physical objects in the playground, plus certain parts of the children's bodies "especially their arms, fingers and feet all show immense potential for possible play. Each object will acquire meaning or value through its relative position with other objects or the specific context"... "The contexts which define the meanings of the environment are the imaginary situations agreed upon by the group." (pp.95-96)

Excellent! It seems that we have to deal with just that stage in development where bits of the body and physical environment are used for social communication, not in the 'animal' way but rather in the uniquely human, meaning-laden way. That's just fine as a description, and in so far as social anthropology is content with description then it's fine social anthropology. Also, in so far as social anthropology is concerned with explanation, if it's content with Lévi-Strauss type structuralist explanation, then Charlotte Hardman has arrived; she's found her anthropology of children and her question is answered. But I don't think anthropology (and note that I say 'anthropology', not 'social anthropology', and note too that Charlotte Hardman says 'anthropology', not 'social anthropology' in her title and last sentence) should or can afford to rest content with explanations that simply take people's ideas, whether adults' or children's, and relate them on a to-fro basis to the world of knowledge in which they live. It's no great trick to do this, although it may be fashionable, or have been so. There remains the stubborn fact, which anthropologists by the generation have chosen to ignore or demote to irrelevance, that humans, children and adults, are biological entities with nervous systems, eyes, ears and so on.

Do I then advocate some sort of Robin Fox-Lionel Tiger approach, by which I mean a consideration of man as a 'cultural animal', a creature evolved and pre-programmed with a 'biogrammar' that predisposes him to develop in certain directions, both in face-to-face interactions and in his social arrangements? (see e.g. Fox 1967, Tiger and Fox 1966, 1972). No, I don't, and I think an article such

as Charlotte Hardman's is sufficient to dispose of most of the Tiger-
Fox arguments, which seem to me (although for a while I myself was
attracted by them and even engaged in them) to be metaphorical,
un-productive of empirical research and too speculative for comfort.
Having lived with and thought about that approach for a few years I
have felt myself forced to reject it or at least modify it drastically.
Charlotte Hardman's approach, however, is much more enlightening and
leads to a direct need for more empirical research and more theorising.
(Incidentally, she might be interested in a little piece of research
I recently did with Anne Guest, on children's conceptions of the meaning
of Easter (Guest and Reynolds 1972)).

But to return to the basic problem. What of the complex and
skilfully worked out approach of Piaget to children's mental development?
Hardman writes "In certain other aspects Piaget is surprisingly anthro-
pological in his approach, or rather he links with anthropology through
structuralism. He sees his own theory of cognitive structure as
intimately connected with Lévi-Strauss' doctrine of the primacy of structure
in social life, and like Lévi-Strauss is seeking that conceptual structure
which lurks behind the social structure" (p.94)...."We might perhaps link
the works of Piaget and Lévi-Struass as a means to understand child thought".
(p.95). An excellent idea, but it leads to certain problems which are
elucidated in an article to which Hardman does not refer, namely Howard
Gardner's recent paper "Structure and Development" (1973), which contains
a step-by-step comparison of the methods of and results achieved by Piaget
and Lévi-Strauss.

So relevant is this article to the issues here discussed that I
quote from it at length:

'Piaget poses a crucial question: "Le problème central de tout struc-
turalisme: les totalités par composition, sont elles composées de tout
temps, mais comment ou par qui, ou ont-elles été d'abord (et sont-elles
toujours?) en voie de composition? Autrement dit, les structures
comportent-elles une formation ou ne connaissent-elles qu'une préformation
plus ou moins éternelle?" (Piaget 1968).

'Here Piaget is challenging structuralism of the Lévi-Strauss variety,
for he goes on to maintain that a full comprehension of the structure can
only result from the realisation that a structure is always in the process
of being formed and that one cannot understand the structure without ap-
preciating the nature of its formation and its course of continuous transfor-
mation and auto-regulation'. (Gardner 1973 p.56). And further on:

'Piaget's approach, then, involves a continuous dialectic between the
flux biological processes and the formal precision of structural models...
In a way suggestive of Lévi-Strauss, the structures discerned are viewed as
intermediate between the nervous system and conscious behaviour.' (p.57).

Gardner goes on to compare and contrast Piaget and Lévi-Strauss in
a number of respects, but note his comments on the developmental issue:
'Lévi-Strauss is explicitly not concerned with...the manner in which, over
time, the individual member of the society acquires the cultural system...
In his disregard of individual actions Lévi-Strauss's thoughts about
development are reflected in a very instructive way Lévi-Strauss believes
that the five-year-old in a society has already acquired the ways of thought

of adults...(Piaget) scorns the kinds of a priori formulations about the nature and quality of thought which Lévi-Strauss finds attractive." (p.58-59).

Gardner concludes: "This review of the two positions suggests that the discrepancies between Lévi-Strauss and Piaget predominate. Yet it is only because they are in many ways close to one another that a detailed comparison is even possible." (p.60).

Gardner continues with a discussion of some of the work of Jakobson, indicating that it might well provide a bridge between Piaget's developmentalism and Lévi-Strauss's structuralism. To go into the details of this is not my intention, in any case Gardner's article is in print. However I will just give his conclusion: "Indeed, combining the developmental perspective of Piaget and the structural linguistic approach of Jakobson and Lévi-Strauss would seem a promising step for students of psychology and anthropology. It should bring into closer alignment those approaches which stress the sensory aspects in relation to specific cultural codes, and those which stress the active, organising aspects in relation to the world of objects and persons." (p.66). Clearly, Gardner would feel that Hardman's idea of combining Piaget and Lévi-Strauss was a good one, and that the way to do it would be via Jakobson.

As for my own comment at this stage, I feel that there is here a problem of extraordinary interest for anthropologists, but one which needs widening out further than authors have hitherto been prepared to do. I want to see much more early non-verbal communication brought into the developmental picture, and a concern among anthropologists for a frame of reference that will do justice to the amazing transformations involved in child development. And as if that alone were not enough I want to see the actual underlying (neuro-) physiological processes brought in as well. I don't want to see a quick jump into genetic or para-genetic arguments or evolutionary rationalisations.

Just to arouse the reader's curiosity, let me end by saying that my own current research concerns the estimation by biochemical means of the catecholamine content of children's urine. Children like those studied by Charlotte Hardman. Why? Let us continue at another time. But let us work together and not create artificial barriers. If we do, children of all ages will have a right to laugh at us.

V. Reynolds.

REFERENCES

Blurton-Jones, N.G. 1967. 'An ethological study of nursery school children.
In: D. Morris (ed) Primate Ethology, London.

Blurton-Jones, N.G. (ed) 1972. Ethological Studies of Child Behaviour.
Cambridge U.P.

Fox, R. 1967. 'In the beginning, aspects of hominid behavioural evolution'.
Man, 2, 415-433.

Guest, A. and V. Reynolds 1972. 'The Easter message: bunnies or crucifixion?'
New Soc., 30 March, pp. 638-640.

Gardner H. 1973. 'Structure and Development' The Human Context, 5, 1, 50-67.

Hardman, C. 1973. 'Can there be an anthropology of children?' J.A.S.O., 4, 85-99.

Illingworth, R. 1972. The development of the infant and young child. 5th ed.,
Edinburgh.

Loizos, C. 1967. 'Play behaviour in higher primates: a review' in D. Morris (Ed).
Primate Ethology.

McGrew, W.C. 1972. An Ethological Study of Children's Behaviour. Academic Press.

Piaget, J. 1968. Le Structuralisme. Paris.

Tiger, L. 1969. Men in Groups. Methuen, London.

Tiger, L. and R. Fox 1966. 'The zoological perspective in social science'.
Man, 1 (n.s.), 1.

Tiger, L. and R. Fox, 1972. The Imperial Animal. Secker & Warburg.

Van Hooff, J.A.R.M. 1967. 'The facial displays of the catarhine monkeys and apes'.

Wolff, P.H. 1963. 'Observations on the early development of smiling.' In B.M.
Foss (ed), Determinants of Infant Behaviour, Vol. 2, Methuen,
London.

Bachelard's Epistemology and the History of the Sciences

The interest of Bachelard's philosophical position lies in its radical and deliberate 'non-positivism' especially in the anti-evolutionism of his notion of the 'epistemological break'. The 'non-positivism' and anti-evolutionism are related to the link he perceives between epistemology and the actual practise of the history of the sciences; the radical displacement undergone by epistemological problems is by virtue of this unity. It is the perception of this unity that links Bachelard's work with that of his pupil, Canguilhem, and later, of Michel Foucault.

In his (re)affirmation of the scientific character of Marx's 'Capital' Althusser makes use of certain of Bachelard's epistemological categories, notably the notion of epistemological 'rupture'. Bachelard's approach to the philosophy of the sciences is analogous to Althusser's - just as Althusser attempts to discover the effects in philosophy of the emergence of a new science, historical materialism, Bachelard's project is to elucidate the effects in philosophy of the development of new concepts in the sciences.[1] Instead of finding a philosophical solution to the 'crisis' in a theory of knowledge Bachelard argues that the change does not take place with the aid of a new philosophy against the old,[2] but without the benefit of philosophy at all, against all existing philosophies, and that this is the rule.

There is thus a strong polemical element, with the purpose of defining the new 'theoretical space' in which Bachelard's epistemology lies; Bachelard announces the time of the 'Anabaptist philosophers'[3]- Anabaptist in that they forswear all the beliefs and dogmas of traditional philosophy; philosophical only in a strict sense to be defined below. They will establish themselves on the territory of scientific knowledge itself, in its actual practise.

