

The idea for this Journal has come from the graduate students of the Sub-Faculty of Anthropology at Oxford: in particular from those at the Institute of Social Anthropology. Papers given at graduate seminars, and preliminary ideas arising from work for the Diplomas and higher degrees, very often merit wider circulation and discussion, without necessarily being ready for formal publication in professional journals. There is a need for some intermediate form of exchange. The Oxford University Anthropological Society has agreed to act as publisher for this venture and has established a Journal Sub-Committee for this purpose.

We hope to produce one issue per term. Articles will be welcomed from students in social and other branches of anthropology, and from people in other disciplines interested in social anthropology. Letters, comments, and reviews will also be welcome. It is hoped that these essays in anthropology will provide a focus for the discussion of work being done at Oxford. For the present, it is preferred that the main emphasis should be upon analytical discussion rather than on description or ethnography.

We have been extremely gratified by the interest shown in the Journal so far. We have as yet not recouped all the expenses of the first two issues and have therefore been obliged to adopt a somewhat less expensive format for this present issue. We ask indulgence for this and also apologize to those subscribers in other universities who have, in the past, been kept waiting for their copies.

There are still a number of Vol.1 nos 1 and 2 available. Those wishing to purchase any back issues should write to the Editors enclosing 3/- for no 1 and 4/3 for no 2.

Papers should be as short as is necessary to get the point over. As a general rule, they should not exceed 4,000 words. For future issues, papers should be submitted following the conventions for citations, notes and references used in the ASA monographs.

Communications should be addressed to the Editors at the Institute of Social Anthropology, 51 Banbury Road, Oxford.

CONTENTS

	<u>Page</u>
EDITORIAL NOTE .....	i
SOCIAL ANTHROPOLOGY AT OXFORD E.E. Evans-Pritchard, Professor Emeritus, Institute of Social Anthrepology.	103
DESCRIPTION, MEANING AND SOCIAL SCIENCE Jairus Banaji, Institute of Social Anthropology.	110
WINCH AND THE SOCIAL DETERMINATION OF TRUTH Ross Bowden, Institute of Social Anthropology.	113
GALILEO AND THE TOPOLOGICAL SPACE Edwin Ardener, Institute of Social Anthropology.	125
£. s. d. VARNA Grant McCall, Institute of Social Anthropology.	131
AFRICAN MEDICAL TAXONOMY: with special reference to mental illness. John Orley, Warneford Hospital.	137

SOCIAL ANTHROPOLOGY AT OXFORD

Introduction

These notes may be of interest to past, present and future students at the Institute of Social Anthropology. Further details will be found in the following articles and memoranda, from which most of the facts cited have been taken:

1. E.E. Evans-Pritchard 'The Institute of Social Anthropology', The Oxford Magazine, April 26, 1951.
2. .. .. 'The Teaching of Social Anthropology at Oxford', Man, 1959, 180.
3. Anthropology at Oxford, Holywell Press, 1953.  
Memorandum to the General Board on 'the Professorship of Social Anthropology' submitted by representatives of the Faculty Board of Anthropology and Geography, 1969.
4. Proposed new Honour School of Anthropology: Communication from the General Board, 1949.

Position in the university.

Without going into the complicated structure of the university - Convocation, Congregation, General Board and all that - this note should suffice. When Tylor lectured he presumably did so (as Keeper of the University Museum) to the few who were interested enough to attend, mostly ladies. When Marett became Reader in Social Anthropology in 1910 I suppose he lectured in that capacity to those who registered for the Diploma in Anthropology, the University having given its recognition of the existence of the subject by the setting up of the Diploma in 1905 (the first examination was held in the academic year 1907-08) under a Committee for Anthropology. This was the first course in Anthropology in a British university. In 1914 Social Anthropology was recognized as 'the Department of Social Anthropology' with administrative and financial autonomy. Radcliffe-Brown changed, I think to no purpose, its title to 'the Institute of Social Anthropology' by so heading its notepaper. So we are an Institute in name, but I suppose that we are in reality a Department of the Faculty of Anthropology and Geography.

In 1938 the Committee for Anthropology and the Board of Studies for Geography (together with the Committee for Geography) were joined together to form a new Faculty, the Faculty of Anthropology and Geography. This seems to have been done for administrative convenience rather than for academic reasons, for the only academic association has been that Ethnology is an optional paper in the Geography Preliminary Examination. Although we are numerically one of the largest 'schools' in the University, Social Anthropology has a very modest representation on the Faculty Board: one statutory member (the Professor) and one elected, in effect nominated, member. This is because Ethnology, Physical Anthropology (Human Biology) and Archaeology count as 'Anthropology' since they were grouped together with Social Anthropology in the old Diploma in Anthropology.

One or the other of our two representatives on the Board, or both, is, or are, on the Applications Committee of the Board which meets before the Board sits and advises the Board on admissions and on the appointment of examiners.

There are two Sub-Faculties of the Faculty, those of Anthropology and Geography, and everybody teaching the one or the other subject ('Anthropology' in the sense as given above) belongs to the respective Sub-Faculty. These are primarily consultative Bodies to which the Board refers certain matters for consideration and guidance; though I suppose that there is nothing to prevent a Sub-Faculty initiating a discussion on any matter. Normally the Sub-Faculty of Anthropology meets once a term.

Since 1969 student consultative committees have been set up for the four component departments of the Sub-Faculty of Anthropology. The committee for social anthropology at present includes three research students at the Institute, elected by their fellow students, and representatives of this committee attend meetings of the Sub-Faculty for the discussion of curricular and other matters relating to the running of the Institute.

#### Accommodation

Tylor taught at the Pitt-Rivers Museum. Marett taught first at Exeter College and then from 1914 in the adjacent 'Barnett House' at the corner of the Broad and the Turl. In 1922 he moved the Department across the Broad to 'Acland House', a building next to Blackwell's bookshop. It was there that I joined it. Then in 1937, when 'Acland House' was pulled down in the demolitions to make room for the New Bodleian, we were housed with the School of Geography at the corner of Mansfield and Jowett Walk. The Geographers soon needed the whole building for themselves and we were also cramped, so in 1948 we migrated once more, this time to Museum House, Tylor's old home, in South Parks Road. Museum House was pulled down in the academic year of 1951-52 to allow an extension for Inorganic Chemistry and we moved to 11 Keble Road, once the home of Spooner, the arch-enemy, according to Tylor of anthropology. By this time, however, staff and students had so increased in numbers that we had to ask for more space and in 1966 were allotted our present abode, 51 and 53 Banbury Road.

#### Teaching Staff

What is now known as Social Anthropology was taught at Oxford under the general title of Anthropology by Tylor from 1883. On his retirement in 1908 his work was continued by Marett (later Rector of Exeter College), who from 1910 held a University Readership in Social Anthropology. On his retirement a University Chair in Social Anthropology was instituted through the generosity of All Souls, to which College the Chair was attached. Radcliffe-Brown was elected to it in 1937. (He was absent in Brazil from 1942 to 1944). I succeeded him in 1946 and I vacate the Chair this year (1970). As far as social anthropology is concerned, Marett taught single-handed till I joined him in a rather insecure post of Research-Lecturer in African Sociology at a salary of £300 in 1935. When I was able to get out of the army in 1945 I took up a Readership at Cambridge for a year and Fortes took my place at Oxford with a Lectureship (and the personal title of Reader); so when I took the Chair the staff consisted of myself, Fortes and a Secretary-Librarian (Miss P.H. Puckle). In 1947 we were joined by Gluckman and in 1948 by Srinivas. In 1949 Gluckman was elected to the Chair at Manchester and was replaced by Peristiany. In 1949 R.G. Lienhardt was

appointed to a Lectureship in the Institute, both Srinivas' and his appointments being made by the Oriental Studies Board. In the same year, since it was no longer possible for one person to act as both secretary and Librarian we obtained the services of a part-time Librarian (Mrs M. Sloss). In 1950 Fortes was elected to the Chair at Cambridge and Steiner took his place. At the same time Miss Tew (now Professor Mary Douglas) was appointed to a Lectureship (under the then Committee for Colonial Studies). So in 1951 the staff were Evans-Pritchard, Peristiany, Srinivas, Lienhardt, Steiner and Tew.

In 1951 Srinivas left to take a Chair at the University of Baroda and Dumont took his place. Miss Tew also left (to get married) and Bohannan took her place. In 1953 we suffered a great loss in Steiner's death. His place was taken by Beattie. In 1955 Dumont resigned to take a Chair in Paris and Pocock took his place. In 1956 Bohannan left us to take a Chair in U.S.A. and was succeeded by Needhari. In 1961, after many years of devoted service to the Institute, Miss Puckle retired as secretary and was replaced by Miss Edmed, who left in the following year. Miss Allaway joined us as secretary to the Institute in 1962. In 1963 Peristiany resigned to take up the Directorship of the Social Sciences Centre in Athens. Ardener took his place. In the same year P.A. Lienhardt was appointed to a Faculty Lectureship in Middle Eastern Sociology (an appointment made jointly by the University and St. Antony's College) in which capacity he became a member of our Institute. After many years of notable service Mrs. Sloss resigned as librarian. In 1966 Pocock resigned to become Reader in Social Anthropology at the University of Sussex. Jain took his place. From that time to now the teaching staff has remained the same. Our present Librarian, Miss Anderson, joined us in 1968.

#### The Library

The library began with Tylor's personal collection of books presented in 1911 and added to by his widow in 1917. It has constantly been added to and the total library now comprises some 7,500 volumes. Included in this estimate is the Skeat collection of Indonesian books and manuscripts. There are valuable runs of many journals and a large number of off-prints and brochures based on the Solignac collection. The care and maintenance of a library of this size is too much for one librarian and we really need an assistant, or at any rate a part-time assistant librarian. As befits a largely research Institute, the books are all on open shelves and we have sustained losses. It is difficult to know how these can be avoided without detriment to research. On the whole the Tylor Library, and the Balfour Library of the Pitt Rivers Museum supplement rather than duplicate each other.

#### Number of Students

Before the second European war there were never more than about 10 students, and often fewer than ten. A few figures will show how considerably the number has increased since. For the years 1946 to 1951 there was an average annual total of just under 43. Between 1946 and 1958 the annual average of students who sat for the Diploma in Anthropology was 8, for the degree of B. Litt. and B.Sc. 18, and for the degree of D. Phil. 15, giving a total average of 42 (excluding 'recognized students'). During the period 1961 to 1968 there were 185 candidates for the Diploma. Since 1949 143 students at the Institute have been awarded the degree of B. Litt. or B.Sc., and during the same period 83 dissertations for the degree of D. Phil. have been successfully submitted. I should add that it is not just that the number of students has increased but that, in my estimation, their intellectual and scholarly standard has, on the whole, risen also.

For various reasons the University has had to restrict its intake of students, and Boards, Colleges and Institutes (like ours) have been told that they may not take in more students than the quota allotted to them. We have about a 30 intake, though we can go up to 36. As it is very exceptional for us to permit a new student to register for a B.Litt. (or B.Sc.) or D. Phil. without having first taken the Diploma this means that the quota is more or less for the Diploma.

### Distribution of Students

Having in mind that we should contribute not only to Oxford but also to scholarship in a wider world we have always welcomed students from foreign countries and a fair number have come to us from Europe, the Americas, Asia, Africa, Canada, Australia and New Zealand. The provenance of candidates for the Diploma in Social Anthropology in the years 1961 - 1968 was Oxford 45; other British Universities 44; U.S.A. 39; other Overseas Universities 46; others mainly total 185. The 45 who took their degrees at Oxford were from the humanities or semi-humanities, and this has been the case since the Diploma was first instituted.

### Courses and degrees

We are a postgraduate department, so all students who come to us have already a degree in one or other subject. We advise all students, whatever their academic background, to take the Diploma in Social Anthropology - a year's course (though occasionally a man will take two years over it). Some stop at this point. Others spend a second year in working for the degree of B.Litt. or B.Sc., (our Board permits a man to work for either) from literary sources, and this is so common that we are inclined to regard the Diploma year and the B.Litt. (or B.Sc.) year as a single two year course. The B.Litt. requirements are a thesis and a written paper based on the thesis. Those who wish to continue, with the intention of making a career in social anthropology, then work for a D. Phil. degree, usually based on field-research, which generally entails 2 years in the field and a further year for writing a thesis for the Doctorship. Thus a professional training takes about 5 years. From time to time we have what are called 'Recognized Students'. These are persons of senior status who reside in the university for a short time and are not registered for a degree but are permitted to attend lectures, use libraries, and so forth, as though they were.

Taylor had tried to get a degree examination for anthropology but his project was rejected by the University, much to his chagrin. I and others spent four years in drawing up a syllabus for an Honour School, but in 1949 the proposal was rejected again, this time almost nen. con. by the General Board. I must confess that I was much relieved when it was turned down. I believe that we are the best, and best-known, postgraduate school in the world and I think we can be happy if we can remain so. It is possible that the new Honour School in the Human Sciences will develop into something like a School of Anthropology. It is well, however, that we are going to participate in it without loss of our autonomy.

### Diplomas

Until recently the Diploma was a combined course of Social Anthropology with Physical Anthropology, Ethnology, Comparative Technology and Prehistoric Archaeology. The examination papers were however heavily weighted in favour of Social Anthropology (three papers on Social Anthropology, one general paper on all subjects, a prescribed area paper

(mainly Social Anthropology), an essay, a practical ( in technology) and a viva voce); and the great majority of students have almost always been primarily interested in that subject. As each and all of these subjects expanded it became obvious that they could not continue to be combined in a one year's Diploma course save on a very superficial level. So in 1965 we were split up, though still under the general title of 'Anthropology and Prehistoric Archaeology', into four separate Diplomas: Human Biology, Prehistoric Archaeology, Ethnology and Social Anthropology. This was not entirely what we wanted but it was all that could be obtained at the time and was a move in the right direction.

The papers in the Social Anthropology Diploma examination now are: (1) History, Theory and Relation to other Disciplines, (2) Ecology, Economics and Technology, (3) Kinship, Descent and Marriage, (4) Political and Jural Relations, (5) Ritual and Symbolism, (6) Prescribed Area or Topic, (7) Essay. There is also a viva voce.

### Supervision

The Institute practises the tutorial system traditional in the university, that is to say that each of the Diploma students writes an essay for his 'tutor' (now called his 'supervisor') once a week and receives individual instruction in what used to be called a 'private hour'. The teaching staff have always divided up the Diploma students between them, and since the load of teaching is heavy (added to by one of the staff being usually absent in a sabbatical year) the Professor has taken his share of instruction. Each tutor decides for himself what lectures he shall advertise in the University Gazette. There is no attempt at planning, but it works out that we more or less cover the Diploma topics. During the first two terms seminars are held, and we usually get anthropologists from outside to give the papers. B.Litt., B.Sc., and D. Phil. students receive such guidance as they require and ask for, on their reading and in the writing of theses for examination. They are divided among the teaching staff in accordance with their special interests.