The new philosophy lies in a 'new dimension', which is portrayed diagramatically in Bachelard's philosophical spectrum.[4] Sciences are produced in opposition to philosophies; the truth of a scientific statement is not founded upon any philosophical guarantee. It is in its very refutation that the necessity of philosophy appears. Philosophy is defined by its function (not by its object), as an intervention in the area of the sciences. "The role of philosophy must be 'reversed' - it must no longer be the spokesman of ideologies vis-à-vis the sciences, - rather its task is to neutralize their discourse and so hinder as much as possible the emergence of obstacles. At least it must distinguish within a given discourse between what derives from scientific practise and what originates in ideological discourses."[5]

So the function of Bachelard's epistemology is to 'escort' the progress of the sciences, and in so doing it treats problems completely alien to traditional philosophy. The new discipline will be an 'open' philosophy[6] - as the problems vary; as a science progresses the 'values' it may secrete change, and the footholds it gives ideology shift. The emergence of a new science may change the theoretical conjuncture, and the position of a given science in this conjuncture change. Being open the new philosophy rejects the characteristic of traditional philosophy to form a system. This stems from the very nature of scientific knowledge: science is not a unity, and between the different branches of scientific knowledge, development is uneven.[7] "This new discipline will be attentive to the real conditions of scientific works...the different regions...and their inter-relations, i.e. a historical philosophy."[8]

The 'openness' of the new philosophy, that which gives it its historicity and delineates it from an ideological philosophy, is achieved by Bachelard's concept of the dialectic.[9] In the place of the subject-object relation[10] Bachelard substitutes the relation comprehension-extension. These categories demonstrate that in science the thought object is

constructed, and comprehension is a function of the concept which thinks the thought object.[11] Sciences themselves produce their objects and phenomena in their theories and their materialization in experimental proofs. "The materiality of the real world - its existence independent of thought - and the possibility of its appropriation by the sciences (the primary categories of materialism), are sufficiently confirmed by the practise of the sciences themselves, by their ability to inscribe their theories in experimental forms, in what Bachelard calls a phenomeno-technics."[12] So the dissolution of the entity as the object of science dissipates the myth of immediate comprehension.

Having defined the theoretical space that Bachelard's epistemology occupies, it is now time to show how he erects the concept of a theoretical mode of production.

The place of the concept of the dialectic is between, on the one hand hypotheses and theories, and on the other, experiments; the dialogue is a historical process of an exchange of information whose final result is to adjust theory and experiment. It is this reorganization of knowledge, "where the language of theory and experiment are in contradiction", that Bachelard calls the dialectic.[13]

The dialectic of reason and application ensures that a science will go through a series of recastings/recrystalizations, each of which will redefine the basic concepts used by a science i.e. scientific thought progresses by reorganizations of its bases proceeding from its summit - this movement takes place only in and by scientific experiment. While the successive concepts are not equivalent there will be an 'epistemological profile' linking them in the mind of the scientist, which means that the extent to which each scientist uses the concept at any given time corresponds to each phase of development of the theory in which the concept has been used.

Hence sciences progress by breaking with pre-existing modes of thought: progress is then discontinuous, and can be seen as overcoming 'epistemological obstacles' secreted by those modes of thought;[14] e.g. immediate experience, general knowledge, pragmatic knowledge. Bachelard gives no general theory of epistemological obstacles, only examples. Three things emerge about them; (1) Once a science has been constituted they arise within the science.[15]
(2) Epistemological obstacles are a trap for scientific knowledge and concepts set by the 'thought-habits' of everyday life and experience.[16] Bachelard opposes the abstraction necessary to scientific thought to the 'revery' - the dream-like character of everyday experience. To constitute itself a science must break away from revery, but the latter does not thus lose its right to exist - the domains of knowledge and art are simply separated.
(3) The two poles of the 'philosophical spectrum', realism and idealism, are the most characteristic epistemological obstacles, whose psychological power gives a foothold to the philosophies which claim to guarantee the knowledge produced by the sciences, whilst really only battening onto and supporting the epistemological obstacles produced at each stage of scientific development.[17]

In this way scientific thought progresses by successive recrystallizations. As stated above this takes place only in and by scientific experimentation. Bachelard lays down a theory of scientific instruments as 'materialized theories' and of their assembly, forming a new body of

doctrine - technical materialism - the study of the material which
science uses for the organization of its experiments.[18]

The essential element of the activity of scientific thought is to
produce couplings of the abstract and of the concrete via the installation
of theoretically defined instruments and via assemblages of apparatuses
following programmes of rational realization - i.e. to "concretize the
abstract". In this way 'scientific phenomena' are produced - termed
phenomenotechnics - and ideological intervention prevented. This is
radically different to a phenomenology that can only talk about phenomena,
never produce any.[19] 'The true scientific phenomenology is therefore
essentially a phenomenotechnics. It reinforces what shows through behind
what appears. It instructs itself by what it constructs...Science raises
up a world not by a magical force immanent in reality, but rather by a
rational force immanent to the mind."[20]

It is in this 'concretizing of the abstract' that Bachelard's
epistemology is situated - thus experience again becomes a central
philosophical theme, but with a completely new meaning - "In the end
experimental conditions are the same as preconditions of experimentation."[21]
The 'objects' of these experiments must be understood in a new manner.[22]
Bachelard concludes "if one is to hold one's position at the centre of
the working mind and of worked matter, one must abandon many philosophical
traditions of the native translucence of the intellect and of the reality
of the sensory world."[23]

Here we have defined the epistemological disciplines that fill the
blanks on the philosophical spectrum, on the level of scientific activity.
They are 'Applied Rationalism' and 'Technical Materialism'. The two
disciplines are reciprocal - in the production of concepts attention must
be paid to the conditions of application of the concepts; conversely the
problems of assembly must integrate into their solution the theoretical
conditions of their formulation.[24]

So Bachelard studies the realities of research, at the level of the
difficult formulation of problems. He introduces[25] the concept of the prob-
lematic to indicate a structural field, to explain the set of concepts
of technical materialism within the metaphor of a field structured by
two operations - experiment and definition. The problematic is the
positive notion which 'stands in for something else' - for the philosophical
idea of the given.[26] "(Against the parade of universal doubt), scientific
research demands the setting-up of a problematic. Its real starting point
is a problem, however ill-formulated. The scientific ego is then a programme
of experiments; while the scientific non-ego is already a constituted
problematic."[27]

Bachelard goes further in the determination of the structure of all
production of scientific concepts. The problematics of the different
sciences are not wholly independent of each other, but only relatively
autonomous, and zones of over-lap may appear. (What Bachelard terms
transrationalism establishes itself at the end of prolonged theoretical
labour, by the intermediary of algebraic organization. It is at the level
of ever-more precise variables that interferences between domains of
rationality can arise).

The organization of the production of scientific concepts is
materialized in the form of institutions, meetings, colloquia etc., i.e.
a 'city of science'. This has communications within itself (a 'mutual

pedagogy'), so theories circulate more rapidly, and permit an
acceleration of discoveries. The city's cohesion allows the elimination
of every aberration related to the subjective character of any
researcher - thus modern science is freed from reveries. In this sense
it is more difficult for epistemological obstacles to form, thus the
apparent acceleration of present scientific time, although their
appearance is inevitable (above).

Thus the 'city of science' creates its own norms, maintaining
the criteria of objectivity and truth; this function is shown in the
technical region, where one can read in material form the general
characteristics of the 'city of science';[28] thus the 'city of science'
stands in for the 'Reason' of philosophers.[29]

Bachelard's epistemology erects the concept of a theoretical mode
of production. This allows: firstly the characterization of the obstacles
in an ideology of science, and secondly to think the transition from one
given mode of production to another - the new concept of the history
of the sciences.

The obstacles in an ideology of science which oppose the construction
of the concept of its history are threefold:[30]
(1) That science is a unity - "Science (in the singular) is neither
a philosophical category nor a scientific concept, but an ideological
notion. The object it designates does not exist. Sciences (in the plural)
however do exist."[31]
(2) That the development of science is continuous and uniform - a
temporal aspect of the essential unity of science (no breaks, recrystall-
izations etc).
(3) The unity of the different interpretations of what science is in
its essence (positivist, pragmatist, conventionalist) is the empiricism
which lies in the definition of the 'pseudo-object' adopted by the history
of science conceived as a history of methods and results.

The history of a science can only find the concept of its object
in the science of which it is the object; the real history of the science
is the real conditions of the production of its concepts. Further, each
science is irreducible, a practise. A science is born by constituting a
body of concepts with their rules of production, the development of a
science is the formation of the concepts and theories of that science.
Different sciences have different forms of development, and within the
nominal unity of a single science, concepts or theories may have different
developments, types of constitution of formation. The history of a
science implies an epistemology - the theory of the scientific production
of the concepts and theories of each science. (The exact point of contact
between the epistemology and the history of the science is difficult to
delimit).

The question thus posed is to know how a philosophy of science in
action can think its relation to the history of science - "the modern
point of view thus determines a new perspective on the history of the
sciences, a perspective which poses the problem of the current effectivity
of this history of the sciences in scientific culture."[32] This history of
the sciences cannot therefore be a "history like the others",[33] it is not
only the narration of events, but a duplicated history in which the unfolding
of values duplicates that of facts.[34] It is not a question of reliving the
past but of judging it, for "once the solution is found, its clarity
lights up the previous data." "This new perspective on the history of the
sciences is precisely recurrent history."[35]

The history of the sciences therefore presupposes the filtering or critical function of an epistemology directly informed by the activity of science - it constitutes its object by judging the claims of past judgments to truth on the basis of contemporary scientificity. This critical function allows the distinction between an outdated history and sanctioned history - the current past.[36] The dialectic of the liquidation of the past is translated into that of obstacles and epistemological acts. It is in respect of recurrent reflection that the acts are confirmed as such and the obstacles recognized as overcome or avoided.

Historical epistemology teaches that science progresses by means of mutations, reorganizations of its principles. The history of the sciences must itself be dialectical. The use of historical recurrence is only legitimately founded if the science concerned has itself attained the level of rigour which makes it possible to reorganize the hierarchy of epistemological values and through it to discern the real state of the genealogy of the concepts. One must beware of false recurrence, but affirm the progressive value of the scientific past. In a recurrent history "the consciousness of modernity and the consciousness of historicity are rigorously proportional",[37] and the history of a science is never completed for an epoch.