### Scholarships

We are not very well off for these. The oldest is the Coltart Scholarship in Anthropology which is in the gift of Exeter College, of which the successful candidate becomes a member. It is awarded for a year in the first instance but can be extended. It is worth about £150. The Bagby Bequest, which comes under the Faculty Board, is for research in urban, literate cultures in accordance with anthropological principles and methods. It is worth about £750 a year and is renewable up to three years in all. Lastly, the Iona Evans-Pritchard Scholarship is (in consultation with the Professor of Social Anthropology) in the gift of St. Anne's College. It is for a woman conducting research in the field of African studies and working for an Oxford degree, and it is at present advertised at £400 a year, and like the other scholarships is renewable. The only other university resource we have to help a student is our Graduate Assistantship worth £500 a year. This post in the Institute requires certain duties to be performed, but may be regarded more or less as a scholarship.

Another scholarship (worth about £3,000) which should be mentioned here is the Swan Fellowship awarded by the Curator of the Pitt Rivers Museum in consultation with the Professor of Social Anthropology and the Reader in Physical Anthropology, for Studies in connexion with the Batwa, which is to say for research among the pygmy and pigmoid peoples. Since this scholarship was accepted by the University in 1955 it has been given to ethno-archaeologists and not to students of existing peoples.

The Social Science Research Council has ten subject committees, one of which is for social anthropology. Candidates for the Diploma and the B.Litt. are eligible for quota and pool awards. In 1968 twelve awards were made but only ten were taken up, In 1969 eleven awards were made.

### Publications

The only official University series (coming under the direction of the Faculty Board) is Oxford Monographs on Social Anthropology. This was started in 1957 and is restricted to theses of unusual merit. So far six volumes have been published and there are a number awaiting publication. What might be called a semi-official series, since it is published by the Clarendon (University) Press, and is in effect run by our Institute, is the Oxford Library of African Literature. The first volume in the series was published in 1964; 15 volumes have since appeared. There are others on their way. I might add that though there is no direct institutional relationship between the Institute of Social Anthropology and the Clarendon Press there has always been a close personal relationship between members of the Institute and the Press, both as publishers and as friends. Under this heading I must also mention the recent venture of some of our students, the Journal of the Anthropological Society of Oxford. It should also be mentioned that the Professor began in the academic year of 1946-47 to submit to the University a brief annual report on the activities of the Institute and this is now expected of him and is published at the end of Trinity Term in the University Gazette.

### Relations with other disciplines

I think I may say that we are now, after some strained periods, on good terms, both academic and personal, with our colleagues at the Pitt Rivers Museum (Ethnology and Prehistoric Archaeology). Co-operation might be closer if the Museum had a new building in which it could display its magnificent collections to better purpose. We are also on good terms with the Professor of Race Relations and the Reader in Physical Anthropology, and also with our colleagues at the School of Geography.

Our Institute and its members have a long record of co-operation with other humane disciplines, and several of us have supervised postgraduate students working under the direction of other Boards, e.g. Theology, Litt. Hum., History and Social Studies. In the past we had much to do with the teaching of Cadets and Officers in the Sudan and Colonial Services. This stream has dried up, but something perhaps more important has taken its place. Social Anthropology is a compulsory paper in the B. Phil. in Indian Studies. It is a scheduled subject in the B.Phil. in Latin-American Studies. In the proposals for the B.Phil. in Modern South Asian Studies the schedule of subjects includes Social Anthropology. Social Anthropology is a subject in the schedule of the proposed B.Phil. in African Studies. As earlier mentioned it will also participate in the new Honour School in the Human Sciences.

### Lectures

The Frazer Lecture on some social anthropology subject is delivered at Oxford every four years (at Cambridge, Glasgow and Liverpool in the other years). The Vice-Chancellor has always consulted me, in effect asked me, to nominate the Lecturer. The Marett Lecture on some social anthropology subject is delivered at Exeter College every third year (in the intervening years it is delivered on a philosophical or archaeological subject). I have usually been consulted by the Rector of the College. The annual Myres Memorial Lecture is given in rotation on a subject within the field of ancient history, European and Near Eastern archaeology, historical geography and ethnology, with special reference to



Mediterranean lands. The lecture is delivered every other year. I suppose that a social anthropologist could be invited to lecture under the title of 'ethnology' but so far no one has been asked to do so. The Professor is not ex-officio a member of the Board of Management, though he, or any of his colleagues, could be appointed by the Faculty Board to serve on it, as the Faculty Board nominates two of its members.

#### Stipendary Fellowships and Hon. Degrees

All Souls College now advertises each year Visiting Fellowships for a year or six months, as those selected wish. The Professor, as a Fellow of the College, may put forward a name, but the competition is severe. There is no restriction with regard to nationality. So far the only social anthropologist to have been appointed is Prof. Fred Eggan (1970). The College also from time to time advertises Research Fellowships and one of the subjects listed in the advertisement is social anthropology. So far no social anthropologist has been awarded one of these Fellowships. Other colleges sometimes advertise Research Fellowships for which an anthropologist might be eligible. The advertisements appear in the University Gazette (and sometimes elsewhere). Four such appointments have recently been made (at St. Catherine's, St. Hugh's, New College and Merton). Council decides who are to be given Hon. degrees. So far the only social anthropologist who has been honoured is Prof. Claude Lévi-Strauss. These degrees are sparingly given and any proposal has to be strongly backed

#### Extra-Institute Activities

Closely associated with the Institute is the Oxford University Anthropological Society. It was formally constituted at a meeting in Exeter College in January 1909 and held its 500th meeting on Wednesday, 25th February, 1953. It is thus one of the oldest University societies. It holds about 10 meetings a year at which lectures are given. It has recently (Hilary Term, 1970) brought out the first number of the Journal of the Anthropological Society of Oxford, the editors of which are students at the Institute.

#### Appointments

All I have to say on this topic is that since I have taught at Oxford no student who endured the full course of his academic career has failed to obtain a good post in a department in a University, were he so minded. I think that these favourable circumstances still persist. During the past 20 years 179 students have been awarded post-graduate degrees in Social Anthropology. Of these, over 20 now hold appointments at full professorial level and well over a further 100 hold Readerships or Lectureships. Between them they have published more than 70 books, as well as innumerable articles, on a wide range of subjects in Social Anthropology.

E. E. Evans-Pritchard.

Description, Meaning and Social Science

I would like to isolate and refute, briefly, some of the gross misconceptions advanced by Clammer in his essay "The Analytical and Phenomenological Approaches to the Social" (JASO 1.1 Hilary 1970). The following three are on the whole representative of the tenor of his argument:

1. Description (in some sense which phenomenology attributes to this term) is "fundamental to accurate understanding of what is happening in the world".
2. The role of the social sciences is to understand the meanings that people give their social behaviour.
3. Society is the object of philosophical enquiry.

Merkeau-Ponty has advanced the phenomenological standpoint in simple unequivocal terms. Phenomenology is a matter of describing, according to him, not of explaining or analysing. When Husserl recommended the return to the "things themselves", he was rejecting science at the very start. The demand for pure description excludes equally the procedure (i) of analytical reflection and (ii) that of scientific explanation. The axiomatic basis of this position can be put as follows: the world is there before any possible analysis of mine. Looking for the world's essence is not looking for what it is as an idea once it has been reduced to a theme of discourse; it is looking for what it is as a fact for us, before any thematization.

In short, phenomenology assumes that a theory-independent description of the world is possible and advocates a return to such description. But can there be such a thing as a theory-independent description in either the natural or the social sciences? Kuhn has argued, quite plausibly, that the "facts" of natural science are only determined as facts within a pre-given theoretical framework, a paradigm (Bachelard's term is "problematic"). "No language restricted to reporting a world fully known in advance can produce mere neutral and objective reports on 'the given'" (Kuhn:126). Thus the "scientist who sees a swinging stone can have no experience that is in principle more elementary than seeing a pendulum. The alternative is not some hypothetical "fixed" vision, but vision through another paradigm, one which makes the swinging stone something else" such as constrained fall. If this argument is valid (and Clammer does not show why it isn't), what would a phenomenological description be? 'Describing what there is' does not seem to correspond to any known experience or procedure in the practice of natural science.

Now this argument applies a fortiori to the social sciences. If the natural sciences know what a "scientific fact" is, the social sciences do not - at least not at the moment. As in the natural sciences, so in our experience of the social, our perceptions are fixed in advance, structured by models which we have each internalized unconsciously. The difference is that in the former the scientist's perceptions are determined by paradigms, that is, models which have been rigorously constructed as part of a scientific practice and which the whole scientific community accepts for a given epoch; in the latter our perceptions are determined by non-rigorous models, and there is no single model accepted unanimously by the entire community: these "models" are not paradigms in the strict sense; they are closer to what Marx calls "ideology" and Levi-Strauss "conscious models". This radical difference between the two situations, that of the natural scientist and that of the social scientist, explains why in the second it is more

difficult - and has hitherto proved impossible - to elaborate a coherent uniform concept of a "scientific fact."

So far the argument has asserted two separate theses: (1) that neither in the social nor in the natural sciences are there such entities as "pure facts", because in both cases our experience is theory-dependent. In neither case are pure descriptions ever possible. (2) that the kind of "theory" which determines perception in the one and in the other diverges radically. Thus it would be difficult to find in the social sciences any homologue of the following fact of chemistry: that a molecule of water is made up of two atoms of hydrogen and one of oxygen.

Clammer's first proposition is therefore based on a radical misconception of the structure of experience. It could only make sense for a world in which the "data of experience" were fixed and neutral, i.e., for a world of which we have no experience.

The second proposition is asserted as a dogma, it is nowhere argued for. In the form in which it is presented, it is clearly untenable or only tenable at the cost of eliminating from the field of the social sciences two of the most decisive advances that were made in it in the 19 century: historical materialism and psycho-analysis. It also, incidentally, makes nonsense of structural anthropology - a consequence which is perhaps not immediately obvious.

"The meanings that people give their social behaviour and institutions": subjective meanings. The implication seems to be the following: either (a) there are only 'subjective meanings' in the world, meanings which men consciously produce and internalize or (b) phenomenology is inadequate, because there are certain meanings which can escape the consciousness of social and historical "actors", i.e. objective meanings. As far as I know Being and Nothingness was the only work to argue for (a). Since then Sartre has abandoned this position. The Critique de la Raison Dialectique is about a world in which people's conscious intentions, their pro-jects, are constantly producing other-meanings: a process which Sartre describes variously as "alienation", "reification" and contrefinalite. That (a) is a completely untenable position is obvious from psycho-analysis which takes it as axiomatic that behind the meanings men consciously attribute to their acts are other deeper meanings of which they are wholly or only half conscious; from marxism which precisely holds that the meaning men give (i.e. consciously confer on) their "behaviour" and "institutions" is never identical with the real meaning of their "behaviour" and "institutions" (theory of ideology); from structural anthropology which holds that social structures are entities independent of men's consciousness of them ( i.e. the way men apprehend them consciously through a certain system of meanings/ conceptual scheme) and from the image which men form of them. What unites marxism, psychoanalysis and structural anthropology is precisely the theory of illusion which each elaborates. If the role of the social sciences is to understand the meanings that people give their social behaviour and institutions - and only that - then they must imprison themselves within illusions - they "must" because that is their "role"! But if that is not their only role, if beyond comprehending conscious meanings, they must disengage the gap (distortion) which separates the illusion from the reality, the spontaneous consciousness of a structure from the structure itself, then phenomenology is, as Levi-Strauss has said, only a point of departure.

If phenomenology were identical with the totalization which is science, neither historical materialism, psychoanalysis nor structural anthropology would be possible, or, at any rate, not as sciences. If they are not scientific, and this is what Clammer is arguing, he does not show why.

It is difficult to see what the last of the three propositions means beyond saying what has already been said in (i) or (ii). If by "philosophical enquiry" we are to understand "phenomenological enquiry", then the arguments against this have already been proposed above. If the enquiry is "philosophical" for a related reason, namely that it resorts to "motive" explanations rather than casual ones, then the arguments against (ii) are valid against (iii). It is however, worth making the point by a different route.

The idea that only "motive" explanations are valid in the social sciences springs from a fundamentally idealist conception of society. And this precisely is the reactionary and inept conception Winch offers us in his little book. We are told, for example, that "social relations are expressions of ideas about reality" (Winch:23). There are two concepts involved here and it would be worth separating them for a moment.

First, there is the notion of "social relations". These to Winch are the particular relationships which are established, by sets of rules, between roles. Winch's concept is therefore the traditional one familiar, for example, from functionalist anthropology. It refers to a more or less immediately perceptible world of social interactions. Next there is the notion of "reality". This, however, seems to be only a more comprehensive term which includes social relationships as one component and everything else as the other. So the proposition seems to amount to the circularity: "social relations are expressions of ideas about (social relations)" i.e., social relations are what men think they are. Who are these "men" however? They include monks and workers - to quote two of Winch's examples. So the social relationships into which workers enter are the relationships into which they think they enter. What happens however, if two groups of workers conceptualize their relationships in diametrically opposed terms? If some workers believe that they are the objects of exploitation, that a fraction of their labour is stolen from them by the boss - while another group thinks that by their work they are benefitting the "national economy" - that as members of a "country" (rather than say as members of a class) it is their duty to work as hard as the management, who of course "know" (this too is part of their "ideas"), requires? What becomes of their social relationships? Can these be different for any two workers though they work in the same factory, for the same wage, and in most other respects have a similar "status"?

Or, to transport the argument to a slightly less mundane level, which are the true social relationships in those societies which superimpose on an asymmetrical class system a symmetrical moiety system? Obviously the relationships pertaining to their moiety system. In short, Winch's idealism radically eliminates the distinction between conscious and unconscious models, experience and reality, ideology and science. It makes science impossible, for if the "appearance of things coincided with their essence" what would be the purpose of science? And what else does "social relations are the expressions of men's ideas of reality" mean except that? Winch would like to privilege 'common sense' which is and has always been "the practical wisdom of the ruling class" (Gramsci) How would Clammer reconcile this with the concept of a "critical philosophy" (Marcuse)?

Clammer's synthesis would put the social sciences back three hundred years, successively eliminating the work of Montesquieu, Marx, Freud, Durkheim, Levi-Strauss... Perhaps this work is the product of an overactive imagination prone to metaphysics? In which case this interesting view should be justified at greater length. What, for example, does Clammer think about the following passage from Durkheim (quoted in Winch: 23) as an example of a view which conflicts with his own:

"I consider extremely fruitful the idea that social life should be explained not by the notions of those who participate in it, but by more profound causes which are unperceived by consciousness...."?