Since Bachelard's epistemology is a history in action, its history continually threatens to dissolve into the current epistemology, so it seems that the historicity of science is biased much more towards its future than its past.[38] So a history is a discourse (based on a current past) on a past as such - the history of recognized errors.

Historical time is abolished in this logical time which the epistemology creates. The construction of this 'real' time of science poses two problems, which lead onto a consideration of the limitations of Bachelard's epistemology.

(1) That a critical history is the 'fruits of past errors', and that it finds the norms of its jurisdiction in the current rationality of the work of science.

(2) The epistemological problem of the status of past truths.

The source of the limitations of Bachelard's epistemology is that the epistemologist and the historian of the sciences are located by Bachelard only with respect to the development of the science in question.[39] Its progressive side comes from (1) its denial of any empiricist history of the sciences - the history will always be epistemologically grounded departing from scientific rationality. (2) It never departs from givens - so the events of any history are never equivalent as elements, - there is at every point a division between scientific objectivity and ideology.[40]

However, Bachelard's epistemology is ideological because it has no concept for the possibility of its hisotry; he does not pose the conjunction of science and ideology and their reciprocal determination. Thus there can be no 'eternal truths' that are not relative to a 'current past', indeed to select elements (sanctianed and errors) from their particular problematic is to fall into empiricism.[41] For the recurrent judgement must conflate the epistemological break that originates the science with the

reorganization of the problematic of the science - thus removing
the concept of the epistemological break from the corpus of the concepts
of history insofar that it is continually shifted forward.[42]

"Scientific knowledge only exists in opposition to, by overcoming,
epistemological obstacles. But to theorise epistemological obstacles, i.e.
what prevents the existence of science, a theory of something other than
science is required; a theory of ideology - i.e. a theory of the ideological
instance in the social formation. Hence a theory of the history of a science
cannot entirely be independent of the theory of history in general, historical
materialism."[43]

In thinking the problem of epistemological obstacles solely from the
side of science and the scientist Bachelard turns to psychologism - epistemolo-
gical obstacles have no historical location; they are assumed to be universal
and natural products of the human mind.[44] Thus the natural tendencies
of the mind are anti-scientific, and science is a constant struggle against
the psychological traps lying in wait for each scientific concept, the
epistemological obstacles.

This concept of the 'scientific mind' explains the nature of Bachelard's
polemic, that of philosophy secreted in the development of each science against
the claims of other philosophies over the sciences; these latter lodge in
the niches provided by the ideological obstacles that the mind creates
for the concepts of science, forming the philosophical spectrum.[45] Thus
there is no positive role for philosophy, it has no history; the true
epistemology of the science in question has only a fleeting existence in
the science's rejection of the claims of these philosophical hangers-on.

This shows no internal inconsistency.[46] However, the anhistorical
nature of the psychological obstacles to be overcome in the establishment
of a science has its converse side - a necessary set of stages, a hierarchy
of rationalities in the constitution of scientific concepts — the
epistemological profile. Three consequences follow:

(1) This demands an evolutionist conception of the history of the
sciences - but what then of recurrence and the double history of the
sciences? (c.f. n38).

(2) Each science is seen in isolation according to its place in the
evolutionary scheme, and each science is essentially similar in kind -
but what then of the histories of the sciencies?

(3) A general theory of scientific rationality is set up corresponding
to the last phase - that of discursive reason; but what then of the attack
on philosophy for attempting to prefound the truth of scientific statements?

Another contradiction arises from the individualism of this psychology
of errors. Although Bachelard correctly sees scientific knowledge as a
collective activity - uniting collective rational activity and collectively
controlled experimentation - he conceives error as individual, for although
archetypal, it manifests itself in the individual scientist. Thus the
role of the 'city of science' is to guard against the aberrations of the
individual psyche, and to guarantee the progressive sequence of the
epistemological profile. The 'evolutionism' of the epistemological profile
is accompanied by a historicism of the social conditions of existence of
the historical mind.[47]

Thus the philosophy of knowledge re-enters by the back-door of psychology, and it can do this because Bachelard can only make general statements from a position outside particular sciences,[48] and, lacking any theory of the outside of science, he lapses into a traditional philosophy of knowledge with its categories of subject and object.[49]

This contradiction can only be resolved by situating science with respect to the ideologies with which it breaks - i.e. by a theory of 'epistemological obstacles' as part of the ideological instance of the social formation.

<div align="right">Tim Jenkins.</div>

Notes

1. "of the recrystallization of concepts in modern physics and chemistry associated with the name of Einstein."

2. "Agnostic idealism" against the previous "mechanical materialism."

3. In the 'La Philosophie du Non', (1940), tr. as 'Philosophy of No', (1968).

4. The 'new philosophy' will fill the blank spaces in the 'philosophical spectrum' - from 'Le Rationalisme Appliqué', (1949):

<div align="center">

Idealism

Conventionalism

Formalism

(   )   Positivism   (   )

Empiricism

Realism

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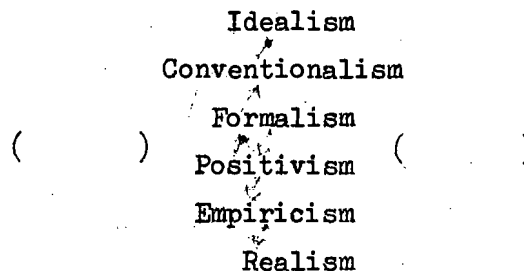
"Bachelard seeks in the use of the 'philosophical spectrum' to demonstrate the hierarchization of philosophical doctrines - 'inner' submitted to 'outer' doctrines in the unity of both sides of the spectrum. The blanks, situated on a different plane, indicate the philosophy adequate to think the sciences,which is thus conceived of as partaking of none of the ideological invariants of the philosophy of Idealism-Realism." - Lecourt in "L'Epistemologie historique de Gaston Bachelard", (1970).

5. Lecourt ibid.

6. "It will be the consciousness of a mind that founds itself by working on the unknown." 'Philosophie du Non', p.9.

7. "There cannot be any unitary epistemology - it is at the level of each concept that the precise tasks of the philosophy of the sciences are posed." ibid. p. 14.

8. Lecourt op. cit.

9. This 'historicity' enables Bachelard's epistemology to comprehend the invariance of the discourse of ideological philosophy. In this (latter) the invariant of knowledge "is the comprehending subject confronting the entity which has the character of a given empirical reality." - Bachelard.

10. equals: "comprehension/entity."

11. Thus there is no philosophically defined world of things in themselves which empirical science appropriates either asymptotically or piecemeal. Nor is there any philosophically defined consciousness to which all scientific statements can be reduced.

12. Brewster, 'Theoretical Practise' 3-4, (1971), pp. 25-37.

13. Under this definition the concept of the dialectic does not coincide with any in traditional philosophy - nor could it given the situation of Bachelard's philosophy with respect to traditional/previous philosophy. (See Canguilhem (1970) - 'Etudes d'Histoire et de Philosophie des Sciences').

14. It is worth noting here that this dialectic operates after the epistemological break that constitutes an open science from a previous ideology, and therefore the historicity of Bachelard's epistemology only operates within this openness. Science is therefore a given. This will be returned to below.

15. "It is not a matter of considering external obstacles such as the complexity and fleetingness of phenomena, nor of blaming the weakness of the human senses and mind: delays and disturbances occur intimately in the very act of knowing, by a kind of necessity."

16. "Over-familiar scientific ideas become charged with too much psychological concreteness, they collect too many analogies, images and metaphors, and lose little by little their abstract vector, their fine abstract tuning."

17. Philosophies are produced as a result of scientific advance with the aim of reuniting the world of knowledge and experience which each new science and each new scientific advance shatters. Hence the philosophies can be defined in a spectrum around ongoing science in terms of their displacement from science. - Brewster op.cit.
    Thus a science is not the exhaustive investigation of a closed domain defined a priori (by sensory experience, philosophical fiat or scientific hypothesis). Once it has made its break with common sense experience and the theoretical modes of thought anchored in common sense experience by an epistemological break, its future is completely open.

18. See 'Le Rationalisme Appliqué'.

19. Thus "Phenomenotechnics extends phenomenology. - A concept has become scientific in so far as it has become technical, i.e. that it is accompanied by a realizatory technique," in 'La Formation de l'Esprit Scientifique', p.61 (1947).

20. 'Le Nouvel Esprit Scientifique', (1934), p.13.

21. ibid. p.9.

22. "The meson, at the junction of the most abstract theory and of the most painstaking technical research, is now a particle with that double ontological status required of all the objects of modern physics." - 'L'Activité Rationaliste de la Physique Contemporaine', (1951).

23. ibid.

24. This distinction devalues the notion of 'method' - "the Cartesian notion of 'general scientific method' is vacuous, lacking the real movement of knowledge."

25. In 'Le Rationalisme Appliqué'.

26. To elaborate-"Compared with the Cartesian method; if one admits the existence of a general method of scientific knowledge, the doubt which is the first moment of that general method can never achieve specificity -

it's purely formal, not allowing the production of any correction and hence of any knowledge". (All this depends in the last analysis on the philosophical idea of looking outside knowledge for an object to serve as its foundation. - Lecourt, op.cit.).

27. 'Le Rationalisme Appliqué', p.51.

28. e.g. Bachelard points to the standardized chemical reagent.

29. It is here that Bachelard attempts (in 'Le Rationalisme Applique') to found the apodicity of scientific values in a psychologistic vocabulary.

30. Michel Fichant: 'L'Idée d'une Historie des Sciences' in 'Sur l'Historie des Sciences', ed. M. Fichant and M. Pêcheux, (1969).