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Winch and the Social Determination of Truth

The issues I want to raise here can be regarded as a direct follow on from some that have been raised in previous issues. Basically the issue at stake is how do we understand an alien belief system. This I want to suggest comes very close to the question of how do we understand another language at all.

The way I shall approach this question is through some purely formal considerations relating to the possibility of alternative logics. My main task will be to reject what might be called a Winchian approach to some of these issues.

A wide range of writers has been attracted to the idea that truth and logic are culture or context dependent.<sup>1</sup> Sociologists of knowledge such as Mannheim, and Durkheim and Mauss agree that the genesis of a proposition is not under all circumstances irrelevant to its truth. For Mannheim the task for the sociologist of knowledge is to analyse the "perspectives" associated with different social positions, the "orientations" towards certain meanings and values which inhere in a given social position where an individual "outlook" and "attitude" is conditioned by the collective purposes of the group and to study the concrete reasons for the different perspectives which the same situation presents to the different positions in it. His interest is in situations where social structures come to express themselves in the structures of assertions, and in what sense the former concretely determine the latter. (Mannheim:1936).

In Primitive Classification Durkheim and Mauss argue that originally there is a casual, genetic relation between the categories in different languages (such as space, time, quantity) and logical relations (such as deductive validity) and that society's social relations. "Thus logical heirarchy (i.e. of exclusion and inclusion) is only another aspect of social heirarchy." Again logical relations between things are based on the social relations of men. "Logical relations," they argue, "are thus, in a sense, domestic relations." (Durkheim and Mauss: 1969).

This view is also shared by some philosophically-minded social anthropologists and philosophers interested in the social sciences. Levy-Bruhl suggests that "prinitive thought violates our most deeply rooted mental habits." (Levy-Bruhl:1922: 48). It is prelogical in that it is "indifferent most of the time to contradiction" and committed to a view of casuality "of a type other than that familiar to us." (ibid:85) Winch argues to a conclusion very similar to that of Durkheim and Mauss at the same time attempting to give his argument a general philosophical justification. For Winch, "our idea of what belongs to the realm of reality is given for us in the language that we use." (Winch: 1958:15). Similarly "criteria of logic .... arise out of and are only intelligible in the context of ways of living or modes of social life"(ibid:100) to the extent that "logical relations between propositions themselves depend on social relations between men."(ibid:126). For Winch, standards of rationality between societies do not always coincide. Indeed rationality itself in the end comes down to "conformity to norms". (Winch:1964:318).

Whorf has also claimed that what counts as true and/or what counts as valid reasoning is relative to particular groups. "When anyone, as a natural logician, is talking about reason, logic and the laws of correct thinking, he is apt to be marching in step with purely grammatical facts that have somewhat a background character in his own language or family of languages but one by no means universal in all languages and in no sense a common substratum of reason." (Whorf:1956:211). For Whorf, then, logic and ontology literally recapitulate philology.

Also philosophers of science such as Kuhn (if Lukes is to be believed here) have been tempted by this view. For Kuhn, when scientific paradigms change, in an important sense, worlds change too. After Lavoisier discovered oxygen not only was the world seen differently, but it was different. Accordingly, Kuhn suggests, there is a need to revise the traditional epistemological viewpoint of Western philosophy that changes in scientific paradigms carry us closer and closer to the truth. (Kuhn:1964:125):

Similarly logicians have spelt out in some detail what alternative logical systems might look like in purely abstract terms. Intuitionists' objections to the traditional propositional calculus have led to the development of a propositional calculus that neither contains the law of excluded middle nor admits of its subsequent insertion. And in logics based on quantum mechanics the distributive law breaks down.

In the article Are there Alternative Logics? (Waismann: 1968), Waismann suggests ways in which it is possible to construct languages to which our familiar Aristotelian two-valued logic does not apply, that is, a language in which a proposition is not always true or false. In fact, Waismann argues the possibility of multi-valued logics, which involve relinquishing what might be regarded as intuitively obvious logical axioms such as excluded middle,

non-contradiction and so on is already implicit in ordinary language. Ordinary English, e.g. he suggests is a loose conglomeration in which fragments of different logical systems are discernable. A logic, he suggests, is always an idealisation of the conditions we meet in a given language, just as mathematical geometry (e.g. a Euclidean geometry of three-dimensional space) is a refinement of the rough data obtained by measuring solids. And as the existence of non-Euclidean geometries demonstrates, just as observations obtained in this way can in principle be built into various geometries, so the conditions we find in a given language allow of an idealisation in more than one direction. In other words the process that leads to a different logic is not uniquely determined by actual usage.

I now want to consider specifically Winch's position. His arguments have been rehearsed sufficiently in earlier editions of this journal to make repetition here unnecessary. Let me start by assuring Winch is arguing for an extreme form of logical relativism.

Consider the different ways in which a belief or set of beliefs could be said to be prima facie irrational. (A belief for convenience can be characterised as a proposition accepted as true).

- Beliefs are said to be irrational
- a) if they are inconsistent or self-contradictory
  - b) if they are partially or wholly false
  - c) if they are nonsensical
  - d) if they are situationally specific or ad hoc; ie not universalised because bound to particular occasions
  - e) if the ways in which they come to be held or the manner in which they are held are seen as deficient in some respect. For example (i) the beliefs may be based on irrelevant considerations (ii) insufficient evidence (iii) they may be held uncritically or unreflectively.<sup>2</sup>

Now I think, with Lukes, one can give good a priori reasons for regarding some criteria of truth and validity (or more generally criteria of rationality - and by criteria of rationality I mean rules specifying what would count as a reason for believing something (or acting) ) as universal, as relevantly applicable to all beliefs in any context while others are context-dependent, that is are to be discovered by investigating the context, and are only relevantly applicable to beliefs in that context. And I shall argue (with Lukes against Winch) that all beliefs can and must be evaluated by both context-dependent and context-independent criteria.

In any set of beliefs in society S one can ask two different types of question:

- 1) What for S are the criteria of rationality in general
- 2) What are the appropriate criteria to apply to a given class of beliefs in S.<sup>3</sup>

1) Now as Lukes has rightly put it, insofar as Winch seems to be saying that the answer to the first question is culture-dependent, he must be wrong, or at least we could never know if he were right; indeed we could not conceive what it would be for him to be right. (Lukes:1967:260).

For in the first place the existence of a common reality is a necessary precondition of our coming to understand S's language at all. This does not mean that I and members of S are going to agree on all the facts. As Whorf put it "language dissects nature in different ways". "What must be the case is that S must have our distinction between truth and falsity if we are to understand its language, for if per

impossible it did not, we would be unable to agree about what counts as the successful identification of public (spatio-temporally located) objects." Similarly if S is to engage in successful prediction it must presuppose a given reality of events which are predictable." Both primitive and modern men predict in roughly the same ways; also they can learn each other's languages. Thus they each assume an independent reality which they share."

This argument, and I have been following Lukes' statement of it here, is put fairly rapidly. The main points can be made clearer in the following way. In The Limits of Irrationality Hollis spells out this argument as follows; attributing what have been called universal criteria of rationality to S is not a matter of empirical discovery, but is presupposed by the very process of coming to understand S's language.

To understand utterances in S's language Hollis suggests the translator must relate them to another and to the world. "To translate them into English he needs to relate some of them to the world, since in relating an utterance to others he does not learn what it means unless he already knows what the others mean. Ultimately he needs a class of utterances whose situations of use he can specify. Now these can be specified either as he himself sees them or as his informant sees them. But this seems to suggest the specifications might be different." But if this could be possible he couldn't begin at all. "For his only access to native perceptions and specifications is by translating what they say about what they perceive. He would therefore have to translate before discovering what they perceive and to know what they perceive before translating. There would therefore be no way into the circle. The class of utterances which form the bridgehead of his advance must be one for which his specification and his informant's coincide."(Hollis:1967:266).

That is there are two critical assumptions which are made in the very act of coming to understand S's language viz 1) that the informant perceives more or less what he perceives and 2) that they will say more or less the same about it. That these are assumptions is demonstrated in the following way.

Suppose the translator gets his bridgehead by pinning down the native counterpart to the English sentence 'Yes, this is a brown cow'. There are no counterparts to pin down unless the native perceives brown cows and asserts that he does. For since these are the conditions for truthfully asserting the above in English they are also the conditions for truthfully asserting the above in S. Now this, as Hollis suggests, is banal enough. But it is not a hypothesis that anthropologists share certain percepts and concepts, hypothesis which later success in translating confirms. For this hypothesis would be irrefutable. In order to question the perceptual and conceptual basis of the bridgehead, the translator would have to ask his informant what he perceived when confronted with a brown cow and whether his utterance was to be construed as an assertion. Also he would have to understand his answer. But he can neither ask nor understand unless he has a bridgehead. Consequently he cannot refute the hypothesis by establishing a rival one. At most he can draw a blank and fail to produce a translation at all. But even this would not justify the translator in attributing idiosyncratic linguistic or perceptual processes to members of S. It would only serve to suggest they had no language at all.



Nor is the hypotheses confirmed with success. The translator has discovered (roughly) what native sentence to pair with the original; but he has not discovered that the natives perceive a brown cow when they utter the sentence. For if that were in doubt so would the pairing be. And, as has been argued already, if both are in doubt, there is no way into the circle. Similarly, although it is an empirical matter to discover how the informant signals the difference between assertion and denial, 'yes' and 'no', 'true' and 'false', and by implication our notion of verification, it is not a hypothesis that they have such distinctions. "For to check such an hypothesis the translator would have to establish the meanings of utterances in the bridgehead independently of whether they were used to correct what was taken to be true. But this cannot be done as their translation depends on what linguistic function they are taken to perform. Consequently the only alternative to finding an overlap in concepts and percepts is to find nothing at all." (ibid:266).

If this is right then the assertion comprising the bridgehead will have to be coherent and indeed true. Again it looks as if notions of coherence and truth in S need not coincide with the translator's. But if this is taken as a hypothesis another vicious circle is generated. "For the only way to find terms (in S) for relations among utterances is to translate the utterances and then to interpret the linking terms so that the utterances are linked coherently. Equally the only way to find the native sign of assent is to translate the utterances and then to interpret whatever sign accompanies most of the true ones as assertion. But this makes it impossible for alternative concepts of coherence and truth to show up. If these concepts were in doubt, the translator would have to know what they were, before he could translate the utterances which they linked, and would have to translate the utterances in order to find how they were linked. Again there would be no way into the circle." (ibid:267).

I should add here that although these arguments seem to me to be valid I think Hollis's account of the notion of 'bridgehead' is rather misleading. Clearly one doesn't decide that 'Yes, this is a brown cow' is true by fiat, so to speak, and then go on using that as a point of leverage into the language. Any translation of a native utterance is always hypothetical and open to confirmation or revision. Rather it is the specification of the situation in which the translator elicits the native sentence and which has to be common to translator and informant if translation is to get going at all that is not open to conjecture and refutation or confirmation.

My argument so far then has been that in order to attribute a language to S at all they must possess our concept of verification, negation and affirmation as applied to assertions about a common reality.

It may be objected that there is nothing here that Winch would in fact deny. Well even if this is the case it is certainly not clear from what Winch himself says.

Now Quine (Quine:1969) has taken this argument about the inevitable grafting of the translator's logic onto the language of the informant a step further (and although it is not strictly speaking relevant to my argument here I think he raises some central questions for translation theory).

Quine's argument can be outlined simply as follows. Picture the anthropologist in the proverbial jungle situation starting from scratch when learning a native language (the presence or absence of an interpreter makes no difference to the philosophical point). Suppose a rabbit runs by and the

native utters 'Gavagai'. The anthropologist duly notes down 'Rabbit' in his notebook, subject of course to further confirmation. But although this is the necessary starting point of any process of translation (and by implication, any understanding of the linguistic utterances of a person using even the same language). It is also the starting point for problems in translation theory, at least for the anthropologist, sensitive to the possibility of fundamental differences between conceptual systems of the Whorfian kind.

Quine illustrates this in the following way. Stimulus synonymy of the sentences 'Gavagai' and 'Rabbit' (stimulus synonymy means the stimulus conditions that prompt the two sentences 'gavagai' and 'rabbit' are the same) does not even guarantee that 'gavagai' and 'rabbit' are coextensive terms (i.e. terms true of the same things.) The informant's sentence 'Gavagai' could refer to rabbits, or mere stages, or brief temporal segments of rabbits. In either event the stimulus situations that prompt assent to 'Gavagai' would be the same as for 'Rabbit'. Again stimulus meaning would register no difference when Gavagai is taken as a singular term naming a recurring universal or a general term. The same problems Quine argues arise for our articles and pronouns, our singular and plural, our copula and our identity predicate. The important point is that over any range of given stimulus conditions, the informant may achieve the same net effects through linguistic structures so different that any eventual construing of our devices in the native language and vice-versa can prove unnatural and largely arbitrary.

For this reason, Quine suggests, translation (or understanding) suffers from a very radical kind of indeterminacy. By this he means simply that conceptual schemes can vary radically but undetected by the translator. In its simplest sense this can be put by saying two men (i.e. translator and informant) and also two speakers of the same language) could be alike in all their dispositions to verbal behaviour under all possible sensory stimulations and yet the meanings or ideas expressed in their identically triggered and identically sounded utterances could diverge radically for the two men in a wide range of cases.

Now although it looks as if Quine is running an extreme Winchian relativism here the emphasis is I think quite different and in fact distinctly un-Winchian.

Consider truth functions such as negation, logical conjunction and alternation. By reference to assent and dissent Quine argues we can state semantic criteria for truth functioning, i.e. criteria for determining whether a given native idiom is to be construed as expressing the truth function in question. For example the semantic criterion for negation is that it turns any short sentence to which one will assent into a sentence from which one will dissent and vice versa. Quine's point is that when we find that a native construction fulfils one or another of the semantic criteria we can ask no more towards an understanding of it. And as Quine points out, this ill accords with a doctrine of prelogical mentality. To take the extreme case suppose the informant asserts as true a sentence in the form 'p and not p'. Now this claim is absurd under our semantic criteria. And, not to be dogmatic, Quine asks what criteria might one prefer. "Wanton translation can make natives sound as queer as one pleases. Better translation imposes our logic upon them and would beg the question of prelogicality if there were one to beg".

And as Quine points out, Malinowski spared the Trobrianders the imputation of prelogicality by so varying his

translations of terms, from occurrence to occurrence, so to sidestep contradiction. Leach protested but provided no clear solution for the issue. And as Quine remarks, it is understandable that the alternative of blaming the translation of conjunctions, copulas or other logical particles is nowhere considered, for any considerable complexity on the part of the English correlates of such words would of course present the working translator with forbidding practical difficulties.

The maxim underlying Quine's logical and methodological charity then is that one's interlocutor's silliness is less likely than bad translation. For translation theory, as Quine puts it, "banal sentences are the breath of life".