31. Althusser.

32. 'L'Activité Rationaliste de la Physique Contemporaine', p.24.

33. 'L'Actualité de l'Historie des Sciences', (1951), p.6.

34. ibid. p. 10.

35. 'L'Activité Rationaliste de la Physique Contemporaine', p.26.

36. ibid. p. 25.

37. 'L'Actualité de l'Histoire des Sciences', p.9.

38. "Recurrence is then substituted for teleology - the concern for sources, ancestries etc.
(1) Teleological analysis treats the statements of sciencs as things. It dissociates them, separates and reduces them, links them together as container to content, or cause to effect. This reduction makes two confusions:
(a) that of the statements of the sciences with the object to which they refer.
(b) that of this object (the object of science) with things offered to perception, whereas the object of science is a theoretical, constructed object, an object 'in thought' and not a concrete thing given as the support of its perceivable properties.

(2) Teleological analysis rests finally on the confusion of the real and knowledge, in an empiricist mode which confers the properties of the real onto knowledge. Science is the disclosure and the formulation of the real. - This empiricism reduces the concept to the word (nominalism) - also to a formalism - it conceives the statements of science not as the registration and production of a concept, but as the formulation of a pre-existing real - succes-sive formulations only 'translate' this 'real' diversity, without affecting it in itself". - Fichant op. cit.

39. Brewster op. cit.

40. Cutler 'Theoretical Practise' 3-4, (1971), 'The Concept of the Epistemological Break', 63-81.

41. ibid.

42. ibid.

43. Brewster op. cit.

44. e.g. the 'Psychoanalysis of Fire', (1938), tr. 1964. - Psychoanalysis develops into a general 'poetics of revery' - close to Jung's theory of archetypal images.

45. Lecourt points out that the 'city of science' stands in for the 'Reason' of philosophers, but says of the psychologistic vocabulary in 'Le Rationalisme Appliqué' - "It is as if Bachelard hoped in this way to resolve a problem whose very terms were forbidden to him ever since he broke with the conception of a norm-producing Reason like the one constituted by the philosophical problematic" - he (weakly) attributes this to 'philosophical guilt', and claims (wrongly) that it is marginal.

46. Nor is it incompatible with a certain conception of Marxism, which similarly refuses philosophy anything more than an imaginary reality, reducing knowledge to a psycho-physiological faculty of the human brain - cf Godelier, 'Myth and History', New Left Review 69 (1971), 93-112.

47. c.f. "Founding the objectivity of rational knowledge on the union of experimental workers and the validity of rationalism on the consistency of a co-rationalism; founding the fertility of my learning on the division of the ego into an ego of existence and an ego of super-existence i.e. of co-existence within a cogitamus, is on the whole an ingenious attempt...but not wholly convincing." Canguilhem, Etudes p.205.

48. The way Bachelard uses examples demonstrates this. In the concrete example Bachelard takes up a position on a particular scientific development from within the science in which it takes place. Whenever he makes a general statement about the sciences he substitutes a constitutive subject in place of the concepts term in the couple used in the example, a subject which first appears in negative form as the psychological subject of error, but then positively as the scientific 'mind' of the advanced phases of the epistemological profile. - Brewster. op. cit.

49. For the theme of the impossibility of philosophical abolitions of philosophy, see Jacques Derrida 'De La Garmmatologie', (1969). Also Althusser in 'Lenin and Philosophy', (1971), pp.59-60.

The Meaning of Life and the Meaning of Words:
The Works of I.A. Richards.[1]

> "The proper study of mankind"
>   Could be
>   This proper realm to free
>   By ridding it,
>   Day by hour by minute,
> Of what deforms a mind.

> Richards, 'General Election'.

Throughout his work, Richards' standpoint is that philosophy is
never separable from life; and his concern is with communication. This
being so, it is easy to understand the wide range of his interests, for
philosophy, poetry, criticism, education, psychology and religion are
all subjects with something to contribute to the problems Richards has
chosen to deal with:

> Criticism, as I understand it, is the
> endeavour to discriminate between experiences
> and to evaluate them. We cannot do this without
> some understanding of the nature of experience,
> or without theories of valuation and communication.
> Such principles as apply in criticism must be
> taken from these more fundamental studies.
> 1967a, vii-viii.[2]

His standpoint (that philosophy is an act of living) and his main concern
(with communication) are of course logically interdependent, but they are
analytically separable.

The position that philosophy and life are inseparable is by no means
fully developed in the earlier works. In The Meaning of Meaning Principles,
Science and Poetry, and Practical Criticism it is peotry, rather than
philosophy, which is to be the saviour of the world (see Schiller, Chapter 5).
As he was confronted with more and more examples and types of misunderstanding
(in academic debate, in education, and in politics, as well as in literary
criticism), Richards became aware of the need to broaden his earlier
formulation. By the early thirties it had become language, and the philosophy
of language, which was our failing and our only hope of salvation (see
Mencius p.35 and Coleridge p.xi). The formulation was completed by the
second half of the thirties in Rhetoric and Interpretation:

> Words are not a medium in which to copy life.
> Their true work is to restore life itself to
> order. 1965, 134 (see also p.136).
> A deeper and more thorough study of our use of
> words is at every point a study of our ways of
> . living. 1973, ix (see also p.5),

and the same sentiments are maintained in the later works:

> Language is an instrument for controlling our
> becoming. 1955, 9.

This view of the world is interdependent with Richards' notion
of value. The relationship between value and criticism is too large a
question to enter into here. Suffice it to say that Richards' position

(which is expounded in Principles, especially the Preface, and which is only slightly modified throughout his work) is that criticism and life both entail judgements, and that our judgements depend upon our valuations. Richards would, however, be the first to admit that the shifts in the word 'value' here cause immense problems.

Richards' concern with communication has been worked out by dealing with the problems of human misunderstanding in all its many guises, Taking The Meaning of Meaning as a general theory of the problems of understanding and meaning, the later works can all be seen as to some extent specializations to deal with the various aspects of those problems. Principles, Science and Poetry, and Practical Criticism each deals with the problem of literary, and in particular poetic, meaning. Mencius is concerned with the problem of communication between languages, Coleridge again with literary meaning, Rhetoric with meaning in ordinary speech, Interpretation with understanding in speech and reading, the works on Basic English with the problems of translation, both in learning a foreign language and within a language, the poetry with communicating feelings and emotions.

The central problem in all this is the opposition between a monosemic and a solipsistic view of language. A monosemic view argues that (or finds it more profitable to act as though) words carry fixed meanings pre-assigned to them. A solipsistic theory argues that we, as speakers, writers, hearers and readers, give words their meanings, the words themselves being no more than fluid masses of associations. In which case, it is hard to know whether we communicate with each other at all, since our meanings for the same utterance may differ; moreover, we can make no judgements on language since the traditional criteria of value disappear.

This conflict is closely parallel to many other oppositions which have been drawn both inside philosophy and outside it: for example Aristotelianism/Platonism. In particular it is related to the opposition between positivism (with its ally scientism) and idealism. It is impossible for a positivist to adopt a solipsistic position, or for an idealist to hold a monosemic view of language.

Much has been talked and written of Richards' early scientism. The Meaning of Meaning and Principles are probably his most widely read books, and it is in these that Richards often offers a psychologism of the crudest kind, for which he has been rightly criticised. But to some extent this criticism has been unkind to Richards, for from his later works, and what he says in them about his earlier ones, it appears that the critics have over-reified some of his conceptualisations. What were taken to be descriptions of how the mind works turn out, on a more sympathetic reading, to be instead no more than ways to help us imagine, conceptualise, and deal with thought. Whether the fault for this lies with Richards' writing or the critics' reading is, for my purposes, irrelevant. The fact remains that, taken in conjunction with his later writings, the earlier ones are far less rigidly positivist than has been suggested.

This can also be justified to a certain extent by a close reading of th earlier works themselves; Richards may never explicitly state that his psychological images are no more than tools to think with rather than things to think about, but he comes close to it. One can certainly find many indications of a bias against positivism and monosemy, which surely argue against any charge of scientism. To take just one example from each of the earlier works:

> We ought to regard communication as a difficult
> matter, and close correspondence of reference
> for different thinkers as a comparatively rare
> event. 1972, 123.
> A   single word by itself, let us say 'night',
> will raise almost as many differentthoughts
> and feelings as there are persons who hear it.
> 1967a, 4.
> What an individual responds to is not the whole
> situation but a selection from it, and as a
> rule few people make the same selection.
> 1970, 37.
> The reception (or interpretation) of a meaning
> is an activity, which may go astray; in fact,
> there is always some degree of loss and dis-
> tortion in transmission. 1964, 180.

These   statements hold an incipient, if not an explicit, leaning
towards solipsism, which develops in the later works into an almost
purely idealist position.

Richards has not, however, spent all his time arguing for a purely
solipsistic view of language; outright solipsism is as unsatisfactory
a philosophy of language as outright monosemy. It is hard for us to
conceptualise any answer other than these two to the question 'How
does meaning work?' and I suspect that, as far as philosophy is
concerned, there is none. There may be no answer within the terms of
logic, but for practical purposes we need one, and most individuals
have no trouble finding one. Richards' workcan be seen as a working
out of just this progression: from a practical problem (misunderstanding),
through a philosophical investigation, to a practical solution.

The practical solution which Richards offers has the merits of
(comparative) simplicity, and some of the advantages of each of the
opposed positions. Its disadvantage, which it shares with all other
proposed working theories, is that if we investigate it at all closely,
if we try to make it do more than it was designed for, it proves to
contain the faults of both opposed views - an unrealistic fixity of
language on the one hand, and an exaggeration of our failure to
communicate on the other.

Richards' compromise, although it develops through his writings,
is in essence that provided by the context theory of meaning he first
expounded in The Meaning of Meaning. This allows words to be fluid in
their meaning, yet provides for their specification by their context.
By 'context' here Richards claims to mean something other than a
word's setting in a sentence (or a piece of discourse of any other size).
Rather he takes it to mean the way in which a word assigns its referent
to a class: the other occasions on which it has been used, the occasions
on which it has not been used, and so on. In other words, the history
of that word for the individual concerned (see The Meaning of Meaning,
pp. 52-59). Unfortunately, Richards' use of 'context' in this specialized
sense is not as consistent as one might have hoped. This is probably
because either sense of the word causes problems for the general theory:
the 'setting' sense suggests a position close to the Usage theory of
meaning, of which Richards is rightly scornful, calling it,

> On the whole, the most pernicious influence
> in current English teaching, doing more than
> all other removable errors together to inhibit
> the course of self-critical and profitable
> reflection about the conduct of thought in
> language. 1973, 174.

On the other hand, the 'historical' sense of context leads straight back into solipsism, for each individual's history of any word will differ.

These are not the only contradictions to be found in Richards' later works; but contradictions are to be expected in the attempted compromise of incompatible positions. Richards is aware of this as he shows by the limitations he is always ready to put on his theories:

> In thinking about how we think, our aim must
> be to perceive as distinctly as possible what
> we are doing rather than to arrive at any
> final-looking positive theories. As we do so
> a great number of theories that are too crude
> to sustain the examination and have only at a
> distance been supposed to apply, are discarded;
> and to be rid of them is a great gain. We may
> be left without any theory, but we are at least
> freed from the interferences of mishandled
> abstractions. 1973, 249.

Although we can arrive at no final logical solution to the question 'How do we mean?' it is still necessary to ask the question, lest a false and over-rigid view of the nature of language distort our view of its meanings. Richards is constantly reminding us, as we must constantly remind ourselves, that

> We shall do better to think of a meaning as
> though it were a plant that has grown - not
> a can that has been filled or a lump of clay
> that has been moulded. 1965, 12.

Martin Cantor.

Notes

1. This article was prompted by the issue of a second edition of
   Interpretation in Teaching 1973, London, Routledge and Kegan Paul, £5.
   I consider Interpretation to be Richards' best work, for it contains alm-
   ost all the major points which he makes elsewhere, in their most coh-
   erent formulation.

2. All references are to works by Richards (or in one case Ogden and Richards)
   unless otherwise stated.

BIBLIOGRAPHY

Ogden, C.K. and Richards, I.A.

    1972 The Meaning of Meaning: A Study of the Influence of Language upon Thought and of the Science of Symbolism. Orig. 1923. London, R.K.P.

Richards, I.A.   1932 Mencius on the Mind: Experiments in Multiple Definition. London, RKP.

    1943 Basic English and its Uses. London, Kegan Paul, Trench and Trubner.

    1955 Speculative Instruments. London, RKP.

    1962 Coleridge on Imagination. 3rd. ed., orig. 1934. London, RKP.

    1964 Practical Criticism: A Study of Literary Judgement. Orig. 1929. London, RKP.

    1965 The Philosophy of Rhetoric. (The Mary Flaxner Lectures on the Humanities for 1936). London, O.U.P.

    1967a Principles of Literary Criticism. 2nd. ed. (1st. ed. 1924). London, RKP.

    1967b How to Read a Page: A course in Effective Reading with an Introduction to a Hundred Great Words. Orig. 1943. London, RKP.

    1970 Poetries and Sciences. A re-issue of Science and Poetry (orig. 1926) with commentary. London, RKP.

    1973 Interpretation in Teaching. 2nd. ed. (1st. ed. 1938). London, RKP.

Schiller, J.P.   1969 I.A. Richards' Theory of Literature. London, Yale U.P.

A Note on 'Mooning' and 'Streaking', as forms
of non-violent Protest.

Events taking place in the very week that this issue of J.A.S.O.
goes to press have prompted me to submit a few ethnographic notes and
preliminary comments from a study which I am making of various non-violent
forms of protest, soon to be completed. In particular I am interested in
the use of nudity and vulgarity in situations of conflict. Some aspects
of this research were included in a paper entitled 'Sexual Insult and
Female Militancy'.[1]

The phenomeno which has been brought to our attention now is
popularly known as 'streaking'. Although this takes a specific form, it
can be seen as belonging to a wider class of behaviour which occurs when
normal modes of exhibiting deference and mutual attention are inverted. Often
such inversions originate as expressions of hostility, but they may take on
secondary characteristics involving questions of identity of individuals
and groups. There is no space in this present brief note to give many examples
of the wide variety of forms which this mode of communication takes in
different space and time, but it may be of interest to draw attention here
to one type of vulgarity, known as 'mooning' which emerged in the United
States in greater frequencyin the late 'fifties and sixties'. I give as
examples two instances, one of which was initiated by females and in one
of which the predominant actor was a male.

In the autumn of 1966, in a co-educational residential college in
Iowa, the girls and boys both lived in one H-shaped building. It was a
three-storey building with a block of rooms for girls on one side, and a
block for boys on the other, joined across the middle on the ground floor
by single-storied common rooms. Each room occupied by girls had a large
plate glass window. Twelve such windows faced the rooms occupied by the
boys opposite. I collected the following account from one of the female
residents.

> 'At night, after we were locked in at midnight,
> stripping and dancing in the windows took place
> to tease the boys. Normally this never went beyond
> bra and pants. One night, when the ring-leaders
> were happy-drunk, instead of a peep or strip show,
> it was decided to do a 'mass moon'. In each room
> one girl was ready in the dark, standing on a
> chair bending over with her bottom pressed against
> the window. Another girl then flicked the light on
> for a few seconds. It was like a dare. It was
> considered naughty and wicked - they decided to
> have a go.'

When a closed window is involved as in this case, the activity is
sometimes known as 'pressing a ham', 'hamming' being a variant for 'mooning'.
My informant stressed the fact that the girls at the College referred to
were normally considered to be very 'moral'; and little actual sexual
intercourse took place - for one thing, because at that time there was no
opportunity. She felt that 'mooning' was essentially a female manifestation,
and that when men engaged in this activity, they were modelling their behaviour
on that of women. I will return to this aspect below.

My second example is a case I recorded of a young man who was involved in
an episode which was dubbed by my informant as 'the Mooning of Yale'.

'In the fall of 1966 there was the first vague
rumblings of drug problems on the campus of
Yale.   At least a couple of people were involved
in what amounted to a kind of raid and the first
publicity began to appear in such well-known
publications as the New York Times.   One freshman,
I remember, was quoted as saying that, by the end
of his four years at Yale, he would have turned
on the whole campus with his little chemistry set.
So this is the background for the mooning incident.
One of these people who had been more or less
confronted by the administration with the possibility
of facing some sort of disciplining as a result of
the drug problem was subsequently involved in the
mooning incident. It happened like this:

> One afternoon he was standing in the window
> of a five-storey building overlooking the old
> campus, where all the freshmen lived in a kind
> of quadrangle, all together. On a few days
> before this he would sit in his window and just
> perch out like a bird overlooking the campus.
> When he perched he would squat down and just
> sort of stare very intently in the same direction
> that he eventually mooned in. He was doing this
> conspicuously. He was sitting right in the
> window-sill, perched with his toes curled over
> the edge looking out. But on this day, instead
> of perching like a bird, he waited for what he
> must have thought had been the opportune moment,
> turned around, and mooned in the direction of the
> old campus where most of the University lay.

This   was the   subject of great conversation and
laughter thereafterwar??   It was pointed out that the
opportune moment came when apparently two girls were in
visible sight of the moon and this seemed to make this
sort of larger protest at the University more effective.
It's hard to say whether or not the University itself
had a kind of a female aspect.'

When these incidents took place, mooning was well-known in various
parts of the States, but not a common practice. It was much talked
about in the early sixties in schools, particularly in boys' schools,
being often used as a boast. 'A would say to party B that he was
going to moon party C in order to put down party C and to elevate himself
in the eyes of his peers'. The seeming paradox of persons acting in
ways which would normally be thought to degrade them, in order to
degrade others, I have discussed elsewhere.

These practices allowed the actors to be readily identified, but
in parts of the United States, at about the same time, individuals
sometimes engaged in 'doing a moon' from the windows of passing cars.
A similar kind of activity, of milder impact, was known as 'Drop Trou',
when males let their trousers fall. This, although sometimes enacted
at parties and the like, is more commonly found in its verbal form
'Wow! He is really going to Drop Trou when he hears about this!', and
primarily expresses extreme surprise and astonishment.

What sparks off 'mooning' and similar behaviour? In the case of the girls in Iowa, in answer to my questions, the following points were made:

'There had to be men /‾witnessing this_7 and we had to
think they were there. It had to be witnessed and we had
to think there were witnesses - we wouldn't have done it
to women except perhaps as a joke. There didn't have to
be many men - not specific men. We just had to be under
the impression that men were witnessing. It was not
merely a friendly gesture towards the boys. It was a gesture
of independence. The men were free to come and go and we
were locked in. It was a gesture: 'Look, we are locked
in because of you'. We resented being locked in. It was
a defiance against authority: 'You locked us in to prevent
this'. We weren't hiding it from authority, we were like
shop-lifters who wanted to attract attention, but didn't
really want to be caught.'

The response to my enquiries as to the general background and motives behind the events at Yale (which was made in the knowledge of some of my general conclusions concerning the use of vulgarity in protest movements) was as follows:

'As much as I can see just from knowing the people involved,
this act was a total expression of indignation and rage,
where very young people of eighteen years old were involved
in all kinds of pressures in a changing environment. The
actual date of the incident was in the fall of 1966 - it
was a very confused time and clearly the means of expressing
such indignation and rage were quite limited. It's certain
that /‾the man who mooned_7 had a lot of reasons for to
feel closed in, confronted by things that were different,
confronted by things that he didn't like at all. He was
subjected to an environment in which he was sort of the
lowest man on the totem pole, without much positive
feedback or ego satisfaction, and there was a strong craving
among his age-group in general to have some sort of social
recognition, that simply wasn't there. And most of them
were uprooted from their home environments as well, where
they had been well-known people. And it had to have involved
some kind of desire for recognition. At the same time /‾it was_7
a kind of muted protest against all of these pressures
which built up so fast right at the beginning of University.'