Behind all this is Quine's main point that all translation proceeds only by means of a number of analytic hypotheses which extend the limits of translation beyond where independent evidence can exist.

Such analytic hypotheses of the translator, for example, involve segmenting heard utterances into conveniently short recurrent parts thus enabling the translator to compile a list of words. Various of these he hypothetically equates to English words and phrases in such a way so as to conform to the presupposition that for example observation sentences can be translated or that truth functions can be translated.

In other words it is "only by the outright projection of prior linguistic habits that the anthropologist can find (e.g.) general terms in the native language at all, or having found them match them with his own."

The method of analytic hypotheses as Quine puts it "is a way of catapulting oneself into the jungle language by the momentum of the home language. It is a way of grafting exotic shoots on to the old familiar bush until only the exotic meets the eye." From the point of view of a theory of translational meaning however the most notable thing about analytical hypotheses is that they exceed anything implicit in the natives' disposition to speech behaviour.

It is worth mentioning here that Quine's principle of charity is interpreted by Gellner in Concepts and Society (Emmet and MacIntyre:1970) as being not an indispensable methodological requirement but as evidence of a moral desire on the part of the anthropologist to be "tolerant, understanding and liberal, to refrain from an uncomprehending and presumptuous superiority in one's attitudes to other (notably 'primitive') societies."

This leads me to my second objection to Winch. This is that S's language must have operable logical rules and not all of these can be purely a matter of convention. Winch states that 'logical relations between propositions... depend on social relations between men.' But if this implies that the concept of negation and the laws of non-contradiction and identity need not operate in S's language then it must be mistaken for if the members of S do not possess even these how could we ever understand their thought, their inference and arguments? (This follows from Quine). Winch half sees this, as Lukes rightly suggests, when he writes that the possibilities of our grasping forms of rationality different from ours in an alien culture are limited by certain formal requirements centring round the demand for consistency. But these formal requirements tell us nothing about what is to count as consistency, just as the rules of the propositional calculus limit, but do not themselves determine what are to be values of P, Q, etc.

But as Lukes points out, this is merely a misleading way of saying that it is the content of propositions, not the logical relations between them that is dependent on social relations between men. (1967:262).

It follows that if S has a language it must minimally possess criteria of truth (as correspondence to reality) and logic which we share with it and which simply are criteria of rationality, in that they constitute the formal conditions for the possibility of understanding utterances by members of S.

So far I have been concerned with fairly formal objections to the most extreme interpretations of Winch's pluralistic social solipsism.

Now I do not want to deny that members of S might not, against a background of universal criteria of truth and logic, adhere to beliefs which systematically violate these criteria. This in fact seems to be typical of the ethnographic situation. What I do want to argue however is that these context-dependent criteria are in Lukes' phrase 'parasitic' on non-context-dependent criteria. That is where there are second order beliefs about what counts as true and valid, those beliefs can only be rendered fully intelligible as operating against a background of such criteria.

Consider the following example from Gellner's Saints of the Atlas (Gellner:1970).

According to Gellner the concept of 'baraka' possessed by Moroccan Berbers which means variously 'enough', 'blessedness' and 'plenitude' and is believed to be manifested amongst other things in prosperity and in the power to cause prosperity in others by supernatural means has the interesting character of violating three of the most advertised categorical distinctions favoured by contemporary linguistic philosophers.

1) It is an evaluative term, but it is used as though it were a descriptive one; possessors of baraka are thought of as possessing an objective characteristic which is empirically discoverable

2) In as far as it is treated as an objective characteristic of people manifest in their conduct it could only be a dispositional one - but it is treated as though it were the name of some 'stuff' (e.g. it can be transmitted between persons by means of spitting into the mouth),

3) its attribution is really a case of a performative use of language - people become possessors of baraka by being treated as though they were possessors of it - but it is also treated as though its possession were a matter wholly independent of the volition of those who attribute it. This is essential to the working of the Berber political life. Two comments can be made here:

1) Concepts which like the concept of 'baraka' consistently ride roughshod over the performative and descriptive use of language would only be socially (and indeed logically) possible against a background of social behaviour where the logic of performatives was not confused systematically with the logic of description. Social behaviour such as making promises or economic contracts would be inconceivable unless in general the social implications of performatives were clearly seen and adhered to.

Now all this raises the general question of what understanding in this sort of situation will consist in. To say with Winch that use is meaning is justification simply seems unhelpful. What is added in the way of comprehension

by saying that as far as Berber political concepts go the Berbers always live, as it were, in a conceptual dimension of their own in which our categorical boundaries do not apply? But as Gellner rightly suggests, we can sometimes only make sense of the beliefs in question by seeing how the manipulation of concepts and the violation of categorical boundaries helps it work. It is precisely the logical inconsistency of 'baraka' which enables it to be applied according to social need and to endow what is social need with the appearance of external, given and indeed authoritative reality.

My third objection, then, is the one Lukes makes although in a slightly different context. He points out that it is only by assuming non-context dependent criteria of rationality that one can "raise questions about the social role of ideology and false consciousness." (Lukes:1970). And he quotes the Soviet historian Joravsky as saying that the only way to prove which beliefs have performed what functions in the social process is to study the beliefs and social processes from the vantage point of genuine knowledge. Consider the belief, Joravsky suggests, that was mandatory in Soviet Russia during the thirties: that land belongs to the people and therefore collective farmers hold their land rent free. This presents a specific verifiable statement as a logical consequence of a vague but stirring principle. But the historian of Soviet ideology in his effort to discern the social functions of various types of thought should begin his analysis with the observation that rent has existed in the Soviet Union, whether or not Soviet leaders have been aware of it. Similarly we can add that the student of Berber political ideology should begin with the observation that 'baraka' is an ideological construct of Berber political imagination. Gellner makes roughly the same point when he suggests that Winch's extreme form of logical charity blinds one to at least one socially significant phenomenon viz the social role of absurdity.

Winch however does have something to say on this point, in criticising Weber's account of sociological understanding. As Winch interprets it this consists on the one hand of 'interpretive understanding' of the meaning of a piece of behaviour which is basically a psychological technique, a case of imaginatively putting oneself in the other fellow's position, and on the other hand providing a casual explanation of what brought the behaviour about. Casual explanation for Weber involves formulating statistical laws based on observing what happens, thus enabling the observer to predict what the agent will do on a future occasion. Now Winch disagrees with the latter part of this when he suggests 'understanding' a piece of behaviour or utterances is quite different from formulating statistical laws about the likely occurrence of those same words in the future. "A man who understands Chinese is not a man who has a firm grasp of the statistical probabilities for the occurrence of the various words in the Chinese language (Winch:1958:15). Understanding rather consists in "grasping the 'point' or 'meaning' of what is being done or said." (ibid:115).

But although Winch gives no further examples of what he means here I think one can fairly easily provide one. To understand why a Nuer holds his fighting spear in his right hand is not to be able to predict that on certain occasions in the future he will hold it in his right hand, but is rather, as Evans-Pritchard does in his chapter on spear symbolism, to spell out the symbolic significance of the right hand for the Nuer, how it stands for masculinity, virtue, the patriline and so on. And as Winch rightly suggests, the notion of meaning here should be carefully distinguished from ~~that~~

of function (although of course this does nothing to refute Gellner's or Joravsky's point).

My fourth objection can now be put in this way: Winch's rather amorphous notion of a form of life provides no means of deciding what is relevant to understanding a belief system. Does understanding a belief system consist only in elucidating what the informants normally say a set of beliefs mean? I can illustrate very simply what I mean with the following example: In *Twins, Birds and Vegetables* (Firth:1966) Firth found sufficient evidence in extraneous, unverbaliſed bits of Nuer behaviour, both in and outside Evans-Pritchard's particular volume, to cast considerable doubt on what Evans-Pritchard and Levi-Strauss interpreted the twins = birds formula to mean. So, how, even in a minimal sense, are we to construe what the equation signifies for the Nuer? It is worth adding here that Nuer Religion is the one work of Evans-Pritchard's that Winch recommends for accurately applying a Winchian methodology. My point here is that there is in fact no such methodology in Winch.

My fifth objection concerns a second kind of issue that can only be raised by assuming non-context-dependent criteria of rationality, i.e., why certain beliefs continue to be believed or cease to be held. For it is only by means of the application of rational standards of truth or validity that the mechanisms and secondary elaborations that protect inconsistent or unverified beliefs against predictive failure and falsification can be identified; this would apply both to the working of Azande magic and, according to Kuhn, the practice of 'normal science'.

This point relates generally to the question of social change. It seems that if, as Winch argues, that truth and validity as applied to belief systems is entirely internal to them why do people abandon religion or magical beliefs or scientific paradigms in the face of intolerable anomalies which as Lukes points out clearly cannot be internal to the paradigms. This applies not only to the rejection of a set of beliefs by rational criticism but where, as Durkheim observes, conflicts arise not between a society's notion of the ideal and the rationally discernable real but between two different (possibly equally irrational) ideals - such as when a cargo cult replaces the missionary's Victorian Christianity. Winch either seems to be offering a view of society as a perfectly integrated system in the old extreme functionalist sense or else must be regarded as having nothing to say on this at all.

(The next point I take straight from Lukes' The Social Determination of Truth.)

Only by assuming the existence of non-context-dependent criteria of rationality can one raise questions about the discrepancy between, say, the conscious model of a tribe's marriage system and its actual structure. The issue here is not just one of the differences between an unverbaliſed and a verbalised structure (e.g. the Iatmul work with several principles for determining the preferred spouse, although as Francis Korn has suggested not all of these will be given equal verbal emphasis) but where the stated rules conflict with actual practice. I take an example from Lukes. Marx's description of the 18th century ideas of society as being composed of abstracted and isolated 'natural' individuals as 'insipid illusions' presupposed the verifiability of the further claim that it is in the 18th century, the very period in which the view of the isolated individual became prevalent, that the interrelations of society have historically reached their highest state of development.

My seventh objection concerns the 'reason' versus 'cause' controversy which is clearly central to Winch's thesis. My only point here is that this seems to be a rather sterile explanatory opposition at least in the way Winch puts it.

Levi-Strauss's structural analysis of totemism or say Needham's analysis of left-hand/right-hand symbolism demonstrate clearly a method of conceptualising social relations by using natural concepts possessing the requisite logical powers in terms of opposition and assimilation. This is done by showing how some part of nature is used as a model for certain social relations and groupings. The model is not a purely abstract one but a concrete one which is employed both as a logical matrix and as concrete analogy.

Now Bell, (Bell:1967) who has made the same point, rightly suggests something is gained in understanding by the revelation of the structural analogies in symbolic systems. Yet such understanding is not assimilated either to casual explanation or explanation in terms of reasons. Rather it is based on structural and hence formal analogies between empirically discernable realities and a system of concepts employed to communicate about some of these realities. It is this notion of structural analogy that needs to be introduced into Winch's discussions of sociological explanation. For example diachronic change at the level of demography, such as that involved in Riviere's discussion of the uneven distribution and rate of acceptance of different types of instruments for hunting among some South American Indians can be understood in terms of the preservation of formal relationships in a conceptual system although they now become relations between different contents. But the structural analysis of diachronic change hardly seems to fit with sociological understanding as Winch represents it, for Winch's philosophical argument based on what constitutes meaningful action operates at a level far higher than that of the sociologist. The sort of explanation which Winch expresses as the central core of sociological explanation misses the point of structural explanation and also, incidentally, seems to commit him to a radical conservatism, in sociological explanation as Bell rightly observes. I am now in a position to answer the second of two questions I raised earlier, viz, what are the appropriate criteria to apply to a given class of beliefs within a society. For any or all of a class of beliefs there are already 1) context-dependent criteria of rationality which specify for example which beliefs may acceptably go together; 2) there are also contextually provided criteria of truth - it is these which make 'twins are birds' true for the Nuer; 3) there are obviously contextually provided criteria of meaning. These last two points seem to me to sum up all that Winch is really at in his 1964 article.

It is one thing to say (and this is something with which I wouldn't argue) that in order to discover what for example the physicist means by 'neutrino' and 'mass' in the assertion 'neutrinos lack mass' we have to see how these notions operate within the language of physics, which includes observing the physicist's criteria for identifying and re-identifying abstract entities such as neutrinos and the conditions under which he applies or does not apply the term 'mass'. But it is another thing to suppose that it follows from this that there is no way of evaluating the truth of claims that occur within such a system or evaluating the truth of the theory itself. In fact the history of science shows there are a fairly clear set of criteria for evaluating rival theories and hence the truth of claims which arise within a theory. There are such considerations as the elegance of a theory, its simplicity, predictive success and ontological economy. Certainly the notion of 'truth' here is not a

simple matter of correspondence to an empirically discoverable fact; but we do have good a priori and practical reasons for preferring a germ theory of disease to witchcraft explanations. And this holds even if all truth is, as Winch suggests, ultimately theory-dependent. 5) There are also contextually-provided criteria which specify the best way to arrive at and hold beliefs. 6) In general there are contextually provided criteria which specify what counts as a good reason for holding a belief.<sup>4</sup>

Sometimes context-independent criteria of rationality will not take the analysis of religious beliefs very far in the form of relations between beliefs that are to be explicated in terms of "provides a reason for" as Fuller for example shows. But this does not as Winch seems to imagine mean they are dispensible. Both would seem to be necessary for the understanding of a belief system, the explanation of why they are held, how they operate and what their social consequences are.

Ross Bowden.

#### Notes.

1. I am borrowing substantially from Stephen Lukes' summaries of some of these positions that occur in On The Social Determination of Truth.
2. I am following fairly closely Lukes' statement of this in Some Problems about Rationality, p. 259.
3. In following Lukes' statement here I don't want to give the impression that I agree with everything in his two articles. In On the Social Determination of Truth, there seem to be eight separate arguments, or more accurately four arguments and four crucial "sorts of questions" that can only be raised for the sociology of belief if the four arguments are valid. Only one of these arguments (the two parts of which I reproduce here as my first two objections) seems to be valid and the possibility of raising only these (although I only mention two) of the crucial questions seems to follow given the validity of Lukes' central argument.
4. Lukes summarises these points in Some Problems about Rationality, p. 263.