In the over-all American scene of the time, the student group may have been in the position of what Edwin Ardener has termed a 'muted group'. Such a group is one that has no ready means of expression for variant views which it wishes to present, which would be given a hearing in any effective sense.[2] He has suggested that women may perhaps be best seen as a 'muted group', and I have shown elsewhere that in certain circumstances when such a group feels that its identity is threatened, if resort to direct violence is ruled out, it may invert the normal modes of expression by the use of obscenity and vulgarity (see 'Sexual Insult and Female Militancy'). The evidence suggests that mooning caught on in the United States at a moment of transition, when the stereotype of the 'all-American Boy' (or Girl), the well-groomed, bobby-soxed, conformist college student, relatively uninvolved in political movements, no longer fitted the 'model' which the student had of himself or herself. This 'identity crisis' led to frustration which turned into hostility, but at this period of time, the release of this aggression through

direct action, and in particular through violence, was generally ruled out. As I have found elsewhere, in parallel circumstances, the use of vulgarity was resorted to. Sometimes, a display which starts off as an expression of hostility may acquire secondary properties. It may, for instance, for a while become a badge for group identity.

Mooning, I suggest, faded away when students eventually took direct action. In due course, violence itself became unacceptable, partly, perhaps, because some of the deep seated frustrations and fears were lessened. This may have been due, among other things, to the recognition of a new model for students, and because the isolation of students from University authorities was decreased, and also because the call-up for military service in Vietnam was abandoned. Probably also the effects of violence were in the actual event, found to be unacceptable for many.

The eruption of 'streaking' is too recent for a thorough analysis of its particular history. It is possible to speculate, however, and to suggest that, perhaps, the frustrations felt by the seeming impossibility of affecting the resignation of President Nixon, despite pressure of public opinion, may have played a part. A new generation of college students is on the scene, trying to establish its identity, probably wishing to avoid the violence which became associated with the college generation immediately ahead of it. That the mode of expression known as 'streaking' has been copied in various parts of the world outside the United States, may be due to similar frustrations over the inability of the young to affect world affairs, or be due to a desire to identify with those who have cause to feel hostile, or for many other reasons. 'Streaking' may be regarded as a 'dare' or a 'game', as an attempt to establish a reputation for courage in defying more commonly accepted modes of proceeding, but it may also be a form for expressing anxiety and hostility. If such expressions are not responded to, then the possibility of ensuing violent action is not something which should be overlooked.

1. Ardener, Shirley, 1973. Sexual Insult and Femal Militancy, Man, September, Vol.8, No.3, pp. 422-440.

2. Ardener, Edwin, 1971. Belief and the Problem of Women, in The Interpretation of Ritual (ed. J. La Fontaine). London: Tavistock.

Shirley Ardener

BOOK REVIEWS

Logik und Leben. Kulturelle Relevanz der
Didinga und Longarim, Sudan. Andreas Kronenberg.
Wiesbaden: Franz Steiner Verlag, 1972. Pp. 191,
41 illustrations, 31 diagrams, 3 maps.

As Government Anthropologist to the Republic of the Sudan, Herr Kronen-
berg worked among the Lango-speaking Didinga and Longarim between 1958 and
1960. Those were years of comparative peace for the Sudanese Nilotic
peoples: it seems likely that the ethnographic data contained in this book
have, by now, mainly historical value.

They are presented in a deliberately idiosyncratic way. We start
with theoretical considerations, and an elaborate mathematical analysis
of kinship terminology and end (after chapters on the cattle idiom, sexual
relations, age-stratification and magico-religious beliefs) where the authors
of most field monographs still feel obliged to begin, namely the geographical
and economic base. Theory first, ethnography later; the pitfalls are obvious,
but the method does have the advantage of honesty: Herr Kronenberg at least
makes no bones about where his real interest lies.

It is a philosophical interest - Herr Kronenberg's intellectual patrons
are Frege and Wittgenstein, Lévi-Strauss being not so much as mentioned in
the bibliography - in what Evans-Pritchard used to call problems of trans-
lation. Empirically observed facts can have significance, "cultural relevance"
only within a given semantic system. But semantic systems are culturally
discrete; thus, even within Western anthropology, "common-sense terms" bear
the imprint of Empiricism in England, of Romanticism in Germany, of the
Enlightenment in France...And even to the extent that a common "scientific"
terminology has been evolved, that terminology is still a classificatory system
of the same order as those it seeks to interpret: a system, in its own way,
as closed and self-confirmatory as that of the Azande. Seen in this light,
whole volumes of carefully checked ethnographic data can have no more
significance than so many compilations of statistics about individual moves
in chess: they will not, Herr Kronenberg argues, enable us to deduce the
rules of an unknown game - the rules, say, of a kinship terminology which
will enable us to produce the correct term for a particular genealogical
specification as unerringly as the native speaker.

Put in these terms, the problem is not soluble; which may lead us to
suspect that it is not in fact correctly presented. It is possible to
discover the rules of an unknown game, but only by the same kind of mental
process that makes possible the discovery of an invisible molecular struc-
ture in chemistry: namely by the methodical testing in the light of observed
facts of what started out as a series of intelligent and consequent guesses.
But there does remain the problem of finding a language for the guesses,
such that they can be tested in other semantic contexts; and Herr Kronenberg
believes that he has found such a language for Didinga kinship terminology.

The Didinga themselves express such things, as good Nilotes should,
in terms of cattle; or rather, they express agnatic relationships in this
way. They recognize, in fact, two modes of kinship. "Natural" kinship is
given by the fact that women bear children; it is thus definable only in
terms of female fertility, and can be transmitted only from mother to child.

All other kinship is "cattle kinship" - i.e. it depends on a
woman's fertility having been legitimately acquired in exchange for
bridewealth cattle. It can therefore be expressed in terms of such
cattle, or rather in terms of proportions of an ideal unit, the
total herd, which corresponds to a brother-sister pair. At each
generation, half a man's inherited cattle are given in exchange for
a wife; but he, in turn, receives half another man's herd in exchange
for his sister, thus reconstituting the ideal complete herd.

Agnatic kin are thus "cattle" kin: the father-son relationship,
for instance, has been created only by the father's legitimate
acquisition, by means of cattle, of a woman's fertility (as among
the Nuer, there is nothing to prevent a woman from becoming a pater
by the same means). And the term applied to a category of kin
denotes, precisely, the number of "cattle-links" (i.e. agnatic links)
between members of that category and the speaker; "natural" (i.e.
uterine) links being immaterial for the purpose.

This Herr Kronenberg chooses to express, ingeniously and elegantly
enough, in binary notation; the symbol I corresponding to agnatic or
"cattle" links, and the symbol O to uterine or "natural" ones,
while positional values correspond to what European terminology would
call generations. Thus:

Genealogical specifications			Binary expressions	Number of agnatic links	Term used
B (FS)	, Z	(FD)	1		
FDS	,	FDD	10	one	A
FDDS	,	FDDD	100		
		etc.			
BS (FSS),	BD	(FSD)	11		
FSDS	,	FSDD	110	two	B
FDSS	,	FDSD	101		
		etc.			

This, is, in itself, a pleasing solution, certainly more illuminating
than labelling such a terminology "Crow" or explaining it by a
"skewing rule". Herr Kronenberg rather handsomely attributes his
reasoning to the mathematicians of ancient Egypt (who, he believes,
derived binary notation from their own kinship reckoning) and backs
it up by a great many pages on the nature of binary series. These
can, of course, be skipped by the mathematically unsophisticated;
but the skipping, inescapably, brings up another question.

Could not the binary notation itself be skipped? Is it, on the principle of Occam's razor, really necessary? Or has Herr Kronenberg simply succumbed to a more seductive form of the mathematical _fata morgana_ that seems at times to bedevil modern anthropology? He has rightly condemned uncritical statisticism; he has had the good taste to ignore Lévi-Strauss's propensity for littering his pages with pseudo-algebraical formulae; but if the Didinga can operate their system by differentiating between "natural" and "cattle" links and counting only the latter, could we not get along with some pair of terms like "uterine" and "agnatic"? Natural language may have its limitations, but the premature importation of mathematical symbols can only compound the problems of translation, besides leaving us no language in reserve for higher levels of abstraction if we should ever reach them. So keep your powder dry, Herr Kronenberg, and save up the binary numbers; with luck, we might really need them some other time.

<div align="center">Eva Gillies</div>

Rethinking Kinship and Marriage.   Rodney Needham (ed.)
A.S.A.11. London, Tavistock, 1971, £4. (hardback),
£1.95 (paperback).

Whatever the merits of the rest of this book, clearly most interest is going to be aroused on the gossip circuits by Needham's long, long "Introduction" (p.xii to p.cxvii, as it were), and his following chapter of "Remarks". The first takes a swipe at a number of targets but the bulk of it consists of a history of the prescriptive marriage argument and the various indignities that have been perpetrated in the name of truth, theory, and the like. On this let us record that Needham is unquestionably right, his opponents are undoubtedly disreputable, and the whole thing is becoming a bore. The temptation to use such an opportunity for elaborate self-vindication is obvious, and one can sympathise, but there is a time to leave well alone. Needham is not content simply to slay the dragons that have so ridiculously plagued him over this issue, but he feels it necessary to turn his sword, in the name of 'competence and authority' on Radcliffe-Brown, Lévi-Strauss, and so help us, Fortes. Perhaps one can be forgiven for thinking that the lady doth protest too much in this instance. Needham's response - that he is 'right' - is of course unanswerable. But then so was Joan of Arc and no one thanked her for it.

Having destroyed the classic authorities in the first salvo Needham then murders kinship itself in the next; Southwold kills kinship terminology in chapter two; and Rivière finishes off marriage in chapter three. Since we are now left with nothing to discuss it is not surprising that in chapter four Leach has to resort to another piece of convoluted cleverness on the subject of phonology and affect, or the "mama/papa" syndrome. It's all good clean fun, and at least he concludes that there might be some

signals of a 'species-specific, cross-cultural type'. Incidentally, it borders on the hilarious that after so many shining reputations have had to go down in the name of competence and authority, that the book should be dedicated to Edmund Leach!

Francis Korn continues the needless Lévi-Strauss bashing that is so fashionable nowadays, and is, I suppose, a natural reaction to the rather silly adulation paid earlier. It seems we don't know what an elementary system is anymore, or rather that Lévi-Strauss doesn't know. Bateson showed that the Iatmul had a series of contradictory structures of marriage, and Korn nicely demonstrates that only one - marriage with FMBSD - is totally compatible with the structure of five assymetrically related descent lines as given in the terminology. 'Sister exchange', for example, and FZD marriage are not. This is very interesting, and clearly reflects a transitional system as Bateson saw (although he got the transition the wrong way round, I think). In The Keresan Bridge I describe a similar situation among the Keresans where terminology reflected both "Crow" and symmetric tendencies, and marriage was preferred between a man and a woman of his mother's father's clan (MFZDD). Of course, in a simple symmetrical system MFZDD, FZD, and FMBSD are one and the same, and therefore, following Lévi-Strauss, I interpreted this as a transition from elementary to complex structure (i.e., in this case, Crow.); these two categories being 'trends' in any case. What I think Korn has here is an "Omaha" version of the same trend. I cite all this, (and I could go on at length about the details), simply to protest against using this kind of material merely to put Lévi-Strauss down, rather than, as we are so often admonished to do, "getting on with the job". This arid 'anti-classificatory' business largely misses the point and I suspect its motives.

Forge adds some notes on the Sepik to Korn's article. McKnight clears up some points about the Wik-mungkan, showing in yet another instance that at base there is a simple, symmetric ("two-line") system. Wilder works out how many descent groups the Purum have. Well, that's the way the Old Kuki crumbles. It's a bit of a relief to get to Beidelman on Kaguru incest notions and Fox (the other one) on sister's children as plants and other analogies. All very lively, and a good read if you can digest the indignation.

Robin Fox

African Culture and the Christian Church: An
Introduction to Social and Pastoral Anthropology.
Aylward Shorter, W.F. Geoffrey Chapman, London,
1973. (also in paperback edition)

In his introduction, Father Shorter tells us that the term 'pastoral anthropology' is likely to cause some confusion, especially among professional anthropologists. But, he explains "the term aptly symbolizes the marriage of two disciplines: social anthropology and pastoral theology". We would ask who officiated at, and who consented to, this marriage?

There are missionary groups, apart from the Pastoral Institute
of Eastern Africa at Gaba, who are trying to find out how the study
of anthropology and sociology might help to broaden Christian pastors'
understanding of peoples of the world and to set new developments in
pastoral work into motion. These groups would not wish to make an
underlabourer out of the discipline of social anthropology, nor would
they simply moralize with reference to various church practises and
legislation, going against the spirit of Vatican II, which represented
a sincere appreciation for genuine religious and ideological values
of non-western and non-Christian peoples.

This book represents a double problem: from the pastoral
theological point of view, the Church in the past has not always
handled problems of tradtional belief and religious practice in the
way Fr. Shorter suggests. From the social anthropological point of view,
hardly any of the points raised would be recognizable as social anthropology
to a professional anthropologist. A concrete example of this is the way in
which the notion of marriage is handled. We would have thought that the
contribution of social anthropology to the question, ¢an other forms of
marriage be Christianized?' would be (1) the facts about other forms of
marriage and different forms of kinship systems throughout the world and
(2) the questions about forms of marriage which an anthropologist might
raise. Most modern anthropologists would hold the view that all forms
of marriage are honourable; the Christian theological view is that only
monogamy is acceptable. The anthropologist would ask, 'what were the
forms of marriage existing in the Holy Land at the time of Christ's birth?'
and 'to what extent is monogamy an imposition of Roman law onto Christianity?'
In such cases, the pastoral question largely boils down to the anthropological
ones, but the pastor would ask (going further than the anthropologist), does
the present legislation of the Church take account of the fact that among
many peoples of the world, marriage is not a contract between two individuals,
transferring "uxorial" rights, but an alliance between two groups of people
involving an intricate constellation of mutual rights, duties, assistance
and prestations? In fact, it is these kinds of considerations which
recently prompted Professor D'Arvack in Rome to brand the Roman Catholic
Church's legislation on marriage as "anachronistic, inhuman and grotesque".

From the point of view of the pastoral theologian, Fr. Shorter does
not go to the root of the problem, he merely skates over the surface of
the problem within the framework of present day Church legislation,
although he seems aware (p.176) that other suggestions have been made.
In so doing, he does both pastoral theology and social anthropology a dis-
service and lumps it all under the heading of a "new discipline" called "
"pastoral anthropology" which does not exist. In one way, this book can
be seen as a missed opportunity; it could have been an attempt at a break-
through, instead, it is a re-hash of old norms and legislation with new
names attached.

While we can readily undertand that Fr. Shorter's book is directed
towards an audience of non-anthropologists, we regret that in the name of
social anthropology (which we understand he studied at Oxford) he does not
demand of laymen that certain intellectual efforts be made to understand
such complex notions as, for example, "social facts". We know of few
anthropologists who would attempt a three paragraph 'potted definition' of
such a fundamental social anthropological concept (pp.6-8). Many
anthropologists would object to the looseness with which terms like 'social
fact', 'behaviour', 'network', 'structure', 'adaptation', 'product', etc.
are used. It does not seem to matter to Fr. Shorter which anthropological
theory or methodology he uses to "prove" his points. Each chapter represents

a curious mixture of ideas: in one paragraph, Malinowski is brought
to the fore, in the next, Lévi-Strauss, then we might find R.G.
Lienhardt or Evans-Pritchard cited to "prove" (p.45) that non-western
religions are really religious! What we protest against can be
summed up in this statement of Fr. Shorter's, made on p.7:

> In social science it is customary to speak of
> sociology and social anthropology, but these
> terms refer to differences in emphasis and
> method rather than to any really significant
> differences between the two.

We are here told that theory and method (which we thought selects
what 'facts' are, which selects what facts will be examined, which
selects how they are examined) are not really significant. This
is simply naive and we disagree, especially in cases such as that in
the chapter on ritual where we find such disparate theoretical and
methodological views as those held by Malinowski, Mary Douglas, Radcliffe-
Brown and Victor Turner all cited to "explain" the same phenomenon.
A study of each of these anthropologists' methodology and explanations
of ritual might well enrich the understanding of any non-anthropologist;
might introduce him to the complexity involved in explaining sequences
of human actions. If nothing else, such study would demonstrate the
care and concern which anthropologists have for the categories and
classifications of others, or it might at least indicate that for some,
anthropology is a life-work. But Fr. Shorter, no doubt unintentionally,
neglects to give such impressions; rather, he gives the impression
that social anthropology is something which can be "mugged-up" over a
short period of time and then applied, carte blanche, according to the
whims of such 'students' of the discipline.

For example, on p. 152 ff., we find that the foreign western
classifications of religion/worship/sacred and medicine/magic/secular
are superimposed onto African religious traditions. Moreover, Fr. Shorter
suggests how judgements may be made by the African pastor with reference
to traditional forms of belief and worship as to whether they are against
"faith", i.e. Christian religion. Any consideration of the genuine
"salvation-value" (in the Christian sense of the word) in non-western
religions, clearly recognized by Vatican II, seems to have escaped his
notice. He does not, anthropologically or theologically, really go into
African religions. He merely puts forward a framework for judging what
is "good" and "bad"; a regression to antiquated missionary attitudes.
Anthropologists have long known, at least in principle, what the
difference is between making judgements and describing what happens in a
society.

Finally, we should like to point out David Pocock's clear distinc-
tion, made in Social Anthropology in 1960:

> It is evident at the outset that the anthropologist
> working in another society (or in his own society
> regarded as "other") must take a certain stance quite
> different from that of, say, a government official
> or missionary, who is concerned to bring about change
> in accordance with certain beliefs which he holds (p.86).

of optimism - which seems appropriate. One would have liked a long
reflective conclusion. Instead, the last chapter (which is only ten pages
long) hurriedly brings us up to date, and quickly glances at the contrasting
situations in France and America.

Some will be slightly disappointed that the survey does not end with
a more positive or theoretical section. But all research is conducted with
certain limits imposed, and it would be asking too much to have expected a
two year period of investigation to have lead to a bolder type of climax.
Miss Henson has served us well in covering the ground in a more descriptive
way, so we now have a useful starting point from which to raise larger
issues. Indeed, her book is never a catalogue, so many of the problems one
would like to follow up have already been indicated by the author. We could
ask, for example, what is the significance of inquiring about the relations
of social anthropology and language? What have been the consequences of
their separate development and why is it important to relate them more
closely? When the latter problem is answered, which is the best means of
linking the two fields?

There are plenty of facts in Miss Henson's history to set us thinking
about these difficulties. If we say, for instance, that language is
important to us because we need a source of ideas to help us tackle semantic
problems more efficiently, then the framework for our research obviously
becomes social anthropology, language, and meaning. And now we are presented
with some rather more precise problems. Malinowski's linguistic work did
not lead to an anthropology more concerned with meaning whereas Evans-Pritchard's
more general sensitivity to language did. On an international scale, the
discipline has been transformed by Lévi-Strauss' idiosyncratic vision of the
role of 'linguistic' models in the human sciences; nothing of equivalent
potential has been produced by the 'linguistic anthropology' of the United
States. It is difficult to envisage 'ethnographic semantics' revolutionising
the subject. If social anthropology and language are to be related,
'linguistic anthropology' is perhaps not the best way of doing it. Even so,
we can still ask whether Lévi-Strauss' use of language has led to a better
understanding of meaning, or whether its dominant tendency has been anti-
semantic. Perhaps, too, it is not out of place to remember that linguistics
has been less successful with semantics than with any other phenomenon.
This being so, might we expect more valuable guidance by looking to linguistic
philosophy than to linguistic theory?