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### Galileo and the Topological Space

In the intellectual history of a discipline combinations of ideas appear almost de novo, and yet upon closer examination they may turn out to have been part of the common store of thought for some time. The search for Galileo and the idea of the topological space are themes which may seem to have little or no necessary connexion. Their appearance as two motifs in Leach (1961) produced a paper of great analytical effect. Its title Rethinking Anthropology was of striking symbolic value: the date of its publication, or perhaps the earlier date of the public address (1959) upon which it was based, mark in retrospect a boundary time between the immediate post-Malinowskian period in British social anthropology, and that phase (however it be characterized) in which it is now. The content of that paper may be assessed, a decade afterwards, in different ways, but its symbolic quality still remains. Rethinking Anthropology is now part of the myth-dream. It is surely not ultimately comprehensible in all its parts to those many undergraduate and graduate students who have read it line by line, with so much apprehension and hope? No more perhaps than it was to its first audience in London in 1959. But a message was received then, and a message is still received now, novelly encoded although it is. However much its argument be dissected, with its maddening semantic jumps and ellipses, the symbolic Rethinking Anthropology remains immune to purely logical analysis. Yet it came into existence from common elements among which were the two I have already mentioned: the 'search for Galileo'; and 'the idea of the topological space'. Suitably Wagnerian motifs to accompany this, undoubtedly one of the most memorable and influential of those 'episodes in polemical, socio-anthropological tourneys in which the contestants, astride their conceptual systems, canter across the sparse empiric field....' (Derek Freeman, 1962:125).

### The Search for Galileo

The comparison of the state of the social sciences with that of the natural sciences at some earlier period has become commonplace. More precisely, there has been the expectation of a revolution in which a figure of the stature of one of the great innovators will appear: 'we are told this revolution has not yet taken place in the social sciences, or at least it is only now in process of taking place. Perhaps social science has not yet found its Newton but the conditions are being created in which such a genius could arise.' (Winch 1958:1).

In 1937, Radcliffe-Brown made a characteristic statement for social anthropology:

'The whole of modern mechanics did not become possible until, as a result of the work of William of Ockham and his followers and of Galileo, Newton was to formulate the concept of mass - a fairly simple and obvious thing - but no one of Newton's time had thought of it, no one had begun to think of it. Only after this concept had been thought out, developed and defined scientifically did we begin to get a science of mechanics. I am suggesting that we have not yet thought of the important concepts for social science. These are still to be discovered and developed and defined.' (1957:29).

He adds:

'There is always beyond (accidental discovery) an imaginative perception of a Newton and (a) Galileo. That is one reason why really important discoveries have to wait on genius' (1957:30).

Although delivered at a Seminar in Chicago in the spring of 1937, these remarks (which the editor refers to as containing Radcliffe-Brown's 'authentic style') were not published until 1957. They were, as a result, prescient in embodying the more typical concern of the '50's with Galileo. He, however, made the further statement: 'Newton's and Galileo's procedures were both fundamentally taxonomic' (1957:35), a view which Leach specifically refuted in 1959.

As Radcliffe-Brown had spoken in 1937, so Malinowski, posthumously in 1944:

'by the advance of modern physics since Copernicus, Galileo, Newton or Faraday, we would find the same differential factors which distinguish the scientific from other modes of human thought and behaviour. Everywhere we find, first and foremost, the isolation of the relevant factors in a given process. The reality and relevancy of these factors are discovered by observation or experiment, which established their permanent recurrence. Constant empirical verification as well as the original founding of scientific theory and experience, is obviously of the very essence of science' (1944:11).

He adds, quaintly:

'It is at this point that the claims of anthropology might be pegged out'.

So much for an older anthropological scientism. With Levi-Strauss (1953:540) we find that: 'It is by means of (certain) studies, which exhibit a truly "Galilean" outlook, that one may hope to reach a depth where social structure is put on a level with other types of mental structures, particularly the linguistic one.' He notes that he means by Galilean: 'aiming to determine the law of variation, in contradistinction to the "Aristotelian" outlook mostly concerned with inductive correlations..' a distinction which he specifically derives from Lewin (1935), of whom more later.

It is interesting that in a 1942 paper Lewin also connected 'Galileanism' with the advance upon simple classification that later appears with Leach:

'In the time of the Greeks, geometry shifted from a "classificatory" method (which groups geometric figures according to "similarities") to a "constructive" or "genetic" method (which groups figures according to the way they can be produced or derived from each other). Ever since, the "genetic definition"

has dominated mathematics. In physics a similar development occurred at the time of Galileo. Biology tried to take a major step in this direction when the system of Linnee was superseded by that of Darwin.' (Lewin, 1942/1952:61).

Although the Galilean image thus in one form enters social anthropology from social psychology, the mainstream of its more analogical use is better illustrated by Popper (1944-45 and 1957:1):

'Scientific interest in social and political questions is hardly less old than scientific interest in cosmology and physics; and there were periods in antiquity (I have Plato's political theory in mind, and Aristotle's collection of constitutions) when the science of society might have seemed to have advanced further than the science of nature. But with Galileo and Newton, physics became successful beyond expectation, far surpassing all the other sciences; and since the time of Pasteur, the Galileo of biology, the biological sciences have been almost equally successful. But the social sciences do not as yet seem to have found their Galileo.'

He specifically opposes this analogy with Galileo to Ginsberg's analogy with Newton in the passage (op.cit.:59-60):

'My point about the technological approach might perhaps be made by saying that sociology (and perhaps even the social sciences in general) should look, not indeed for "its Newton or its Darwin" but rather for its Galileo, or its Pasteur.'

He asserts:

'It must be admitted, however, that the success of mathematical economics shows that one social science at least has gone through its Newtonian revolution.'

With the full emergence of the image of Galileo, comes naturally the contrary image of the Ptolemaic system. Leach (1961:26-27) himself now says:

that it was wrong but  
'The trouble with Ptolemaic astronomy was not that it was sterile - there could be no real development until Galileo was prepared to abandon the basic premiss that celestial bodies must of necessity move in perfect circles with the earth at the centre of the universe.. We anthropologists likewise must re-examine basic premisses and realise that English language patterns of thought are not a necessary model for the whole of human society.'

He says: 'Of such cycles and epicycles there is no end'(p.26). He repeats (1962:240):

'The Ptolemaic system of astronomy which finally crumbled under the onslaughts of Copernicus and Galileo was just such a model of ideal types...Some of my anthropological colleagues appear to believe in a similar way that certain traditionally accepted sociological conformations are a "law of nature".'

We may compare this with Wiener (1948, 2nd edition 1961:viii):

'When I came to M.I.T. around 1920, the general mode of putting the questions concerning non-linear apparatus was to look for a direct extension of the notion of impedance which could cover linear as well as non-linear systems. The result was that the study of non-linear electrical engineering was getting into a state comparable with that of the last stages of the Ptolemaic system of astronomy, in which epicycle was piled on

epicycle, correction upon correction, until a vast patchwork structure ultimately broke down under its own weight. Just as the Copernican system arose out of the overstrained Ptolemaic system, with a simple and natural heliocentric description of the motions of the heavenly bodies instead of the complicated and unperspicuous Ptolemaic geocentric system, so the study of non-linear structures and systems, whether electric or mechanical, whether natural or artificial, has needed a fresh and independent point of commencement.'

Wiener acknowledges useful discussions with Dr. K. Lewin.

In the two such different worlds of non-linear electrical engineering and of unilinear descent systems the language of crisis looked back to the destruction of classical astronomy. It is not necessary to add to such quotations to show that the search for Galileo, (or Newton, or Darwin or Pasteur) and the perception of out-of-date Ptolemaic systems crumbling and tottering, were part of a widespread mode of expression in many disciplines - already analogical in its preciser usages: metaphorical or rhetorical in other applications.

### The Topological space

Kurt Lewin was responsible for the first important discussion of topology in relation to social studies so it is worth citing him at some length. His major work was the Principles of Topological Psychology (N.Y. 1936). Elsewhere he has this to say about the concept of the topological space in psychology and sociology:

'Psychology has to deal with a multitude of coexisting facts which are interrelated and have a relative position to each other; in mathematical terms, it has to deal with a "space".

'Mathematics knows a variety of different types of spaces. It is an empirical question as to what kind of geometry is best suited to represent the dynamic interdependence of that realm of facts which is treated in a particular science. Since Einstein it has been known that Euclidean geometry, which previously was the only geometry applied in physics, is not best fitted for representing the empirical physical space. For psychology, a recently developed non-quantitative geometry, called 'topology', can be used satisfactorily in dealing with problems of structure and position in a psychological field. This space permits representation of the position inside or outside of a certain region, the relation between parts and whole, and a great number of structural characteristics. All of this is done in a mathematically exact way but does not presuppose the quantitative determination of size, which is generally not possible in a psychological field...

'It is, I suppose, beyond question that sociology, too, deals with a "multitude of coexistent interdependent facts" - in other words with the "empirical space". The sociologists and psychologists should recognize what has been long known, that the empirical space is nothing other than a multitude of facts existing at a given time and showing certain types of interdependence... Better insight into the meaning of space in mathematics and physics should readily lead to the understanding that the social field is actually an empirical space, which is as "real" as a physical one.' (Lewin 1939, reprinted in 1952:150-151).

He goes on:

'For in sociology, as in psychology, one is frequently able to determine relations of parts and whole and changes in distance or direction without being able to determine quantitative relations of size, distance, or angle. In addition, these geometries seem to be particularly suitable for representing the peculiar combination of "cognitive" and "dynamic" factors which is characteristic of psychological and social fields, as well as a number of other fundamental properties of the social-psychological dynamic.'(p.152).

Levi-Strauss, in the same essay in which he specifically uses Lewin's 'Galilean' concept, (1953,1964:283), refers to topology as one of the fields in which it has been possible 'to develop a rigorous approach to problems which do not admit of a metrical solution.' He also says that 'social structure may have to deal with prehistory, archaeology, and diffusion processes as well as with psychological topology, such as that initiated by Lewin or Merino's sociometry'(1953:532; 1963:290; cf. also Nadel 1957:145).

When Leach(1961) introduces a topological analogy into his Rethinking Anthropology, it is therefore surprising that he does not refer to Lewin, whose well known system illustrates the complexity (and even the dangers) of a topological model. In this connexion it is worth recalling Braithwaite's criticism that 'to be profitable the system must be representable by a calculus in which formulae are genuinely derived, according to the rules of the calculus, from other formulae'. In referring to Kurt Lewin's Principles of Topological Psychology he says: 'the mere translation of tendency statements into mathematical language is not sufficient to make a quasi-deductive system out of them. The essence of mathematics is not its symbolism, but its methods of deduction.'(Braithwaite,1953,1960:366 note; my emphasis.)

There is absolutely no reason why social anthropologists should not explore these fields. A simple statement of the basic mathematical concepts involved may be cited from one of the most elementary works:

'In general any set of objects is called a topological space if a collection of its subsets are singled out so that the collection has the three properties we found in the open sets on the line: 1) The whole space and the empty set belong to the collection; 2) The union of any number of sets in the collection is also in the collection; 3) The intersection of any two sets in the collection is also in the collection. When these three conditions are satisfied, the sets in the collection are called the "open sets" of the "space"'. 'Under this definition, any collection of objects can be converted into a topological space, usually in more than one way.' (Adler,1958,1960: 120).

Leach's presentation of topology through the rubber-sheet analogy was possibly the more evocative one to use to introduce the matter to a group of functionalist anthropologists in 1959. It may be expressed so:

'We say that two topological spaces are essentially the same or are homeomorphic if there is a one-to-one correspondence between them that preserves the topological structures embodied in the system of interlocking open sets.'(Adler op.cit.:123).

As is well-known the topological space can thus be approached from set-theory or from geometry; from the latter Euler's Theorem is an illustration.

Science and Myth

Galileo and the topological space were motifs united by Lewin in the thirties. They were united again by Leach in 1959/1961. Levi-Strauss lies somewhere at the heart of the transmission. Lewin's application of the natural scientific analogy was, as we saw, serious enough to be sternly rejected by Braithwaite. The temptation to do the same for Leach should perhaps be resisted. Strangely enough we should, in these less positivistic days, even be prepared to say: 'the essence of mathematics is its symbolism' (by a twisting of Braithwaite's use of the term 'symbolism').

The formal systems of science and the images of science seem to form co-existent and interrelated semiotics. The search for a new synthesis, and for a non-mensurational view of systematic relationships, could be apprehended only symbolically in the fifties by most social anthropologists, given the characteristic bases of their training. Leach's paper, as he no doubt would be the first to agree, is brilliant myth rather than mathematics. Yet the great interest of mathematicians in topology is itself part of the general intellectual movement of our time, of which the structuralist or 'neo-anthropological' trends in social anthropology are another expression. Topology was for Leach as the phoneme was for Levi-Strauss - something good to think with.

We may finally note that Galileo was chosen by Popper, for one, to symbolize the awaited new era for social science because of the essentially experimental and technological breakthrough associated with the invention of the telescope. This sort of expectation is generally less appealing nowadays: the computer once appeared to embody it; but we shall probably need a Newton after all. At least we already have a few Keplers about.

Edwin Ardener.

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£.s.d. varna

In what was both a theoretical and a methodological note, the French sociologist and indologist, Louis Dumont made the following suggestion:

'The time has perhaps come when the mirror which anthropologists direct at other societies should be turned back by them on ourselves, when we should try and formulate our own institutions in comparative language, i.e. in a language modified by what we have learnt of different societies, however incomplete it still is.' (Dumont:77).

Along this line of thinking, we might develop further the comment by the Indian sociologist, G.S. Ghurye that of all the social systems he has studied in the world, it is the classical English estate system that most resembles the Hindu varna scheme of caste inter-relationships.

Using the varna scheme of Shudra, Vaiysha, Kshatriya and Brahmin, I intend to apply this to an analysis of traditional British society as it is reflected in the currency system and in certain drinking habits as a system of signs (Barthes) tangibly representing attitudes in the formation of their social structure. At the end of the analysis, it will be shown how these systems relate and how the forthcoming D-Day (15 February, 1971) symbolizes recent changes in the traditional social structure.

The system of organizing units of money into a four-tiered system, related in unequal units is unique in the world. The three tiered units of currency is usual because these units are unrelated by any common divider. I propose the following alignment:

Shudra.....	Pence
Vaiysha.....	Shillings
Kshatriya.....	Pounds sterling
Brahmin.....	Guineas

The lowest of the English estates was the peasant farmer and he conducted most of his daily business in pence. The urban proletariat also used this as their primary medium of exchange. There is much historical evidence to show that the food and "luxury items" (i.e. tobacco and alcoholic beverages) purchased in previous times by this lowest rung of the social ladder in both India and Britain was calculated in pence since the next unit up, the shilling, was a large sum of money at one time. The British Shudra rarely saw shillings.

The Vaiysha or merchant class are often referred to by the Francophile (reluctantly) British as "those of the town" by a French term - the Bourgeoisie. These traders dealt in larger amounts and required larger units for their commerce.

This resulted in the shilling which was calculated, after the Conquest, to consist of twelve Shudra units. To this day, items appropriate to this merchant class, such as books, are still quoted in shillings.

The Indian Kshatriya varna of warriors and rulers finds its equivalent in the urban "civil servant" of Britain. This group, well-paid and dealing with the affairs of government, required a larger unit of currency with which to conduct their affairs and this resulted in the construction of a currency tier equal to twenty Vaiysha units.