There are lots of large problems like these that need to be considered.
That they are not part of the problem Miss Henson set herself does not lessen
the value of her book. One could not begin to grapple with such topics
without a knowledge of the historical background. Social anthropology and
language was a slice of our development which had not previously been
charted, and those interested in the sorts of issues I have outlined will
be in Miss Henson's debt for her covering this ground and laying bare some
of the landmarks.

Malcolm Crick.

J.W. Berry & P.R. Dasen (eds), <u>Culture and</u>
<u>cognition: Readings in cross-cultural psychology</u>.
London: Methuen, 1974.

The publication of this collection of papers should allay the
fears of those anthropologists who find themselves overtaken by visions
of Human Relations Area Files, tortured ethnographies and coughing
computers whenever the word "cross-cultural" is muttered by their
uncautious colleagues. It demonstrates once again that not all
psychologists are totally insensitive to the problems of translation
and cross-cultural comparability, that some are aware of the role of
situational factors, and that there are even a few psychologists who
feel that numbers are not essential to the conduct of a science.

This volume has brought together some of the more important papers
on cross-cultural studies of thinking. It contains Fishman's impressive
review of work on the Whorfian hypothesis, and, by including papers by
Gladwin and Sturtevant, may serve to remind psychologists that ethnographers
are also interested in similar topics. There is also a very striking
paper by Cole and Bruner on the potential problems of inferring cognitive
differences from differences in behaviour. The last third of the book
is devoted to papers on Piagetian developmental psychology. Among these
there is an early essay by Piaget and a good review of the field by
Dasen.

The introductory essay by Berry and Dasen raises several important
theoretical issues. Instead of engaging in empty name-dropping and the
passing genuflections that cross-cultural psychologists usually make
to aspects of theory, these authors have taken the trouble to detail
the contributions of early writers on the subject, and have been so
bold as to offer some thoughts on the problem of cross-cultural
comparability. The problem of comparability is without doubt the most
serious problem confronting cross-cultural psychology. Yet for all the
attention it has received one would think that researchers in the field
regarded it as the piffling decision of an over-zealous touch-judge
rather than what it really is, the rule upon which the entire cross-
cultural game depends. In discussing the problem of comparability,
Berry and Dasen elaborate some ideas presented in an earlier paper by
Berry. They propose among other things, that behaviours in different
cultures be matched in terms of their "functional equivalence". This
solution is clearly inapplicable in several areas. What is more, where
it might be appropriate, it would merely serve, like slum clearance, to
remove the problem to another quarter.

This volume will certainly be of value to anthropologists and
psychologists. It will show anthropologists what has been happening
in the study of cognition and, more importantly, will enable psycholo-
gists to take stock of what they have produced thus far. It seems
likely that cross-cultural psychology will begin to abandon its strictly
comparative approach, and that it will turn instead to the study of
relationships between behaviours within a community. We may still see
the day when cross-cultural research is conducted by people who enjoy
a psychological way of thinking and who, but for their concern with
data and the elimination of competing explanations, could be taken to
ethnographers.

Peter Collett.

F.G. Bailey (editor), Debate and Compromise:
The Politics of Innovation. Oxford: Basil
Blackwell, 1973, 343 pp., £5. (Pavilion Series).

A critique of Shakespearean humour based exclusively on synopses of
the first acts of the "comedies" would match this book for intellectual
teasing. Each ethnographic chapter begins in a new location; few readers
will want to be "just getting into it" so often in a single work. On the
one hand we are told that, in effect, ethnography is theory (p.254);
on the other, there is little to convert the reader who suspects that,
instead, he is being led to ultimate generalities distilled from these
ethnographic "shorts". There is clearly more to the complete ethnographics
than such synoptic presentation displays; some, at least, surely deserved
separate and full-scale publication. Moreover, so strong a resemblance
to Gifts and Poison suggests the birth of a new methodological oligarchy
(compare "ekistics", "cantonometrics"); though, encouragingly in a sense,
the vagueness of the concept of "innovation" and the brevity of the
editor's summary show that no restrictive constitution has as yet been
drawn up.

There is clearly much here that is worth having, and in far more detail.
As it is, arguing from Wolf to the peasant will not suffice: the peasant
should be at liberty to falsify Wolf (and others). Specifically, the
symbolic universe of each community tends to receive stunted treatment
at best. Heppenstall suggests that in St. Martin the "fringe members'"
inability to "participate in traditional customs of reciprocity "
leads them to "think of their labour in terms of cash return". With a more
exhaustive discussion of traditional reciprocity customs, we should
perceive the "fringe" in diachronic context. One wonders whether some
so-called innovations might not be most usefully considered in relation to
changes, not in the reciprocity system itself, but in the mode
of its realization. "We" and "they" remain opposed, while the
specific range of reference changes; in the essays on Saburneda and Gema
we see how new means can serve old ends (such as family solidarity). But
to relate "change" to a continuity of any kind requires fuller ethnographic
presentation of traditional practices and beliefs. In the East Tyrol Essay,
the whole emphasis on the "local council" demands supportive data on the
composition of such a council and on the traditions which the members
support or contravene. Similarly, in Barrett's assessment of informant
views on the failure of the Aiyetoro fish-ovens (pp.260 ff.), the
ethnographer's "practical" objections may be valid, but we have no means
of judging their acceptability in Aiyetoro terms. Barrett surely does not
mean that one type of explanation is exclusively wrong or right; but the
inference might easily be drawn here.

Bailey's view of crisis as loosening the minutiae of normative
behaviour, thus creating greater freedom to innovate, is falsifiable and
clearly delineated, and could provoke useful discussion of other ethnographies.
However, the relation of the society to the source of crisis raises further
questions of bounding. Crisis may lie in internal smugness as much as in
internal threats: external grandeur may go with internal stagnation. But
Parkinson's Law is falsifiable too.

Michael Herzfeld

SHORTER NOTICES

When the Golden Bough Breaks: Structuralism·or Typology? Peter Munz.
Routledge and Kegan Paul. London. 1973. £2.25. xiii, 143 pp.
Professor Munz's historical approach to myths consists of classifying
them into typological series. The progression in each series from
more general to more specific versions yields the historical and
inherent meaning. There are some insights on the way, but on the
whole they do not justify the journey.

Urban Anthropology: Cross-Cultural Studies of Urbanization. Aidan
Southall (ed.). Oxford University Press. London. 1973. £2.25. vi, 489 pp.
A collection of essays hoping to deal with questions like 'What is meant
by terms such as 'urban' and 'urbanization'?'and 'Is there a clear
rural-urban dichotomy?' Particularly interesting are the contributions
by E. Bruner and O. Lewis; but even these are of small-scale interest,
dealing with the minutiae of social life. The 'cross-cultural' emphasis
of the title is noticeably absent, but the book does offer a collection
of sound ethnographies.

The Translation of Culture: Essays to E.E. Evans-Pritchard. Thomas O.
Beidelman (ed.). Tavistock Publications. London.1973 (paperback).
£2.50. ix, 440 pp. This excellent collection of articles (reviewed
JASO vol.2 no.3) is now available in paperback.

Cause and Meaning in the Social Sciences. Ernest Gellner. Edited by
I.C. Jarvie and J. Agassi. Routledge and Kegan Paul. London. 1973.
£3.25. ix, 228 pp. A collection of well-known articles and unknown
book reviews in the essayist tradition of Voltaire, Heine, Twain, Butler,
Shaw and Borges, according to the editors. Anthropologists will not find
much of any interest here. Social scientists, no doubt, will be
fascinated and impressed. English style sui gellneris and irritating in
the extreme.

Race. John R. Baker. Oxford University Press. London. 1974. £6.50p. xviii,
625 pp.Baker's interest is 'with the question whether there is reality behind
the idea of race'. From the point of view of sheer industry one can have
nothing but praise for his attempts to rectify this state of affairs. If
only his appreciation of social anthropology was better founded: ' A language
may reveal its superiority or inferiority...by the scope of meaning attached
to its words'! Remarks like this throw doubt on his controversial conclusion,
'One must deny...the "fine dictum of morality" that men are everywhere the
same'. Criticism aside, however, Race contains as comprehensive a survey
of the subject as could be expected in one book. The work is also
entertainingly illustrated.

Urban Ethnicity. A.S.A. conference edited by Adrian Mayer £4.50p. Tavistock
Publications. London. 1974. xxiv, 391 pp. 'Urban ethnicity' involves the
study of 'the anthropology of the complex structure of the new state'. The
anthropologist's job is therefore to deal with 'the socio-cultural problems
raised by the developing interdependence between these parts and by the
processes of socio-cultural change involved in this development'. Such an
approach suits the current interest in group/group relations, but is hardly
likely to provide 'heuristic and theoretical considerations' of 'major
importance' to our discipline: the functionalist tone of many of the
contributions, the general concern with 'definition', the existence of much
jargon, and the occurence of simplistic remarks of the type 'I (Deshen)
have operated with a conception of ethnicity as a strategy whereby people
set bonds of inclusion or exclusion', all suggest the extent to which 'urban
ethnicity' is a species of that type of sociology which dates from Homans'
The Human Group (1950). What is so surprising, given the 'heuristic' and
'theoretical' pretensions of the work, is that so many contributors write as
though their subject is only just beginning. There are articles, amongst
others, by Mitchell, Parkin, Lloyd and Schildkrout.