These differential amounts may be explained as signs of separation. The threat of Shudra to Vaiysha was low and so the amount between them slight, while Vaiysha ambitions towards the Kshatriya were greater and so a greater difference was required. It is still not uncommon for British Vaiysha to attempt to boost their social prestige by attempting to find Kshatriyas in their ancestry.

The hereditary and spiritual heads of British society, the aristocracy, are Brahmin and would have liked to have put as much differentiation between themselves and the disliked but necessary Kshatriya. But, the Kshatriya, as military figures and powerful civil servants were too influential. All that was possible was to construct a fourth tier in the seventeenth century called the Guinea, only being equal to one Kshatriya and one Vaiysha unit. However, as the Empire grew and Kshatriya power increased, the Guinea was driven out as a tangible unit of currency and was last minted in 1813. The Kshatriya had triumphed in the area of their greatest competence and power - the formal running of civil government. But, in areas of ritual, the Brahmin were still dominant and until this day have maintained the usage of the Guinea when referring to the prices of prestige articles such as the price of paintings, prestige holidays, or other articles of quality ritually sanctioned by the Brahmin.

In the latter part of the twentieth century, the Kshatriya have become more and more important. The national Panchayat, which formally was rigidly divided into Brahmin (Lords) and Kshatriya (Commons) consultative function is now to all intents and purposes, a single practical body, with the Brahmins being reduced to merely ritual functions.

With the coming of the change in the monetary system to a system of decimals, the last ubiquitous ritual power of the Brahmins is being threatened. The Kshatriya unit, the Pound sterling, is the basis of the new system. The old symbol of Vaiysha subservience, the shilling, is to disappear altogether and reflects their rising importance. The new penny, symbol of the Shudra, is to be revalued by two hundred and forty percent to make it a viable separation between Kshatriya and the Shudra as it never was before. The Guinea, with its dependence for symmetry upon both the shilling and the pound, will be made to look ridiculous, as recent publications on decimalization suggest.

A significant point here is that there were, from the Vaiysha, suggestions that it would be more practical for business affairs to base the new currency on the "new pound", or "old" ten shillings. The battle for symbolic dominance was brief but even though a vestige of the Vaiysha (the sixpence) will remain for a short, unspecified time, the Kshatriya victory was complete. It may be remembered that in other sterling area countries of the Commonwealth, where the Vaiysha tend to dominate, their unit of currency has been retained in the form of a "dollar" - that is, the shilling base, but without the name.



Now, we may shift to another important locus of attention in British society - the pub. Endless activities circulate around it, and the British will make any excuse to enter it and take refreshment. There may be something to the sharp division between Public and Saloon when applied to the scheme above, but it is the various different drinks served in these two sections which carry more luggage as a system of signs.

This most characteristic of institutions, where behaviour is ritually prescribed and circumscribed in a variety of ways has its own internal coherence or "boisonlogique". By carefully assigning certain characteristic beverages to the afore-mentioned four groups an obvious analytic paradigm emerges

Shudra.....Beer (Scotch)  
 Vaiysha.....Cyder (Wine)  
 Kshatriya.....Scotch (Beer)  
 Brahmin.....Wine (Cyder)

Processed	Beer(WC)	Cyder	Wine	Formented
Culture	Scotch	K	B(SE)	Nature

The first chart names various classes in British society and postulates the drinks characteristic and alternative (in parenthesis) for them. The second diagram interprets their inter-relationships or, "les structures élémentaires de la parenthèse."

The Shudra working class identifies itself with the nation's beer, but also latently enjoys the prestige of Scotch. The Viaysha merchants (especially of the characteristic south-western English) take cyder as their daily drink, but aspire to "the better things in life," with wine as its symbol. The Kshatriya civil servant is a firm scotch drinker, but will often take a pint of beer at his local. Lastly the Brahmin aristocrat has wine as his symbol, but for a sort of rustic sensibility will often drink draught or even bottled cyder.

The second diagram shows the inter-relationships (arnatures and axes) of the scheme in the first diagram. Wine and scotch are on the "strong and expensive" axis (SE axis), and reach their apogee in the Brahmin, whereas cyder and beer are on the "weak and cheap" axis (WC axis). The oppositions of scotch to wine and beer to wine are based upon both societal and digestive criteria. Cyder and wine enjoy the affinity, on the other hand, of being at once fruit products and also the result of simple fermentation, while beer and scotch are processed. This derives an "F" axis (Fermented) and a "P" (Processed) axis, respectively.

Further, it must be pointed out that there is a con-  
 . gurence in an "R" (for Regional) axis between cyder and  
 scotch, whereas beer and wine are made in many places. A  
 fuller statement, in more rigorous fashion of these relation-  
 ships, is as follows:

<u>F axis</u>	<u>SE Axis</u>	
C = W	S // W	⇒ C :: W : B :: S (F + F axis)
	B // W	
<u>P Axis</u>	<u>WC Axis</u>	C :: S : B :: W (B + F axis)
<u>B = S</u>	C // B	

It is clear that the Shudra have an affinity for the Kshatriya and they swap beverages to symbolize this. They may also exchange women hypergenerously, while the Vaiysha look to the Brahmins in a similar manner. This may be symbolized as:

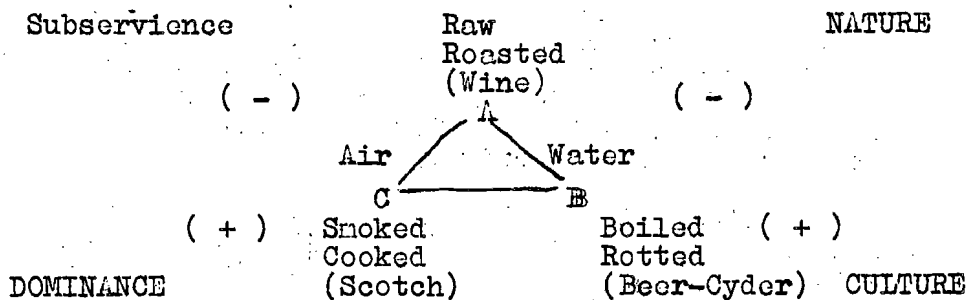
w/b : S/k ⇒ s : k : w :: k exchange

In terms of class origin, this is predictable and is what I would term the principle of alternative opposition. There is in the sociological literature (Cohen, Simmel) much to show that enmity exists between closely competing groups in a society and though traditionally the estate system in Britain may have functioned as a system of inter-relationships, conflict may have become greater in recent years due to influences from the West (principally the United States). While this enmity may be less noticeable in the urban British centres, where the system ceased to exist as a viable unit some years ago, it shows appallingly in village Britain. Therefore, to understand the operation of British social structure, it is to village Britain we must turn rather than the highly Westernized urban centres.

Our second figure can also tell us about attitudinal and stereotypic features of the social structure. As we saw, there is a WC axis centering on the Shudra. WC jokes, as most know, are of "low" character and are considered "dirty", which is precisely the popular stereotype of the Shudra. This lies in opposition to the SE axis of strength culminating in the Brahmins - the strongest group in the society. The R axis reflects the regionality of the groups from their characteristic drinks - the most regional products (scotch and cyder) are representative of Vaiysha and Kshatriya who, oppositionally, are the most mobile group in Britain, whereas the least regional products on the R axis (Wine and beer) are characteristic of Shudra and Brahmin groups most tied to the land in a particular region. The F axis, not yet mentioned, represented the relationship of the drinks to food and centres on the Vaiysha who, as the society's merchants, are most tied to this function as suppliers of sustenance to the social order.

I am, of course, not the first to show congruences between gastronomic preferences and social structure. (see Levi-Strauss: 411).

In order to see better how this works out in terms of social dominance patterns, I have drawn-up a "triangle boisonaire" from the previous data:



At the peak of the triangle is the wine-drinking Brahmin. He drinks a beverage which is made from a raw fruit which is not allowed to rot fully, as is the case with cyder. The "roasted" must be taken metaphorically as this refers to the carefully prepared and refined technique of the food of this group, but may also have something to do with a wine once characteristic of this group which is, in effect, roasted - Madeira. At angle B, are the beer drinking Shudra and the cyder drinking Vaiysha. Beer is boiled in its preparation, while cyder is the product of rotting fruit. The fact that these two groups are found here should not be surprising as they came historically from the same low rank and have only recently (the middle ages, perhaps) been differentiated.

At angle C, are the scotch drinking Kshatriya, whose beverage is slowly cooked in large vats and often poured into smoked casks for curing. Significantly, air (often smoked and cooked, therefore hot) is the only separation between the Kshatriya and the Brahmin, while water (a more solid and therefore more prohibiting agent) separates the lower Vaiysha/Shudra from the higher Brahmin.

Le triangle boisonaire offers the final confirmation for the hypothesis that the dominant caste, following Srinavas, among the British, is the Kshatriya, as they are in the most favoured and strongest part of the triangle, enjoying a complete plus dominance over the wine drinking Brahmin, who are, respectively, the "cultural" and the natural rulers of Britain.

It is also clear that the Kshatriya understand the significance of such a construction as they jealously hoard their strengthening beverage to themselves by a high tax. The relatively lower tax imposed on beer and cyder insures it for the masses, while Brahmin ritual power obfuscates the understanding and use of wine by others. It is clear, therefore, that while higher cultures organise their lives on the basis of preferences of taste and reason, "chez les sauvages" of Britain, things are done "to protect the purity of their beings." (Levi-Strauss: 419).

Further analysis of Britain reveals a continuing obsession with congruent primitive classifications based upon the four-part scheme:

	Land Divisions	Radio	Entertainment
Shudra	Ireland	Radio I	Television
Vaiysha	Wales	Radio II	Cinema
Kshatriya	Scotland	Radio III	Theatre
Brahmin	England	Radio IV	Concerts and Opera

	Pets	Transport
Shudra	Budgies	Public
Vaiysha	Dog	Taxi
Kshatriya	Cat	Private cars (parked)
Brahmin	Spec. pets	Private cars (unparked)

Two points ought to be made with respect to any judgement of the ideas and analysis contained in this paper. First, I have intended this effort to be suggestive of a point of view and I hope that I manage, (to give ...ideas even when (you, the reader, doesn't)..really know what ( I an)... saying' (Leach:1967:xvii). Second, and with most particular reference to the unsupported correlations of British native, or 'practical' (Leach:1968:1) schemes of symbolic classification of experience as it relates to social structure, I would hope that the following point would be taken seriously:

'Generalisations on such a grandiose scale are likely to provide many easy targets for the hostile critic and there are some weak patches in...(the) argument but I cannot see that this really matters. In a comparable way it is easy to show that Freud was very often wrong on points of detail; this does not detract from the massive validity of Freud's major generalisations. Even if time should show that some of the items of evidence have been misplaced, the fundamental method of (the)...analysis is an innovation from which there can be no retreat' (Leach:1970:185)

Grant McCall.

Note 1. Motor cycles enjoy a somewhat less than respectable image and they serve extremely adequately as a symbol of defiance

by the young. They come between the Kshatriya and the Brahmin shared use of private vehicles, the former being 'parked' and the latter being 'unparked.' The Kshatriya, in spite of his being able to afford to purchase a private vehicle of great power and prestige is thwarted in the full expression of this ownership by having to park his machine, a most frustrating experience and one which the Kshatriya is willing to spend large sums of money on to facilitate even to the point of destroying beautiful urban features which obstruct his building plans for parking lots and multi-storey car parks. The Brahmin, on the other hand, owns a private vehicle, but it is chauffeur-driven - that is, it requires no parking space about which the Brahmin must be concerned. He is free to take his private vehicle (or to be taken in it) to where he wishes without being concerned where the machine will rest while he is not in it. When the Brahmin is ready to depart, his vehicle is brought to him at his will. As was shown before, the Brahmin and the Vaiysha share a number of affinities and the latter's taxis are entirely compatible with this as their appropriate form of transport. The motorcycle, is a non-chauffeur driven vehicle, but, on the other hand, requires no or minimal parking space! It is, in short, outside of the classification system and is, thus, polluting. (see Douglas). It is an abomination for most of the society as such and only those similarly "impure" for other reasons and thus outside of the system will find it appropriate for their use. Perhaps recent attempts of the Kshatriya to mark cycle parking spaces in large cities is an attempt to bring impure cycle owners into the system or, at least, to detract from their affinity with the Brahmins. It is probably no accident, in this line, that deviant cyclist clubs often take on names associated with royalty (i.e. The Knights, The Kings) or, in another vein, use names to symbolize their out-of-place and paradoxical conflicting low/high status within the classification scheme (i.e. The Hell's Angels).

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African Medical Taxonomy:  
with special reference to mental illness.

It has been said that primitive classification of illness is completely unrelated to Western categories, and that in particular all forms of illness are said to be caused by witchcraft or supernatural agencies in primitive societies. Field discussed the problem briefly:

'The first problem of classification - whether to adopt an ethnological or a psychiatric basis - was easily disposed of. Had I chosen the former, each category would have remained hopelessly heterogeneous.' (Field:1960:149)

Since she felt that "witchcraft cases" would have covered most of their diagnosis, she used Western diagnostic labels such as depression and schizophrenia.

Over the last few years more interest has been taken in primitive classification. Kinship has always been a central area of investigation of such systems by anthropologists, but in the last decade other folk taxonomies have been studied, the field now being called "ethnoscience," by some. Before ever discussing the classification of illnesses in different cultures one has to ask if there is the category "illness" as such. There seems to be no report of a society that does not use a category closely resembling what we call illness. (It can thus be called an "etic" category since it is universal) (Romney and D'Andrade:1964). In the case of spirit possession, there may be areas where certain states are defined as illness in one society and not in another, but even in our own culture there are those who are difficult to classify as "ill" or "not ill". There may be doubts, for example, as to whether a person should be called eccentric or mentally ill.

The early papers on the subject of primitive concepts of illness tended to concentrate on concepts of causation, and although these could be classified, they were not the only system used and in fact seldom relate to the actual naming of illnesses. Clement's paper on "Primitive Concepts of Disease" was the first major work in English and deals entirely with theories of disease causation, discussing the ideas found throughout the world and postulating various patterns of diffusion to account for the presence of these ideas. He actually defines his subject as:

Primitive concepts of disease are those ideas held by primitive people as to the cause or genesis of sickness. (Clements: 1932:126).

His classification of causes has been modified slightly but is still the basic text on this subject. He firstly extracted three basic categories of cause:

- (1) Natural causes.
- (2) Human agency.
- (3) Supernatural agency.

He preferred, however, to call the last two Unnatural Causes of Disease and divide these into:

- (1) Sorcery. This is the action of human beings usually using contagious or imitative magic. He includes the evil eye in this.
- (2) Breach of Taboo. This may be unintentional. Confession is the usual form of treatment.
- (3) Disease - Object Intrusion. This is a tangible form of some disease producing agency and is thus a material form of the next.
- (4) Spirit Intrusion. This is the presence in the body of spirits. These may come by themselves, they may be sent by a sorcerer, or they may come as punishment for breach of taboo. Spirit intrusion and spirit possession may be similar and can be confused by ethnographers.
- (5) Soul Loss. This may occur by accident or the soul

may be abstracted by spirits or sorcerers.

Clements mentions three cures for spirit intrusion:

- (a) Commands to depart or exorcism.
- (b) Mechanical means such as cupping, bleeding or purging.
- (c) Brushing into an animal or object by transference.

Clements however does not mention the method of taming the spirit so that intrusion changes to controlled possession, which is the fundamental step in most instances of healing; by initiation into a formal 'possessed' relationship with the spirit that is causing the illness.

Hallowell in his discussion of Clements' work points out that sorcery can make use of several of the other causative techniques mentioned. Sorcery involves (1) the human agent (2) the technique employed (3) the specific proximate cause of the malady which actually produces the symptoms. This includes object or spirit intrusion and soul loss. (Hallowell:1935) Murphy uses Clements' categories in her account of the beliefs in disease causation amongst the Eskimos. (Murphy in Kiev (ed) 1964:61-69).

There has been some discussion as to whether there is a belief in natural causes at all in primitive medicine. This point is well reviewed by Ackerknecht in his paper "Natural Disease and Rational Treatment in Primitive Medicine." Most authors report that minor illnesses are the ones usually considered "natural". At the beginning of a disease no supernatural danger may be felt and home remedies are given. There may not be sufficient concern to consult a diviner. Some authors claim that all diseases are believed to have a supernatural origin, but in mild cases the patient will be content with palliation and not spend the money on a diviner. Only when simple remedies fail is the complicated supernatural machinery put into motion. These diseases considered natural by primitives are: (1) The very slight. (2) The very common ones. (3) Diseases imported by Whites. (Ackerknecht:1946:272). The reason for this latter is not obvious. It may be that it is an artifact due to informants being polite to White investigators, or it may be that the explanatory system is so rigid that it cannot accommodate new diseases. Ackerknecht himself does not believe in the notion of rationality in primitive medicine and says that mild illnesses are just not explained, it is not that their explanation is naturalistic. (ibid: 478).

Prince, in his study of Yoruba beliefs about mental illness found that 20% of cases whom he saw in an indigenous treatment centre were said to be caused naturally. The factors involved were faulty diet, small insects, worms, black blood or watery blood, hemp smoking and other toxic effects and hereditary factors. (Prince in Kiev (ed): 1964:96). Presumably some of these could be thought to be due to magical processes also. The Ganda believe that epilepsy can be due to a lizard in the brain which may be there at birth and grow or be introduced by magical means. (Orley: 1970).

The distinction between sorcery and witchcraft has been emphasised by Middleton and Winter. The term sorcery should be used to describe instances where objects or medicines are used against victims. Such acts can be performed by anyone and are only called magical because there are no grounds in Western science for believing in them. Witchcraft, however describes a mystical power, residing in particular people. It may be enough for a witch just to wish evil against a victim.

Witches are usually thought to have other powers such as travelling at superhuman speed, turning into animals, or dissociating their spirit from the body and thus going about doing evil while apparently asleep in bed. (Middleton and Winter (eds): 1963).

The Ganda believe that illnesses can be brought by spirits or witchcraft substances. The spirits are of two types that can work in two different ways:

1. Those that do not kill people but merely require to enter into some form of relationship with people, and be placated.

2. Those spirits which are exceedingly dangerous and whose aim is to kill people. These latter are sent against people by other humans.

Amongst the nearby North Kavirondo Bantu a visitation from a spirit is usually a slow illness, not a sudden violent outbreak. This latter would be attributed to human agents. (Wagner: 1959:164).

The Ganda do not have a strong belief in witches, but rather in sorcerers. Some witches do exist who prepare substances which may be placed in such a way that the victim passes near them. These may cause a sudden unpleasant or even fatal illness but if not immediately effective, they may go on to produce a more gradual and persistent illness. I saw two cases which, I was told, were certainly due to witchcraft substance. One was a progressive wasting disease which lasted two years and ended in death, and the other was a dementia. When these substances are walked over in the path, they produce a painful swelling of a part of the body, typically a foot, spreading to the rest of the body. It may produce small sores over the legs. There is a separate Ganda word for poison. Poison is characterised by having to be taken by mouth and it produces severe abdominal pain, vomiting, and ultimately death if not treated. It is sometimes called the witchcraft substance of the Europeans. There is no difference in kind between witchcraft substance and poison, and there is considered to be no essential difference in their mode of action, even though the one is swallowed and the other can work from a distance. Similarly there is considered to be no difference in kind between those medicines which are given by mouth, those that are rubbed over the body and those that are worn wrapped in a piece of cloth around the arm or waist. (Orley:1970:19-20). Beattie has made a similar point about the neighbouring Nyoro:

'So for Nyoro poisoning is a kind of sorcery, indeed the most typical kind: as Westerners we distinguish, in terms of what we know of the operation of chemical and physiological laws, the act of putting noxious medicine in a person's food, from the procedure involved in buying another kind of medicine in a path where an enemy will pass: Nyoro make no such distinction. Even to burn a person's house secretly at night with the intention of killing or injuring him and his family is a kind of burogo (sorcery).' (Beattie in Middleton and Winter (eds): 1963:29).

There are difficulties enough for those who wish to relate Western disease categories to primitive categories. For those who wish to relate Western categories to primitive categories of causation the position is even more difficult as is illustrated by Le Vine's study of the Gusii of Kenya. (Le Vine in *ibid*). He states that in any one instance, death or an illness may be ascribed to different causes by the different people involved. For the uninvolved observers it is most convenient to ascribe misfortune to natural causes or

neglect. An ill man himself is likely to say that he has been bewitched and blame a relative or neighbour rather than to think that he is being punished. The relatives in their turn, either use the occasion to express their hostility against a third party whom they claim is bewitching the ill man or, if they wish to avoid disruption, will claim that it is caused by the ancestors. Women tend to be more prone to make witchcraft accusations, perhaps because they are outsiders who care less about their disruptive effect. Lineage elders on the other hand, who wish to preserve unity, try where possible, to turn blame of others into self-blame.

The Ganda recognise the way in which different people attribute different causes to an illness. They have a proverb; a Lubaale, Hero Spirit, punishes with reason, provided that it has not killed one of your own relatives. Bennett found difficulties when asking Ganda parents about their childrens' diarrhoea. Although relatives admitted a possible cause of the diarrhoea was obusobe, a ritual mistake made in pregnancy or infancy, none admitted that this was the reason for their own child's diarrhoea (Bennett et al:1964). Although there are said to be some diseases that result from the breaking of certain taboos, it does not seem to be an important reason given in these days. Madness in a mother following child-birth is thought to be brought by her committing adultery during pregnancy. Southwold investigated the beliefs associated with a rash which may be seen in infants because the mother ate salt during the pregnancy. He states that although he had read about this belief,

'I never heard of a specific case of anyone with the disease, and when I approached it from the other end, by asking what would happen if anyone did break the taboo, people were pretty sceptical whether anything would.' (Southwold:1959:45).

'People will tell you that buko is an illness that people get through committing incest, but when you ask what happens to a man who commits incest and is not prosecuted they will say "nothing".' (ibid:p 46).

There appears to be no literature on the classification of "natural" causes in primitive medicine, but these are not necessarily clearly distinguished from supernatural causes. What we may call "natural" are those explanations which are most sensible in Western terms. In many cases these causes are thought to be secondary to supernatural factors. The "natural" causes that appear in the literature may be classified:

- (1) Invasion of the body by external agents;
  - (a) Living agents, worms, lizards or insects.
  - (b) Non-living agents such as poisons. Poisoning is only an instance of disease-object intrusion which happens to fit in with Western notions of causation.
- (2) The mal-function or movement of certain organs or the blood.
- (3) Dietary factors which includes eating foods which are prescribed for ritual reasons.
- (4) Hereditary factors which may be expressed in terms of some family spirit.
- (5) Environmental factors such as the moon's phases or cold winds.
- (6) Contagion, the mechanism of which is seldom if ever specified.

Thus Hoernlé says of the Bantu of South Africa that: 'Worms they have seen in their stools, and often think that the pains in their bodies may be caused by such wandering about among the different organs, or even by the organs getting displaced within the



body.' (Hoernle & Schapera (ed):1937:227).

Amongst the Ganda, stomach aches of all kinds are usually referred to as *enjoka*. Whilst in general this word has the connotation of "worms", not all forms are thought to be due to worms. An early European traveller in Uganda reported an encounter with worms amongst the Ganda.

'Another difficulty is that natives often come for treatment for imaginary complaints. One of the great Waganda Chiefs, the Kago, used to come to me regularly with his story of the "worm". One day the "worm" was in his heart; another day in the small of his back, another time it had travelled to his arm, and so on. I gave him the benefit of the doubt, and treated him for rheumatism, oppression, or anything rational bearing on his symptoms. In spite of all, his ailments grew daily more stout and strong. One day I gave him a strong purgative. He did not reappear for a week; when he came, he was accompanied by one of his men leading a fat sheep. He had never given me the slightest acknowledgment for the scores and scores of times he had come to me for the treatment which he was receiving gratis. This day he solemnly made me a present of a fat sheep. He reassured me that my last medicine was splendid. The effect was such, he said, that he really thought he was about to die, and that it had utterly prostrated him for days. He felt, however, that he was cured and he came to thank me publicly. It was many months before he was troubled again by his old enemy the "worm". I was interested when one day the Mission doctor, Dr. A. Cook, incidentally mentioned to me that some natives came to him with imaginary diseases. They cause a serious loss of time to him whom I know to be one of the most able and hard-working men I have had the privilege of meeting either in professional consultation or in private life.' (Ansonge:1899:191-2).

The Ganda also classify some diseases as "those of fornication". These include the venereal diseases but also include the madness that occurs after child-birth. Although they have the notion of contagion they cannot describe the mechanism by which they think it occurs. They believe that epilepsy, leprosy and consumption are contagious and they isolate people suffering from these diseases. (Orley:1970:35).

The relation between ideas of natural causation and witchcraft amongst the Azande have been clearly laid out by Evans-Pritchard.

'In speaking to Azande about witchcraft and in observing their reactions to situations of misfortune it was obvious that they did not attempt to account for the existence of phenomena, or even the action of phenomena, by mystical causation alone. What they explained by witchcraft were the particular conditions in a chain of causation which related an individual to natural happenings in such a way that he sustained injury.' (Evans-Pritchard: 1937:67).

Thus a victim of some accident, although seeing clearly some of the natural events leading up to it, wants to know why it should have happened to him rather than someone else and at that time rather than at another. Misfortune requires a fuller explanation in addition to that which can be given by crude observation.

The naming of diseases, however, is seldom related to causation in primitive medicine. Naming tends to depend on the part of the body affected and the symptoms of the disease. Sometimes the response to treatment is used to define the illness in broad terms. Junod says that:

'Thonga call the complaint from which they suffer by the name of the organ affected: for instance "I have a foot, I have a hand, I have a neck", means; "I have a pain in my foot, my hand or my neck". "He has a head" means "He is mad". For "I have a headache", the expression "I feel my head", would generally be used.' (Junod:1913:430).

The Tallensi name illnesses by reference to the part of the body most affected, but there is a vague notion that all forms of illness are manifestations of disorder either of the head or of the belly or of both. (Fortes and Mayer:1969:41). The Azande know diseases by their major symptoms. Diseases are named (1) After the part affected. (2) After the sensations they produce or their effects on the organism. (3) After something in nature to which they bear a resemblance. (4) After their cause. (5) After their cures. (Evans-Pritchard:1937:482).

The Ganda also have a tendency to think of their illnesses in terms of that part of the body affected. Thus a cough may be referred to as ekifuba (chest), provided that the context indicates that illness is being talked about. By changing the prefix of the word, one can indicate different diseases of the chest. Thus there is akafuba (consumption or tuberculosis) and olufuba (asthma). Because of this already established way of thought, the Ganda seem to have readily taken to the idea of a group of illnesses within the category 'diseases of the brain'. (Orley,1970:p4).

The Subanum also use the part of the body affected in their naming of disease, but they use other criteria as well. (Frake:1962). Some causes are important criteria in classification such as a wound, a bite, a burn or a worm, whereas others are not important as criteria in naming since they appear to cover a very disparate collection of phenomena. Such are the sorts of causes listed by Clements such as object-intrusion or soul loss. It is only the exceptional case that is re-named as a result of a seance or divination. Frake's already classic description of Subanum naming of disease emphasises the way that symptomatology is used. Symptoms may be localised internally or externally in certain parts of the body. Deformity in a particular area or pain, itch or irritation in particular areas may help to refine the diagnostic name.

Mental illness appears to be recognised in all areas of the world and is defined by its symptomatology as reflected in the patient's behaviour, as also is epilepsy. Jilek and Aall-Jilek report from Tanzania, however, that:

'The Wapogoro do not possess a concept of mental disease. They have, however, a fairly clear notion of what constitutes a deviation from culturally accepted patterns.' (Jilek and Aall-Jilek:1967:208).

It is uncertain what they mean by this. Edgerton reported a study of four East African tribes. (Edgerton:1966). All had terms corresponding fairly closely to Kichaa in Swahili which is the word usually used for madness. No great variety of terms were found in any one language, and where there are several terms they do not seem to imply different symptomatology.

Questions about aetiology produced no consistent replies and certainly witchcraft was not seen as the sole cause. Some attributed it to a worm in the brain, not usually introduced by witchcraft, others said it came for no reason. All tribes recognise the possibility of multiple causation. Further enquiry in each of the four tribes for a description of what was meant by that tribe's word for madness brought very similar answers but with some differences in emphasis. The Sobei (Uganda) most often mention nudity, shouting, talking nonsense and violence. The Kamba (Kenya) mentioned violence and nudity most frequently as the features of madness. The Hehe mentioned nudity frequently, but most often mentioned a timorous retreat from people to a solitary life in the bush. They did, however, talk of two types of madness, the violent and the passively fearful. The Pokot most often referred to talking nonsense, but murder and arson were also mentioned. They also distinguished between "wild" and "mild" mad people, but these were not regarded as mutually exclusive types. A notable emphasis in all the tribes was upon nudity. Even the Pokot whose men are typically nude, are horrified by nudity among women. Violence seems very frequently mentioned, as has been noted over much of Africa. Hallucinations were very seldom mentioned, but all mentioned that psychotic acts occur without good reason. None of the behaviour regarded as mad in these tribes would not also have been so regarded in the West. Two of the tribes recommend treatment more than the other two, and this may be related to their belief that it is caused by witchcraft and at least temporarily curable, whereas those who regard it as incurable tend to recommend harsh treatment more than medicine.

There have been very few studies of the indigenous classification of mental illness. Those attempted have usually done nothing more than list the names given to various conditions that would be called mental illness in the West. Examples of this type of study from Africa have been reported from amongst the Bemba, the Shona and the Yoruba. It is very difficult from these accounts to distinguish what are the indigenous systems of classification and what has been imposed upon the terms to make them fit Western diagnostic groups. Thus Brelsford discusses various classes of the insane, in many of which there are various sub-classes given Bemba names. He lists the classes as (1) Idiots and imbeciles. (2) Madness. (3) Temporary violence or fits. (4) Hysterics. (5) Melancholics. (6) Bhang intoxication psychosis. (7) Eccentricity, (not madness). (8) An aimless wanderer. There are several terms given for varieties of sub-normals. Cases which show violence, even though sub-normal, are called "mad". One informant described the typical madman:

'This person does not mind anything. He stabs another without fear. He jeers when he is most painfully tied up. He swears and curses without fear and reason, and he walks naked without shame. He does great violence and is the proper Legion spoken of in the Bible.' (Brelsford:1950).

Gelfand gives the names of many disorders in Shona, but there seems no system in his description. This may be due to the fact that there is no system behind these names. We see, however, the name ebenzi included in three disease names, and we can assume that they should be classed together in some way, but we are not told what meaning this word has. This same applies to the word kupenga which also is included in three disease names. (Gelfand in Kiev(ed):1964:165-170).

Among the Yoruba the term were is used for all forms of insanity, particularly chronic forms. (Leighton & Lanbo: 1963:106-8). The symptoms covered by this term are; talking

to oneself, obvious hallucinations, aimless activity, refusing food because it is thought to be poisoned, sleeplessness, tearing clothes, inappropriate defaecation, urination and smearing, and sudden attacks on people with loss of memory afterwards. The category were is modified by other words. Thus were alaso (that wears clothes) refers to a patient who is normal most of the time but becomes mad periodically. Were agba is the psychosis of old age. Were d'ile (of the lineage) is hereditary psychosis. Ipa were is madness associated with epilepsy. There are different words for an acute psychotic episode, mental deficiency, and postpartum psychosis. The Western category of neurosis is not so easily used in dealing with primitive categories.

'Yoruba healers often do not make a clear distinction between physical disease and psychoneuroses. This failure is understandable because many Yoruba neuroses present largely physical symptoms.'  
(Prince in Kiev(ed)1964:86-8).

Grand mal epileptic seizures and childhood convulsions are given distinctive names.

Amongst the Tallensi the stereotype of the mad person is that his talk is wild and confused and his behaviour erratic and sometimes violent. (Fortes and Mayer:1969:48). Laubscher makes brief reference to the fact that the Tembu distinguish two forms of madness. The one is characterised by odd behaviour without undue excitement. This is thought to be a sign of the necessity for ritual training to be a doctor and if prevented such a person will lose his senses, and become afflicted by the second major category of madness, characterised by:

'Confusion of mind, stupor and stuporous dementia, and has special reference to epilepsy and catatonic stupors.' (Laubscher:1937:222).

The Nyakusa can also use the same term for running mad and having a fit. (Wilson:1957: 53 fn). Some Bemba use the same word for madness and epilepsy - this latter being called the "madness of a hawk." The Lenje of Zambia, however, do not consider that epilepsy is related in any way to madness. (Haworth:1969:6).

The Ganda believe that there are four main kinds of diseases of the brain. These are madness, epilepsy, foolishness and dizziness. They distinguish two kinds of madness, the wild and the mild. Dizziness is considered to be an illness, not merely a symptom, and is thought of as "the brother of epilepsy" in the same way that sleep relates to death. Just as a cough may be referred to as "the chest", so when one asks what is wrong with a mentally ill person, the reply is "the head", although this term can also be used for headaches. While most people acknowledge that the present classification of madness and foolishness as diseases of the brain is "correct", many of the older people said that these diseases were originally thought to affect a person's heart. Epilepsy and dizziness were probably always thought to be diseases of the head and this is indicated by what is probably a fairly old custom of cupping the head to cure epilepsy. (Orley:1970:4).

The Ganda refer to an illness which strikes only small children causing them to have febrile convulsions as eyabwe. The word itself means "theirs" (the children's) and refers to the fact that it is thought to be brought by "their bird" which is an eagle. This is a reference to the suddenness of the onset of the illness, just as a bird of prey swoops down to catch its prey, and also to the fact that during a

convulsion, the eyes turn up as if to see the bird flying above them. If such a bird is seen, the women with the children may shout up at it that the child is really quite an old one, hoping to deceive the bird into thinking that the child is too old to be attacked. There are various other preventive measures, such as the tying of a small bell onto the left wrist, the noise of which will frighten the bird, or attract the mother's attention if the child moves to fend it off. (ibid: 9-10).

The association of birds with epileptiform fits and convulsions seems to be common to many parts of Africa. Amongst the Bemba the term used for fits in children is the same as that used for a sparrowhawk that is found unconscious on the ground, presumably after missing a swoop. (Brelsford:1950). In another part of northern Zambia the word used for epilepsy means "the madness of a hawk". (Haworth:1969:6). Turner has reported that the treatment for epilepsy amongst the Lunda consists of the beak of a bird which "flies up and down spasmodically like an epileptic, making a whirring sound." (Turner:1963:29). The Wapogoro believe certain birds should not be killed, especially the fish eagle, because it circles and then drops to the earth like an epileptic in an attack (Aall-Jilek:1965:71). (It is of interest to note that the word used for fits by the Mohave of North America means "hawks copulate". (Dovereux:1961:73) ).

There are few ethnographic accounts of primitive concepts of body function. A few investigators give some information as to the ways in which mental disorder is related to the body. On the whole they are associated with the head or the heart, but occasionally abdominal structures are implicated. Thus, as we have already mentioned, the Thonga associate madness with the head. (Junod:1913:430). The Tallensi have a vague notion that all forms of illness are due to disorders in either the head or the belly or both. (Fortes and Mayer: 1969:41). In much of the African literature there is probably a greater emphasis on the association between madness and anger as expressed in the American colloquialism "to get mad" meaning "to get angry". The Nyakusa say that a madman may recover but:

'When he is angry his heart seems to be full of madness again.

They say a mad person talks to himself like someone in a passion of grief, or anger, or fear, and moulds the earth.' (Wilson:1957:80,83).

The Bemba describe the melancholic as one who "has a heart", or is sick in the heart. (Brelsford:1950:47). The Lozi considers that epilepsy is caused by an insect which when it attacks the heart of a patient causes foaming at the mouth and irregular movements of the legs and arms. (Howarth:1969:7). Amongst the Wataita (related to the Kanba of Kenya), illnesses of the heart include anything involving abnormal cravings, fears or urges. Thus a type of kleptonania is said to be a "heart" illness, also:

'....Saka (a type of spirit possession) is said to belong to this category (of the heart) because it is an illness of "wanting and wanting". It does not belong to the other category, illnesses of the head, which are called isu, madness.' (Harris:1957:1050).

The Subanun beliefs about the liver are somewhat akin to Western notions of the heart and Frake recorded no instances of diseases being attributed to the heart there. (Frake:1962).

The Ganda believe that both the heart and brain think, but this probably represents an incursion of western thought, for it is still possible to find older people who say that only the heart thinks. The heart is thought not only to be the centre of emotions such as fear, anger, joy and jealousy, but in the past it was also considered the place where wisdom and memory reside. Whereas older people believe in the primacy of the heart, the young normally say that the brain and the heart work together, the heart deciding things and sending a message to the brain which in turn passes it on to the body. (Orley: 1970:1).

There are two conditions of the heart which in some cases may represent neurotic illness. In the first the heart is referred to as being agitated or fearful and in the second it is said that the heart has fallen or failed. The first condition refers to a pounding of the heart with fright and may be used merely to refer to feeling startled. It is most commonly thought of as an illness in which the person experiences an intense fear causing him to run away and hide in the bush. (Orley:1970;p8). This running away into the bush has been commented on by authors working in Africa as an aspect of the "mad" syndrome. Field describes it in Ghana as a typical feature of most acute psychotic episodes, and Fortes and Mayer note it amongst the Tallensi(Ghana).(Field:1968:32). (Fortes and Mayer:1969:66).

The word used for the heart in these conditions is omene and refers not only to the heart but to the small bone at the base of the sternum (xiphisternum) and to the small bone at the base of the spine (coccyx) and the associated region of the anus. The condition in which it is said to fall or fail may affect either or both of these. It manifests itself in a general weakening of the body and failure to eat. Sir Albert Cook referred to the condition in the usual term taken by many physicians and surgeons to such disorders. He wrote of "the stalwart man with a frame of Hercules, who wastes ten minutes trying to persuade you that his heart has fallen from its right place." (Cook:1954:124).

The importance of the heart in African thought has been emphasised by Muwazo, a doctor working in Kampala.

'Africans of the present day resemble Europeans of previous centuries in regarding the heart as the centre of life; the soul is also thought to reside in or near the heart. Africans consider that the heart is normally motionless, they have no knowledge of the circulation of the blood. The exact position of the heart is not understood clearly, but the whole of the front of the chest and the upper abdomen is regarded as a dangerous area. Palpitations and any sensation which can be interpreted as a movement of the heart are considered to be specially dangerous, for the soul may be moving and may leave the body, and life may thus be in danger. In some patients fever is chiefly noticed and attributed to the cardiac palpitations which accompany slight exertion, especially if anaemia is present; and many Africans are anaemic.' (Muwazo and Trowell:1944:149).

He goes on to state that many of these cases are probably neurotic and others are malingering. These latter:

'naturally complain of the heart, for this would appear the best illness to feign. It would never occur to them to feign a peptic ulcer or blindness, for diseases of the "Heart" or the "Soul" are, in their opinion, the most serious. (idem.)

The final method of classification of illnesses is by "nationality". This usually depends on treatments and comes into prominence when there are competing systems of treatment. Illnesses may be said to have come from neighbouring tribes and this may be due to a similar mental process that attributes illness to witchcraft from a neighbouring homestead. Where Western treatment is offered, it seems that some diseases are seen to be very amenable to this treatment and are perhaps Western diseases. Those illnesses not amenable to Western medicine (particularly psychogenic illnesses) are said to be indigenous. The Zulu recognise disease as being susceptible to Western medical treatments, usually obviously organic illnesses, whilst psychogenic illnesses are distinguished from them.

'These things are "kwaZulu" (of the Zulu Race), and cannot be put right by a White man with an injection.' (Lee:1950;10).

Loudon also discusses a conversion hysteria called ufufunyana:

'The Zulu class ufufunyana as a Bantu disease, a term they use to cover all conditions which they believe to occur only among their own people and to be susceptible only to indigenous Zulu methods of treatment. (Loudon in Opler (ed):1959:361).

These diseases are thought to be due to spirits or witchcraft and are accompanied by stereotyped dreams and are usually associated with rather ill-defined or generalised pain. It may be diagnosed as such by a diviner.

A similar approach seems to be taken by the Eskimos. Murphy describes a shrewd shaman:

'He was one of the shamans who changed, not by giving up shamanism, but by adjusting shamanism to fit new circumstances - recognising, for example, that Western medicine works for white-man's diseases like tuberculosis, while shamanistic medicine works for different illnesses.' (Murphy in Kiev(ed): 1964: 77).

In South Iran it is not the lack of response to Western medicine that is the principle criterion but a positive response to rituals aimed at the Zar spirit. The patient attends a Zar ceremony and if he becomes unconscious during the ceremony, it is an indication that he is afflicted by Zar. Otherwise the patient must go to a Western style doctor.

It is interesting to note that the patient who is diagnosed as being possessed by Zar will not respond to any treatment administered by a Western doctor. Not only do they fail to respond to medical treatment, but in many cases they become worse. If a doctor gives an injection to these patients, they become extremely manic or withdrawn. In the native dialect, they say that Zar and the needle are not compatible, and that the more you give injections the worse the patient becomes. (Modarressi in Prince (ed): 1968: 151-2).

This type of classification into indigenous and non-indigenous illnesses is used by the Ganda. Thus not only do the Ganda ascribe diseases to certain parts of the body, but they also classify them according to three sets of

dichotomies.

1. Those that come by themselves and those that are sent or caused by witchcraft.
2. The strong and the weak.
3. Kiganda and non-Kiganda.

In general the Kiganda illnesses are those that the Ganda believe to have been already afflicting them before the Arabs and Europeans came to their country, but the Ganda imply certain other things when they refer to illnesses as Kiganda. The Ganda think of their illnesses as "strong" illnesses, and they are usually thought to be sent by another although there are many exceptions. There are traditional forms of therapy for them although in these days the traditional art of healing is thought to have been largely lost. Western medicine is not considered to be particularly effective in treating such illnesses. The underlying feeling is that Europeans know how to treat their own diseases with their own medicines. Those illnesses which are untreatable by Western medicine or are difficult to treat, as in the case with much mental illness, are thought therefore to be Kiganda diseases, and are of course strong since traditional forms of therapy are not often very useful either. There are other reasons for strong Kiganda illnesses being thought to be brought by witchcraft. Madness, epilepsy and other strong illnesses bring an enormous amount of trouble to a patient and their family and usually tend to follow a chronic course extending over many years, if not a lifetime. In order to cope with such a stressful situation an explanatory model (paranoid in nature) is formulated by the family, which apart from helping them to talk about the illness, also absolves them from blame and opens up a course of action. No one bothers to use such a model when referring to a cold, but the "strong" diseases are almost always said to be sent. Thus the typical Ganda illness such as madness or epilepsy are said to be sent by another Kiganda and strong. (Orley:1970:p15).

The classification into "come by themselves" and "sent by another", is confused by the notion of contagion. This is a recognised mechanism even though some illnesses are regarded as contagious which we would not so regard in the West, an example of this being epilepsy. It would seem that there always was an indigenous classification of illnesses into contagious and non-contagious, but that this was disrupted by the advent of Western medicine, which includes a very strong notion of contagious diseases. These diseases thought contagious in the two systems presumably varied widely and the result has been to leave a certain amount of disorder in this field. These mechanisms of the genesis of illness are not mutually exclusive and although a "natural cause" is postulated there is still the desire to explain why this natural cause operated at a particular time and in a particular way.

The evidence presented shows that while African medical taxonomy varies considerably in detail, there are some common features, which are well illustrated by the Ganda case. The six most common criteria used for classifying diseases in Africa are:

1. The part affected
2. The kind of symptom
3. Some convenient natural symbol.
4. The cause
5. The cure
6. The national origin

Western medical science uses categories based on different criteria. We have seen, for example, that contagion



is important in Western medicine as a cause. To the Ganda, cause may be differently determined and the notion of contagion will then only be important in determining social treatment. For the ethnographer it is more important clearly to describe his people's taxonomy than to devote futile effort to matching Western scientific and folk analyses.

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